SCIENCE HISTORY INSTITUTE

ISABELLA KARLE and JEROME KARLE

Transcript of an Interview
Conducted by

James J. Bohning and David K. Van Keuren

at

Naval Research Laboratory
Washington, District of Columbia

on

26 February, 15 June and 9 September 1987

(With Subsequent Corrections and Additions)

THE NAVAL RESEARCH LABORATORY ORAL HISTORY PROGRAM

On 26 February, 15 June, and 9 September 1987 I Isabella L. Karle, the interviewee, voluntarily participated in a taped interview with David K. van Keuren representing the Naval Research Laboratory. The interview was made in connection with the NRL Oral History Program.

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[redacted]
Witness

[redacted]
Interviewee*

March 14, 1989
Date

Accepted for the Naval Research Laboratory by [redacted]

14 March 1989
Date

*This and other signatures must be identical with the name of the interviewee as it appears in the first paragraph, page 1.

THE NAVAL RESEARCH LABORATORY ORAL HISTORY PROGRAM

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Upon Isabella Karle's death in 2017, this oral history was designated **Free Access**.

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ISABELLA KARLE

1921	Born in Detroit, Michigan, on 2 December

Education

1941	BS, University of Michigan, chemistry
1942	MS, University of Michigan, physical chemistry
1944	PhD, University of Michigan, physical chemistry

Professional Experience

	University of Chicago
1944	Associate Chemist
	University of Michigan
1944-1946	Instructor
	Naval Research Laboratory
1946-2009	Head, X-Ray Diffraction Section of the Laboratory for the Structure of Matter

Honors

1965	Superior Civilian Service, Navy Department
1968	Annual Achievement Award, Society of Women Engineers
1970	Hillebrand Award, American Chemical Society
1973	Federal Woman's Award
1976	Garvan Medal, American Chemical Society
1980	Dexter Conrad Award, Office of Naval Research
1984	Chemical Pioneer Award, American Institute of Chemists
1986	Lifetime Achievement Award, Women in Science and Engineering (WISE)
1988	Gregori Aminoff Prize, Royal Swedish Academy of Sciences
1988	Rear Admiral William S. Parsons Award of the Navy League

JEROME KARLE

1918 Born in New York, New York, on 18 June

Education

1937 BS, City College of New York, chemistry and biology
1938 MA, Harvard University, biology
1942 MS, University of Michigan, physical chemistry
1943 PhD, University of Michigan, physical chemistry

Professional Experience

New York State Department of Health, Albany, New York
1939-1940 Laboratory Assistant

Manhattan Project, Chicago, Illinois
1943-1944 Research Associate

United States Navy Project, Michigan
1944-1946 Research Associate

Navy Research Laboratory
1946-1958 Head, Electron Diffraction Section

Navy Research Laboratory
1958-1967 Head, Diffraction Branch

Navy Research Laboratory
1967-2009 Chair of Science and Chief Scientist of the Laboratory for the Structure of Matter

Honors

1959 Research Society of America Award in Pure Science
1961 Elected Fellow, American Physical Society
1968 Navy Distinguished Civilian Service Award
1970 Hillebrand Award, Washington Section of American Chemical Society
1972 President of American Crystallographic Association
1976 Navy Robert Dexter Conrad Award

1976	Elected to National Academy of Sciences
1981	President of International Union of Crystallography
1985	Nobel Prize in Chemistry
1986	Sigma Xi Centennial Lecturer
1986	Thomas A. Edison Memorial Lecturer
1986	Distinguished Lecturer in Chemistry, University of Michigan
1986	Karl Herzfeld Memorial Lecturer, Catholic University of America
1986	Albert A. Michelson Award, Museum of Science and Industry, Chicago, Illinois
1986	Honorary Member, International Academy of Science
1986	Golden Plate Award of the Academy of Achievement
1986	Rear Admiral William S. Parsons Award of the Navy League
1986	Townsend Harris Award, Alumni Association of City College of New York
1986	Secretary of the Navy Award for Distinguished Achievement in Science
1986	Paul Harteck Series Lecturer, Rensselaer Polytechnic Institute
1986	President's Award for Distinguished Federal Civilian Service
1986	National Library of Medicine Medal

ABSTRACT

Isabella and Jerome Karle met while both were pursuing doctorates in physical chemistry under Professor Lawrence Brockway at the University of Michigan. After earning their degrees (and marrying), they worked on the Manhattan Project at the University of Chicago's Metallurgical Laboratory. After a brief return to the University of Michigan, the Karle's moved to the United States Naval Research Laboratory, where they focused on the development of X-ray crystallography. They worked together to develop a direct method for determining crystal structures, work for which Jerome Karle, with their colleague Herbert Hauptman, was awarded the Nobel Prize in Chemistry in 1985.

In the first of three interview sessions, the discussion focuses on Isabella and Jerome's family backgrounds, their education in the public schools of Detroit and New York, respectively, their undergraduate careers, and their meeting and graduate work at the University of Michigan under Professor Brockway. The second interview session covers the Karles' work on the Manhattan Project at the Metallurgical Laboratory at the University of Chicago, their subsequent return to the University of Michigan, the growth of the field of electron diffraction, the challenges of pursuing dual research careers, and their move to the Naval Research Laboratory. This second session concludes with a look at their early years at NRL, during which they designed a new apparatus for gas electron diffraction and developed theoretical and experimental approaches to solving the crystal structure problem. The third and final interview session begins with a discussion about the early opposition to the Karles' theoretical work on the crystal structure problem, and the process by which that work gained acceptance through Isabella Karle's X-ray diffraction work beginning in the mid-fifties, through which she was able to solve the structures of spurrite, p,p'-dimethoxybenzophenone, and arginine, among others. Discussion then turns to the nature of the Karles' working partnership in research and in their family life, their children's interests and eventual careers in science, their collaboration with Herb Hauptman, and the effect of the 1985 Nobel Prize in Chemistry on their lives and work. The interview concludes with Jerome Karle's thoughts on the present state of science education and training in the United States, and his concerns for a global future marked by overpopulation, pollution and natural resource depletion.

INTERVIEWERS

James J. Bohning was professor emeritus of chemistry at Wilkes University, where he had been a faculty member from 1959 to 1990. He served there as chemistry department chair from 1970 to 1986 and environmental science department chair from 1987 to 1990. Bohning was chair of the American Chemical Society's Division of the History of Chemistry in 1986; he received the division's Outstanding Paper Award in 1989 and presented more than forty papers at national meetings of the society. Bohning was on the advisory committee of the society's National Historic Chemical Landmarks Program from its inception in 1992 through 2001 and is currently a consultant to the committee. He developed the oral history program of the Chemical Heritage Foundation, and he was CHF's director of oral history from 1990 to 1995. From 1995 to 1998, Bohning was a science writer for the News Service group of the American Chemical

Society. In May 2005, he received the Joseph Priestley Service Award from the Susquehanna Valley Section of the American Chemical Society. Bohning passed away in September 2011.

David van Keuren earned a PhD in history and sociology of science from the University of Pennsylvania in 1982, following a master's degree from the University of Wisconsin at Madison (1975) and a bachelor's from the University of Wisconsin at Eau Claire (1972). His graduate studies were concentrated on scientific thought in Europe and America from the Middle Ages to the present. In 1986, he joined the staff of the Naval Research Laboratory as its historian, documenting the agency's significant research and development achievements past and present, and contributing to national awareness of the broad impact of military scientific research on civil society. He died in a hit-and-run bicycle accident in March 2004, in southwest Washington.

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.

This interview was conducted in collaboration with the Naval Research Laboratory Oral History Program. A prior edition of this transcript, which included edits requested by the interviewees, was kept under seal during the interviewees' lifetimes, in accordance with the terms of the signed release agreement. This new edition of the transcript was prepared following the death of Isabella Karle in 2017; it retains the edits made to the original edition. A full audit-edit of the interview and transcript was performed, so that deviations from the recording could be noted typographically. The recordings, particularly of the first session of the interview, were noticeably poor in quality with some of the interviewer questions nearly inaudible.

Changes in this edition of the transcript include: reformatting to adhere to current Center for Oral History standards, and the preparation of chronologies, an abstract, table of contents and index, none of which were included in the previous, sealed version of the transcript.

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INTERVIEWERS:	**James J. Bohning** **David K. Van Keuren**
LOCATION:	**Naval Research Laboratory** **Washington, District of Columbia**
DATE:	**26 February 1987**

VAN KEUREN: [. . .] This is David Van Keuren from the NRL [Naval Research Laboratory] History Office with James Bohning from the Center for the History of Chemistry, and we're talking today, February 26, 1987, with Drs. Isabella and Jerome Karle of the Laboratory for the Structure of Matter at the Naval Research Laboratory. Dr. [Isabella] Karle, could you begin by telling us something about your own and your family background? [. . .]

ISABELLA KARLE: Both my parents were born in Poland. My father came from a family that had, at one time, been wealthy estate owners but because of the difficult history of Poland, they still owned the land, but [they] were no longer wealthy. My father was the youngest son of my grandfather's second family and it was imperative that he do something other than depend upon the land. So, at age fifteen, he came, all by himself, to the United States to make his fortune. He had had some education. He lived on what was then the border of Germany and Russia, and he had gone to some German schools, to some Russian schools, and then he had a private tutor, which apparently was illegal for a few years. So he had a basic education.

My mother's family were small landowners, farmers. It was a fairly large family. My grandmother died when my mother was about four years old, at which time my grandfather decided to emigrate to the United States, and he arrived in Baltimore [Maryland] with three children. He left the eldest daughter to look after the three little ones in Poland until he could raise enough money to bring the other three. So one year later, at the age of five, my mother was brought by her older sister, who was sixteen. She brought a five year old, a three year old and a one year old. Steerage, I suppose; my mother doesn't remember. All she remembers is that she held on to her sister's skirts for the whole crossing.

By that time my grandfather had remarried, and it didn't prove to be a good situation for the children. The three eldest got married in fairly short order, and each one of them then took the three youngest to live with them and the middle one, who was about eleven, fended for himself. Well, [because of] all of this turmoil, there was just no education in any organized fashion. My mother said she had never gone to school. By this time she was living in Wilmington, Delaware, and apparently there were no compulsory education laws at the time.

<T: 05 min> By the time she was thirteen, she and the sister with whom she had lived, moved to Detroit [Michigan], and there she went to work in a cigar factory. Later she learned to become a seamstress. In all of this, she learned to read and write, do arithmetic, speak Polish and English—[all] on her own.

To get back to my father . . . when World War I—when the United States became involved in World War I, my father joined the Army. By this time he knew some English, but the Army was interested in the fact that he could speak Russian. So they sent him to Archangel [Arkhangelsk Oblast, Russia] where he was an interpreter for the staff there, and he also was the town censor for the newspaper, so that nothing would be written in Russian that was not appropriate, and so forth. Well, eventually, after the war, he came to Detroit, and he and my mother met, and they got married. I was born in Detroit, Michigan.

BOHNING: Excuse me, [what was] your father's name and your mother's name?

ISABELLA KARLE: My mother's name was Elizabeth, and her maiden name was [. . .] Graczyk. My father was Zygmunt. Now the family name [. . .] was Lugowski. But when my father got his citizenship, whoever wrote out the papers didn't like the "w," omitted it, and my father never had it reinstated. I never used the "w" in my name.

We spoke Polish at home. I didn't know any English until I went to school. I was about seven at the time. My father was a painter; my mother was a seamstress. But my father was always a history buff. He read an awful lot of material, mostly European history but some American history. And, again, his English was self-taught, but it was [pretty] good. I made pretty fast progress in school. I started at age seven. On my sixteenth birthday, I graduated from high school.

BOHNING: That would have been in 1930?

ISABELLA KARLE: Let's see. I was born in 1921, in December. I graduated in January of 1938. [. . .]

VAN KEUREN: You went to the public schools in Detroit?

ISABELLA KARLE: I went to the public schools, and it was an interesting experience. My parents being sort of upwardly mobile, as one would say nowadays, had bought a house on the outskirts of Detroit. It was still within the city limits, but it was on the outskirts of Detroit where new homes were being built that were middle-class homes. In every single home was a family whose origins were in Europe, but in different parts of Europe. So our neighbors were

Hungarian and Russian, Italian and Swedish, Belgian and German, and the children spoke that many languages at home, and we all had to learn English in school. And it worked.

VAN KEUREN: What was your high school education like? Did they stress the humanities or the sciences? What do you remember?

ISABELLA KARLE: The high school education was on a three-track system. There was a commercial track, and that was mostly for girls who [learned] typing, shorthand and such. There was—they didn't call it industrial, I don't remember what they called it—but at any rate there was a track in which the students were highly encouraged to study all kinds of trades. Mostly boys, but the girls went in for the cooking and sewing and such. If you remember the dates there, both of these were really very popular even amongst some of the very good students, because they could see that perhaps they would have a trade and try to make a living, otherwise **<T: 10 min>** there weren't any jobs available.

Then there was a third track which was college preparatory, and that was rather rigidly set out: four years of English with [. . .] literature associated with it, mathematics and a foreign language. Only French and Latin were offered at the high school I went to, so I [studied] the French. There were science courses; the only course I took was chemistry. At that time, I really had no guidance, nobody in my extended family—and there was a lot of extended family in Detroit—was even aware of what science meant. My mother had hopes that I would be a teacher. My father had hopes that I would be a lawyer. Although I thought that these were suitable sorts of professions, they just didn't interest me sufficiently. It wasn't until I had a chemistry course that a light went on: "Aha! This is what I want to do." And it was all quite by accident. The first semester, I remember, I had a very good teacher. The second semester was a disaster, but the first semester carried through as far as my interest was concerned.

VAN KEUREN: So you would say that your association with chemistry [was] directly linked to this first course you took in chemistry in high school?

ISABELLA KARLE: Yes, most definitely.

BOHNING: What do you remember about your teacher?

ISABELLA KARLE: It was a woman, and I don't think it mattered [whether] it was a man or a woman. Her name was Mrs. Deming. [. . .] I don't remember her first name. And she was very enthusiastic about the subject. Apparently she transferred her enthusiasm to the students. She was lucid in her presentation of the subject. We had laboratory experiments. The high school I went to in Detroit was the Edwin Denby High School, named after some former Secretary of the

Navy. It was built during the Depression days. It was a new school when I began to go there, and its laboratories were well equipped, even though it was built on a shoestring. I remember, at that time the city of Detroit had no money. It was paying its teachers and other [. . .] employees in scrip—it wasn't real money. The laboratories seemed to be well equipped in all of the standard things, and I was very impressed by the kinds of experiments we were exposed to.

BOHNING: What kind of things were you doing? Were you doing qualitative analysis? Was that standard then?

ISABELLA KARLE: Well, we were doing some distillations. We were doing some qualitative analysis—no, quantitative analysis [really]. We were doing some inorganic syntheses and, of course, brilliant changes of color in solutions, precipitates coming out.

BOHNING: What about mathematics?

ISABELLA KARLE: I liked the mathematics. I remember one teacher, her name was Mrs. Coats. [. . .] I had her for a number of courses, and I thought that it was very practical, but it was algebra, geometry and trigonometry, and she was very good in presenting it. She was an older lady with white hair. The only reason I took one science course was because that was all that was required for college preparatory. It was a little too late by the time I took chemistry to then take the physics and the biology before I went off to college. It was later that I was exposed to those courses.

BOHNING: You progressed very rapidly through both elementary and [your] high school education. How did that occur? In other words, did you have someone who was watching out for you and helping you to move at that pace?

ISABELLA KARLE: Well, I think it occurred because of several reasons. One, this was a new area. The elementary school was crowded, and the high school was crowded, even though they both were very new schools. And the teachers took the two or three better students in each class, and if they were too crowded, they would push them up to the next. But that happened a number of times in elementary school. It is strange what one misses with something of that sort. **<T: 15 min>** I don't think I missed anything in reading, writing, and arithmetic because, again, in each class, we always had the three track system, and one third of the students were allowed to progress pretty much as fast as they wanted to. What I did miss was Canadian geography and South American geography. And I still find that I was well drilled in United States geography, in the geography of Europe, of Asia, but South America is just a little bit fuzzy. In high school, it was just a matter of taking more courses per semester, and as long as they could be fit into the schedule, nobody objected.

BOHNING: What were your outside activities, outside of school?

ISABELLA KARLE: Well, there was a large Polish community in Detroit. We did not live near it, but my parents were quite involved in various social clubs. I had a large extended family—not so much on my father's side. He had some first cousins living in Detroit, and I was friendly with some of their children, but eventually all of my mother's brothers and sisters came to Detroit, and they had children so that I had about twenty-five first cousins nearby and another twenty-five further away.

So that was one activity in that we had a lot of family events and then social life. In those days, when parents went to the dance at the social club, they also took their children along. I mean, that we stayed up until two or three [o'clock] in the morning it didn't make much difference. We slept the next day. So there was dancing and singing and things of that sort. Other than that, [I played, of course,] with the neighborhood children. I was interested in sports. I was on the high school tennis team; I was on the high school basketball team. We didn't have a gymnasium, we practiced outdoors. As far as the basketball was concerned, we were always at a disadvantage when we went to play at a high school [that had] a gymnasium. But nevertheless, it kept me running around. And although I was never very proficient at it, there was a lot of ice-skating in the winter time. Detroit was a nice place to do that. There was an island in the river called Belle Isle with many canals, and at the time I was growing up it was a lovely place to take the bus to and ice skate. They had a casino set up to keep one warm and so forth.

In high school, there was a group of us—maybe you could call us the elite—I don't think anyone of them was particularly interested in science, but they were the students who were the good artists, who liked to perform in plays, write poetry, were interested in history or foreign languages. It was sort of loosely knit but there must have been twenty to thirty of us, and we went together to lectures around the city, to social events at each others' houses, to skate. There wasn't that much place to go swimming in the summertime, but [tennis was available]. There was no problem in taking the public transportation [when you were] going across town at night.

VAN KEUREN: You've indicated that your parents wanted you to either go into education or law, so their expectation was that you would go on to college after [finishing] high school.

ISABELLA KARLE: Oh, yes. Of course, finances were tight, so the expectation was that I would, and actually I did, go to a school in the city. I went to what now is Wayne State University. At that time, I think it was called Wayne University because it was the university of the county of Wayne, and Detroit now occupies the whole county. At that time, it didn't.

[Wayne University] was also interesting in that it was set up in the old building of Old Central High School after the building was condemned because it wasn't safe enough to be a

high school. It was [the sort of university] **<T: 20 min> —**I don't think it quite ran twenty-four hours around the clock, but it started around six in the morning and ended at midnight to accommodate the large numbers of students [. . .] who couldn't possibly go elsewhere. [I think it was free.]

I entered in January—it must have been January of 1938, yes—and in the spring, there were statewide examinations for scholarships to the University of Michigan. And I hadn't heard about them, but one of my high school teachers called me up and said, "You must take this examination." I was, I think, fourth in the state, and they had about eighteen or twenty four-year scholarships to the University of Michigan, so I got one of those, and that took care of my tuition, [but it] did not take care of room and board. However, the girls who were good students—and most of us had scholarships of one kind or another for tuition—had preference for living in university-run cooperative houses. [And those were really very nice. I lived in such a house with] sixteen girls and a house mother. It was an old residential place that was large enough for the number of girls. We did our own housekeeping, and that cut down on room and board expenses very, very much, and it was affordable.

BOHNING: Were you the first of this extended family to go on to college, or were there others of your generation who were doing the same thing?

ISABELLA KARLE: I should say yes and no. One of my cousins, a year older than I, started at Wayne State University the semester before I did. However, because of health problems, she didn't continue. So the answer is that I was the first one to graduate from college, yes. Others after me went on to the university.

BOHNING: Had you decided that you wanted to be a chemistry major at that point?

ISABELLA KARLE: Oh, yes, when I entered. It was interesting when I entered Wayne State University, there [were two kinds of chemistry courses, one of which fit my time schedule much better than the other.] I didn't know that the "E" at the end of [the course label] stood for engineering, so I was the only girl in the class of roughly a hundred. I did well, and the professor there took me an interest in me. Joseph Jasper [was the Professor], and he said [at one time], "Of course, you'll go on to graduate school." That was nothing I had heard of. [. . .] Even after I went off to the University of Michigan the following semester, we kept up a correspondence for a number of years. No, there was no question about the fact that it was going to be chemistry. Now what kind of chemistry sort of developed later.

BOHNING: When you got to Michigan, what chemistry [were you taking] then? You would have arrived there as a sophomore?

ISABELLA KARLE: No. I was a second-semester freshman. I had one semester [at Wayne]. But the chemistry I took at Wayne State was a class that was supposed to cover all of general chemistry in one semester, and so I went on to a sophomore course in qualitative analysis.

VAN KEUREN: Do you remember who taught that course?

ISABELLA KARLE: Yes. Robert Carney. In fact, after that year, he asked if I would be his laboratory assistant for testing, preparing samples for his analytical chemistry courses. I then worked with him, at fifty cents an hour and that was a high rate of pay, through my undergraduate days and even through part of my graduate days. We eventually made a movie. He was interested in photography. He was also concerned with the fact that **<T: 25 min>** when he had laboratory experiments that he performed in front of a large class that only the first five rows could see what he was doing, and the rest couldn't, and so he set himself up with photographic equipment, color film, which had just come out for private use. What we were doing was making a film of all the analytical procedures—the elementary analytical procedures—that he would talk about, and then he [would] not have to demonstrate because [of the] film. Eventually we had many hours of film for a whole year's worth of analytical procedures, and copies of that film were made. I know that they were used in other universities [and] other colleges in Michigan. I don't know how much further they [went]. But it was written up in the *Journal of Chemical Education*.[1]

VAN KEUREN: What were your peers like in your chemistry classes? Were there many other women who were taking chemistry courses with you? You said that at Wayne State you were the only woman in the class.

ISABELLA KARLE: Yes, but that was by accident. No, there were not very many, and of course there were fewer as time went on. There were special courses for the nursing school, so that I did not see those girls. A good many of the students were pre-medical students, and there were not very many [women at all] involved. The University of Michigan at that time combined the regular chemistry courses with the engineering school, so that the engineering and the [liberal arts] students took chemistry together until the senior year. So they were very highly populated by boys.

BOHNING: What attitude did the faculty have towards you as a woman and a chemistry major?

[1] C.C. Meloche and R.J. Carney. "Illustration and demonstration in elementary volumetric analysis." *Journal of Chemical Education* 23, no. 4 (1946): 198.

ISABELLA KARLE: Well, the reactions, I think, ranged from one end of the spectrum to the other. Some seemed to go out of their way to make things difficult for me, others went out of their way to help me along. I lost out on one of the nice undergraduate prizes for top student in chemistry, because the committee decided [that this had] been set up for boys, and "she isn't going to continue in this anyway, so why give her the prize." However, after I got to graduate school I did not have much support for my first year because, of course, I couldn't be a teaching fellow. But the second year, a number of the faculty people were very instrumental in getting a fellowship for me called the Rackham Graduate Fellowship, and that was a very nice one. It had a stipend of [about] a thousand dollars. [. . .] It was a lot more than [a teaching fellowship]. And I didn't have to pay any fees otherwise. So I had both kinds of experiences.

BOHNING: Did you do any research as an undergraduate?

ISABELLA KARLE: No, that was not the kind of thing one did then. I didn't start research until graduate school. By the time our daughters went to school at the University of Michigan, they were doing research as undergraduates.

[END OF AUDIO, FILE 1.1]

BOHNING: What were [the] courses you remember taking as a chemistry student? Was it a broad range of chemistry courses?

ISABELLA KARLE: Yes. The University of Michigan had a special degree. I think that was because the chemistry department at one time was a separate school. And when they joined the Literature, Science and Arts School it kept its special requirements for many more courses in chemistry than an ordinary major in chemistry would require. People who took an ordinary major in chemistry [received] a bachelor of science [degree]. Those who took the special courses received a bachelor of science [degree] in chemistry. In addition to chemistry, there were also a good many physics courses that were required, and I liked mathematics, so I went beyond calculus, because I liked it. I took some biology. In the more cultural courses, I think I only took the absolute minimum required courses. [. . .]

VAN KEUREN: Are there any courses or professors that stick out in your memory as really being important to you as an undergraduate?

ISABELLA KARLE: Yes. I think one of the reasons I took a lot of mathematics courses was because I liked [the] people who were teaching the courses. There was a Professor [Earl]

Rainville, although that was more in graduate school than in undergraduate. I remember the face of [Professor Elder], a man I had for a number of undergraduate courses, [. . .] Again, I felt as if I were learning things properly. I took a number of undergraduate courses from him. [. . .]

In undergraduate physics really no one in particular stands out in my mind. In undergraduate chemistry, there was Professor [Werner] Bachmann, who was not only a very talented organic chemist, but he was an excellent teacher. He also liked teaching well enough that he would come into the organic laboratories of courses he was not teaching, just to look over students' shoulders and point out various things that they could improve in their experimentation—all in a very nice way, of course. Not critical at all. Unfortunately, he died at too young an age. Other teachers were good lecturers, presented the subject well, but were not very personable.

BOHNING: Was there any particular area of chemistry that you thought you would concentrate on as you decided to [go on to] graduate school?

ISABELLA KARLE: It was not analytical chemistry. When I took organic chemistry, I liked that. And then when I took physical chemistry, I think I liked that even better. When I went to graduate school, I stayed on at the University of Michigan for all of my education. [The first year of graduate work was mostly spent in taking course work that resulted in a master's degree. There was] a lot of course work which prepared us for all of the examinations which were required for pursuing a PhD. And there was not much research even then. [. . .] Lawrence Brockway had come to the University of Michigan about two years previously. I had him for the physical chemistry course, and he asked if I would like to be his graduate student and I accepted.

BOHNING: That would be graduate-level physical chemistry?

ISABELLA KARLE: No, as a senior. Well, for those people who were getting a bachelor of science in chemistry, the special degree, one took it as a senior. But a good many of the students **<T: 05 min>** who came from other schools as graduate students were required to take that course also, so that there were quite a number of graduate students in the senior physical chemistry course.

BOHNING: And that was where you met your husband, in that course.

ISABELLA KARLE: Yes. I was a senior, and he was a graduate student.

BOHNING: Is it true [that the first time you met] you were opposite each other in the first laboratory?

ISABELLA KARLE: Well, we worked next to each other. We were in alphabetical order.

JEROME KARLE: At adjoining [benches], actually. I sort of vaguely remember that they were up against the wall. [. . .] I had gotten in there before the first period and had already set the apparatus up [for the first experiment]. She came in, took a look at the apparatus all ready to go, and had some negative impressions.

ISABELLA KARLE: He was a true competitor, [and] I never had any true competitors before.

BOHNING: How did you react to [that experience]?

JEROME KARLE: Well, she showed similar signs throughout the year. Actually, it was an interesting experience for both of us. I, as will come out when we chat, was very familiar with young people who were extremely bright and extremely talented and far ahead of the standard timing in schools. But I had always gone to schools in which there were only boys. It was a bit of a shock to me to see such a young girl be so far along in complicated subjects and be so bright. I think I'm correct in saying [this], because I've heard Isabella say it several times, that I was the first real competition that she ever had in school. So I think that there was a little bit of [shock] on both sides.

VAN KEUREN: When you were an undergraduate [student, among] your peers in the chemistry [classes], were many of them preparing to go to graduate school? [. . .]

ISABELLA KARLE: As I remember, by the time we were seniors, the number of undergraduates—the number of students who were now specializing in chemistry for the special degree—had become quite small, perhaps fifteen or twenty. A few of them went on to medical school; I would say perhaps half of them were intending to go on into industry; and the remainder [went] to graduate school. So it wasn't really a very large number after that. Some stayed at Michigan. Most went—should I say most? I'd say that more than half of them stayed at Michigan for graduate school, of those who were going on to graduate school. Of course, we graduated in 1941, and that was [during] World War II. A number of them who were intending to go to graduate school, however, had also been in the Officers' Reserve Corps, so they were obligated to go off to the Army. It wasn't until they came back after the war that they went back to graduate school. They would have [gone] immediately if they could have.

VAN KEUREN: Did the faculty encourage students to [apply for] graduate study?

ISABELLA KARLE: The faculty did—the boys, yes.

VAN KEUREN: Not the girls?

ISABELLA KARLE: Well, by that time there were two of us. No, I'm sorry—three. There was Helen Barnett, and she went [on] to industry. She was a very attractive young lady, **<T: 10 min>** and she went to one of the pharmaceutical houses. [. . .] Do you remember which one?

JEROME KARLE: [Bristol-Myers.]

ISABELLA KARLE: Another one was Charlys Lucht [. . .] who spent one year in graduate school [. . .] and then went on to General Electric. Then she married a crystallographer, John Kasper. I think he retired recently. [. . .]

VAN KEUREN: So [it wasn't that there were no potential] women, but that they were actively discouraged from going on.

ISABELLA KARLE: I don't know if they ever really were encouraged to go on. I think that they had made up their minds some time before [when] they were going to leave [. . .] and go on with their lives.

BOHNING: I had asked you about the attitude of the faculty. What about the other chemistry majors—the male chemistry majors. How did they react to having three of you at that point?

ISABELLA KARLE: Well, they reacted differently to different ones of us. Helen was very attractive.

JEROME KARLE: When they looked at Helen they thought of a different kind of chemistry. [laughter]

BOHNING: But certainly intellectually you were at the top of your class. I wondered whether that . . . and you were much younger than [them] too, so that combination.

ISABELLA KARLE: Well, there was John Dice, and he spent most of his time with Parke-Davis and Company. He retired recently. We always got along very well. We played tennis together a lot during our chemistry days together. He eventually got his PhD from the University of Michigan, also in organic chemistry. There was a young man by the name of Fields. [We got along well], but something happened during his graduate days, [. . .] and he never finished, or at least not at that time. It wasn't a matter of the Selective Service Board because they were pretty good in allowing people to finish up their degree work, unless they were in the Reserve Officers' Corps. I don't know what happened, to him.

No, I had some good friends amongst them. I didn't feel any [great feelings of] antagonism by that time. I guess after all those years they had to get used to me.

BOHNING: Did you have any role model as you were [. . .] developing your science career?

ISABELLA KARLE: No. The closest one could come to it is just about the time that I was perhaps a senior in college, Yves Curie wrote the biography of Irene—no, not Irene, her mother, Marie—and I thought that was great. But that was about the only model, if you could call it that. I did enjoy my high school chemistry teacher, but I didn't want to become a teacher. It wasn't that.

VAN KEUREN: You had an AAUW [American Association of University Women] Fellowship and the Rackham Fellowship [for a while]. How did those fellowships come about?

ISABELLA KARLE: I must have had the AAUW Fellowship the second year I was a graduate student. I just applied for it. I remember there was an interview, and I got it. Then they were willing to renew it the year after that, which was nice of them. Except by that time the Rackham Fellowship [was awarded to me].

VAN KEUREN: Were there any specific professors who were really pushing you for the Rackham [Fellowship]?

ISABELLA KARLE: I don't [really] know who [. . .] was on the committee, but there was a Professor Case who taught thermodynamics. I really hadn't had him for any courses ever. **<T: 15 min>** But he was instrumental. He was the one who told me that it was being awarded to me. I am sure that he had to be very much in favor of it. He was the head of the committee and could

just have well picked somebody else in chemistry to have receive that Fellowship. I don't know if Professor Brockway had any input in this or not. [. . .]

VAN KEUREN: Had you noticed a changing general attitude of the faculty towards you as you progressed in graduate school?

ISABELLA KARLE: Oh, yes. After we got our degrees, and after we went to Chicago and then came back to Michigan, they actually appointed me as an instructor in the chemistry department, and this was the same group of faculty members, for the most part.

BOHNING: Maybe it's time to turn to [. . .] Jerome, to see if we can also get him to the University.

JEROME KARLE: Well, my father's origins were in Austria-occupied Poland. His family lived in a small town that was perhaps fifteen or twenty kilometers from Krakow. The name of the town—its present name [. . .]—is Trzebinia, but he knew it and called it Trzebin, so the "ia" in those days was not included in the name.

His father was an artistic man and throughout his life, in the main, he not only did straightforward house painting, but he [obtained] commissions to sculpt and decorate ceilings and so forth, presumably in the wealthier houses, ultimately in New York City. I understand that he left his native town to study in the capitals of Europe when he was a young man. At the time that there were three children in the family, he left Poland to come to the United States, and I thought that it was rather strange that he wound up in New Orleans [Louisiana], but it isn't at all because in those days New Orleans was a very popular port for new immigrants to use as a port of entry for the United States. But I'm sure that its art colony down there attracted him, and he stayed down there. I don't know what happened exactly, but it was, I think, taking him a little bit too long to get the money together to bring his family over, and so somehow or other my grandmother took the initiative. [. . .] I think [my father's] older brother was about two years older, and his sister was perhaps five years older. [My grandmother] managed to scrape together what it took to bring them to the United States in steerage. She landed in New York. My father remembered all of his life, the awful crossing of the Channel. Apparently everybody was terribly sick. It was very rough weather. He told me that they finally left England at Liverpool and came to the States. When **<T: 20 min>** my grandmother got to New York, she [made contact] with her husband in New Orleans, and he came home. They had numerous more children—I don't know how many, [but I think] there were six who survived to adulthood.

It was indeed an artistic family. Two aunts made their living, later in life, by being part of the commercial art scene in New York City. The oldest sister had an excellent voice and was going to—I guess, at least had the objective of ultimately being in the opera. However, she married a gentleman by the name of Ivan Olinsky and for whatever reasons, that put an end to

her artistic career. But he was an artist. [He] taught for many years, for example, at the Art Students' League in New York City and was very well known.

My father's brother, Dave, the one who was two years older than he, studied for many years as an artist in Europe—perhaps ten—and his profession was to do art, teach art, and do a lot of portrait painting. He made his living that way. He was offered a position [to teach] in the art department at City College in New York, and he told me that he got as far as the subway, but doing anything on a regular schedule was absolutely impossible for him, and he turned around and went home. I knew the head of the art department very well when I [was attending] City College, and he would tell me from time to time that he tried very hard to get my uncle to come to teach but was unsuccessful. He was a very nice gentleman. His name was Mr. [Eggers]. [. . .]

My mother's family came from the southern part of Russia, known as Georgia. She never was sure whether she was born there, or on the ship coming over, or in Manhattan. But in her sixties she made an inquiry and found that she did have a birth certificate in Manhattan, but it didn't indicate specifically where she was born. Her mother didn't survive very long. She died in childbirth with the next child, and my grandfather had four young children. That was too much for him, so he kept two of them, and two of them were adopted out by either close relatives or close relatives of relatives. It so happened that my mother was adopted by relatives through marriage who had come from Hungary. Amongst other things, my mother grew up being [. . .] fluent in the Hungarian language. I had the pleasure of knowing and meeting many Hungarians as I was growing up, and I found this to be a special experience which I actually appreciated from a young age. There are many of them who just have their own views of life, as a kind of a joy of life—a joie de vivre— kind of a spirit about them **<T: 25 min>** that's joyous, fun-loving, free spirited and crazy. That environment made things more enjoyable around the house.

My mother had a good education for those days. She had a very fine talent for the piano; she played piano and organ. She gave lessons when she was still fairly young herself, maybe sixteen [or] seventeen years old. She didn't try to make it a public profession, but she belonged to various sororities and groups of that sort, and she would be the one who would play the piano or the organ at various meetings and festivities and so forth. I learned to play the piano. [My mother] had perfect pitch, and if I ever hit a wrong note, [I remember her] calling out from the other end of the house. Not only bringing it to my attention, which I knew darn well, but telling me exactly which one I should have struck. She had great ambitions for me to become a concert pianist, and much to my chagrin she would enter me into yearly contests [which] were called "Music Week Contests" in New York City that would promote music. They had their good points, but I always had a disdain for those kinds of competitions and disliked [. . .] competitive public appearances. I set my mind against that as a grown-up activity. I [received] some medals, but that didn't change my mind. [laughter]

[END OF AUDIO, FILE 1.2]

What put an end to intensive practicing was my entering City College, which was extremely demanding of my time. It was associated with an three-hour-a-day subway ride. It was just impossible. However, at times in my life later on I got back to [the piano]. When I was employed at the New York State Health Department, I would go in the evenings [to play]. They had a piano there, and I would do various things, play and a do little composing and so forth, just for my private entertainment.

My mother went through high school. They had some fairly high standards in the New York school system, and for those days she was [fairly] well educated. She did not go on to college, however. [. . .]

My father disdained formal education. He went to elementary school, and he told me the records were poor. When he decided that he should be promoted, he just went to the next higher class. I believe he probably got to the sixth grade. However, his facilities went far beyond the formal education. He was certainly well versed in reading, writing, and arithmetic, which he could do all very well. He had an excellent speaking voice and a very good command of the English language. Many times he would get on the phone when some young lady [would] be calling me, and she would tell me, "Oh, what a beautiful voice he has," and then I would tell him, and he would [sound] so pleased about that.

He and another sister of his were not at all oriented towards learning. I wouldn't say that he disdained it in the sense that it wasn't for anybody; it just wasn't for him. He was certainly very encouraging to me, and always did what he could to facilitate my progress in school. The atmosphere was certainly very good for me. The atmosphere that my mother created was [also fine. She had her ambitions for me though]. When it became clear that I was not going to become a concert pianist, the next obvious profession was to become a physician, and I did entertain that thought while I was going through college.

So far as elementary school was concerned, I had the very great advantage of going through a school system that had very high standards and was very concerned about promoting people at a rate consistent with their development. They had what is now known as a track system. There were a lot of youngsters in the elementary school that I went to. For some classes there would be perhaps at the same level, say the first semester of the fourth grade, there could be four or five different classes, and each one was stepped [at a different level indicated by numbers]. "A" represented the fall semester, so that would be 4A. 4Al was the numero uno class and 4A2, well not quite. And you would get down to the 4A4 and the 4A5. Those were great classes, but the people who were teaching them, mostly ladies, were fantastic disciplinarians. There was just absolutely no stepping out of line. **<T: 05 min>** They had those youngsters, no matter how unruly they would be otherwise, they had them scared to death. And there was order in the classroom, very much so. I appreciated it then, but I came to appreciate it very much more in retrospect. As a consequence of this business of having various tracks, and also pushing people ahead as their capabilities developed, I was in the eighth grade when I was ten years old. I graduated a little before my eleventh birthday.

BOHNING: I am sorry, but if I may interrupt, where were you living in New York and what school did you go to?

JEROME KARLE: That's fine. I grew up in a very interesting area. It was Coney Island. [My father owned various kinds of properties and rented them out. One] property had stores on the ground level, and [. . .] there was a rooming house above, which he subletted. Somebody ran it and paid him the rent. Then he had an apartment house, in which there were the usual tenants, dental offices and so forth, a drug store downstairs, that sort of thing. And he had [another property] in which there were summer bungalows which would be rented [for the summer. They] were about a block from the ocean.

Depending upon the variations in the economy, he also did other things. He had a Chrysler-Plymouth agency when they first came out with Plymouths. I know when they first came out with Plymouths. That was in 1928. And depending upon how our economic fortunes went, we either lived in Coney Island or on a fancy periphery which was called Ocean Parkway. Actually, that part of Ocean Parkway is not too far from the Brighton Beach area which has now become a great colony for Russian émigrés. So that's where I lived. It was a great place for a youngster to grow up. In the summertime, there was the ocean, and I had friends who owned rides or worked on rides and so forth. I had free access to [tremendous] amusement parks [such as] Steeplechase and Luna Park [and various] roller coasters. And then there was a huge parking lot that served the area, which was frozen over by the fire department in the wintertime, so we had our ice skating also. I used the swimming pools as well as the ocean. It was just one huge playground for me in the summertime. That was great.

I went to—except for one year I went to Public School 100 during that year, [when I first] started [school, that was one of the times that] I was living on Ocean Parkway. When we moved back to Coney Island, I went to Public School 80 which was just about a block and a half from where I lived. Actually, I was born in an apartment house which is across the street from the Steeplechase Amusement Park. I got used to all sorts of things. You could hear [. . .] the people screaming on the rides across the street. There was a very noisy trolley car that went up the avenue, which was called Surf Avenue. And I remember each night going to sleep when I was about five or six years old, with a striker right outside my window. This was not part of the [amusement] park. You know, [a striker is an attraction where] you hit a flat, rubberized lever **<T: 10 min>** with [a] huge wooden mallet and then [a] rubber cylinder goes up a wire. [If you hit it hard enough,] you can send it far up the wire to ring a bell and [perhaps win] some prizes. Well, that bell used to clank outside my bedroom as I was sleeping. Nothing really disturbed me. I just [tuned] out all of those noises, and everything was fine.

I went to [. . .] a high school that was located on Ocean Parkway, not far from where we lived at that time, called Abraham Lincoln High School. It was newly built at the time. Actually, I did my first year in an annex which was nearby to James Madison, but since that became Abraham Lincoln's annex as soon as Abraham Lincoln opened the next year, my connections [were essentially wholly] with Abraham Lincoln. The students were very good students. There were any number of them who were extremely bright, [as] the record shows. We have three

Nobel Prize winners from the high school and actually, part of a fourth one. The [man] who received the medicine prize last year spent his first year at Lincoln, and then he went off to James Madison. [laughter] There are certainly other measures of success [besides receiving] a Nobel Prize, [and such evidences have emerged on countless occasions among graduates of the] great New York City school system of the thirties.

BOHNING: Who were the other people [who] won the Nobel?

JEROME KARLE: There was a classmate of mine, both [from] Abraham Lincoln and also City College. His name is Arthur Kornberg and he [received] the Medicine Prize, I believe in the late fifties, maybe '59. I'm not exactly sure of the date. And then there was Paul Berg who graduated probably about ten years later. Interestingly, Paul Berg was a student of Arthur Kornberg, and they are both [on] the faculty of Stanford University.

So they were interesting times. As far as becoming interested in science is concerned, I really have no true [recollection of when I recognized my interest]. I know that when I was about nine years old, and I was sent to the library to read and make book reports on books that were written for children in the sixth, seventh and eighth grades, that instead I would take out books from the library written by, [for example], a distinguished astronomer, Sir James Jeans, who popularized science. I don't [remember] if he was doing it for nine year olds, but nevertheless, I really just couldn't get enough of that. This was already a reflection of my interest. Of course, I didn't know specifically what I wanted to do. All of science fascinated me. The *New York Daily News* had a Science Museum [within New York City whose exhibits could be activated by pressing buttons], and I used to love to go [there to] do the little physics experiments, the electrical experiments [and] watching what would happen. To me, it was obvious that there was nothing else [for me] to do in this world, and I knew that when I would grow up, somehow or other I would manage to do <**T: 15 min**> something science-related.

When I [was in] high school, I don't believe I took biology. I'm almost certain that I didn't. But I [did take] chemistry and physics, and there was a gentleman there who taught those courses who very obviously liked me and encouraged me. I was teacher's pet for two years. He taught both the chemistry and the physics courses. Very unfortunately, he had an automobile accident the summer that I graduated, and didn't survive it. It was really a great pity. [. . .] He was a fine [gentleman and a very fine] teacher.

BOHNING: His name?

JEROME KARLE: His name was Goldman. I don't remember his first name. A Mr. Goldman.

BOHNING: What did you do in chemistry in high school? What was the course like?

JEROME KARLE: Well, I felt that it was a good challenge. I don't recall it in [very] great detail, but we certainly learned a variety of reactions [and some elementary calculations]. We had laboratory work in which we would carry out some of the more simple experiments. So far as the details [are concerned], I'm afraid that they are mostly lost in my deeper memory; they're certainly are not in my quick recall. I felt that I got a good insight into balancing equations and carrying out a number of the simpler calculations that are done to find out how much reacts with how much, and how much product you get, and so forth, and understanding the concepts of molarity, pH. I felt that it challenged me. I was fascinated by physics. I'm always fascinated by physics. [Much of the work that I have been engaged in has close connection to that field of science.]

VAN KEUREN: Were there any [professors] or teachers from high school that stick in your mind?

JEROME KARLE: I don't remember the lady's name, but I took an advanced algebra course from her, and I thought that she was one of the greatest teachers ever. She knew her subject absolutely [thoroughly], would lecture and work things out on the board, and after she was finished explaining the material, I really understood it. So I remember her.

I remember a Mr. Green who was a Latin teacher of mine, who was a very nice gentleman, and I enjoyed his course. My very first Latin course was given by an Englishman whose name was Bowker. Was that it? No, I'm confusing him with a math teacher [in college]. I can't think of his name right now. [I remember his name. Mr. Barrow.] He had the most pronounced British accent that I had heard up until that time. He was a very good teacher also. It was very easy to enjoy his classes. I think that is mostly it. I worked in the dean's office, and I remember him very well.

VAN KEUREN: Did you have a peer group at that time? [. . .]

JEROME KARLE: Yes. My [. . .] close friends at that time **<T: 20 min>** did not accompany me through Abraham Lincoln High School. There was a kind of an age displacement that was sort of difficult. I had a lot of friends, but they were not directly from my classroom. Later on I became friends with—this was college days—I became friends with two other people who were not college chums. One of them also did go to City College, but he was a year behind me in school, as I recall. They were close to my age and very outstanding students. For the most part, there was no peer group at all from high school. My association with the people in high school [was] athletic. They had marvelous handball courts there and a swimming pool and so forth, and I took advantage of that. I knew people there but nothing particularly close.

VAN KEUREN: [. . .] Was there any other person who really served as a role model?

JEROME KARLE: I don't really think so. At times I also would read about various people. I would read biographies. Ehrlich, Paul Ehrlich's biography appealed to me. [Hermann von] Helmholtz's biography appealed to me. [. . .] Marie Curie's biography appealed to me. Pasteur, for example. So [. . .] I found inspiration in scientists of accomplishment whom I read about. [. . .] They didn't exactly play a role model, because I did something rather different, but I found [accomplishment] inspirational, [however].

I graduated from high school just about the time I became fifteen. I could have graduated earlier. I was interested in everything, and I sopped up all the courses that I could, all kinds of things, and I had enough credits to graduate in February, but I would have been only fourteen-and-a-half years old at the time. I mention in this little autobiography [that I wrote in connection with my Nobel lecture] that to teach us the social graces the girls in the school came to the boys' gym in the last year of high school, and we [learned] how to dance with each other. [. . .] I hadn't begun to grow yet. [I was fourteen, and about five feet tall]. [. . .] Girls mature earlier, [. . .] they were mostly seventeen or eighteen years old. They just couldn't believe [that I was a senior. The girls got use to my presence and after a while even danced with me.]

BOHNING: Did you think about any other places other than CCNY?

JEROME KARLE: I couldn't have, it was an economic matter, and [CCNY] was free. The only requirement they had was that you had to pay a dollar per year for a library card, and that was it. In fact, the first two years they supplied textbooks, **<T: 25 min>** and all you needed was subway fare, which was cheap in those days. It cost me ten cents to go from Coney Island up to 137th Street in Manhattan, and the reason it wasn't a nickel is because I had to take two different subway lines. Now they are all integrated. There was the BMT [Brooklyn-Manhattan Transit], and then I had to switch to the IRT [Interborough Rapid Transit]. That cost me an extra nickel.

I found City College quite difficult the first year. There must have been about fifteen other youngsters who were also just fifteen years old—twelve to fifteen of them. A number of them had gone to an especially advanced school that was [associated with] City College. It was Townsend Harris High School. [. . .] They were already teaching [the students] as if they were in college, and they had a standard three-year curriculum. It just wasn't four years as for the senior high school. Those young people and their older peers were real competition. At the time, I think that they probably did not accept, or didn't have the room to accept, anyone who had much less than a 90 average in high school. There were still about five thousand applicants, and they could take fifteen hundred in the freshman class. So the sorting out from the excellent [educational] system in New York City [created a student body of fine] brains and virtuosity.

[. . .] I just [did moderately] well the first year, but I adjusted to [the work], and I learned [how to do it well]. The next three years were much better for me. I was [pleased] that I [received] the [first] award in the natural sciences when I graduated—considering the competition!

BOHNING: Did you have to select a major when you entered, or were you allowed some flexibility?

JEROME KARLE: They had requirements, a lot of them. City College was going to turn out a lady or a gentleman. To give an idea of [the nature of their requirements], there were two years of public speaking [as a requirement for everybody]. This was New York City with all the foreign accents and the New York accents. They couldn't completely [remove them], but they did a marvelous job [anyway]. For first semester phonetics you had to learn how to pronounce, etc. It finally wound up by the fourth semester [with] soliloquies. [. . .]

There were a lot of other things. Everybody, whether they were going to become a scientist or not, had to take a science survey course, which was not easy. [. . .] They had a gentleman who was a marvelous lecturer, [and the auditorium] would be filled by returnees who would just come to hear his lectures. He was excellent in conveying the ideas, and at the same time he would make [the students] roar with laughter. He was a Professor Corcoran. Just marvelous.

[We] had to take [a language test]. It was a difficult foreign language exam—just one. Everybody had to do that. Two years of English, and it covered composition, literature, poetry, and so forth. Government and history [courses were required for all]. Everybody had to have two years of math, and one year of that would be calculus, no matter what you wanted to do. They poured it on, and if you couldn't take it, that was it. [laughter]

[END OF AUDIO, FILE 1.3]

VAN KEUREN: Was there a high dropout rate?

JEROME KARLE: Yes. But I was never able to tell how much of it was economic. Even though it was free, so many of [the students] had to add to the family resources.

BOHNING: Well, the Depression effect was still there too at the time.

JEROME KARLE: Yes. It was at the height of the Depression. Or the depths, whatever. There were, I remember, about seven hundred and fifty who were graduated. [I believe that that

the high dropout may have been] largely economic. [. . .] They lost half their original students by graduation time.

I was interested in everything. I took extra math and extra physics. Predominantly, however, I took chemistry and biology, because I was thinking about the medical profession. [So my major subjects were] chemistry and biology. There were about a hundred and fifty people who hoped to get into medical school. About four or five did. I was not one of them. I tried. I continued that pursuit by [taking up graduate biology study] at Harvard. This I had to do at our own expense. I lived on a shoestring. The funds mostly came from the fact that my sister was working at that time. But they were not interested in having me at Harvard Medical School and they weren't interested in encouraging me to go on in the biology department, even though my course work was satisfactory. I received in two successive years from the Harvard Medical School, a very nice letter that said that I was not accepted, but that I shouldn't take this to mean that I did not qualify. So, it goes.

VAN KEUREN: Do you know any more of the background to this?

JEROME KARLE: Of course. Let me not say anything more.

BOHNING: Could I come back to CCNY for a moment, because CCNY was famous for producing chemistry majors. It had a very strong chemistry department and was turning out forty or fifty majors a year at that time. Was your degree in chemistry, or was it in biology?

JEROME KARLE: They didn't bother with that. They weren't so keen on that. They didn't give anybody degrees in biology or chemistry, so I [received] a bachelor of science degree, and that bachelor of science degree could mean all kinds of things distributed amongst individuals. They were more concerned with turning out a well-rounded individual, and to the extent that it was possible, I think they did a really excellent job. Again, I took so many courses that I could have graduated a half a year earlier. But they didn't make me do so, so I stayed on until the end of the year.

VAN KEUREN: The 1930s were really an intellectually brilliant time in New York City. Was this obvious to you at CCNY?

JEROME KARLE: I felt that I was around youngsters who [. . .] were just unbelievably good. They were virtuosos. It was obvious that these were very, very sharp kids. It was obvious in their performance, in the way they performed on exams, in their responsiveness, in their classes, and in their interests. They were motivated. They really wanted to absorb everything that there was to be absorbed. They were very interesting youngsters. They were difficult though, because

<T: 05 min> if you wanted to succeed along with them, the pressure was really on. The school set high standards, the youngsters set higher standards, and it was [. . .] hard work. And if one adds that to all the laboratory courses, there was just no time [for anything else].

BOHNING: We were talking about CCNY. Two or three weeks ago I was up at Cornell [University] talking to Harold Scheraga.[2] He was also from CCNY. He actually entered CCNY the year you graduated. [. . .] He described some of the political activism that existed at CCNY at that time. Did you encounter any of that?

JEROME KARLE: Yes. There was quite a bit of it. I was an observer. These were strongly motivated, idealistic young people who were terribly upset about the Depression. They were easily influenced by leftist philosophies that theoretically could turn things around and make this world a much better place to live in. There were, therefore, certainly a significant number of youngsters who considered themselves aligned with socialistic and communistic philosophies of the time. What they couldn't possibly know, if nothing else, [was] that the reality of these philosophies is very different than the principles of the philosophies. I felt that at least most of them were very decent and well motivated. But I had my suspicions, and so I was never attracted to it and didn't participate in it. But I witnessed it.

There was the Spanish Civil War, for example. And there were a large number of young people, I believe, who had left the college to go over to Spain to fight. I think it was called the Lincoln Brigade, or something of that sort. It wasn't a great part of the scene, but there [were] times [that] I remember [when] there were confrontations [with the authorities over some political issue that the students felt strongly about]. I don't really remember the issues. I remember one issue, I don't remember the rest. [There were] some campus speeches. That's as far as they ever went with demonstrating. The thought of wrecking furniture, or sitting in an office they weren't supposed to be in, never occurred to them. The worst that ever got to be was that they would collect around the flagpole and would make speeches [in the attempt to] inspire each other. The one issue that I remember is perhaps the second year I was there, there were students coming from Fascist Italy, and they were to appear in the auditorium [before] a gathering of the students. There was a confrontation between a [. . .] number of students who objected and the president of the university. That was the only issue that I can recall.

BOHNING: Were there any professors that you had there that had any influence?

JEROME KARLE: After I left the place, I had heard that **<T: 10 min>** one or two of them ran afoul of [Joseph] McCarthy and his people. There may have been [faculty who] identified themselves as leftists, but I don't recall in the course of my going there any political influence

[2] Harold A. Scheraga, interview by James J. Bohning at Cornell University, Ithaca, New York, 10 February 1987 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0064).

whatsoever on me—or [even] an attempt on me or the students. When I went to class, [everyone] stuck to business.

VAN KEUREN: Were there any professors there that had any major intellectual influences on you? [. . .]

JEROME KARLE: I can't say that I remember them particularly in that light. There was a gentleman who was in charge of the embryology department, whose name I don't remember. It may have been Parker. Maybe. I didn't have him for my class. I had another gentleman who was there. I liked them. I enjoyed that course and in fact, to the extent that I did any special work in anything when I went to Harvard, it was in embryology.

I was impressed by the people in the physics and the math department. There was a gentleman who was a very famous philosopher, Morris Rafael Cohen. He impressed everybody. Somehow he had a head and a face that looked like the epitome of intellectuality. It sort of started narrow and got bigger and bigger into the brain cavity. He would just come across as somebody who [understood] everything. He was [very] knowledgeable. He lectured in the logic course, and his logic was unimpeachable. He was an excellent mathematician. This was his specialty at one time. He just seemed to be the sage of the ages. He was a very impressive individual.

BOHNING: Did you do any research as an undergraduate?

JEROME KARLE: No.

VAN KEUREN: What is the background to your decision [to apply to medical school]?

JEROME KARLE: I think my mother probably had some influence in [that]. I also was, I must say, impressed with the physicians whom I knew personally. [. . .] There were two family physicians. One was a rather young man who was a specialist in obstetrics and gynecology, and the female members in the family thought very highly of [him]. He progressed very well, and he taught, I [believe], in New York University Medical School. His operations were attended and that sort of thing. He was an expert in his field. And there was another man, an ear, nose and throat man—an otolaryngologist—who was a family physician, who also had a high position in the medical profession. I found these people rather impressive. So I thought that medicine was attractive.

I think that one of the things that I did that [reduced] my chances to [enter medical school], bad as they were anyway, was that I was very truthful about saying that my interest was

to [pursue] medical research. [Medical practice was a] rather secondary [interest]. **<T: 15 min>** This was at a time when, I believe, [. . .] medical schools [preferred] to train people to be physicians and not researchers. At least I was told that, and I had that impression. I must say that as I have become older in this world [I] have realized how limited medical [capabilities were], particularly in the thirties. It turned around when we got antibiotics, greatly, in the forties. I just read an autobiography of a well-known physician who writes about this. He said that mostly we used to watch people die in the thirties. After that we could do something about it. I really thought that physicians could do more than they could at the time.

VAN KEUREN: You spent a year at Harvard. Could you give us some background on why you applied to Harvard? What were your experiences like?

JEROME KARLE: I thought that part of my difficulties in this world were coming from New York City and going to City College, and I wanted to go to a place that was considered very fine in academic circles. I [also went] there because the name was Harvard. There were a few other places that I also would have considered under similar circumstances. [. . .] I applied, they accepted me, and I attended the [school]. I hoped that it would benefit me, [and] I think in some respects it has, in terms of the fact that I did learn a lot there. I think it [also] helped me get a job in the New York State Health Department which was largely [of] a biological and chemical type.

BOHNING: [. . .] What were you thinking about when you left Harvard? [Had you formulated] any career goals at that time?

JEROME KARLE: I didn't know what I was going to do. I wanted to go on in graduate school, and I did indeed apply to others. I actually did a little research [problem] that I had written up, and I sent out copies of it. [But] I just got negative replies. [. . .] I knew that I needed some sort of financial support because my family couldn't provide it, and I didn't want to take advantage of my sister. So I didn't know what I was going to do.

[. . .] During that summer I had an on and off job, selling tickets to an amusement ride. One of my good friends [. . .] came by the box I was selling tickets from, and he [told me] that there is [an examination for] a lab technician's job at the Health Department. "Why don't you fill [an application] out and send it in?" I did that. Except for this chance circumstance I wouldn't have done anything, I don't suppose I filled it out. They then asked me to take an exam sometime in the fall. I found it not too difficult. Finally, around, perhaps, Christmastime or the first of the year, they asked me to report [in Albany, New York]. I may not have been the highest one on the list because I had no experience, and they counted experience for something, **<T: 20 min>** but I was the first one on the list that they could get to. So I went up there, and I started out by working in a laboratory that examined the bacterial content of water supplies.

Then later on I went into a laboratory which [chemically analyzed] water supplies, and that's what I did there most of the time.

In fact, my first [independent] experience in the research world was to develop a little test for testing fluorine in water supplies. They were just beginning to add it as a civic activity. I was told later by the head of the lab who met me [in Ann Arbor, Michigan] while I was a graduate student at the University of Michigan, that it had been adopted as the general method for water analysis. I was pleased about that. That was first modest entry into the world of research.

During the period that I had left Harvard and before I went up to Albany, I [remained] close friends with one [young man] who told me about the exam. He was going to school at night at Cooper Union [for the Advancement of Science and Art. [. . .] His name is Roger Gilmont—very successful, both as an adjunct professor of thermodynamics at Brooklyn Poly [Polytechnic Institute of Brooklyn], and [as owner of a] chemical engineering supply company. At any rate, he was studying thermodynamics at the time, and I interested myself in thermodynamics also, and so we would chat about his problems and so forth. He had [to carry out a project, and he selected a topic] that he was interested in. He asked his professor [for approval]. His professor said, "Your project can't be done." Then he went to several other people, and they [concurred with his professor]. So he asked me if I would try to do it. This is a little personally embarrassing [to say so], but I solved the problem in two days. [. . .] This wasn't his whole project, but it was something that he needed in order to proceed with his [. . .] project. [. . .]

[The point is that I kept active and interested during very discouraging times.] Actually, I kept up with thermodynamics when I was working at the Health Department, and I wanted to [continue it] in graduate school. The reason why I didn't was that there didn't happen to be anybody at the University of Michigan at the time who was interested in having somebody do a thesis project on thermodynamics. What Professor Brockway was doing was certainly interesting enough for me. It was a long haul between the State Health Department and graduate work at Michigan.

BOHNING: How did you select Michigan eventually?

JEROME KARLE: [. . .] Some people who were working at the Health Department extolled the virtues of Michigan to me. It sounded like a good place.

VAN KEUREN: You spent two years at the Health Department?

JEROME KARLE: Almost. It was about a year and a half. It was an enjoyable year and a half. The atmosphere was good, <**T: 25 min**> and although the work was largely routine, I was

allowed to do special little projects, for example, developing this fluorine test. That made it interesting.

The gentleman whom I worked for, whose name was F. Wellington Gilcreas, was a really fine gentleman. I will tell you an anecdote as to just how fine he was. I didn't realize it at the time. There was a three month probation period. I found [several] nice, competent gentlemen who came up from New York City disappearing after three months. Somehow or other, I didn't. I [had been] slated to go too. The only reason that I didn't was that [Gilcreas] put up a terrific fight for me. I never knew about [that] until [much] later when somebody told me. [. . .] After the various disappointments and tum-downs that I had, I don't know what that would have done to me. I think that I would have recovered again. But it was pretty tough sledding.

I used the money that I [saved] from this technician's job [. . .] for my first year at Michigan. [My] problems weren't over yet. I needed a fellowship for the next year. I applied for a teaching fellowship, and it was turned down. [. . .] I found out just before Easter vacation. I was taking the train home to visit with the folks during the week. I had a [depressing] time thinking about the fact that I was being turned down again. I wasn't going to make it. This was [really rather] depressing. [. . .] I knew I had to do well; my record in school at Michigan was really very fine. They had the special grade of A+ [that was given] in graduate school if a [student] had done outstanding things, and I had collected A+s already. Well, I was doing A work at Harvard too. So I thought that this was the end of my rope. When I came back I had finished the quantitative analysis course in record time, and the professor came out to make a great speech about [how] he [had never seen] anybody get the work done so quickly and so accurately, etc. I was [the virtuoso of] the class, but I still didn't have anything to do the next year. This same gentlemen, Lee Case, who was so nice to Isabella, [. . .] came [into the quantitative analysis laboratory, while I was cleaning up for the semester]. He came over and said [. . .] that he had a request from Johns Hopkins [University] for good graduate students in chemistry. He said, "I'm pretty sure you can get it if you want it." [. . .] So I let him know I wanted it, and he said, "That's fine." I then decided that I would go to Professor Brockway, [with whom] I had a tentative arrangement, to tell him **<T: 30 min>** what my circumstance was and that I was going to take this fellowship at Johns Hopkins unless something could be done here at Michigan. [. . .]

[END OF AUDIO, FILE 1.4]

JEROME KARLE: [. . .] So he said "Let me look into this." Well, the next day I had a teaching fellowship. From that point out, I also had a career. And I stayed there.

VAN KEUREN: Besides Brockway, who were the major influences on you when you were at Michigan?

JEROME KARLE: Well, Professor Bachmann, whose first name was Werner, [. . .] who Isabella mentioned, was just a very fine gentleman. We got along very well. He recognized me as a good student, and he once said to me, [out of the blue and with some surprise to me], "I hope when you get finished you'll stick to chemistry." [. . .] We used to have a lot of [interaction] in the general lecture for organic chemistry. [. . .] He would ask a question about the next thing he was going to say, and I could anticipate him most of the time. He enjoyed that very much. But one day he came in, and I think he said to himself, "I'm going to catch that fellow." So [during] the course of the lecture he said, "By the way, does anybody know the surface tension of water?" So I told him: 78 dynes per centimeter. [. . .] He enjoyed that very much, and [it was fun]. He was very encouraging.

Who else? Well, Hobart Willard, who was a great analytical chemist. [. . .] He was very stimulating. Kasimir Fajans had a [great] reputation. [He headed the] Physical Chemistry Institute in Germany—Munich—and he brought his knowledge and his great distinction to the University of Michigan. [. . .]

ISABELLA KARLE: The graduate school was still small enough that most of the people in the graduate school had the same [professors] for their particular courses. There were [some courses that were] absolutely required and then the other courses that you wanted to take anyway. Another professor was Roger [H.] Gillette.

JEROME KARLE: Yes. I was going to [mention him].

ISABELLA KARLE: And that was in quantum mechanics.

JEROME KARLE: He taught us three courses, and he was very good. He taught us statistical mechanics, quantum mechanics and thermodynamics. Each one of them [was given with] polished lectures.

ISABELLA KARLE: I don't know why we missed Lee Case in thermodynamics—

JEROME KARLE: —he was giving a special course in the [Gibbs'] phase rule at the time, and it just didn't fit with the other things we had to do. We had to take any number of prelims. We had to take two physics prelims amongst the chemistry prelims. We had a prelim in organic and physical—each a separate one—analytical chemistry, and then we had to take two prelims of our choice in physics.

ISABELLA KARLE: And also foreign languages.

JEROME KARLE: And foreign languages.

ISABELLA KARLE: French and German.

JEROME KARLE: Two foreign languages.

ISABELLA KARLE: We were kept busy.

JEROME KARLE: Those were the days.

VAN KEUREN: No Hungarian?

JEROME KARLE: [. . .] No Hungarian. **<T: 05 min>** I had good relationships with Otto Laporte in the physics department. He helped me a lot later on when I started to do my research on theoretical diffraction problems. He had an excellent background in the mathematics of theoretical physics. He himself was a fluid dynamicist. There is a prize named after him in the [American] Physical Society. And then in math there was Earl Rainville, and there was also a gentleman by the name of [George Yuri] Rainich who interested himself in the mathematics of relativity. We had good relationships with him.

VAN KEUREN: [I notice from the course schedule] that there was a course on molecular structure. Who taught that?

JEROME KARLE: It might have been taught by Brockway, [. . .] but we didn't take that course.

ISABELLA KARLE: I don't know who would have taught it at the time we were there.

JEROME KARLE: We didn't take that course. It may not have been taught.

ISABELLA KARLE: Not every course was given every year because there weren't that many students.

JEROME KARLE: I was very unfortunate in that respect. The library course by you-know-who wasn't given. I was so glad to miss that.

ISABELLA KARLE: In those days, not every professor had graduate students. I mean, there just weren't that many in the university.

BOHNING: What was it about Brockway [to which] you were attracted virtually from the time you met him?

ISABELLA KARLE: Well, he had a charismatic personality. He had a love of life. He was younger than everyone else. I think he was the youngest of [all of] the professors at the time we were there.

JEROME KARLE: He was about ten years older than we were, and we were quite young.

ISABELLA KARLE: And he was doing exciting—what we thought was exciting—new research. If Brockway weren't around, I would certainly have asked Bachmann to do research in organic chemistry. And in spite of Bachmann's great knowledge of the subject and being a very nice person, I thought at the time that there would have been one drawback. He had too many graduate students. He had all the graduate students. Organic chemistry was very popular then because afterwards it was easy to get an industrial position. I don't know about inorganic chemistry. There were other organic chemists around, but he had all the graduate students—maybe twenty or so—and that seemed as if he was spreading himself very thin. That, I think, was one of the reasons that also led to his demise. He had a massive coronary. He was just working too hard [. . .] because he was very conscientious.

JEROME KARLE: Before I forget. If you are making notes to this, [Laporte] is spelled not like "the door." It has a small "p" and it is all one word. When he was alive, he didn't like people to say "La Porte." [laughter]

There is a funny story about Rainich. I don't know if you want to hear funny stories. He was giving a seminar on some particular subject, I don't remember what. I think it was something to do with differential geometry which comes into the relativity theory. He gave an hour's lecture and presented this as his own work, Rainich. There is always some bright student. [This one] said, "I'm sorry professor, I read the Russian literature, and this work was done by somebody by the name of Rabinovich in Russia." So Rainich said, "Yes that's true. My name was Rabinovich when I was in Russia." [laughter] **<T: 10 min>**

ISABELLA KARLE: If I may return to my earlier days. I always felt that my parents, despite the fact that they had [very] little formal education, were always very much interested in the world. Somehow or other, in those days, they managed by very careful living to save enough money for a summer trip. So the family consisted of my brother and myself and they would all pack ourselves into a car and we travel as far as we had money to travel. As I was growing up, I think I was just about the only one of my contemporaries who had seen almost all of the eastern United States.

JEROME KARLE: We're sort of jumping around, but I should say that except for one cousin in a rather large and extended family who went to a mid-New York State school, and whose intellectual interests centered mostly on managing the baseball team, nobody had ever, to my knowledge, received an advanced education in my family. I think that the closest anyone would come was my sister. She went to Washington Irving High School, particularly to study art there. They had a particularly good art course, and she did go to places like the Art Students League and Pratt Institute to take some more art courses. She advanced herself.

ISABELLA KARLE: And she has been an artist all her life.

JEROME KARLE: [. . .] Yes. And her husband has been, her son has been, [his] wife has been, [. . .] and her grandson will probably be.

VAN KEUREN: What was the response of your family to your education? Were they pleased? [. . .]

ISABELLA KARLE: My mother and father were, of course, extremely happy that it was possible for them to help me as much as they did, and the fact that I really [wanted] an education. That was all very positive. Most of my relatives—aunts and uncles and cousins—were supportive. Some of them didn't quite understand why [. . .] it was chemistry and not something that was a little more easy to understand. However, there were a good many friends of my parents who did not understand at all. Why send a girl to college? After all, she is just going to get married and have a family and cook. She doesn't need all that education. It wasn't that they really disapproved of it. They just thought it was something very foolish to do.

BOHNING: Did they ever understand how you managed to do all [that] they expected and still be a scientist at the same time?

ISABELLA KARLE: I think by that time they didn't exist anymore.

VAN KEUREN: [To Jerome Karle] What was the response of your parents?

JEROME KARLE: They were very pleased. I never had the impression from my mother that she was terribly disappointed that I didn't become a physician. Actually, towards the end of my first year at Michigan—this was in 1941; you remember the war started in December of 1941—around May or June anyone who qualified could suddenly enter free the University of Michigan Medical School. They had an accelerated training program already in place [. . .] that was supported by the government. There was clear anticipation of the probable need for physicians. I could have easily **<T: 15 min>** taken advantage of that opportunity. But, by that time, I felt more interested and committed to the path that finally was laid out in front of me, and so I didn't [consider medical school]. I remember, however, young men who didn't know what they were going to do otherwise, [during my first year at Michigan]. There were two or three people, I recall, who [did take] advantage of [the opportunity].

BOHNING: Were there any other people in Brockway's group? How big a group did he have?

ISABELLA KARLE: Yes. There was Jack Secrist, who was his first graduate student. Jack [. . .] first taught at a college in New England, I don't remember which one. He would come to do graduate work in the summertime. [During] vacations he tried to take whatever he could back with him. After he got his degree from Brockway, he was a professor at Wayne State University until he died. There was Robert Livingston, who was a contemporary of ours. He came from Ohio State [University]. He got his degree by the same time you did [to Jerome]. Then he went on to Purdue [University], and he was a professor there. At first he did structure work, and then after that he was much more involved in chemical education in the universities. He is no longer alive. He stayed there all his life. If you remember earlier, I mentioned that Charlys Lucht was another girl who was with us. Did she ever get her master's degree?

JEROME KARLE: I think so.

ISABELLA KARLE: [. . .] So she was more or less with us that first year, but then she left.

JEROME KARLE: And then there was Elaine Shafrin. She didn't go beyond the master's degree, but she [. . .] came to work here.

ISABELLA KARLE: Before we did, in chemistry.

JEROME KARLE: With Bill [William A.] Zisman.

ISABELLA KARLE: And she retired recently.

VAN KEUREN: So, you were in contact with Zisman?

JEROME KARLE: That's how I became initially connected with NRL. Brockway had a contract, and Elaine, and I, and possibly Bob . . . yes, yes sure. Bob Livingston had finished his degree work when I did. The three of us were involved in that project.

ISABELLA KARLE: There was one other person who started about the time we finished. He has been a professor at the University of Texas ever since. [. . .] We know him well. Jim Boggs.

JEROME KARLE: Yes. Jim Boggs.

ISABELLA KARLE: But we didn't really overlap as graduate students.

VAN KEUREN: How did you choose your thesis topic?

ISABELLA KARLE: I think it was sort of handed to us. This was really the very early days of electron diffraction. Although Brockway, in his first two years or so at the University of Michigan, built the diffraction unit with his own hands, vacuum pumps, electrical circuits, everything, [. . .] there were still [many] things to be done when we came to work with him. The unit was functioning, but [many] improvements [were needed]. In addition to that, we were studying the smallest of molecules, because [at this time] nothing was known about [their structure]. There were [numerous] molecules [. . .] in the vapor state [to choose from about which little was known]. Some of the [substances I studied] I had to heat [in order to vaporize]. That was the first example of heating a solid to a vapor and shooting electrons [through the gas to obtain the diffraction pattern. Brockway] had a number of [compounds] on hand. [In addition,] people [in other laboratories] had synthesized new small compounds, <**T: 20 min**> and the [. . .] conformation of these simple organic molecules was quite unknown [and needed to be established]. Anything we did was new.

BOHNING: You started working for Brockway the first year you went out there? No, the second.

JEROME KARLE: No second.

BOHNING: That's right. You had gotten your degree. You were a year behind, is that correct [to Isabella]?

JEROME KARLE: [. . .] Not really. The first year that I was there was '40 to '41.

ISABELLA KARLE: I [received] my bachelor's then.

JEROME KARLE: That was the same year as Isabella. We both slowly started to do research for Brockway, she in her first year of graduate school and I in my second year.

BOHNING: And you were married the year after that, in '42?

ISABELLA KARLE: [. . .] Forty-two. That was when I got my master's.

JEROME KARLE: It was the end of that first year when we started to work with Brockway. It was June of '41. The first year was '40-'41 and the next year was '41 to '42.

BOHNING: Was this work related at all to any war effort?

JEROME KARLE: The thesis work, no.

ISABELLA KARLE: Except [that] I got caught, in the sense that I had been working on fluorine compounds. [. . .] When I finished writing my thesis, I had already picked my committee for the final examinations, and everything was set up, the government decreed [that] all fluorine compounds were classified. And so there were two results. One [was] that I had to find the few people on campus who could act on my final committee who had clearance. The other was that I couldn't publish. Actually, I did publish the half of the thesis that was on hydrocarbons, and [I could publish a part of it]. My final examination was rather strange in that

the few people on campus really didn't have the time to study the thesis well. They weren't that familiar with the kind of subject I was involved in.

VAN KEUREN: How did the onset of the war effect graduate studies and graduate life?

ISABELLA KARLE: First of all, it skimmed off all the people who were in the ROTC. I think they were allowed to get their bachelor's degree, but they weren't allowed to go on and do graduate work. It skimmed off [a] group that [represented] maybe 25 percent of those who would have gone on to be graduate students at the time. Maybe more.

JEROME KARLE: I have no idea. That may be a good estimate.

ISABELLA KARLE: As I mentioned before, the Selective Service was fairly generous in allowing people—at least in chemistry, I don't know about the other sciences—[to finish] their PhD's. As soon as they [received] their degree they were obligated in various ways. No new ones were coming along. So the number of graduate students was reduced. Most people were in a hurry to finish. So most people, I [believe], worked much harder than they would [have] otherwise to try to finish up their graduate work as soon as possible, because there was no guarantee that they would be allowed to finish at the time.

[There was] a very select group that stayed at Michigan for a long time. Most of the people who [received] their PhD's stayed on at Michigan doing war work of one kind or another—a lot of it secret work. RDX was developed there, <**T: 25 min**> for example, in Bachmann's group. So that the graduate students turned into research associates. No new ones were coming. Pretty soon the graduate students and the research associates got married and had children, so our big chemistry picnics had a lot of little kids around. It was not until the war was over that the group broke up.

JEROME KARLE: I was teaching [as a teaching fellow] as the war developed. There were a great number of young people who were permitted to come back to school from the overflowing [military] camps. They seemed to have many too many people in the camps. This was a great opportunity for them, but my experience was highly negative, and I came to the conclusion that they emptied the camps of the malcontents and the trouble-makers and sent them back to school. I flunked most of them. They seemed to have absolutely no interest in learning. They were not motivated at all. [There was] a tremendous contrast between them and the classes of serious students that I had who were the regular students at the University.

BOHNING: Was this in the introductory course?

JEROME KARLE: It was the most elementary, and for them it was even more elementary than that. It was [. . .] not quite, but almost at the high school level. But they were really unsatisfactory.

ISABELLA KARLE: There was another [circumstance] too, and I encouraged my brother to participate in it. He was five years younger than I. [At this stage of the war], many of the boys knew that they were going to be drafted at age eighteen. They tried to finish high school early [in order] to get a few years of college [education] before they were drafted. For the most part, this served them well.

I was teaching the engineering courses in chemistry after I got my appointment as an instructor. And there were all these sixteen- and seventeen-year-olds [in the class] who had tried to [take as many] technical courses as possible. [. . .] As the result of [a few] years of college, most of these young men, when they entered the Armed Forces, [. . .] had an advantage in that they became petty officers in the Navy, or they were sent to radar school. You remember, there was a radar school here [at NRL]. My brother [was assigned to] the radar school because of his earlier education in engineering, but he was [sent to Texas instead of NRL]. [. . .] It was interesting for me to teach freshman chemistry to a group of young men who were motivated in a way other than they would have been during peacetime.

JEROME KARLE: There was also a variety of activities. For example, they wanted to train people to speak Japanese very quickly, and there were groups on the campus, [usually with] a nice [. . .] young lady [of Japanese origin], and perhaps a dozen of them sitting around, and all day long they would speak to each other in Japanese. [. . .] In about three months time, these people were speaking pretty good Japanese and they would converse with each other. One of the more amusing parts of the ride **<T: 30 min>** to New York at Easter time—I guess this was another Easter time, because the war had started. Later on, when I was going home to visit the folks, I was to get on such a train, and I could listen to a number of people who were also going to New York speaking to each other all night long in Japanese.

ISABELLA KARLE: One of our [chemistry] friends did learn Japanese, I suppose in that program—Lynn Merritt. He was somewhat older than we. Eventually, he not only became a professor at the University of Indiana, but [also] one of the vice presidents in charge of their global research programs, and he spent a good deal of time in Japan. [It] helped him a good deal to be able to speak the language well.

[END OF AUDIO, FILE 1.5]

JEROME KARLE: Let me make one more personal remark in response to the effect of the war. For two of the summers that I was [at Michigan], Brockway had a program to help [in] welding airplane parts in the Willow Run Bomber Plant, which was in Ypsilanti, maybe eleven miles away. [. . .] I think Ypsilanti was about that.

ISABELLA KARLE: I walked there once.

JEROME KARLE: I worked on that program for each of the two summers. The first summer I built the electron diffraction apparatus, put it together myself, and the next summer I did experiments. So, even though my thesis work didn't concern anything about the war, this particular activity did.

VAN KEUREN: What was it like working on electron diffraction at this time? Was it exciting? Did you feel you were on the front edge of knowledge?

ISABELLE KARLE: Yes. It was exciting because there were so many factors that [all came] together. I mean, we had to become instant electronics specialists. We had to become high voltage specialists, and the [breakdowns] we had, the discharges of 50 kV, were enough to send [you] through the ceiling at times. High vacuum—all of this was really very new in those days, and how to machine joints and put them together so that they would hold a good vacuum. This [experience] brought our attention immediately to the [space] shuttle disaster. We could see how badly the joints were made. No freshman would make a joint that badly, way back forty years ago. So these were all kinds of technologies that we either had to learn in a hurry or work out [for] ourselves. This is all aside from doing any chemistry.

BOHNING: Did you have any support facilities—machine shops or things like that?

JEROME KARLE: Yes. I learned something very early in life, with respect to both the machine shops and the glassblowing shops, that if I wanted something done in a hurry, to let Isabella take it over and ask for it. [laughter]

ISABELLA KARLE: You worked that on me here for many years, too.

JEROME KARLE: It came in very handy at the Naval Research Laboratory also.

BOHNING: I'm not familiar with who the others might be who were working on electron diffraction at the time. Were you in contact with others, go to meetings?

JEROME KARLE: Yes. [Electron diffraction of gaseous substances] was always, in all its history, a very sparsely occupied field, and even today there are probably not more than ten laboratories all over the world working in this field. I think that is as many practically as there ever were. In those days, [during the forties], there were fewer.

ISABELLE KARLE: [Also], in those days, [. . .] the contacts were [pretty much] by mail because there wasn't much travel.

JEROME KARLE: There was a little bit of getting together at meetings. In fact, what is now the American Crystallographic Association was emerging [from] the American Society for X-ray and Electron Diffraction and [. . .] the Crystallographic Society of America. We would see the people from Caltech [California Institute of Technology] fairly often. That's where Brockway came from. [The electron diffraction program at Caltech] was continued by people such as Verner Schomaker. **<T: 05 min>** [. . .]

ISABELLA KARLE: Actually, when we were graduate students there wasn't really much contact, except for Verner Schomaker [. . .]. [Linus] Pauling came once or twice.[3] Brockway got his degree from Pauling. [Was] Brockway Pauling's first graduate student?

JEROME KARLE: He may have been.

ISABELLE KARLE: I think so.

JEROME KARLE: He may very well have been. Because it was right after Pauling was on a postdoctoral sabbatical stint. He came back to Caltech around 1930; he was in Europe in 1929, and that's when Brockway got started. Yes. He may have been his first one. There were about seven years between them.

ISABELLA KARLE: But we didn't really meet too many people until after the war when we started going to meetings.

[3] Linus Pauling, interview by Jeffrey L. Sturchio at Executive Tower Inn, Denver, Colorado, 6 April 1987 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0067).

JEROME KARLE: Oh, that's what I'm talking about. You were talking about during the war? Oh, I see.

ISABELLA KARLE: Not when we were graduate students. We were quite isolated.

JEROME KARLE: No. When graduate students there wasn't much wartime association. But [. . .] I was thinking of the later forties. There were some assemblies at that time.

ISABELLA KARLE: Yes, we met with a number of other people, even the Europeans.

JEROME KARLE: There was a strong school in Norway. There always has been. In fact, they started doing first rate work in the late thirties. A strong school developed in the Soviet Union.

ISABELLA KARLE: But in a different way.

JEROME KARLE: Somewhat later and in a different way. At least some of them had pioneered in really high temperature electron diffraction. There were some [attempts] before that, but they solved a number of problems which [interfered] with the earlier attempts.

ISABELLA KARLE: I did, I think, one of the first higher temperature work. That was on organic compounds, and the temperatures weren't very high. But the Soviets went up to extremely high temperatures.

JEROME KARLE: They were evaporating salts. [. . .] We were evaporating organic vapors.

ISABELLA KARLE: And the Japanese? Not in gas electron diffraction.

JEROME KARLE: Well, after a while.

ISABELLA KARLE: After a while. Yes.

JEROME KARLE: There were [Yonezo] Morino and [Kozo] Kuchitsu.

ISABELLA KARLE: They started also in [electron diffraction of solids].

JEROME KARLE: Well, Morino was doing [. . .] gas work, and he coupled it with his spectroscopic studies.

VAN KEUREN: [. . .] You were recommended by Raymond Keller of the chemistry department at Michigan for a position at the Met Lab [Metallurgical Laboratory at the University of Chicago].

JEROME KARLE: My goodness, how did you know that? Okay. You don't have to tell me your sources. [laughter]

VAN KEUREN: Glenn Seaborg mentions it in his four-volume history of the Met Lab.

JEROME KARLE: Oh, from Glenn. Sure.

VAN KEUREN: Could you tell us something about the background?

JEROME KARLE: He was a young man, Ray Keller, who was brought in [. . .] when we were just finishing our graduate work. There was a lack of interest in [inorganic] chemistry [at that time], and I'm not talking about the University of Michigan, just broadly in inorganic chemistry. It was "one of the dead subjects," and [it was] felt that this was a lack and a loss in the department, so Ray Keller was brought in to fill that gap. Being fairly young and new around the place, and being on the same floor that we were, **<T: 10 min>** we got to be good friends. I used to have long chats with him about everything, including the department and so forth. Our contacts were not specifically chemistry interactive per se, but just sociability in the context of chemistry.

A colleague of his had written to him to find out if there was anyone at the University of Michigan who might be interested in "we can't tell you what" at the University of Chicago, but it was important and so forth. I would be immediately drafted, as soon as I [received] my PhD, and I had concluded that I might be able to do something more worthwhile for the war effort than simply carry a gun, and so I was interested in finding war work. This project was surrounded by the great [minds] of the time. I think this was probably mentioned to me, and I was quite enthusiastic about joining their company. I was not told what it was going to be.

Everyone who arrived in Chicago under those circumstances had several days of adjustment. [. . .] The terribleness and the destructiveness of the potential application is something that most people at the time could not very easily dismiss and get on with the work. But along with that were two things that most of us found to give us some comfort, and that was that in the first place nobody knew what the Germans were up to, and at what possible stage of development they were at. They knew about heavy water factories in Norway, and so forth, and it scared everyone. [. . .] There was no question in anybody's mind that if someone were to [do] it, we had to do it first. That was perfectly straightforward. That assuaged most of the problems, and then there was a statement by [. . .] president, Franklin Roosevelt, that he would not make an initial use of [the bomb], especially on some human target. I believe that that may have been true. I believe that if he were president, that if there were to be any things [done] to scare the Japanese, he would not have found populated cities to [bomb].

At any rate, with the thought that the Germans may be doing this and ahead of us, I had [. . .] no qualms about pursuing this matter. I didn't notice that anybody else did either. Except for initial feelings, this was not a part of the conversation there. We worked on plutonium. I think that it was fortunate that we only had it in extremely small quantities because I had the feeling that people [working with it] did not fully appreciate how dangerous this material was. I did some interesting experiments. There was **<T: 15 min>** one experiment that I did [for which] they cleared out the whole area that I was working in. I was the only one there who was going to do that experiment. It was an interesting time.

VAN KEUREN: You were working on reduction by the use of atomic hydrogen.

JEROME KARLE: [Yes]. That was one of the things I did. That one was not a space-clearer. The hope there was that the affinity [of atomic hydrogen for oxygen was greater than that of plutonium. [. . .] The tube that we had to discharge in—[in which] we made atomic hydrogen—was actually getting the sample fairly warm, but [the oxygen] didn't budge. [I do not believe that I tried the oxide of plutonium with atomic hydrogen]. The other experiment that I was doing was a later attempt, [. . .] and it did make some metal, probably poor quality, but it was obviously metal. It was done in [an apparatus at very high temperatures. The apparatus in which the experiment was being done was not made to run at higher temperatures]. I can tell you. So we turned it on in a hood [. . .] and let it go.

BOHNING: [. . .] Time is growing short now, but maybe we could pick up again on this because I have a number of questions I want to ask you, and I don't think we have the time. Especially the facility—the laboratories you were working in and the safety considerations.

ISABELLA KARLE: [. . .] They followed the standard for the day.

JEROME KARLE: People did not fully appreciate what radiation might do.

VAN KEUREN: I might note that when Brockway wrote you recommendations to Seaborg he wrote that ,"Isabella and her husband had made the best records there of any recent graduates in chemistry. Mrs. Karle was brilliant with a pleasing personality."

ISABELLA KARLE: How nice of him.

JEROME KARLE: I wonder what that says about me. "What kind of a person is he? Well, he gets good grades." [laughter]

BOHNING: I think we've probably reached our deadline, and we certainly appreciate your taking time out of your very busy schedules. It was a delightful three hours. It was fascinating. We look forward to continuing this, at a later time if you can spare us the time.

ISABELLA KARLE: I think we can.

VAN KEUREN: Thank you very much.

[END OF AUDIO, FILE 1.6]

[END OF INTERVIEW]

INTERVIEWEES: **Isabella Karle**
Jerome Karle

INTERVIEWERS: **James J. Bohning**
David K. Van Keuren

LOCATION: **Naval Research Laboratory**
Washington, District of Columbia

DATE: **15 June 1987**

VAN KEUREN: [. . .] Today is 15 June 1987. This is David Van Keuren and James Bohning, and we are here for a second interview with Jerome and Isabella Karle on their career and work. [. . .] I would like to return us to where we left off at the last interview. We were talking about facilities at the Met Lab in Chicago. What were the facilities like in Chicago?

JEROME KARLE: They were set up specifically for the purposes. Neither of us was there [for] the original construction. [The structures] were annex-type buildings that were specifically set up for the purpose of carrying out the research [program]. They seemed to be quite adequate.

ISABELLA KARLE: Although [of a] temporary type.

JEROME KARLE: Temporary type of construction, but nevertheless, quite adequate.

ISABELLA KARLE: The sorts of things we were doing, one would say, [were] mostly physical chemistry, and there were no sophisticated instruments at that time for physical chemistry. We built our own vacuum lines. We had some technicians to help us. We used centrifuges, and [. . .] I had to build some high temperature ovens, but I did that by myself. But that is what one would have done anyway in private research at a university laboratory.

JEROME KARLE: We did our own glassblowing.

ISABELLA KARLE: The laboratories now are much different than they were in those days in that there weren't very many instruments available commercially. Most of the specialized instruments the scientists either thought of themselves, designed themselves and built themselves, or looked at what his neighbor [or friend] was doing [. . .] and copied them.

JEROME KARLE: For intricate glassblowing, there was always such a facility with an expert at university sites, and there were also machine shops.

VAN KEUREN: What were the safety facilities like?

JEROME KARLE: We handled extremely small amounts of material, and it was only really necessary to wear a mask over our noses so that [we] wouldn't breathe in [any] naughty compounds that would do bad things in your system.

BOHNING: What was the attitude towards radiation at that time? The reason I ask is that, for example, with above-ground tests we marched troops through the ground merely an hour after the tests, so I'm wondering what the attitude was towards radiation in the laboratory.

JEROME KARLE: They tried to be very careful, actually. We used to have blood tests every month. They had machines there for counting any radiation that might be circulating; it could do the hands and feet and so forth. They tried to protect people from radiation damage, and there were certainly monitors around to tell when you had hot sources or not.

ISABELLA KARLE: But we didn't have any special rooms or any special facilities for handling our materials. Each scientist sort of did it on his own, and for the most part, a lot of the precautions were taken because the materials demanded it. We had to have isolated boxes, isolated from air or isolated from **<T: 05 min>** moisture so that the materials we worked with didn't decompose in the moisture of the air. And so, I was introduced to dry boxes for the first time. It sort of looks like an incubator—they haven't changed much over the years—and there are two long gloves that one puts one's hands into. We used to use obstetrical gloves so that they would fit well on the hands, and we could handle the materials inside. And then there was a lock, so the air was displaced with nitrogen. We could introduce samples through the separate lock where the air was pumped out and the nitrogen pumped in, and it was dried in the usual ways. I think people still use that [system]. We made our own dry box. I don't know whether it was something—the dry box—that originated there, that someone thought of, but [by] the time I arrived, one was being constructed for our small laboratory.

BOHNING: Last time we had talked somewhat about the work you were doing, but we didn't talk about the work that Isabella was doing. Were you doing more synthetic work?

ISABELLA KARLE: In a sense, yes. It was thought that if the halides of plutonium were made, then it would be easier to make the pure metals from the halide. So this would have been an intermediate step. There were not more than a half a dozen, maybe only five people in our group. Norman Davidson was the group leader, and each one of us was assigned a different halide. I was the first to make plutonium chloride, which forms absolutely gorgeous dark green crystals. The way that was made was to put some plutonium oxide—I don't remember which oxide it was—it was yellow and greasy, and we got that from where? Oak Ridge [National Laboratory]?

JEROME KARLE: It was extracted from solutions that we had gotten.

ISABELLA KARLE: But at any rate, when I saw it, it was already as a solid oxide [in] very small pieces, because I had to look through a microscope to pick up a few pieces of oxide to do my experiments with.

JEROME KARLE: I think that it was PuO_2.

ISABELLA KARLE: I think so. I think it was a dioxide.

I had never had any training whatsoever in any of my education in inorganic chemistry, so I had to search through the library on how one makes chlorides from metal oxides, and there were a number of suggested compounds to use as starting materials for the chlorides, and they all worked. The process was to put a bit of the oxide in a ceramic boat in a vacuum line, to put a furnace around the vacuum line, so the vacuum line in that place had to be made from silicon glass, and I used to blow that myself, with some intricate paths for the hot gas to pass over the hot oxide. And the various gases that I used—of course, not all of them were gases at room temperature, but they were at the high temperature—were carbon tetrachloride, chloroform, thionyl chloride . . . oh, there must have been several others. Small organic compounds with chlorine in them that would vaporize to a gas and then break down in the high temperature to have the chlorine [available]. The chlorine reacted with the plutonium oxide and, again, I suppose I could look up my old reports. After a few minutes at the very high temperature, I would let everything cool down, seal it off, put it in the dry box, open it up there, and I had beautiful green crystals which again I would seal into a capillary to keep them isolated from air and moisture.

BOHNING: You were working in milligram amounts?

ISABELLA KARLE: Yes. [. . .] No. I think I started out with micrograms **<T: 10 min>** and eventually went up to a milligram or two because only one or two crystals would grow.

JEROME KARLE: And they were very tiny.

ISABELLA KARLE: They were tiny. They were about the size of the point on a ballpoint pen. Other people in our group succeeded in making the bromide. The fluoride was difficult because that went up the flue.

JEROME KARLE: Too volatile.

ISABELLA KARLE: It was rather volatile. And that gave us a lot of concern because we didn't know where the plutonium went to. I don't know—I don't remember if the iodides were made. I'm sure that somebody must have been trying to make the iodides. Maybe Joe Katz. Joe Katz worked closely with me physically, although we worked on different materials. He may [possibly have] worked on the iodides.

VAN KEUREN: I noticed from Seaborg's report that in C-1 you had very frequent meetings in which the different sections got together and discussed their research. What was the feeling like both at C-1and at the Met Lab as a whole in terms of community spirit. Did people interact an awful lot? How would you characterize it?

JEROME KARLE: I think that the morale was good and that the people were generally quite friendly. In fact, [there are] several people from that time whom we are still in contact with.

ISABELLA KARLE: I think as far as working in the laboratory, nobody was secretive. They shared suggestions, ideas, and told other people what they were doing, and I think that helped a good deal in developing something that was completely unknown. At the same time, I think a lot of instrumentation and procedures were developed that were unknown at the time.

VAN KEUREN: Was there any aspect of your work at the Met Lab that you felt was particularly helpful to you in your later research? Anything that sticks out in your mind?

ISABELLA KARLE: Well, I don't know about that. I think what really sticks out in my mind is just the creativity that had to go on to handle unknown problems and the way we approached our problems. [. . .]

BOHNING: I wanted to ask you about the administrative structure of this. Was this conducive to the work that was going on? Was it well organized?

JEROME KARLE: Well, in my view, scientific laboratories that are well organized are not conducive to doing good work. Organizing things is what you do when you don't know what else to do. Good science goes on when there are talented free spirits. There was, of course, some kind of an organization, but the things that were accomplished for the most part were accomplished by the people who were free spirits. There was no real attempt to do anything more than exchange ideas and achieve agreement on direction. So far as Isabella and I were concerned, we both had an immediate group leader. Norman Davidson, was hers. The name of the gentleman who was mine escapes me for the moment. And at the head of all of it was Glenn Seaborg. Paths could be taken that were just broadly agreed to; then you went ahead and did your work. So I would say that the laboratory was ideally organized for accomplishing creative things because the laboratory was not overly organized. **<T: 15 min>**

BOHNING: Was that typical of Seaborg? Was that his doing? Did he understand that?

JEROME KARLE: I would think so. Nobody ever verbalized on this. Certainly the lab was run the way he would have liked it to be.

BOHNING: Can you tell me more about him at that time? Did you—you were in those meetings with him?

JEROME KARLE: Yes. He was a very talented, bright, energetic gentleman who knew his subject thoroughly. This was the impression that always came off. He was the leader and the major participant in the discussions, and he [always gave] very favorable [. . .] impressions.

ISABELLA KARLE: In addition to Seaborg—I don't know if you had mentioned this—we also had meetings at which [Enrico] Fermi spoke every once in a while. Then people who had come from the other laboratories—Oak Ridge or Los Alamos [National Laboratory]—would also speak about the work going on and the progress being made and the problems in the other laboratories.

JEROME KARLE: These were not in any way informal conversations. Of course, there were questions at the end. [The more formal] meetings filled the auditorium of the physics building. [. . .]

VAN KEUREN: So you had a good knowledge of what was going on in the Manhattan Project as a whole, in terms of the other labs?

JEROME KARLE: To a certain extent. There was a need for informational security, and so there were legitimate limitations that were put on how much detailed information was widespread. But we had a general knowledge of what was going on in various places.

ISABELLA KARLE: I think the one exception, at least in my mind, was that I never really knew what was going on at Columbia.

JEROME KARLE: That's true.

ISABELLA KARLE: I don't even remember anybody coming from Columbia. That may be an error, but I don't remember anybody coming.

JEROME KARLE: I don't recall any real exchanges with the people at Columbia.

BOHNING: That would have been Harold Urey and . . .

JEROME KARLE: Yes. Of course, one knows now better from the accounts of the entire project what he was doing, but at the time they were quite secretive.

BOHNING: I wanted to ask you about Norman Davidson as a group leader.

ISABELLA KARLE: He was fine.

JEROME KARLE: We knew him earlier, actually, because he spent a [year] postdoctoral with our research professor. So, in fact, we shared an office with him. He is a nice gentleman. As I said, the [Met Lab] was loosely organized, so there were not deep interactions scientifically. Isabella knew what she wanted to try to synthesize and how to go about it, and that's what she did

ISABELLA KARLE: The organization was such that perhaps once a month or so we would write a very informal report of what had happened during the month. They said that I was quite

fortunate in making the chloride in a number of different ways. At least in the crude way in which I had made it, all of [the processes] seemed to be quite equivalent.

BOHNING: Was that a successful route then to plutonium?

ISABELLA KARLE: Yes. Oh, a successful route to making the plutonium?

BOHNING: Yes, from the chloride.

ISABELLA KARLE: That I don't know.

JEROME KARLE: We do not know what the ultimate method was that was used to make the bomb.

BOHNING: Your chlorides went elsewhere, then, from Chicago?

JEROME KARLE: We don't know ultimately what method was used. I could guess, but there is no point to it. [laughter] **<T: 20 min>**

ISABELLA KARLE: Eventually the various plutonium materials that were synthesized—I shouldn't say eventually—at the same time, but there were too many of them to be done all at once. I used to take my chloride samples to Professor Zachariasen who was doing the X-ray diffraction. Was he already head of the physics department?

JEROME KARLE: No. [Arthur H.] Compton was there.

ISABELLA KARLE: He eventually was head of the physics department.

JEROME KARLE: And then he became Dean of Science, but that was much after the war.

ISABELLA KARLE: And he was one of the very early X-ray diffractionists from the time he was a student. So at that time he was characterizing the new materials that were brought to him. And there were, of course, too many for him to have completed without computers and

[automation], not being available in those days, and he worked on them for quite a number of years afterwards and published the various structures.

There was a somewhat amusing incident to me. I used to seal up these materials and carry over a crystal or two to him across campus. After a while when security found out about that, they said that I couldn't possibly do that all by myself. [So] I had two guards walking on either side of me to do the delivery.

JEROME KARLE: Then everybody who knew that there was something going on in Chicago could tell perfectly well that Isabella was carrying hot stuff. A little girl with pigtails [would not] have attracted that kind of attention. We always thought that that was a self-defeating operation.

VAN KEUREN: Who insisted on that?

ISABELLA KARLE: We had a security group around the laboratory, around our building certainly, because it was one of the temporary buildings put up devoted to plutonium chemistry.

JEROME KARLE: And we had to wear badges.

ISABELLA KARLE: Yes, and there were guards at the door. They must have somehow or other found out that I was carrying these samples over. I don't really know how it came about.

VAN KEUREN: You left the Met Lab in July of 1944 to return to Ann Arbor. Can you give us some background on your departure? Did you have a job offer in Ann Arbor? Why did you leave the Met Lab at this particular time?

JEROME KARLE: In some sense, we had each completed the projects to the extent that we could there, and Lawrence Brockway had a project going on with the Navy that seemed quite interesting to us. It was still war work, so we didn't have any conscience problems about going from one place to another.

ISABELLA KARLE: It was a kind of stopping point, and we would have had to go on to different sorts of projects at the University of Chicago. Both [Jerome] and I had accomplished what we had set out to do.

JEROME KARLE: In a lot of respects the activities were slowing down a bit there because they had fulfilled a number of the interests that they had. So it seemed to us at the time that it would be just as well if we went on, and then there was the opportunity to do things that were a little bit closer to our specific training.

VAN KEUREN: Had Brockway contacted you about this project? Did you know before you went to the Met Lab that he was doing it?

JEROME KARLE: We were always good friends. He would come out to see us in Chicago and that sort of thing. We remained good friends [and] used to socialize quite a bit. [That] continued very much after we **<T: 25 min>** [returned to Ann Arbor]. It was not unusual to have a knock on our door at midnight, and there would be Lawrence Brockway outside the door [saying], "How would you like to go swimming in the Huron River?" But the [river] was so contaminated that we would never dare to put our heads in. We would drive out a while and go to a place where it wasn't flowing too fast and it wasn't over our heads and get in for a while. There were always crazy larks like that. That was the way things were—spontaneous and so forth. But we enjoyed each other's company very much and remained good friends until he died.

ISABELLA KARLE: At that time, I also got an offer to be on the faculty of the University of Michigan, which was most unusual. I think I was the first woman to be on the faculty [and to have] the grade of instructor. I was teaching the elementary chemistry classes.

JEROME KARLE: That was a real step up for Isabella, and it stood her in good stead for the future.

BOHNING: I don't know if we covered that ground at the Met Lab, but as a woman at the Met Lab did you feel comfortable with your interactions with your colleagues? Were there other women there?

ISABELLA KARLE: If there were, there were not many. I can't think of any.

JEROME KARLE: Women scientists . . . there were probably some, but certainly not close to us.

ISABELLA KARLE: Not close. There were women, of course, who were secretaries, who were technicians [and] who did quite a number of things in the laboratory. No, there was no problem in being a woman there.

JEROME KARLE: There weren't very many women scientists in those days, or PhD trained [women]. Very few. But colleagues were extremely friendly.

BOHNING: Was Brockway instrumental in getting you the faculty appointment?

ISABELLA KARLE: I don't know. [. . .] Most of the universities at that time were teaching mostly the sixteen- and seventeen-years-old boys, because by eighteen they were all being drafted. And girls. A good many of the younger faculty had to be involved in defense-related projects, and there was a shortage of faculty. I was available, and they knew about me. Who actually was instrumental in hiring me, I don't remember.

BOHNING: Who was chairman of the department then?

ISABELLA KARLE: That was a man by the name of [Chester S.] Schoepfle. [. . .] His background was organic chemistry, dyes. [. . .] Something that was not at all common. I mean, we had no common ground.

JEROME KARLE: He was a student of the very famous organic chemist at the University of Michigan by the name of Moses Gomberg.

BOHNING: Oh yes, the free radical chemist.

ISABELLA KARLE: That's right. [. . .]

[END OF AUDIO, FILE 2.1]

BOHNING: [. . .] Did you work on Brockway's project together as well as teaching?

ISABELLA KARLE: No. I did some other kind of research, although I did teach many courses; I did not have that much time for research. But Jerome was, of course, employed full-time doing that sort of thing. There were some student assistants that he had to do a lot of the routine preparations.

BOHNING: Could you tell me something about the work you were doing with Brockway then?

JEROME KARLE: Yes. There were two kinds of projects. One was an attempt to protect ship hulls from corrosion and another project concerned boundary lubrication.

BOHNING: Where was the support for this coming from? Was that NRL supported?

JEROME KARLE: Yes. That was a NRL project. That's how I, in a sense, became acquainted with the Naval Research Lab. It led to our coming here two years after we joined [Brockway] in Ann Arbor.

VAN KEUREN: And this was through work being done by William Zisman?

JEROME KARLE: That's right. I became an employee of the Naval Research Laboratory. Even though the work was being done at the University of Michigan, my employment [by NRL] actually started in 1944.

VAN KEUREN: What was this research like? Did you find it interesting? Was it useful to you?

JEROME KARLE: It was quite useful to me. I wrote some interesting papers about it after the war.

ISABELLA KARLE: In fact, you continued it here after we came.

JEROME KARLE: Yes. I used extensions of what I had learned as a graduate student and techniques that I had learned as a graduate student. In fact, professionally it was an opportunity for me to broaden my knowledge of my specialty.

BOHNING: Before we move on to NRL, I wanted to ask [. . .] one question before we [go on]. Were there many others in this group with Brockway working on this project—the NRL project?

JEROME KARLE: There was another man at my level, and then there was a young woman who was also helping him, and then we had some youngsters from the school who assisted us in hand computing, which was the only kind of computing there was in those days, little desktop multipliers.

BOHNING: You used Frieden calculators?

JEROME KARLE: Exactly. Marchant and Frieden.

BOHNING: I remember those.

VAN KEUREN: You were doing electron diffraction research for NRL at this time, I believe?

JEROME KARLE: Yes.

VAN KEUREN: Were you making your own electron diffraction equipment also?

JEROME KARLE: Yes

VAN KEUREN: I understand that NRL had contracted out all its electron diffraction work to the University of Michigan. Do you have any recollection of this?

JEROME KARLE: You mean before I came here?

VAN KEUREN: During the war period.

JEROME KARLE: Yes. That could very well be. As I think I mentioned the last time we **<T: 05 min>** got together, while I was still a graduate student, Lawrence Brockway had a contract with the Willow Run Bomber Plant to help them with welding. One summer I built an electron diffraction apparatus to do the work, and the next summer I did the work. So, building electron diffraction equipment was the only thing feasible if you wanted to do that kind of experiment.

ISABELLA KARLE: The one that was built here [at NRL] was built on a different principle, much more advanced than the ones that we had used either in graduate school or the [ones that we] built for the welding project.

JEROME KARLE: We specially designed new ones here when we came. [The machine shop] actually built two of them according to the same design. They had a marvelous machine shop here that did a very fine job for us. But we—from what we learned about the technique—had a number of plans for improving the experimentation and also the analysis. And so the apparatus that we built here was much more advanced than was currently available in the United States. Some of the advances that we put in had already been made in Norway, where they have always had a very fine group. There are still several laboratories there that do very fine work in gas electron diffraction.

BOHNING: I was going to ask you if there were any others. Outside of Norway and what you were doing in the United States, were there any other groups?

JEROME KARLE: Yes. At the time there were some in Japan. I did mention that.

ISABELLA KARLE: But they never did the gas work [. . .] in Japan.

JEROME KARLE: Sure they did. Morino [and colleagues].

ISABELLA KARLE: That was afterwards.

JEROME KARLE: You mean after the war?

ISABELLA KARLE: After the war, yes.

JEROME KARLE: They did a little bit of gas work during the war, I think, Isabella. But I can't be absolutely sure about that. They improved their facilities greatly after the war. There was some work being done in the Soviet Union. [Z.G.] Pinsker did some. I can't say exactly what the date was that they started. The British became interested in it after the war.

The very first experiments were done in Germany at about the time that Pauling was carrying out his—I guess it was a sort of a sabbatical, around 1929 or '30. He brought back the thought that this might be a [worthwhile] technique, and Brockway, as a graduate student,

developed it. The first experiments were done by Herman Mark, who was very famous as a polymer chemist.[4] He had an associate by the name of [Raimund] Wierl. [. . .] They did the first experiments, but neither of them were interested in pursuing the technique. [They felt that it would be worthwhile to pursue the technique, however.] There were a number of people working in the field of gas electron diffraction who did themselves in in various accidents for some reason. Wierl was killed, not too many years after, in a motorcycle accident. There was . . .

BOHNING: These weren't laboratory accidents?

ISABELLA KARLE: No, external [accidents].

JEROME KARLE: There were very few people working in the field, and the number of catastrophes that were associated was really disproportional. A curious thing.

VAN KEUREN: Can you speculate on that at all?

JEROME KARLE: No. I think it's—well, maybe working in [this] field required a certain amount of adventurousness that would bring people into circumstances **<T: 10 min>** that were a little more dangerous than otherwise. [laughter]

BOHNING: I'm curious about this point, if you don't mind my pursuing this for a moment. What other kinds of accidents befell other workers?

JEROME KARLE: There was an automobile accident, I forget the names of the [. . .] young people who were working in California. Another unfortunate gentleman [had a fatal] brain tumor. [. . .] Another man became [fatally] ill [after taking] a job here at the University of Maryland in the early fifties.

ISABELLA KARLE: Yes.

JEROME KARLE: He became ill [soon after he arrived].

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[4] Herman Mark, interview by James J. Bohning and Jeffrey L. Sturchio at Polytechnic University, Brooklyn, New York, 3 February, 17 March, and 20 June 1986 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0030).

ISABELLA KARLE: There was another young man in Germany who [developed] a mental illness, and his papers had to be retracted by the university [during those] years.

JEROME KARLE: There may have been one or two others, but I can't remember.

ISABELLA KARLE: Was it Spurr or Shand in California?

JEROME KARLE: It was Spurr who was here at Maryland who became ill, and I think something may have happened to Shand also.

ISABELLA KARLE: I think that was the automobile accident. I'm not sure, but I think it was Shand.

JEROME KARLE: But considering that there were maybe fifteen or twenty people in the field, it was becoming decimated.

ISABELLA KARLE: The electron diffraction of vapors just didn't persist in Germany even though it started there. Herman Mark came to this country soon afterwards and turned to polymers.

JEROME KARLE: Well, he was [apparently] not interested in pursuing it.

ISABELLA KARLE: And then the few people who did didn't survive.

VAN KEUREN: Why would you guess that electron diffraction development in centers in which it did develop, in the United States, Norway and [ultimately] Japan? Were there any specific professional, or institutional reasons for that, as opposed to in Germany or France, say?

JEROME KARLE: It's very hard to say. I think in part it was connected with professional interests, but also personalities. Pauling was interested in it as a tool in addition to spectroscopy for getting good information about the geometric structure of molecules. At the same time, he was developing his quantum-mechanical calculations, [in the attempt] to predict what the structures would be. So his interests [lay specifically in relating theory and experiment. In general, I believe, interest in gas electron diffraction] developed among people who were

interested in organic chemistry. There is, of course, an obvious connection between [. . .] structural [analysis] and synthetic organic chemistry, in terms of [. . .] having a three-dimensional picture of what it is you are working with. [Odd Hassel in Norway] was quite interested in this and did a lot of work on how hydrogen atoms would be directed in [substances] like cyclohexane, for example. [Hassel] received the Nobel prize, as a matter of fact, for that work. [It was shared] with Sir Derek Barton. [Why organic chemists in Germany] were not quite as interested in [pursuing gas electron] diffraction, I don't know. There was gas electron diffraction work in Debye's laboratory. **<T: 15 min>** His son, as a matter of fact, made a contribution to improving the method for gathering data.

ISABELLA KARLE: That's P.P. Debye.

JEROME KARLE: Not P.J.W.

Debye was certainly interested in it because he was one of the founders of the theory [for interpreting] diffraction from gas molecules.

ISABELLA KARLE: In fact, when he came to Cornell from Germany, he brought along with him the electron diffraction unit that he had built, or had built in Germany. I don't know whether he had done it [himself or had somebody else build] it for him.

BOHNING: But they actually brought the unit with them?

ISABELLA KARLE: Yes. It was quite some time ago. We saw it in the late forties. At Cornell they had stored it in a room—not in a museum-type room—but in a room. [. . .]

JEROME KARLE: The first attempts were made to use X-rays, but the scattering of X-rays from gases is practically nil. The experiments were very poor. Now, I suppose if you use synchrotron sources with the various devices for enhancing that radiation, you might be able to do gas diffraction. Electrons were scattered so much more strongly that they became the method of choice by far. It is possible to get a diffraction photograph in a matter of a fraction of a second. [. . .] You couldn't use a nozzle [for x-ray diffraction. When it was first tried] you had to have [the gas] in some sort of a closed container for days. The container would scatter as much or more than the gas in it and all that sort of thing. It was really a nuisance; it didn't work out.

ISABELLA KARLE: For the X-rays.

JEROME KARLE: For the X-rays.

BOHNING: Do you think that the difficulty in building the apparatus may have been the determining factor?

JEROME KARLE: Yes. I think [that the adoption of gas electron diffraction] was limited in part because [of the complexity of the apparatus. It was also an expensive undertaking. I say that because] when I went around to universities to interview for jobs, one of the immediate considerations that came up [was] the question, "How much do you think it will cost to build such an apparatus?" This lay heavily on people's minds. [. . .] For those days, [it was] a complex instrument that would be costly.

ISABELLA KARLE: I think there was also another matter. You had to dabble in electronics because [the electronics were built] by us or by whomever built the instrument. You had to dabble in building high vacuum pumps. It was not until we came to the laboratory here that they became commercially available. Before that they had to be built in a university shop, and whoever wanted the high vacuum had to tell the man in the shop exactly how to do it or do it himself. There weren't any plans available. There was the matter of high voltage and measuring it accurately, so you would know what wavelengths you were working at. That was a problem. Keeping an instrument at a high vacuum [was a problem]. So there were many different facets besides doing the experiment itself.

JEROME KARLE: And many technical difficulties.

ISABELLA KARLE: There was no help from the outside. You had to be adventurous and do it yourself.

JEROME KARLE: I think that's quite true and a number of the techniques were simply not in the background of chemists.

VAN KEUREN: What impact would you say this had upon your research and your work, as opposed to **<T: 20 min>** doing [such work] in chemistry or physics [now]? [. . .]

JEROME KARLE: There was a time when gas electron diffraction apparatus became commercially available in Japan and Switzerland. I think it is possible now to still order one from Switzerland, but it would be quite expensive because they are not being made except to

order. I don't know what the situation in Japan is. There has not been a growth of this field, so there has not been a new market.

ISABELLA KARLE: I think if I understood your question, though, you were asking how students are trained differently now in that there are so many commercial instruments available and, [. . .] even though this may not be easy, all they have to do is write a grant to get enough money to buy a commercial instrument. I think they can do many things with it which they would have to spend a lot of time otherwise developing an instrument, which is good. On the other hand, I think they lose something in creativity and understanding of the process that goes on in the instrument itself. I mean, what is it actually that happens when you put in your sample here and get your [information] out over there which is now often in the form of a photograph or a graph written out on a piece of paper.

BOHNING: You were of a generation that, as a generation, you did this kind of experimentation on your own from a young age. [During] the early days of radio there were people who built their own radio and that kind of thing. But today's students don't do that. They don't have to get into that kind of detail and build it. There is something lost, I guess.

JEROME KARLE: Yes, I quite agree with what Isabella said. In addition to that, another aspect of it is the computer program. There are certainly two sides to the computer program situation. One is, it affords a facility to a much broader range of workers. That is very good. On the other hand, it is also used as a black-box-type operation such that students come up with answers, but they don't have the slightest idea how they [arise], and when they run into difficulties they don't know what to do with the computer program.

Certainly, in educational institutions, they can make sure that students do indeed learn how to do things and not hand them everything. I have no idea [. . .] to what extent this is done or whether young professors are in a hurry to get answers and don't care too much about how well the students learn what they should be learning. I think you probably [are getting] a spectrum [that ranges] from very careful training to that which does nothing more than make super technicians out of young people and hand them PhD's. That is something to be very careful of and something we feel very strongly about. Any number of times we [have been] met by young people who are starting out in our field and don't know us. [They] find out that we are in the structure field—crystal structure, crystallography and so forth. Their first question is, "Oh, what program do you use?" That is the wrong question.

VAN KEUREN: If I am understanding you right, you would agree that actually [producing] the apparatus to conduct the experimentation gives you a more thorough grasp of the physical principles underlying the work than you would achieve otherwise.

JEROME KARLE: In fact, if it is of the correct complexity you get an appreciation for apparatus and experimental work in general that is very, very important. Again, talking about another aspect of **<T: 25 min>** programs and computing and so forth, we feel that there has been a trend towards substituting computers for laboratory work to an extent that is extremely dangerous. We certainly have no qualms about theory and theoretical work. We find it very attractive. To the extent, however, that it's used to replace experimentation, that can make a tremendous amount of trouble. We've had experiences with some young people again. In one instance, we were at a meeting looking at posters, and Isabella spotted some work that looked purely theoretical. It was inconsistent with some experiments that she had done. We attempted to tell this [to the person involved, but the individual] would have none of it. The theory and the calculations came out [just right]. "Don't tell me the facts." [laughter] This is an extreme case but we witnessed it, and we see more of it. This is, of course, a danger.

So starting out with using computers which [weren't] even electrified, which you had to crank by hand, building your own apparatus, doing your own glassblowing, puts certain appreciations and certain cautions into one's development which I think, if nothing else, are quite protective as life goes on in the scientific world.

BOHNING: How do you see the preparation of the young people coming into this facility?

JEROME KARLE: It is a little hard for me to really give a statistically accurate impression because I have essentially not hired anyone for the last twenty or so years. I have developed a very stable group [that has] been with me. So I cannot speak firsthand. I can't grade the relative quality of PhD people coming out now. About the only thing I see in brief contacts at various types of meetings with young people is that I find too often that they're too much dedicated to the use of programs and to the use of the computer where a certain amount of hand work, and a certain amount of understanding that would come from the hand work, and an appreciation of the difference between theory and experiment, would be quite worthwhile. One has to hesitate a little bit to always say, "The good old days, why, we were better than everything." But I think certain tendencies of the sort that I have been just discussing have been coming to the fore.

[END OF AUDIO, FILE 2.2]

ISABELLA KARLE: [. . .] I would just reemphasize that in our own fields a lot of the younger people would say, "We can't absolutely solve this structure because none of the programs will solve it." Ordinarily that is not the case. They have not had the experience in knowing how to think through solving the structure because they have been doing everything so automatically. I think a certain amount of real information gets lost, gets put aside, because the more automatic ways of doing things will not provide the answer [for some computer to do it].

JEROME KARLE: I feel very strongly that in our field, there has been a certain regression in the capabilities of the people working in the field simply because computer programs have been too generally available.

BOHNING: One of the things that I've noticed is that you don't even have to write your own program. At least if [these students] had to write their own programs—

ISABELLA KARLE: That's true.

BOHNING: —but the way that the technology is moving so fast, it is rare that one even has to write a program.

JEROME KARLE: Yes. I don't know how many digressions you are interested in, [. . .] but there is another aspect of this program business, which is its commercialization, which interferes with the free circulation of [. . .] scientific [information. Certain] organizations, people, businesses try to make programs proprietary for the sake of commercial exploitation. I think that is an extremely bad thing, and the subject needs to be considered by [organizations such as] the International Council of Scientific Unions, that is, an organization that sets standards for free circulation of scientists, information and so forth.

A small experience that we had: there was a company that was quite interested in our purchasing their equipment. They described to us a program that would be available had we purchased equipment that could be used for macromolecular structure determination. After it was described to us, we recognized it as a program that was developed in [our] laboratory. But they had patented it.

BOHNING: Very interesting. I didn't realize that programs could be patented.

JEROME KARLE: Or copyrighted, or whatever you do to protect it.

ISABELLA KARLE: And not necessarily your own.

BOHNING: That is frightening.

JEROME KARLE: That interferes with what used to be done and that is to offer programs to your colleagues.

ISABELLA KARLE: Not necessarily free of charge but for the basic cost of the tape and the copying and maybe a little extra because you needed a postdoctoral or two to put it together in a decent fashion.

BOHNING: I believe there used to be an exchange at Indiana University. They ran an exchange of these programs for quantum mechanical calculations.

JEROME KARLE: They still do that. We are [one of their customers].

ISABELLA KARLE: Yes, but the exchanges in many aspects of chemistry are getting to be fewer and fewer because of the variety of programs. One of the problems there also is that you cannot get a write-up of how things are done in a particular program. Supposing you have a larger problem that will not fit—more unknowns, more data—and it just doesn't fit into **<T: 05 min>** a particular computer program. You can't get into it. You don't know where to get into it in order to expand it a bit or to do things in a different order. So they are limiting in that respect. If the write up [is] available, then someone with not necessarily great computing experience, but medium computing experience, can adapt programs to other kinds of problems.

VAN KEUREN: When did you first start noticing the problem [. . .] with copyrighting?

JEROME KARLE: Something between five and ten years ago.

ISABELLA KARLE: I would say ten.

JEROME KARLE: Maybe ten.

ISABELLA KARLE: Ten by now.

BOHNING: I know some of the early programs I bought for physical chemistry instructional purposes, I could get a listing. In other words, they gave me the listing and I could go in and do whatever I wanted on them. [It] was really very nice and helpful to the students as well because it wasn't a black box. As you said, now it is impossible—very difficult. [. . .]

[I'd just like] to go back, for a moment, to your teaching at Michigan. You were teaching—would it be freshman chemistry, the first year course?

ISABELLA KARLE: I was teaching general chemistry [and] analytical chemistry, [which are both] first-year courses, mostly in the School of Engineering and that surprised me. I had the sixteen- [or] seventeen-year-olds who were planning to be engineers in the future, and many engineering students have to take general chemistry. [Then] there were many who were intending to [become] chemical engineers. I did not teach—I had perhaps one class—liberal arts students, who were mostly girls at that time.

BOHNING: Do you remember any of the texts you used?

ISABELLA KARLE: No, I don't remember what particular text we used. We had Hildebrand and who, at home?

JEROME KARLE: [Wendell Latimer].

ISABELLA KARLE: It's possible. I did take some of my materials from that particular text, but that was not the text for the course. I don't remember. I may very well have it in my bookcase at home.

BOHNING: How did you find that experience?

ISABELLA KARLE: I found that it was quite satisfactory, but it didn't inspire me sufficiently to want to continue teaching at a college level. [. . .] If I were teaching graduate courses, it might have been different. My only experience was in the first-year chemistry. There were, of course, always some shining stars, but for the most part the students were not all that enthusiastic.

VAN KEUREN: Was that a limited-term appointment?

ISABELLA KARLE: At the level of instructor, it is always a limited term. Even an assistant professor, it is always a limited term appointment. I could have gone on, I know, in 1946, when I came here [to NRL], at the same level. How long it would have lasted, I have no idea. As I said, I had many courses to teach because there were not that many people available for teaching in those years, and there were still a large number of students [enrolled] in the university.

BOHNING: [Had] these sixteen- and seventeen-year-olds already graduated from high school?

ISABELLA KARLE: Yes. You see the war had already been going on long enough so that those with a little foresight took summer [courses]. My brother was amongst them—he was five years younger than I was—so they took summer classes. [. . .] The minimum requirements were a high school degree [for] entry in a university. At the university at that time, we had classes six days a week and every week, every day. No vacations except [for] Christmas Day. Thanksgiving Day, New Year's Day were all regular classroom [days] so that three full semesters were squeezed into one calendar year. It was rather concentrated for [both] the students and the faculty.

BOHNING: Did you do any laboratory instruction <**T: 10 min**> or was it just lecturing?

ISABELLA KARLE: I have to think about this because I had done laboratory instructing as an undergraduate. Not as a graduate student, but as an undergraduate. [. . .] I must have [at the time I was an instructor. Then, of course,] the laboratory instructing accompanied the classroom.

BOHNING: [. . .] It was pretty obvious that your tenure at Michigan was going to be drawing to a close. What were you [both] looking at beyond that point?

JEROME KARLE: Because of our interests we were thinking predominantly about university work. At the same time, we had strong criteria and also the strong constraints that the criteria brought on, mainly that we wanted to be at least in the same city. Isabella was very anxious to have a research career, but we didn't want research careers to break up our marriage.

ISABELLA KARLE: Or separate us.

JEROME KARLE: [. . .] The more we looked around, the more problems there were. If nothing else, there were nepotism rules, and as the war was drawing to a close, there weren't any great and obvious expansions by universities. This did, of course, take place, and it exploded in a few years, but not immediately. The picture was extremely discouraging, and we really didn't know what we were going to do.

It turned out that certain powers that be were anxious to change this laboratory from predominantly engineering to a basic research laboratory, and in conversations we found out that they were interested in hiring the both of us. We were, I have to say, somewhat apprehensive because we weren't sure that there would be a significant basic research program

here after the war, but it seemed very attractive to us and we decided that we would try it. As it turned out, as a place to work and as a place to do basic research, it couldn't have been better.

BOHNING: [What] was Brockway's attitude towards the move? Did he encourage it?

JEROME KARLE: There were two sides to that. He was sorry that we were leaving. We were good friends, and he also enjoyed working with me. But, he certainly thought that it was worthwhile coming and trying it, and it worked out quite well.

VAN KEUREN: Had you been interviewing with other universities at some point? You said you had been doing some interviewing.

JEROME KARLE: Yes. I remember some of the ones that I had gone to. I went to Princeton, I went to Columbia and I went to Michigan State. That's the one I came closest on. I came in second. There may have been one or two others, I don't recall.

VAN KEUREN: I have some questions about how you came to NRL. You said that the powers that be at NRL were interested in hiring both of you. They knew of your work through Zisman and the contract work you did? Was Zisman integral in your coming to NRL, [do you think]?

JEROME KARLE: At first, he was interested in hiring us, <**T: 15 min**> and I don't know all the intricacies as to why that wasn't pursued. But he was good friends with Herbert Friedman and simply told us that [Friedman] was interested in hiring us, and it would be easier for Herb than for him at that time. What actually was involved in all of the arrangements and all of the decisions, we don't know.

ISABELLA KARLE: It, again, had been a matter of our interests in that Herb Friedman [was already] involved in various kinds of X-ray activities. Not crystal structure, per se, but using X-ray diffractions for other sorts of things. He was looking forward to having the electron microscope, also, and so it was already in that sort of a bailiwick, that we would fit well with [all of the] instrumentation.

JEROME KARLE: And there was the optics division, too. Electron diffraction could be part of that.

ISABELLA KARLE: Whereas Zisman's group was surface chemistry, although Zisman did hire another person from Brockway's group—Elaine Shafrin. Her name was Glass at the time, wasn't it? And she stayed here until she retired a few years ago. But she did much more surface chemistry type work than any diffraction.

VAN KEUREN: So if Zisman hired you, you would have ended up in chemistry, but since Friedman hired you, you ended up in optics.

ISABELLA KARLE: That's correct.

VAN KEUREN: What sort of reputation did NRL have, back in 1945 [or] 1946? What did you know about it?

JEROME KARLE: It had done things for the war and, as it turned out, for science, that were quite significant—for example, the development of radar. And Zisman had a good reputation as a surface chemist. The electronics people were evidently well-known, although this wasn't our field, and we didn't know them particularly. [The optics people were also well-known people.] From talking to people, I find that it has certainly been highly regarded as a research institution since the wartime.

ISABELLA KARLE: But at the time during World War II, and at the time we came, at least I was not aware of much of this work. I knew about Zisman's work.

JEROME KARLE: The man who was the head of the Optics Division, E.O. Hulburt, was a distinguished person in the field of optical sciences. It is true, though, that we were not aware of most of these things when we came [here].

VAN KEUREN: You mentioned the fact that important people at NRL were intent on changing the orientation of NRL [into] a major research facility. Were you aware of that at the time? Was that expressed to you when they hired you that this process was [coming about]?

JEROME KARLE: Yes. In fact, when I was travelling out to Wood's Hole to talk to someone about a possible position at Brown University now that I think of it—I knew there were some other [universities]—I met an admiral on the train who was quite conversant with these intentions. He undoubtedly was concerned [with] the office that was responsible for research during the war—naval research during the war—it wasn't ONR [Office of Naval Research] yet. He was telling me about the plans.

VAN KEUREN: It wasn't Admiral [Harold G.] Bowen, was it?

JEROME KARLE: I don't know. I don't remember. I think it might have been.

VAN KEUREN: But it was well known within the Navy.

JEROME KARLE: Yes. [That these plans were well known.] The plans were very much afoot already.

VAN KEUREN: Did anybody else mention it to you? Did Zisman talk about it? **<T: 20 min>** Did Friedman?

JEROME KARLE: Yes. It was part of the discussion in encouraging us to come here.

BOHNING: Chronologically, where did your discussions with this facility fit in with your interviews at the universities? [Did they all happen] at the same time?

JEROME KARLE: The interviews with the universities [. . .] began earlier. I worked for NRL from '44 to '46 at Michigan, and I think that by about '45 when hostilities looked like they were going to be coming to a conclusion in the not-too-distant future, I began to have some contacts with universities. I started to talk to them. And the development of the opportunity at NRL, I would [think], came at least six months after I was making contact with universities.

VAN KEUREN: They initiated that contact?

JEROME KARLE: [With the universities], I initiated the contact with Brockway's help. But it was mostly people with whom he was acquainted. He [wrote letters] of recommendation [for me], and I [visited the schools to] find out if there was anything available. This did not [arise from universities] advertising that they had positions open. [. . .]

ISABELLA KARLE: At that time, as Jerome mentioned earlier, universities did not know that they were going to be expanding much, and they were also obligated to hire back a number of their faculty that had left temporarily for various defense-oriented laboratories. So that it was

not at all easy to get a faculty position. It was only a few years later with the G.I. Bill of Rights and [ONR's] support for research that universities started expanding.

JEROME KARLE: [There was an organization that preceded the establishment] of ONR. In fact, it was going strong all through the war.

VAN KEUREN: Vannevar Bush was the head of it?

JEROME KARLE: Yes.

ISABELLA KARLE: But it seemed, in thinking back, that it wasn't until almost 1950 before there was this big explosion in universities.

JEROME KARLE: In the fifties.

ISABELLA KARLE: Well, in the forties there still wasn't much—between '46 and '50.

JEROME KARLE: It didn't build up all that fast. There were the pressures—the pressure developed certainly in the late forties, because people were going to school on the G.I. Bill. So what these schools did was to build a lot of quonset huts and [other temporary buildings] to house people. [In many universities] in the fifties and the sixties, the population went from twelve thousand students to forty and fifty thousand students. [Much campus growth took place.]

VAN KEUREN: Were you interviewing for other jobs also at this time?

ISABELLA KARLE: No. I wasn't. One of the reasons was that I suppose we figured that it was more important for Jerome to get a more permanent position somewhere, since we were intending to have a family. In fact, we had our first child in '46. The other was that I was still teaching six days a week at Michigan, and there was no one to substitute, so we had planned that if he could get settled, and hopefully in a place or a city or an area where I could also find some research to do, it would be quite convenient for me then to have everything arranged and [go there].

JEROME KARLE: It was really virtually impossible in a university setting.

ISABELLA KARLE: If you go to a place like Michigan State, for example, there was the university and, since the two of us could not work together at the university at that time, there was no other opportunity for any other research position. **<T: 25 min>** It obviously had to be in a larger city that had more than one research institution.

BOHNING: Had you considered any industrial positions?

JEROME KARLE: Yes. Negatively.

BOHNING: Did you visit or interview with any?

JEROME KARLE: No. I would have come close to giving up my profession rather than work for industry.

VAN KEUREN: Why did you feel that way? Can you elaborate?

JEROME KARLE: Yes. Because I felt—perhaps not altogether legitimately—I felt at the time that most industrial laboratories did not do science; they did things simply for profit. And that isn't altogether true. I think I certainly would have accepted a job if it were available in a laboratory such as G.E. [General Electric] or Bell Labs, for example. So I am sure that I would have bent that much, but I didn't feel that all the struggles and the [sacrifices] for what I thought [would be] a constructive life, would have been [justified] by trying to make the next compound which is going to make [X] amount of dollars for somebody and that [might have been] the only reason for doing it.

ISABELLA KARLE: In fact, I think there was about the largest chemical concern that came looking for [people]—interviewing people at the University of Michigan—and in their brochure they had stated that if you are successful as a bench chemist, you can be promoted into sales. [laughter]

JEROME KARLE: This was, really, the ultimate turn-off. This was about when I was getting my degree and finishing my work in '43. They [had a] brochure that said this is your reward, to get into sales if you were successful [at the bench. That] was the image that I had. [It was a very large] corporation.

VAN KEUREN: Do you recall the name?

JEROME KARLE: Yes. I know the name. [laughter] But I have some good friends there.

There was another one that was almost as big that was actually very interested in interviewing us. A man from that company was good friends with Brockway, and he knew us [from] visiting. He was quite interested in attracting us there. We did not even interview. I might have been a little bit strong in my feelings at the time, but that is the way I was. [. . .]

BOHNING: In your negotiations with NRL did you make trips here, come down here?

JEROME KARLE: I did. I came down once. I spoke to—well, I came down more than once, but the first time I came down it was just in connection with the project that I was doing at Michigan, to visit with Zisman. But I remember coming down here once to talk [with] Herb Friedman, and also Herb Friedman was travelling with Zisman once when he came up to Michigan, and he met us there. I don't know if you recall?

ISABELLA KARLE: I don't recall that. I recall that I came down here once because I was in Zisman's laboratory, and that was before I worked here.

JEROME KARLE: [. . .] But in any case, yes, there had been some contact with the principals involved in the hiring process.

[END OF AUDIO, FILE 2.3]

ISABELLA KARLE: [But I think that was the only trip that I made. As I] mentioned, I was already pregnant at the time and teaching six days a week. There was not much opportunity for travel.

BOHNING: Did your negotiations make it very clear that Isabella was included as part of the [deal]?

JEROME KARLE: Yes. It was, as one might say, crystal clear.

VAN KEUREN: Did you discuss what [kind] of research you would be doing at NRL?

JEROME KARLE: Yes.

VAN KEUREN: What were the discussions like?

JEROME KARLE: I'm sure that they concerned the point that we had plans for enhancing the techniques of structure research by gas electron diffraction and that we would need to build an apparatus for this and also pursue theoretical aspects. This was all quite acceptable. In fact, one of the things that I found extremely attractive about NRL at the time, was that they had a first-rate machine shop which was at peak capacity, in a sense, because of all the work they had done during the war with excellent machinists. And I knew that they could really build the kind of an apparatus that I wanted, and that made me feel very good.

ISABELLA KARLE: They also had a good vacuum tube shop, as they called it at the time, and helped us design our circuits [. . .] for the very accurate [measurements we needed]. We did very well in measuring.

JEROME KARLE: Yes, we were lucky that all we had to do was measure the voltage, set up the currents, beams, the very well-controlled currents . . .

ISABELLA KARLE: Very well-controlled currents. Measuring the vacuum, [. . .] the high vacuum.

BOHNING: Could you tell us something about those job facilities—the personnel . . .

JEROME KARLE: The people in the machine shop were sort of vague to me because you would operate through their foreman, and although you could come and talk to them a little bit, it was almost a fleeting visit. You didn't get to really know them very well. At the time, there was a Navy commander who later became the commanding officer here at the lab, whose name was Brad Bennett. [He] took a personal interest in seeing to it that the apparatus would get made and [would get made properly]. It was a great thing.

ISABELLA KARLE: In fact, it was he who suggested, "Make two." And that was very convenient because when I was putting together the apparatus there were two of each piece. If

one had a small leak in it—we had to test everything for their vacuum abilities and so forth—or didn't quite fit, the other one did, so we had one apparatus running much faster than . . .

JEROME KARLE: Much sooner than it would have otherwise, if we had to wait for all of—

ISABELLA KARLE: All of the quirks to be—

JEROME KARLE: —straightened out. So there was great support, right up to the man who was in charge of the machine shop. Public Works actually was what he was in charge of, and the machine shops came under his purview. He was extremely helpful. People in the shops, as we mentioned, were [extremely] talented, and we got the apparatus put together in record time.

BOHNING: How long was that?

JEROME KARLE: Well, I think that everything—the drawings, and everything else and the machine—was developed over the period of about a year. That was great.

ISABELLA KARLE: We were already publishing papers in '48, I think.

JEROME KARLE: In '48. We came here in 1946.

ISABELLA KARLE: Well, I didn't come until the fall of 1946.

JEROME KARLE: And we were giving papers already.

ISABELLA KARLE: And we have publications from 1948 using the instrument, and that was really record time. And we didn't have a large group of people working with us.

JEROME KARLE: We just had one man, [and he] was excellent. [He helped] us to put everything together for [. . .] testing and [finally diffraction experiments].

ISABELLA KARLE: The high-voltage testing.

JEROME KARLE: [He helped with the electronics] and everything else. <**T: 05 min**> I don't know how we had time for everything we did.

ISABELLA KARLE: And we had a new baby.

JEROME KARLE: At any rate, everything was fulfilled and the atmosphere in the Optics Division that was created and maintained by Ed Hulburt was just[wonderful]. He was what [one] would call an academic person and [. . .] an excellent researcher on his own. [. . .]

VAN KEUREN: Did you have much contact with Hulburt?

JEROME KARLE: Yes. We used to have nice friendly contacts with him. He was interested in many other things besides optics and amongst them was marine biology. I had a graduate degree in biology so we would have some very nice conversations about [subjects] that were quite distant from optics. Conversations in general.

ISABELLA KARLE: We had a coffee meeting every morning—informal, social. [Richard] Tousey, who was then head of some spectroscopy division, and [John A.] Sanderson, and a good many other people who then moved over to the new building, which is an old building now—the Upper Atmosphere Research Building. [They] were all here on the second floor.

VAN KEUREN: Atmosphere and Astrophysics?

ISABELLA KARLE: That's right. A and A.

JEROME KARLE: All the people in the Optics Division were quite [friendly].

VAN KEUREN: What was the research atmosphere like?

JEROME KARLE: Oh, it was first rate. Just what I described. It was not overly organized by any stretch of the imagination. It was obvious from our research program and the things that we were doing that [we were] not closely supervised. [. . .] It was clearly stated here that the point of view was to find good people and to give them the opportunity to do their work. That is exactly what was happening. It showed in the productivity in the divisions of the [Laboratory].

ISABELLA KARLE: By the time we came there was already another person here—[Laverne S.] Verne Birks—who was interested in the electron microscope and [applications of] X-ray optics of various kinds, and he had made quite a reputation for himself. He retired a number of years ago already.

JEROME KARLE: Maybe four or five years ago.

ISABELLA KARLE: But he was one of the young people here [when] we came. Eventually this group [was] broken up into three pieces when Friedman became much more interested in the upper atmosphere research and got the—

JEROME KARLE: He had [an entire new] division established here.

ISABELLA KARLE: Oh, yes. And then Verne Birks's activities remained down on the second floor, and we moved up here into all separate groups.

VAN KEUREN: There was a coterie of people at the Lab interested in X-ray research, and this included Birks [and Friedman], and [Robert Franklin] Mehl, who was in metallurgy.

ISABELLA KARLE: We did not have that much contact with him.

BOHNING: You could, then, feel free to direct your own work and not have someone—did you need to get approval as you made steps along the way?

JEROME KARLE: It was very casual.

BOHNING: You didn't have a lot of administrative paperwork to worry about, or anything?

JEROME KARLE: No. Nothing to waste time. [. . .]

VAN KEUREN: Was there any expectation that the research you were doing would eventually be of use to the Navy? Was that ever stated?

JEROME KARLE: That the work would be relevant?

VAN KEUREN: At some point in time. What was the feeling as to applied versus basic research?

JEROME KARLE: Formal statements with regard to relevance didn't come into the picture until [outside pressure] got into the act at the end of the sixties. Everybody understood that this was a Navy laboratory and that ultimately **<T: 10 min>** the work would be directed towards Navy interests. But, it was also understood that you get farther and do a better job if you can carry the work out initially in a very broad context. And so, my feeling from being here is that there was always a large amount of work that had a very specific purpose and perhaps fairly short-range goals, but there was also the understanding that there were other kinds of projects that could benefit from the long-range point of view and just from the general development of fundamental science. We had the good fortune to carry out a good deal of our work in that context. There were opportunities all along the line in our work and in other people's work that was carried out on this broad basis to make a contribution to specific Navy problems and, of course, those were never overlooked. But at the same time, the atmosphere for us was not so constrained that we didn't dare take on a big problem without necessarily seeing every step to some ultimate application. Just that this was important science and that if it came through, why then its implications and applications would be very broad and very generally useful. And we appreciated that, and it was the atmosphere, for the most part, in which we worked.

VAN KEUREN: Would you say that was characteristic of your division, optics, or was it characteristic of the Lab as a whole? Do you have any feelings on that?

JEROME KARLE: I really can't say. I was never that deeply involved in the Lab as a whole, but from what I was able to see, there was always [. . .] a certain percentage of the work that seemed to be very deeply basic and very much fundamental and whose implications would have to be long-range.

ISABELLA KARLE: That was certainly true of optics. We didn't have that close contact with very many—of course, we knew people around the Laboratory, but with the workings of the other divisions.

JEROME KARLE: But at the same time, to a very large extent it was obvious that the needs of the Navy were not lost sight of by any means, and there were always many projects going on that had immediate applications.

VAN KEUREN: You suggested that the attitudes towards free research changed in the late sixties due to the intervention of [outside pressures].

JEROME KARLE: I don't know that it was so much that the administration's attitude changed [here], but they were trying to adjust to outside pressures where written statements—

ISABELLA KARLE: —stated goals—

JEROME KARLE: —and much more paperwork had to be put in the form of objectives and implications and applications and so forth. And so there were pressures, and they would vary, and so this gave rise to a certain amount of paperwork and a lot more reviewing. Sometimes you would feel that you were getting reviewed to death. [. . .] This had very funny—I remember one time when the pressures almost forced the lab to run only on the basis of short-range goals—six month, yearly projects and all that. [. . .] And [someone] was telling me—I don't remember [who mentioned it-]**<T: 15 min>—**that they had been down at the Pentagon, and there was the usual turnover of personnel then who were leaving soon, and a new gentleman there was looking at the programs and said, "How come we don't have any long-range goals?" Of course, the answer was obvious, we just didn't have any for a while because they were changed to short-range goals, etc.

I think that the attitude here at the lab has [basically] really always been fine, but there can be enough pressures from the outside that make it a little difficult to accommodate the best of all worlds for research and, at the same time, appear to be obeying the rules as they are set down. It is still quite good here.

BOHNING: Was there any effect when the change in administration was split into a Naval commander and a civilian, and [they] had two people running the facility?

JEROME KARLE: It was always like that. [. . .] We always had a Navy leader here. However, it was only into the fifties that we had a civilian research director per se. Before that the heads of the various divisions used to get together.

ISABELLA KARLE: There were ten of them, I think.

JEROME KARLE: Something of that sort. There may have been ten of them, and they had their periodic meetings, and they just kind of ran the lab, and it was done very harmoniously. It was only later on that the administration became more centralized.

VAN KEUREN: I believe it was ’49 when Hulburt became the first civilian Director of Research.

JEROME KARLE: Was it ’49? It was that early? I thought it was in the fifties.

VAN KEUREN: Did you notice any change after that due to Hulburt’s leadership?

JEROME KARLE: Well, you see, leadership in science is not to try to lead, and Hulburt was not a person who would try to lead. [. . .] In those days, [I did not have] any immediate interactions [with Dr. Hulburt]. However, knowing him and watching the way subsequent directors worked, they, for the most part, were just trying to do what was administratively to the advantage of the research institution per se. This didn’t result in directives and various types of interactions that changed the way the way the [Laboratory] worked at all. In fact, I didn’t notice any influence on my research program, and I was just sorry that [Dr. Hulburt] was up front in some office instead of not still downstairs on the second floor where we used to have great sociability. That was the extent of the change.

ISABELLA KARLE: Even after Page became director—I think he was the second one, wasn’t he, Robert Page?—l don’t think we felt any changes were made in the laboratory. It was after , when the third director came and then, by that time, I think the pressures from the outside . . .

JEROME KARLE: Page preceded Alan Berman.

ISABELLA KARLE: No. He did?

JEROME KARLE: Yes. In between there were several other gentlemen. [Oscar] Marzke and this gentleman from Upper Atmosphere [Franz Kurie] who went out to head up the lab in Phoenix whose name escapes me for now. I think that there were either two or three people before Page, and Page preceded Berman.

ISABELLA KARLE: Well, I remember Page best of all of them. He was a very nice man.

JEROME KARLE: He was a nice, friendly gentleman. Again, whatever he could do for the laboratory, he did. He was one of the really instrumental developers of radar, preceding and during the war. A very nice person. He was just very pleasant.

BOHNING: In that first year that you were building that apparatus, had you given thought to the systems you were going to study with that? What were you targeting?

JEROME KARLE: I was publishing a lot of theoretical papers while all that was going on. I was getting all cranked up. We had to do something else. It wasn't just a matter of apparatus. **<T: 20 min>** It was a matter of how you would analyze the better data that you had. I was very much influenced by a paper that Peter Debye had written in '41 that I read avidly while I was a graduate student. What he had in there was a type of mathematics that would be applicable if somehow or other you could do some interesting things to the data that you obtained from experiments. Amongst the things that you had to be able to do was to transform the data that you got from real atoms into the data that you would have obtained had all the atoms had their scattering power concentrated in a point. And there were some other things that needed to be done. I was very busy working out the techniques for data reduction, and also it was necessary to embellish that theory because it assumed that you had an infinite amount of data which [could not be collected]. That was a classic problem. There was actually another problem. [The interference scattering had to be separated] very accurately from the background scattering. It turns out that in solving that problem, I saw how to do the crystal structure problem. It was that development that actually led to the solution to the crystal structure problem. [. . .] The first paper that Isabella and I wrote on gas electron diffraction structural work had in it a particular criterion which could be generalized to crystal structure analysis. [. . .] We had many substances in mind [that we were intending to investigate by the new techniques for structure analysis].

ISABELLA KARLE: If I may interrupt at this point. What we did, in these early papers—and, of course, later ones—actually these papers are considered classics now, from '48, '49, '50—was that we not only were able to establish how the atoms were related to each other in a gaseous molecule, that is whether they were arranged tetrahedrally or triangularly, or whatever, but we were also able to determine how much each one wiggled, the vibrational amplitude of each one. As you can imagine, in order to be able to do that, in addition to the theoretical work that [Jerome] had done and that Debye had set up the fundamentals for, we had to be able to take the data off the photographic plates and transfer it into numbers in a very accurate fashion. And we came out very well in our measurements, and that had started a whole new era in the electron diffraction of gaseous molecules. We had started out very simply with carbon tetrachloride [and] carbon dioxide, and then went on to benzene. Although these materials had been analyzed by electron diffraction previously, we got much better results along with the vibrational measurements of each atom, and then we went on to the more complicated molecules after that.

JEROME KARLE: We were the first ones to establish the techniques [for quantitative analysis of the diffraction patterns].

ISABELLA KARLE: And that indeed we could get the correct vibrational motions from them.

VAN KEUREN: What was your working group? The two of you were working together. Was [Herbert A.] Hauptman working with you after ’47?

JEROME KARLE: He came towards the end of ’47, and it was just about the time when we were seeing through how to handle gas diffraction data. It was in’47 that we saw this. There was a certain criterion that we could apply, a mathematical **<T: 25 min>** criterion that was called non-negativity. It arose from [something that Debye showed]. If you did [a number of] good things to the data [accurately], a certain mathematical manipulation would give you the probability of finding interatomic distances in the molecules. [Evidently, probabilities have] to be non-negative. A negative probability means nothing. Zero means [no chance, plus one means certainty and you have] everything in between, so it is a non-negative function. [We perceived that non-negativity would be useful and] made great use of [it].

This worked out so well—surprisingly well—that I started to look around for other applications. It was obvious to me that there may be another application in the crystal structure problem because what actually represents a structure is the electron distribution around atoms that [is obtained from X-ray diffraction analysis]. The greatest electron density is in the center of an atom so that you know where your atom is if you know where the highest part of the electron density is. Now, the electron density is also a non-negative function. Negative densities don’t mean anything. You either have no density or something that builds up to a positive density, and this is the idea that stimulated [us]. It seemed [to be a] rather ideal [problem], since Herb had joined us, and he had mathematics in his background. He was primarily a mathematician. It seemed to me that this would be a great project for us to pursue. It was very long-range because we couldn’t see any practical solution in sight when we first started it. At the same time, we knew that if we could solve this problem, it would have very broad implications. [It is in that context that] I talk about not having short-range goals or immediate applications. But to know or to see that if you can do something which is worth doing, and which we also knew—made an analysis of—we knew that we had enough information [to know] that the problem had a solution. In fact, it was tremendously overdetermined. It is not just borderline or hairline, but very much overdetermined. Whether it was too complicated for us to solve, this we didn’t know. But that it was rational to do it, and with important implications, this was obvious to us. And we had the opportunity to do that here at NRL, [as] the atmosphere that I have been talking about was so favorable for doing research. So that was it. We decided we were going to get into the business of the mathematics of non-negativity and the work went on.

[END OF AUDIO, FILE 2.4]

ISABELLA KARLE: Herb Hauptman was not involved [. . .] in any of the electron diffraction work. People who were, were John Ainsworth, who had worked with us in the early days.

JEROME KARLE: [. . .] John Ainsworth.

ISABELLA KARLE: And David Swick was here for quite a number of years.

JEROME KARLE: He came a little later.

ISABELLA KARLE: Yes. But he was here doing the electron diffraction work.

JEROME KARLE: Hoober? What was his first name? He was a draftsman who drew up the plans for us.

ISABELLA KARLE: Yes, but he didn't do the experimentation.

JEROME KARLE: He was not an experimental scientist.

ISABELLA KARLE: He was a draftsman

JEROME KARLE: [. . .] The last name was Hoober.

ISABELLA KARLE: Daniel.

JEROME KARLE: Daniel Hoober. But as far as scientific people at first [are concerned], I guess John.

ISABELLA KARLE: John Ainsworth was the one.

BOHNING: What was his background?

JEROME KARLE: He was a physicist. [. . .]

BOHNING: Did you know Hauptman at City College?

JEROME KARLE: Not really. When I saw him, I remembered his face. He may have been in some of my classes that people had to take—general English, science survey or something. But I didn't know him.

BOHNING: [Not so far as] your paths had crossed previous to NRL.

JEROME KARLE: No. He wasn't an unfamiliar figure, but I don't know that we ever had any conversations.

VAN KEUREN: [. . .] How did you divide up the work in your research?

JEROME KARLE: It just [fell] naturally into place. People do what they like to do and what they [do best. When people collaborate, they converse a great deal, examining ideas].

ISABELLA KARLE: Most of mine was from the experimental part, working in the laboratory. Most of his was pencil and paper.

JEROME KARLE: It is very important to point out that the crystal structure problem is one in which there is a very big gap between the theoretical work and the experimental application. In fact, this comes into an area which is largely overlooked in science. I have written some articles about this, something that I and some other people call "bridging," which is an obvious term. A simple statement of what bridging is, is to manipulate and accommodate the mathematics so that it can be applied to experimental data and at the same time to manipulate the experimental data [. . .] in such a way that it becomes applicable to the existing theory. This required a great deal of work so far as the mathematics was concerned. What I mean by that is that there are any number of ways of writing down the mathematics which states the answer in terms of the measurements. But you can't get anywhere with it. That kind of mathematics is not amenable to any of the analytical techniques or the size of computing machines that exist in this world. So you have to use it in such a form and in such a way that progress can be made. On the other hand, data always come in the form that is not quite what you want and so you have to do many

things to it and do it carefully to get it into the form in which you want it. As a matter of fact, **<T: 05 min>** one of the things that is important to do is the same thing that we were doing with the gas diffraction: to manipulate the data in such a way that it acts as if it had come from point atoms instead of atoms that had electron distributions around them. That was just one small point. And then the math was still so complicated that you had to proceed through it in probability ways.

ISABELLA KARLE: If I may interject a point before you go on. Data [are] always limited and the mathematics assumes that there is infinite data.

JEROME KARLE: That's another one of [the cute] little aspects. Now, what we found was practical was to go through this in a stepwise procedure making use of probability measures and finding out just how to tiptoe through that and not get off on paths that lead you to oblivion. [That] was a very, very difficult thing. [. . .]

Isabella made very important contributions to the practical aspects and to the bridging. This has been totally lost on people who are not familiar with our work because the award went to Herb and me. But it is very well known amongst our colleagues, the contribution that she has made to this. I just wanted that to be in the record.

BOHNING: If I am correct, that first paper with Hauptman was in '53. Was that it? Were there others?

JEROME KARLE: No. There were others. This is what happens in newspapers. The first paper that really established everything was written in 1950.

ISABELLA KARLE: Published in 1950.

JEROME KARLE: Right; sorry about that. The results—and you can see how feverishly we worked in those days—the results were actually announced at a meeting early in '49. We already knew all the mathematics that was basic to this field. Now its aspects had to be developed, and it took about six more years to do that. The probabilistic aspects had to be developed. We had to learn which of the infinitely many formulas that we had in there were the ones to use and how to relate them to the probability [measures], etc. But the key paper was one that was published in 1950 and, as I say, announced at a meeting. [. . .]

BOHNING: What meeting, for the record?

JEROME KARLE: This was a meeting of the Crystallographic Society of America. I think I still have the abstract from there. They published the abstract of their meeting in their own journal. It might have been the Mineralogical Society of America, so let me make absolutely sure. There was no American Crystallographic Association in those days. [. . .] I don't have the publication, but I have a copy from it. It very clearly states what the results [were]. This was, I believe, in April of 1949. We probably [completed the problem by] end of '48. So that was really the basic paper, and everything was a development from that. The '53 [publication] was a monograph [in which we developed] the probability measures that were associated with the formulas that already came from [the 1950] paper. [The 1950] paper was a statement of what happens if you take advantage of the non-negativity criterion. <**T: 10 min**> You use something called Fourier series to represent the electron density in a crystal. [The 1950] paper spelled out the conditions for non-negative Fourier series, and the conditions were in the form of inequalities—not equalities but inequalities. [We put them in a form for crystallographic application. The first work on non-negative Fourier series was published by Toeplitz in 1911.] The inequalities also had probabilistic characteristics that made them useful for a complicated structure. Somewhere I have a copy of the 1949 abstract.

ISABELLA KARLE: I couldn't find that at the moment.

JEROME KARLE: I don't think it's filed, but I have it in a folder. I know where to look for it. I can get it for you before you leave because I think I can find it.

That's when we already knew what to do with [the mathematical problem]. You don't know practical aspects because you have to not only develop the mathematics into a form where it becomes eminently useful, but you also have to bridge the gap [between] what you get from an experiment and [the appropriate form for the data for use] in an analytical procedure.

VAN KEUREN: So the initial basic work was done in a two year period, from '46 to '48?

JEROME KARLE: Yes. Everything was actually downhill after that. We saw through an awful lot in those few years.

VAN KEUREN: How did your colleagues react to the initial paper? Was there any response?

JEROME KARLE: Oh, there was response. It was one of the most entertaining types of meetings people tell me that they ever went to—[meetings of crystallographers]. Most people were intelligently cautious. There were those who felt [that] they were really being treated to pie

in the sky, and there were a few who developed hostility. Those were the ones who were the most outspoken at the meeting.

There was good reason to think that what we were telling them was untrue. What you had to get out of the mathematics were phases, and every physicist knows that you never get in an experiment—or, at least, not an ordinary experiment—phase information from measuring diffraction patterns. All you do is get certain intensities. Actually, the intensity is associated with how high an X-ray wave oscillates as it goes along its wavy path. The phase, however, has something with to do with what part of the oscillation the beam is at as it is scattered away from [an imaginary plane cutting through the crystal]. And that just disappears. All you really get is an intensity which is proportional to the square of the amplitude at its highest point in the oscillation of the wave. So you don't have information, and here are some crazy people telling us that they can find out what the phases are by looking at the amplitudes. [. . .] The physics background made them just absolutely incredulous.

The reason why you can actually do this, though, is because you can set this problem up in terms of algebraic equations [based on discrete atoms] in which it is perfectly obvious that you have more information than the unknowns, and among the unknowns are the phases, [the problem is overdetermined]. It is not obvious that phase information is very subtly contained in the intensities.

ISABELLA KARLE: In actual fact, [the theory] was just not applied by any of the experimental X-ray people, [except Howard Evans, Charles Christ and Joan Clark at the Geological Survey], and so a number of years later Jerome decided that **<T: 15 min>** I should collect some X-ray data.

JEROME KARLE: We developed the theory in the first half of the fifties, and Isabella started collecting data in the second half. There were some few labs—very few—who were doing a few things, but they were only very special types of crystals—those [with a] center of symmetry. [Analyses of] crystals that did not have a center of symmetry [were] completely out of reach. It was her work done in the sixties with complicated noncentrosymmetric crystals that really established [our work]. Later on, at the end of the sixties, people really took up [the procedures in earnest]. For the most part, [however, NRL] was always at the forefront in the development, and the particularly hard structures were done here.

So, there—first it was impossible, and then it wasn't impossible in principle, but it would be impossible in practice, and that's about the way it stood [in the early fifties]. It behooved us to show people that it could be done practically. I didn't mind that. I think that they had legitimate reason to be doubtful about the practicality. I was myself. But there was no question about the principle, and that's what all the excitement was about.

[Our early work] was not greeted with unbridled enthusiasm. But [there was another positive aspect to] NRL. They knew that we weren't given to frivolous activities so we didn't

have problems [simply] because our work was not well-accepted. In fact, we continued to be encouraged. That was good. I think if I were trying to get tenure in a university, I would have had some real troubles, especially if they asked the opinion of peers: "That insane character." [laughter]

ISABELLA KARLE: It appeared in print. We have a number of articles to that effect.

BOHNING: Really?

JEROME KARLE: Oh, yes. But being attacked in print has very interesting consequences. I had no reputation in crystallography, and what was being attacked was the 1953 monograph that established the probability characteristics very well in the mathematics that we were working with. One of the more hostile colleagues wrote a book review for *C&E News* that just said from one end to the other that this is damn foolishness. Since I had no reputation in crystallography, I was never asked to review papers for publication. As soon as that review came out, and they spelled my name correctly, I began to get papers to be reviewed for publication. So the notoriety had an immediate payoff. I now had a reputation as a crystallographer. [laughter] This is true.

BOHNING: Do you think this might be a good point to end? I'd like to thank you again very much for your time and energies this morning in sharing more of your career with us. Thank you.

ISABELLA KARLE: You are quite welcome.

[END OF AUDIO, FILE 2.5]

[END OF INTERVIEW]

INTERVIEWEES: **Isabella Karle**
Jerome Karle

INTERVIEWERS: **James J. Bohning**
David K. Van Keuren

LOCATION: **Naval Research Laboratory**
Washington, District of Columbia

DATE: **9 September 1987**

BOHNING: [. . .] Today is the 9th of September 1987. This is Jim Bohning with David Van Keuren talking with Dr. Jerome and Isabella Karle, continuing previous interviews. When we finished our second interview we were talking about some of the early difficulties that you faced in the acceptance of your early work by your colleagues. I wonder if we could pick up at that point and continue. Could you give us some more information about troubles and problems that you faced at that time?

JEROME KARLE: Yes. Well, we never regarded the problems as serious. The positive side was that NRL was very supportive, and though they were perfectly well aware of the fact that in the crystal structure area our new work was not accepted with great enthusiasm, they nevertheless continued to support us well and were not themselves concerned about this. That was a very excellent circumstance in which we found ourselves, and one that has always been appreciated by me. I have had occasion as time went by, to consider what the situation might have been for me if I were at a university still worrying about getting tenure.

There was a very vocal opposition, but it was limited to a very few people. The attitude of most others was what good scientists really do, and that is that if they feel as if they don't have the time to devote to make detailed studies of the sort that we were making, or felt that it was the kind of thing that they weren't prepared to do, they'd just wait and see what happens. And although there was a certain amount of opposition, we found that among several of the leading people in the field [there was good support of our] new work, and they treated our papers, which they reviewed and identified themselves in so doing, as new and worthwhile research. We did not run into any serious problems with regard to publication.

In fact, we did not run into any significant problems. About the only thing that happened to us—and it happened to everyone—was that when we published in an appropriate place, *Acta Crystallographica* at that time, they took an exceedingly long time to publish papers. It could easily be a year or more between the time the paper was submitted and it finally appeared in print. That again is a minor consideration.

If there were any feelings of frustration at all, they derived from the fact that we were not interested at all in having <**T: 05 min**> angels-on-a-pinhead-type debates with anybody, in terms of will it work or won't it work, and so forth. This is ultimately something that can be tested, and we were anxious for it to be tested. In that respect, around 1953 and '54 we had colleagues at the Geological Survey who were interested in making applications. They had some minerals that were causing them some difficulty in analysis. It turned out that we did make some applications. They were not so difficult as problems that they really stressed the methodology. But it was nevertheless heartwarming that these problems went through very readily. They were easily solved.

As time went on we realized that we had to do things ourselves, and that was when Isabella took up X-ray diffraction—about 1956 I think. She got a good elementary book on the subject that told how to take diffraction photographs, index photographs, collect data and so forth. It wasn't too long before we had our own experimental laboratory here, and were able to start turning out structures that were significantly difficult.

ISABELLA KARLE: Very significantly difficult.

BOHNING: Which structures had you targeted in those early days, to work on?

JEROME KARLE: The plan was to do things that would show the features of the method and at the same time help us with developing the practical aspects. Again, I would stress that this was not a simple matter of substituting experimental data into theoretical formulas. The theory concerned certain nebulous aspects like infinite data and point atoms—all kind of things which do not exist in the real world.

At the same time, the real world involved certain limitations on the data, ways of collecting the data or certain inherent features of the data [such as] real atoms instead of point atoms, some [aspects that required adjustment], a variety of [adjustments]. Then there are the standard corrections that need to be made—things called Lorentz and polarization corrections are normally made on X-ray data. So a certain amount of manipulation was required. [. . .] Beyond that it [was] trickier. The procedures had a probabilistic characteristic: they were step-wise, and it was necessary to follow paths of very high probability. Tempting paths of lower probability were always there, and a methodology had to be developed to make sure that one would optimize <**T: 10 min**> the procedure. Very many attempts were made [. . .] by people who wanted to pick up on the method. [These attempts were generally] characterized by taking [. . .] advantage of the easier low probability paths. These more often then not did not succeed, and [that implied] in people's minds that the procedure was not working. It was really very tricky.

There were other things—I think I covered them in the past—barriers that had to be overcome. For example, for noncentrosymmetric crystals, the [desired] phases [. . .] can have an

infinite number of values between 0 and 360 degrees, and the procedures did not permit that. What do you do? A problem like that had to be solved. Fortunately, there was a good solution to it. What happened was that there was a period of about four or five or six years in which we developed applications of the original mathematics by making adjustments in the math, making adjustments in the experimental data and, as a consequence of that, we were led to quite a general scheme for carrying out structure determinations. [. . .] It took a period of about four to six years to develop.

ISABELLA KARLE: If I may interrupt at this point. We've already mentioned this in the past, but you have to remember that there were no large computers at the time. Most of the computations were done by hand or on a desk calculator, except for the very few computers. IBM was just beginning to have its larger ones in New York City, where we had to send our data for some of the calculations. [It] took several months before we could get the results. And it cost an awful lot of money.

You asked earlier what kinds of substances we tried at the very beginning. Those were dictated, more or less, by crystals that were available—colleagues, [for example], who had materials that they did not solve by existing methods. One of them was a mineral called spurrite. That was an unusual kind of mineral in that it was both a silicate and a carbonate. It occurs at boundaries between siliceous rocks and carbonaceous rocks. Looking back at it now, it seems like a fairly simple thing. But it has five calcium atoms, two silicates and a carbonate, all in general positions in the unit cell. That made it [very] difficult for the procedures as they existed at the time. [. . .]

The important lesson in that particular structure analysis was that some of the formulas that we were using were [. . .] interfered with because of the pseudosymmetry. However, we found **<T: 15 min>** a scheme to overcome pseudosymmetry. Another very early structure was given to us by a colleague from Bell Laboratories. That was p,p'-dimethoxybenzophenone. It was actually more difficult than what the formula indicates. It doesn't have any [disproportionately heavy nonhydrogen atoms it just has] carbon, hydrogen and oxygen atoms [. . .] and there were two independent molecules in the asymmetric unit. So the problem was actually twice as large [as it might have been. It] proceeded very smoothly, no problems at all. [. . .]

JEROME KARLE: We did do some tricks to get negative phases, if you remember.

ISABELLA KARLE: That's true.

JEROME KARLE: We treated it as if it were a [triclinic crystal] and used a sigma-3 formula to get negative phases. That was the first use of quartet formula to get negative phases. [. . .]

ISABELLA KARLE: Both of these materials belonged to the general class of centrosymmetric crystals which are considerably easier to derive phases for than noncentrosymmetric ones. And so we must have solved a few more centrosymmetric ones. But soon we were intrigued with noncentrosymmetric materials. [. . .] Most natural products fall into the noncentrosymmetric case. They are all handed molecules, and usually one hand is preferred over the other in nature. They crystallize in noncentrosymmetric space groups. The first noncentrosymmetric structure we [solved] was arginine—the amino acid arginine—and there, as Jerome has already mentioned, we had to address the problem of how to represent phases that could have values anywhere from 0 to 360 degrees. We decided to approximate them by 90 degree intervals. That was, I think, quite a breakthrough, because as long as we had phase [values] in the correct quadrants, [the structure would be sufficiently well represented as a first approximation for location] of the atoms. This was not [. . .] expected, but it was [fortunate] because it allowed us to proceed rather readily with noncentrosymmetric structures.

JEROME KARLE: Once we had the phases in the right quadrant we could then refine [their values].

ISABELLA KARLE: After arginine, which was a fairly small molecule, [we] became quite adventurous and [obtained] catalogues of natural products. [We looked for larger structures to investigate]. We proceeded then to reserpine which had about forty-five atoms in it, and I'm trying to think of the <**T: 20 min**> other early ones—panamine.

JEROME KARLE: Well, our colleagues doing organic and biological chemistry became quite interested in the possibilities of helping their chemistry with structural research, and for the most part, the substances that we worked on were substances of considerable interest from a chemical point of view and [. . .] applications that would be relevant to the program here. [Often], they happened to be substances of interest to colleagues, and so it was very easy for us to get numerous samples of materials that were involved in first-rate research programs. That helped us, and it also helped our colleagues.

BOHNING: Were these colleagues here, or were they scattered around?

JEROME KARLE: They were scattered all over the world.

ISABELLA KARLE: Actually, Dr. Bernhard Witkop at the NIH [National Institutes of Health] was one of our early colleagues, and [we] collaborated with him for many years. Through him, I met many other people—met not [always] face to face, but by the mail, from around the world. There were many Japanese people who were interested in

photorearrangements in [physiologically active] materials. [We have had a long, continuing collaboration on antitoxins with] Professor Wieland, Theodor Wieland, in Heidelberg. [. . .]

JEROME KARLE: Huisgen.

ISABELLA KARLE: Yes. Rolf Huisgen. He synthesized by various rearrangements all sorts of strange materials. [He had a good insight into the chemical formula for some of them] but still wanted confirmation. For others, he had little idea [concerning their structure]. Those were really quite interesting problems. All we knew was how many carbons, hydrogens and nitrogens to look for, but we had no idea [as] to how they were connected.

JEROME KARLE: Various natural products like frog toxins involved Japanese colleagues also.

BOHNING: Was there any particular molecule or piece of work that was a benchmark that firmly established the methodology, or did it just slowly accumulate?

JEROME KARLE: I think that the first structure that we did for noncentrosymmetric crystals, the arginine dehydrate, was kind of a benchmark problem. I would say that both things happened, [benchmarks and the evolution of methodology, because] we learned as we went along. [. . .] A key development so far as noncentrosymmetric structures was concerned was something called partial structure development by use of the tangent formula. The tangent formula in this case is a special kind of a phase-determining formula. The basis for its significance is that **<T: 25 min>** it is quite usual, particularly with noncentrosymmetric crystals, to only get part of the structure as a consequence of the phase determination. It was very valuable to have a good scheme [for developing] the entire structure from knowledge of the partial structure. This is something that developed as we did noncentrosymmetric structures.

ISABELLA KARLE: There is another substance, it is one of the most quoted ones in the citation index collection. It was a cyclic hexapeptide.

JEROME KARLE: Cyclohexaglycyl.

ISABELLA KARLE: Yes. With six glycines in a ring, and there were four different conformers in the asymmetric unit. Although this was a centrosymmetric structure, because glycine doesn't have a hand. This was published when? . . . in '63. That structure [still creates interest. The structure had] about a hundred [nonhydrogen] atoms that had to be located.

JEROME KARLE: [There were] actually ninety-eight nonhydrogen atoms.

ISABELLA KARLE: Independent ones.

JEROME KARLE: Independent ones that had to be found. And after people saw that structure, they were [generally convinced of the efficacy of the methodology].

BOHNING: During this period, were you [. . .] not only publishing, but were you active at meetings presenting your work verbally?

JEROME KARLE: Yes. We didn't hesitate to let people know that the scheme was working. It was a curious thing; I won't mention one laboratory, but someone who was working as a young assistant—perhaps a postdoctoral in the laboratory—told me later on that the man at the head of the laboratory was absolutely convinced that we were using some kind of a Patterson method to solve the structures and were not telling the truth [when we reported] that our scheme for phase determination was working. [laughter]

VAN KEUREN: You said, Isabella, that you took up X-ray diffraction studies in '57, was it?

ISABELLA KARLE: It was in the middle fifties.

VAN KEUREN: Could you give us more background on that?

ISABELLA KARLE: By that time, Jerome [realized that it behooved this laboratory to develop and apply the practical aspects of the mathematics that was developed]. I had been doing electron diffraction work here with vapors, so one day he said, "Why don't you do a crystal structure?"

So down on the second floor at that time Laverne Birks had an X-ray generator and some cameras. I borrowed the use of those and, as Jerome said, I [obtained] Buerger's book on elementary crystallography [. . .] and starting out in cookbook fashion, followed the steps, and was able to take the photographs, and index the spots, and read all of the intensities by visually comparing them to a film strip with calibrated spots on it. [. . .] The phase-determining formulas required complete three-dimensional data. That meant reading several thousands spots. [. . .] <**T: 30 min**> One of the interesting features of this was that not too many people collected

three-dimensional data [at that time. Admiral] Tom Owen, who was one of the directors here in the past, [had done] his graduate work at Cornell with [Professor] Hoard. He collected three-dimensional data—very good three-dimensional data—on a particular molecule which he said he carried around with him for about ten years at his various duty posts and was trying to solve when he had spare time.

[END OF AUDIO, FILE 3.1]

About a week before the change of command ceremony, he came to visit me with these sheets of paper, all dog-eared: "Here's the data. Can you solve this?" Fortunately, he did a very good job in collecting and estimating the data. Too, it was [a very difficult] problem in that the molecule was not at all what the chemists thought it should be. It was an entirely different arrangement of atoms, and besides, [. . .] it was disordered, [. . .] alternately or [. . .] at random, either [of two] orientations. So it took a bit of unravelling. Again, in time, I was able to unravel the structure, so there was a paper by Karle, Karle, Owen and Hoard.[5]

JEROME KARLE: It ought to be mentioned with regard to Tom Owen that [it was impossible to solve the problem] with the tools that were available at the time [he collected the data]. The disorder and the incorrect chemistry made it impossible to even get a start on the problem.

BOHNING: Did he—you said he did at it at Cornell, did he get a degree from that?

ISABELLA KARLE: [Yes, a PhD for his work there that included other studies.]

BOHNING: I see.

ISABELLA KARLE: This particular problem annoyed him because it was unsolved.

VAN KEUREN: This gets into the question of your working partnership. You have a relatively unique partnership, being married and being able to interface your work so very well. Could you talk some more about this? How it has affected your work, and how it has affected your family life.

[5]I. L. Karle, J. Karle, T. B. Owen, and J. L. Hoard. "The structure of C8F12, a saturated dimer of hexafluorobutadiene." *Acta Crystallographica* 18, no. 3 (1965): 345-351.

JEROME KARLE: There is obviously an overlap in our interests, and I suppose if one had to make some estimate, perhaps 25 to 30 percent of the work has been collaborative. On the other hand, Isabella has her own research program. She works on all kinds of materials that I do not participate in, and I have my own research program that she doesn't participate in. I only mention this to point out that it is only partly collaborative. That is not intended to address the fact that in human relationships people always expect close partners to be arguing with each other. It doesn't happen in our case. It is very simple. We don't feel that either of us threatens our status in the world. We respect each other's talents, which are complementary. And I think there is even a built-in resistance, so far as we are concerned, [since] we consider it childish foolishness to bring in the kinds of emotions that do often arise when close partners or married people are competing with each other. This has never occurred, and we wouldn't permit it to occur.

BOHNING: Now, you have raised a family through all of this as well. That is also an interesting part. Also, from my standpoint, as I recall, at least two of your children are involved in science. And a number of people that I have interviewed, <**T: 05 min**> I have found that their children never followed in terms of being science oriented. So I wanted to pursue that part a little bit too.

ISABELLA KARLE: Well, they naturally seemed to gravitate toward the natural sciences. As youngsters they were always curious about how things worked, about how bugs crawled, all that sort of thing. In fact, my housekeeper complained most bitterly that she had to empty all these worms, beetles and so forth, from our oldest daughter's jackets before she could put them in the washing machine. She didn't appreciate that at all. I think in some sense, we were always partners with our children. The oldest daughter did a lot of plotting of [electron] maps when she was really very young. Somebody had to do it, and she enjoyed it. As soon as they could more or less look after themselves, we started to take them to scientific meetings in the summer time, and they were very well-travelled children. They went to Europe quite a number of times. They understood very well that they could come along if they could take care of themselves, didn't get into trouble and so forth. I don't know how serious this was, but our second daughter said that she always thought she was going to become a scientist because she liked all the travel. But obviously, she was [also] interested in science.

JEROME KARLE: I think we helped them without pushing them. There are several instances that I am familiar with [in which] parents tried very hard to push their youngsters into scientific careers, but it seemed to build up resistance on the part of the children.

BOHNING: May I ask, just for the record, what your children are doing today? I read it somewhere.

ISABELLA KARLE: The eldest one is at Brookhaven National Laboratory. She does theoretical work [and spectroscopic experiments on electron transfer in biological systems. Metalloporphyrins are a type of substance of interest to her].

BOHNING: [. . .] The oldest daughter is the one who plotted the density diagrams?

ISABELLA KARLE: Yes. Actually, she thought, as she was growing up, that the biological sciences would be what she was most interested in, but during college she confided to me that "the biological sciences are so messy." [laughter] Pencil and paper were much to her liking. She has a family of two children, and her husband also is a scientist [. . .] at Brookhaven. At the moment she is about to leave to give some invited talks at a school in France. She is excited about that.

The middle one, Jean, is not married. She works now at the Walter Reed [Institute of Medical Research]. She works on malaria problems and seems to be quite interested in [them]. She occasionally collaborates with me on crystal structure analyses of materials, of new drugs that are being developed in the malaria field.

The youngest one did get her bachelor's degree in geology <**T: 10 min**> but museum work beckoned, and she is at the Smithsonian, in the Natural History [Museum].

VAN KEUREN: You have your own institute [. . .], the Lab for the Structure of Matter. Can you give us some background on the organization and founding of the Lab?

JEROME KARLE: It seemed as if the idea started in the sixties that people who were doing special work successfully might be supported in a different context than the division structure, and several special laboratories were set up. About five different laboratories in the history of this lab were set up—maybe six, I'm not exactly sure. I would have to think about it for a while. At the present time there are still two extant, this one and the Laboratory for Computational Physics that is headed by Jay Boris. The philosophy behind it was to, in the first place, honor the individuals for their work, but at the same time, isolate them as much as possible from administrative duties so that they and their group, which was usually held to some small number, perhaps ten or fifteen people, could concentrate on the research at hand as well as possible. So that was the philosophic basis for setting these laboratories up. And I had the good fortune [to be] one of the people for whom such an arrangement was created. [. . .] I have enjoyed [the arrangement very much], and it has been advantageous to the research program.

VAN KEUREN: In what way would you say that it has been advantageous?

JEROME KARLE: Being isolated as a smaller group. One might say, "Well, you could still be a branch in a division." There was more recognition for a laboratory than a branch in a division and somewhat more consideration for funding stability. So being small, isolated, appreciated and better funded, I regard as advantageous.

ISABELLA KARLE: And less administrative work than if you were a division head. [. . .]

VAN KEUREN: Do you know where the idea for organizing individual labs came from? What was the genesis of the idea?

JEROME KARLE: Who originated it? I am not sure how the idea originated. I am almost certain that it originated in the administration. [. . .] But who was the director at the time the first ones came up and where the idea within the administration came up, I really don't know. <**T: 15 min**> Someone who might know, who had such an arrangement earlier than I did, would be James Shulman. So he might be queried on that. He might know.

BOHNING: Were you approached by someone in the administration in terms of setting it up?

JEROME KARLE: I don't remember exactly how it happened. At that time I had had, from time to time, offers to become a division superintendent. And I didn't want to do that because I was certain that it would interfere too much with my research program. And I may have very well said that. [I had been] quite well aware of [the special laboratories] that had already been set up. I may very well have said that I would "prefer" . . . [laughter]

ISABELLA KARLE: Maurice Shapiro already had one.

JEROME KARLE: Maurice Shapiro had one, [Bill] Zisman had one, and Jim Shulman had one. I can't think off hand of any others, so with mine and Jay Boris's, that would make five.

VAN KEUREN: There are only two extant. Is the idea being redeveloped?

JEROME KARLE: I really can't speak for the present administration. I have the impression [from the fact that] nothing much has happened for many years, that it is not widely favored. But, as I say, I don't really know.

BOHNING: How long did your—we touched on this briefly before—did your work with Hauptman continue? He didn't stay at the Lab very long.

JEROME KARLE: He stayed at the Lab, but he left our group around 1960 or '61. Actually, he began to do independent work around '59 and '60. We didn't collaborate that much anymore. Even though there are publications that extend beyond [1960], it was just unfinished things that I finished up, and since he had collaborated with me on [them], I included him as co-author. [Collaborations] stopped around '59 or '60, and then he had various other positions within the Laboratory, but we didn't collaborate.

VAN KEUREN: What was the basis of your earlier collaboration with him? Did you collaborate closely? What sort of working relationship did you have with Hauptman?

JEROME KARLE: It was very close. He joined our group sometime in late '47, I think it was. As I probably have mentioned previously, Isabella and I were developing much more precise ways of [performing] gas electron diffraction [structural research. In the course of this work we discovered a] non-negativity principle that was tremendously helpful to us. When Herb first came here, I was thinking about expanding the work on electron diffraction and he designed, along with our draftsman, an apparatus for [performing] slow electron diffraction. [. . .] The difference was that we normally used what were **<T: 20 min>** [known as] fast electrons at that time, at an accelerating voltage of about 40 to 50 kilovolts. The kind of experiments we had in mind would just be done at an acceleration of perhaps anything between 10 and 100 volts, instead of kilovolts. That was an entirely different type of an experiment. It looked very interesting. There were a lot of challenges. We became intrigued by the usefulness of the non-negativity principle, [however, with] the gas electron diffraction experiments, and Herb had had the [appropriate] mathematical background [to work with me on extending its applications]. I thought, and he agreed, that it would be very interesting to pursue the non-negativity [principle] in other fields of research. It very soon led us into the crystal structural problem because of the characteristic feature that its structure is represented by an electron density distribution in the unit cell, which is a non-negative function. At that point, we worked very closely on the mathematics together, until about 1956 when, broadly speaking, we had most of the math. After '56, things just naturally started to taper off. [Herb] did collaborate on some of the earlier structure determinations that we performed in our laboratory.

ISABELLA KARLE: I think the first two.

JEROME KARLE: Also with the ones that were done [at the Geological] Survey.

ISABELLA KARLE: Yes. And the first two we did here.

BOHNING: I heard a story, but rather than repeat it I am going to ask you how you heard about the Nobel Prize.

JEROME KARLE: I was at a meeting in Munich, and present there were several colleagues whom I knew well, who were members of the Royal Swedish Academy of Sciences. They [told me] nothing. I recall one of my good friends at a banquet on the last evening casually coming over to chat with me. As [such conversations] often go at the end of a meeting, we discussed when we [would be] leaving and how [we would be traveling]. Little did I know that he wanted to know what plane I was going to be on. So about two hours before I [landed in Washington on the] Pan Am trip that started out in Munich [. . .] an announcement came over the loudspeaker. It started out [describing] the weather in Washington and [telling] what a nice day it was. It sounded a little bit strange to me. It was [like] one of those captain's announcements which [attempts] to encourage people and then goes on to say, "It so happens that we lost a couple of engines, but we'll get there anyway." However, he got around to saying that "he doesn't know it but the latest recipient of the Nobel Prize in Chemistry is aboard." [. . .] Had this been an announcement for the United States or [some large territory], I would not have paid too much attention to it. But I figured with three hundred people on an airplane, I had a pretty good chance. [laughter] <**T: 25 min**> Then he said my name, said where I was sitting, and suggested that I come up to first class.

I'm glad that I was informed two hours before the plane landed because there was a huge number of reporters at the airport and [innumerable] microphones. If this were thrust on me without [any knowledge of] what was happening, I don't know if I could have had as much composure as I did. But I had time to regain my composure [on the plane], and I could handle the [meeting with the reporters]. That's how it happened.

BOHNING: What was the reaction of the people on the plane?

JEROME KARLE: They were delighted. They stood up and cheered. I had to stand up [so that they could see] who I was, and so forth. It was a very nice event.

BOHNING: It was certainly a most unique way of announcing it. [. . .]

[END OF AUDIO, FILE 3.2]

VAN KEUREN: What was the response of your friends and colleagues to the award?

JEROME KARLE: Excellent. I [realized once again] that the people who had had a negative attitude early [on were a small] minority. The letters I received were very warm and made me realize that I had many more friends in the audience [when] acrimonious discussions were imposed on meetings than I had any thought of. This was very pleasant for me.

In particular, I [received] letters from young people who were just breaking into the field, and they wrote me about what their reactions were when they heard [the acrimonious] discussions. They said that they could never understand, and I could never understand either, why instead of having discussions about "yes, it will work" or "no, it won't work," why they didn't just try it. I received hundreds of nice letters. The letters also didn't hesitate to mention the really important contributions that Isabella made, and this was a great source of satisfaction to me too. In fact, I would say that more than half of the people mentioned this. [. . .]

VAN KEUREN: How has the award affected your research and life since 1985?

JEROME KARLE: I don't think that it has interfered very much. If the kind of research that I would do was laboratory work, on my own, that took time, it would certainly have made a big inroad because I have [had many] speaking engagements. [. . .]

But the kind of research that I have been doing in recent years is theoretical work, which I can do [readily] in my head. I have someone to program [the results]. As a consequence, my research program [has continued] without any significant interruption. Now, my publications have decreased. The reason for this, though, is that I have been starting [work] in an entirely new area. It is [an area] that I picked out that is very difficult, high-risk [research]. It is going to take awhile before I have something to publish. The decrease in publications has not been due to the award, but simply due to the fact that I have another research area that I am [getting] into.

BOHNING: What area is that?

JEROME KARLE: It is a problem [. . .] in numerical analysis. It is purely mathematical in one sense, but of course I have applications to structure analysis and other [applications] in mind. I am trying to tackle the problem of nonlinearity. As soon as systems become nonlinear, there are tremendous problems very often in getting answers. I am looking for a more general approach to the solution of nonlinear problems than has so far been available. I am trying to make life somewhat easier for people in the future than mine has been with nonlinear problems. **<T: 05 min>** That's only one of the areas. I do have other little things going, and if I really want to come up with something publishable, I can take a month off and work it through. My main concern, [however], is to try to do something about [obtaining] answers [from] nonlinear systems.

VAN KEUREN: Do you find that you have become a public spokesman for science?

JEROME KARLE: To a certain extent, yes. I am asked for my views abroad, I would say, more than I am asked for my views locally. In a way, I think that that is too bad. But it just turns out that way.

VAN KEUREN: That's a leading remark. Why do you think it is too bad? If you had a chance to make your opinions known, what would you want to say?

JEROME KARLE: Well, I don't have an immediate thesis that I wish to present, I would like to collect my thoughts. I do have them collected somewhere. I think that—just off the top of my head—I think that in our educational system, the tendency to treat, or try to treat, everyone the same, is a [great] mistake. The talent ought to be sought out and supported, and for the youngsters who are brilliant, they ought to be treated as brilliant youngsters and ought to be supported educationally as if they were brilliant youngsters. That's one thought that I have. The educational people in the United States have not asked me what my opinion on that subject is.

I think that science ought to be supported over much greater lengths of time. This business of having a yearly budget interferes with the progress of science in a way that almost nothing else that I can think of could interfere. It would be extremely valuable to budget science over longer periods of time and take the burden off extremely talented people of writing proposals from one year to the next. [They] hardly have time to breathe and talk to their co-workers and their students. I have to think some more about my views on this subject. I would rather not be taken unawares. [. . .]

BOHNING: The more general problem, though, is an intriguing one; the fact that your stature internationally has not been recognized locally in terms of getting your expertise. Certainly it is recognized here in NRL, but there's a broader picture, and that's what David was trying to look at. It is really unfortunate that it is not being utilized.

JEROME KARLE: Yes. There is also the problem in the United States of the serious decrease in the number of American graduate students in science. **<T: 10 min>** This has been dropping off precipitously, and many of our colleagues in universities are able to maintain graduate schools and graduate programs on the basis of foreign students who are visiting. One might ask what are some of the reasons for this? One of the reasons is perhaps the lack of preparation and the poor preparative environment in which students who would be inclined to go on to graduate work might find themselves. [Another aspect], however, is the fact that young people, early in life, become apprised one way or another about what the future offers them [in particular professions]. The conditions for young scientists in the United States leave a great deal to be

desired. Their lives are fraught with lack of tenure, lack of security, temporary jobs, endless postdoctoring, and an environment in which their talents are exploited in a way that makes it very difficult for them to lead a secure life, secure in terms of the work that they will be doing and like to do, and economic security. I feel that until the future for young people can be made to look brighter than it does now, that there is going to continue to be a tremendous problem about attracting people to making the sacrifices that are required for a career in science.

BOHNING: In the meantime, where does all our talent go?

JEROME KARLE: Well, for a while it was being attracted to the medical profession. I don't know if that's still true. People with a mathematical bent will go into accounting.

ISABELLA KARLE: The computer field, of course. That's very attractive.

JEROME KARLE: It goes into the computer field, but there is [much] in that field that does not ultimately result in basic science. It can result in good salaries, however. I don't really know what all is happening with [our students]. Conceivably a lot of people with [an aptitude for science] are unmotivated. They are not using and developing their talents.

BOHNING: I've seen that. At the undergraduate level you see that happen.

VAN KEUREN: Isabella, what are your present and future research plans?

ISABELLA KARLE: Well, ever since I [investigated] cyclohexaglycyl in 1963, I have become interested in peptides. Since then [I have analyzed] many peptide structures, both cyclic and linear, and getting into larger and larger ones. Recently I solved a 16-residue peptide. The ones I have been concentrating on more recently, are the kind of linear peptides that penetrate membranes—cell membranes <**T: 15 min**>—and mediate ion transport. That is still a mystery as to [the mechanism of action]. A few years ago, before any of the structures were available, it was assumed that the peptides [were wound] into a rather wide helix [with] a pore down the center that somehow or other attracted and pumped potassium or sodium ions through. That is not the case. All of the structures that we have [determined] so far show a very tight helix with no pore down the center. The current thought is that there is an aggregation of these peptides in the cell membranes. An aggregation of [. . .] perhaps six or eight peptides, and these [peptide helices] come together and make a pore. Just recently—in fact I am proofreading the paper now—a very apolar synthetic peptide has shown that in the helix there are water molecules that spread the helix apart and become part of the helix. One side of the peptide molecule becomes polar because of these water molecules [that] distort the helix enough so that carbonyl oxygens

protrude out from it and attract other water molecules. In fact, the drawings that I have been making of this crystal structure [. . .] show a polar channel [that contains water molecules] between these strictly apolar [helical peptide molecules]. This was the very first indication of such [an occurrence] and is quite exciting. I have had the good fortune to collaborate with a man by the name of Balaram [. . .] from India [whom] I have never met. He has been isolating these kinds of peptides from spores in the soil, and they have these properties of mediating ion transport through membranes. The natural materials have resisted crystallization. However, he has been making synthetic analogs that are quite close to the natural materials, and I have been doing the [single] crystal structure [analyses. At present], we are very excited about how the apolar peptides can have polar areas [associated with] them, and that they do seem to be making polar channels.

BOHNING: Maybe in sort of summing up at this point. do you have any comments about what you have seen in your career, let's say in X-ray crystallography. What the major parts are that you have been a part of and a witness to, and maybe in science in general? We have touched on both of those. [I guess] I'm looking for a summary statement.

JEROME KARLE: Just some general remarks about science, life in science, and so forth? Well, I've said these things before. Very little good is accomplished, in general, and also in science without hard work. Science progresses in fits and starts. Every once in a while it [appears] that there [may be] a big breakthrough or new idea. It does happen that way. **<T: 20 min>** It's sort of a quantum jump, insight, or a capability, or what have you. But it just about always has a long history during which concepts and ideas have been developing. It's really a culmination of a long process. It's very rare that anything instantaneous comes [forth] in science. If nothing else, the preparation for making the next step is generally a long and tedious process.

For myself, the greatest satisfaction is to learn something new. One of my colleagues who received the Nobel Prize made the remark that nature gives the best prize, and the Royal Swedish Academy gives the second best prize. [laughter] People who go into science should do so looking for their satisfactions in what they may contribute and discover. But if they do it with the prime purpose of being recognized and winning [. . .] awards, they are almost guaranteed to be disappointed. Being appreciated is [just] the icing on the cake. But the cake is what you can personally appreciate and accomplish for yourself. With that kind of a philosophy it is possible to do things such as science in this world and be at peace with the world and, I think, a lot happier than otherwise.

ISABELLA KARLE: I suppose this is just saying the same sort of thing in a different way, but it takes a lot of perseverance. Some ideas will work out in a short period, but others take many, many years before they come to fruition. And sometimes they are even passed on from generation to generation. But they shouldn't be abandoned just because the time isn't quite

right, or the instrumentation isn't quite available, or the computers aren't quite available at the time. Eventually it will work out.

BOHNING: Do you have any prognosis for the future? That's a dangerous question, I realize.

JEROME KARLE: I'm apprehensive about the future, not so much in the context that [concerns most people, namely] nuclear war, [although] I do not rule out the possibility. I feel that it is unlikely that the big powers will get involved [in such a confrontation]. With respect to the dangers of nuclear explosions, [I believe] that it's possibly more likely that fanatical groups might try to [. . .] use them as a threat [or weapon]. [It is] the exploitation of the earth which is very frightening. Population is growing, in my view, [very] much too fast. The resources are being exhausted much too fast. **<T: 25 min>** The pollution problems are getting out of hand. I just worry that we might be [like] bacteria in a petri dish who ultimately do themselves in by their own wastes. [. . .] There are many important groups around the world who are concerned about this. I have been invited abroad [. . .] by [various] groups and governments that are [greatly] concerned about these questions. The real problem, and the big problem, is not that we do not know the solutions to these problems. The problem is implementation. How do you get governments and societies to face up to the realities of these problems and do something about them? In that context, I must say I am somewhat apprehensive.

ISABELLA KARLE: I don't think I have anything to add.

VAN KEUREN: [. . .] Thank you very much.

BOHNING: Thank you very much. We certainly enjoyed your sharing all of your very valuable time with us. We appreciate it.

JEROME KARLE: We enjoyed it.

[END OF AUDIO, FILE 3.3]

[END OF INTERVIEW]

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