

CHEMICAL HERITAGE FOUNDATION

DONALD S. NOYCE

Transcript of an Interview
Conducted by

Leon Gortler

at

University of California at Berkeley

on

22 January 1981

(With Subsequent Corrections and Additions)

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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Donald S. Noyce

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DONALD S. NOYCE

1923 Born in Burlington, Iowa on 26 May

Education

1944 A.B., chemistry, Grinnell College
1945 M.A., Columbia University
1947 Ph.D., organic chemistry, Columbia University

Professional Experience

1946 National Institutes of Health Fellow, Columbia University

University of California, Berkeley

1948-1950 Instructor
1950-1955 Assistant Professor
1952-1960 Assistant Dean for Undergraduate Affairs, College of Chemistry
1955-1960 Associate Professor
1960-1986 Professor
1974-1981 Assistant Dean for Undergraduate Affairs, College of Chemistry
1981-1986 Associate Dean for Undergraduate Affairs, College of Chemistry

Honors and Awards

1957 Guggenheim Fellowship (Europe)
1964 National Science Foundation Senior Fellow
1986 Berkeley Citation, University of California, Berkeley
1987 Establishment of the annual Donald Sterling Noyce Prize, University of California, Berkeley

ABSTRACT

Donald Noyce begins the interview with a discussion of his childhood home in Iowa. He discusses his family and their strong academic tradition, his years at Grinnell College, and his early training as a chemist. He also details his graduate training at Columbia University, including his work with Bill [William von Eggers] Doering, his courses, research, and the University's atmosphere. Next, he discusses his position at the University of California at Berkeley. He describes the faculty, the chemistry administration, and the changing atmosphere with respect to organic chemistry. He describes briefly his interaction with other faculty, his research, and his graduate students. Finally, he discusses the development of physical organic chemistry from the turn of the century to 1980.

INTERVIEWER

Leon Gortler is Professor of Chemistry at Brooklyn College of the City University of New York. He holds A.B. and M.S. degrees from the University of Chicago and a Ph.D. from Harvard University where he worked with Paul Bartlett. From 1961 to 1962 he worked as a postdoctoral fellow for Donald Noyce at the University of California. Professor Gortler has long been interested in the history of chemistry, in particular the development of physical organic chemistry. He has conducted over fifty oral and videotaped interviews with major American chemists.

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INTERVIEWEE: Donald S. Noyce
INTERVIEWER: Leon Gortler
LOCATION: University of California at Berkeley
DATE: 22 January 1981

GORTLER: Donald, you were born in Burlington, Iowa, on 26 May 1923. Why don't you start by telling me a little about your family, your parent's influence on you, and your early education?

NOYCE: I was the oldest of four boys. My father was a Protestant minister, so we moved frequently when I was a young. We lived in four different towns before I finished high school; all of them in Iowa.

GORTLER: Where else did you live besides Burlington?

NOYCE: Atlantic, Decorah, and Webster City. We moved to Grinnell, Iowa, when I began attending college. So I went to Grinnell College and lived at home.

GORTLER: What is your father's name?

NOYCE: Ralph [Noyce].

GORTLER: And your mother's name?

NOYCE: Harriet [Noyce]. They're both still alive. They've lived in Berkeley since they retired, for about twenty-five years.

GORTLER: What about the rest of your family? You have a few brothers, correct?

NOYCE: Yes, Gaylord [Noyce] is the next in line. He's on the divinity school faculty at Yale [University]. Then there's Bob [Robert N. Noyce], who is five years younger than I am. He

has a Ph.D. in solid state physics from MIT [Massachusetts Institute of Technology] and is now vice chairman of the board of directors at the Intel Corporation, in Santa Clara, California. My youngest brother is Ralph [Noyce]. He is a development engineer for IBM [International Business Machines Corporation] down in San Jose.

It's obvious that education was always an important facet of our lives, because both my parents were college educated and all four of my grandparents were college educated. All four of them went to Oberlin [College] for at least part of their college educations. It's a long tradition. And so I just went to college naturally.

I suppose I chose the natural sciences to find something that had more likely answers and solutions than what I saw in the personal relationships of a ministerial career. There are many questions that come up for which there are no answers, like illness and death and that sort of thing, so I migrated away from the personal sciences to the physical sciences.

I always enjoyed geometry in high school, and I think parts of organic chemistry are a natural outgrowth of that.

GORTLER: As a result of moving around, in retrospect, how do you picture your early education; elementary school, junior high school, high school?

NOYCE: I don't think moving had any great effect on my early education because that kind of homogeneous Midwestern society looks pretty much the same wherever you go.

GORTLER: Was there any particular instructor who influenced you during that period, or any books that you read that you remember?

NOYCE: I can't honestly remember. I do remember, with some very cordial feelings, my high school chemistry teacher. She was nearly middle aged and had a Master's degree from the University of Nebraska, and I'm sure we had an above average high school course.

GORTLER: Were there any other people, from your family or anyone else, who might have influenced your interest in science? I realize that you were escaping from the "personal sciences" but was that the only factor that drove you towards the physical sciences?

NOYCE: Not really. Though I enjoyed geometry, differential equations were something that escaped me. So it was quite obvious that I should not be on the highly mathematical side of the sciences. Further, I've never gravitated strongly towards the biological sciences, so the structural sciences and organic chemistry looked like a good place. And I guess, in a sense,

inorganic chemistry in the early 1940s was sort of a “Dodo” situation. It hadn’t been rejuvenated yet.

GORTLER: When did you enter Grinnell College?

NOYCE: About 1940.

GORTLER: How were you supported at Grinnell?

NOYCE: I lived at home, so I didn’t have to pay for room and board, and Grinnell still had the vestige of its congregational founding at that point, so the children of ministers in Iowa were offered half tuition. That meant the net cost of tuition was only two hundred dollars a year at that time, and I had a two-hundred-dollar-a-year scholarship for three of my four years, so my personal costs were minimal.

GORTLER: Tell me something about life at Grinnell?

NOYCE: Society at Grinnell was undergoing very rapid changes because of World War II. The University had about eight hundred students when I started, but within two-and-a-half years, by early 1943, the number of men on the campus had dropped to twenty-five or thirty. Then the women started leaving campus, mostly for sociological reasons, and so there were only about three hundred and twenty-five students on the campus by the time I graduated.

The college administration did try to recoup their losses. There were two [United States] Army training programs on campus: Officers Candidate School [OCS] for German language specialists who were to go in with the occupation forces in Germany, and then a younger, junior engineering training program for students who were drafted and showed some potential for Army Engineering Corps.

During my senior year, I taught a high school chemistry course for a group of fifteen or twenty Army draftees that had not yet had chemistry, which was a very strange venture. I was inexperienced, and my students wanted things to be laid out in an exact style, to learn so much today and so much tomorrow. I didn’t know quite how much that was, so I’m not sure they got what they needed, but I did have the experience, which certainly made me feel that teaching was a rewarding profession.

GORTLER: Were you threatened by the draft during that period?

NOYCE: I was called up for induction in 1943, but was turned down because I had a history of asthma and allergies. I was classified 4-F, which meant I was one of the unusual people who went straight through and finished all their educational work at a relatively young age.

GORTLER: During that period, were there any people who influenced you? What were your chemistry courses like?

NOYCE: The faculty consisted of two people. There was a delightful old gentleman who was a Christian scientist and had earned his Ph.D. under [John U.] Nef, I think it was, at [University of] Chicago, in 1915. He taught a very sturdy classical organic course.

GORTLER: What was his name?

NOYCE: Leo P. Sherman. Bill Oelke taught analytical and physical chemistry. Both of them enthusiastically encouraged me to continue my studies, and thus, were influential in my decision to attend graduate school.

GORTLER: At what point did you decide to become a chemist?

NOYCE: Probably in the beginning or the middle of my sophomore year. I went to college and I'm pretty sure I put down "science" or something like that. By the time I started to consider a career, I was into qualitative and quantitative analysis.

GORTLER: Do you remember textbooks, the organic text for example?

NOYCE: The organic text we used was written in Illinois by [Reynold C.] Fuson and [H. R.] Snyder (1).

GORTLER: You were saying it was an interesting book. Why?

NOYCE: It's divided in half. The first half goes through organic chemistry rather quickly, and the second half comes around and goes through it in more detail. So it was designed so that teachers could use it in a brief, comprehensive course, and then take the chemistry majors on further.

GORTLER: What courses did you have outside of science that might have been particularly influential?

NOYCE: There is one course in particular that I remember was very interesting. I needed one more course in my senior year to finish the breadth of my general education requirements, and there was a senior honors seminar in contemporary European history for history majors. It was just about the only thing that fit my time schedule because of that Army SAT [Scholastic Aptitude Test] teaching service, and so I talked my way into that course. It was interesting because we discussed the period from 1918 to 1940; the independence of Czechoslovakia, the recreation of Poland, the breakup of the Austro-Hungarian Empire, and so on. There were eight other students in the class and all of them were history majors, so I was certainly out on a limb as far as background goes, but it was very interesting to do something so apart from science.

GORTLER: When you finished you wanted to be a chemist. What were your perceptions of being a chemist at that particular point? Did you want to go into academic chemistry?

NOYCE: I think I wanted to go into academic work. I'm pretty sure that that was expressed subconsciously rather strongly, if not expressed directly.

A couple of other important things happened in that period. When I was elected to Phi Beta Kappa, they offered every new member a moderate prize, like fifteen dollars. I got hold of a couple of chemical book company catalogues and ordered fifteen dollars worth of chemical books, including [Louis Plack] Hammett's text, Pollack's description of nuclear science, and an old reprint of a British treatise on inorganic chemistry, the name of which I honestly forget (2). So I did some extra reading during my senior year.

GORTLER: How did Grinnell's laboratories compare to what you saw later?

NOYCE: Not terribly well. We were in a building that was built in the 1880s, and the wood was soaked with oil. It tended to be greasy. However, it was probably better equipped than similar small colleges of its day. It had had a pretty strong history of excellence in the sciences and one of its senior professors had been recognized nationally during the 1920s. But that was not true while I was there.

GORTLER: Did you do any research as an undergraduate?

NOYCE: No. We didn't have an independent studies program at that time.

GORTLER: Did any other students at Grinnell go into chemistry during that time?

NOYCE: Yes, there were two or three others. One of the students, Claire Patterson, was in the class ahead of me. Currently, he is a leading geochemist at Caltech [California Institute of Technology] and has become well known for his studies of lead in the environment. He's worked there for a long time. [Claire Patterson died in 2000.]

And there were regularly one or two students a year that completed the full set of advanced studies. I've run into someone from 1939 and someone from 1947, just outside of my time.

GORTLER: Then you decided to go to graduate school. What graduate schools did you consider?

NOYCE: I applied to University of Illinois and was offered a teaching scholarship—I think that's what they called it then—and I applied to Columbia University. I was offered a very rare fellowship from Columbia, the [Lydia C.] Roberts Fellowship. To be eligible for that Fellowship, you must've been born in Iowa and you must've graduated from an Iowa college. You could study anything you pleased at Columbia, so long as it wasn't theology, medicine, law, or veterinary science. Apparently, Mrs. Roberts had had a fight with a minister, a doctor, and a lawyer! [laughter]

Anyway, it was a good-sized scholarship, worth eleven hundred dollars. So after paying the four-hundred-dollar tuition at Columbia, I still had seven hundred dollars to live on. That wasn't quite enough but it came pretty close. By working the summer before graduate school and the summer after graduate school, things came out even after the first year.

GORTLER: Had you been advised to apply to those universities?

NOYCE: Yes. My professors said those were the places to go.

GORTLER: So you went off to Columbia in 1944, correct?

NOYCE: Right.

GORTLER: What did you find there? Were they still involved in wartime work?

NOYCE: Yes, but that work was pretty well segregated. It was several months before I realized how involved they were. There were areas of the old Havemeyer Hall in which nothing seemed to be happening. It appeared to be a dead-end corridor, but there was obviously more building over there. It was actually some kind of security area.

[Robert C.] Elderfield had a lot of people working on anti-malarials, and after I was acquainted with some of the people, I recognized that they were working on that very specific kind of project. But it wasn't for some time that I recognized Columbia had these other separate buildings, like the Nash Building, which was three or four blocks up the street which was used for part of the Manhattan Project. People just weren't aware of those programs.

GORTLER: So you just came in and began to take courses.

NOYCE: Yes. Everybody took courses at Columbia for the first year, and you had to pass a qualifying exam to be admitted to research. So I took that exam.

GORTLER: What courses did you take?

NOYCE: I took thirteen units. I took a course in thermo. I took advanced organic lecture, advanced organic lab, and a course in phase rule.

GORTLER: Tell me a little about who might have been teaching.

NOYCE: Thermo was taught by V. K. LaMer, and to me it was a disaster. He was using a text that was written by a physicist but he didn't like the physicist's notations, so he was using chemist's notations and all the F's and mu's and nu's were muddled. It was a problem.

Then old Pop [John M.] Nelson gave a course on natural products that involved a little bit of biochemistry and a little bit of protein and amino acid chemistry. I remember a problem arose in a discussion when they couldn't offer a good explanation for why labeled citric acid lost only one of its two carboxyl groups. So that chiral situation and the 3-point enzyme contact came subsequent to the year that I took that course.

Then I had an advanced organic laboratory synthesis course, which Bill [William von Eggers] Doering taught, and that's when I first met with him.

GORTLER: Did anyone teach a regular advanced organic course?

NOYCE: Yes. It was offered one semester a year. Then in the second year I sat in on a course on alkaloids by Elderfield.

But the major things in the first year were the advanced laboratory synthesis, the course in thermo, the natural products, and then synthetic/organic general/advanced organic. It was mostly Doering and his mechanistic style.

GORTLER: Did he teach the course?

NOYCE: Yes. That was in the second semester.

GORTLER: Do you remember if he used a textbook?

NOYCE: No, he did not. He used his own notes. I now recognize that they are rather closely related to the sorts of notes that [Paul D.] Bartlett would have had at Harvard [University] in the 1941 to 1943 period¹. The course was patterned in that style.

GORTLER: That's what Andy [Andrew] Streitwieser said, too (3). Do you know Doering's background?

NOYCE: Yes, Doering was a Harvard undergraduate and his graduate study at Harvard was with [Sir Reginald Patrick] Linstead. So he also has that cross-reference to the British mechanistic style. Linstead, I think, was one of [Robert] Robinson's students².

Then in 1941 or 1942, I guess, Linstead felt obligated to return to England, even though he had been offered a senior professorship at Harvard. So he went back to England at more or less the same time that Doering was finishing his Ph.D., so I think he sort of finished it up on his own. I'm not quite sure about that. Then, Doering stayed around Harvard as a post-doc for another six months or so, during which time he became associated with [Robert Burns] Woodward on the quinine project, which was not quite finished when he started teaching at Columbia.

¹ Doering was at Harvard during that period.

² Linstead took his Ph.D. with Jocelyn Field Thorpe.

GORTLER: When did he come to Columbia?

NOYCE: In the fall of 1943. As I understand it, he did a lot of commuting, traveling to Cambridge for long weekends—Thanksgiving, the week before Christmas—to finish up the quinine project. They did so during the fall, and it was published early in 1944 (4).

GORTLER: So you took these courses. Then, you took your qualifying exams?

NOYCE: Right. And started research.

GORTLER: How did you decide on a research director?

NOYCE: Finding a research director was quite simple. I was intrigued by the patterning, logic, and sequencing that Doering discussed in his lectures. During the latter part of my junior year, I made a few efforts on my own to learn if there were more than forty-eight different isolated examples of condensation reactions. I learned that you could cross-hatch all of the enolate reactions, many of which were named, and I found that to be very interesting. Also, I had an appreciation for the generalizations of mechanistic style found in subjects like ionic organic chemistry, which Doering emphasized in the course. So it was quite clear that I wanted to work with him.

GORTLER: Please name some other chemists around during that period.

NOYCE: Elderfield was there doing heterocyclic chemistry; alkaloid chemistry. Charles Dawson was working on poison ivy and shellac extracts, which was painful research. And there were two or three young people around. [David Y.] Curtin came there around 1946, I think, but I'd decided, basically, to work with Doering before I became acquainted with Curtin. And then there were a couple of temporary people who left very quickly.

GORTLER: Do you remember [Arthur C.] Cope there at all?

NOYCE: Cope left just before I arrived. I knew half a dozen of his graduate students that finished there, including Lillian Levy and Harold Levy. Lillian worked with Bill Doering and Harold worked with Cope. And I knew Dwight Morrison, who finished up there about 1947.

GORTLER: He had a graduate program going there, but he wasn't there very long. When did he leave?

NOYCE: No, he wasn't there very long at all. He went to MIT around 1944, I think, or possibly a half a year later. I don't think he was at Columbia for more than two or three years.

GORTLER: Interesting, he came and went so fast that Hammett never even knew he was there.

NOYCE: Right. So he came after Hammett left, in 1941, and he left before Hammett got back in 1945.

GORTLER: That explains it. What kind of problem did you work on with Doering?

NOYCE: I worked on a natural products problem.

GORTLER: Was that *Aspergillus ustus*?

NOYCE: Yes, a mold metabolite that killed TB [tuberculosis] germs in a test tube.

GORTLER: Were you trying to determine its structure?

NOYCE: Yes, a classical structure proof with limited quantities of material. And the quantities of material were more limited than we cared to admit.

Bill Doering convinced somebody in the fermentation business to grow an extra-large batch of that mold, so we got material from which we probably should have got 4 or 5 grams. However, they told Doering that they wouldn't grow another extra-large batch because it crapped-up their tanks thoroughly. The mold stuck to everything and it took them a long time to clean their fermentation tanks.

I remember I was finishing the isolation from the crude at the time of the World Series in the fall of 1946. Somebody hit a home run or something, and we knocked a 25 ml Erlenmeyer flask of the ether solution—which probably had about a gram and a half of the pure material, more or less—off the counter and onto the asphalt tile floor. We tried to extract it

from the asphalt tile floor, but we didn't get enough to count, so I had to finish up with about a gram of the material instead of the 3 or 4 that I thought I was going to have.

I got to the point of a good suggestion for the structure, which turned out to be right, but I couldn't prove it.

GORTLER: Did the suggested structure appear in the literature at the time?

NOYCE: It was in my thesis.

GORTLER: Who were the other people around at the time? Who were some of the other graduate students that you interacted with in the Doering group or out of it?

NOYCE: All right. There was Marshall Beringer and Ruth Haines-Doering-Zeiss. Jerry [Jerome A.] Berson started near the end of my time there. There was also Ruth Alice Weill, who was my lab partner for a while. And Betsy Claflin.

[END OF TAPE SIDE 1]

NOYCE: I didn't really know Andy Streitwieser. During my last year, Streitwieser was a senior or a first year graduate student who hadn't started his research. But he says he remembers me because I gave a couple of lectures in one of the courses Doering was teaching, so he [Streitwieser] knew who I was when he got to Berkeley.

GORTLER: What was the nature of communication at that point? Was Doering already holding his seminars?

NOYCE: Yes. His regular weekly seminars, literature seminars, and problem solving seminars were very stimulating. I remember one of them. It was the paper by Lyndon Small on some Grignard reactions with morphine and the products, which resulted there, and a rather unusual combination of isomerizations and epimerizations, which we spent some time trying to figure out, and we missed it (5). But the next week there was a paper, I think it was by Sir Robert Robinson, in *Nature*, suggesting that these were due to hindered rotations in biphenyl systems. So that kind of intellectual exploration was very stimulating, and I'm sure some of us looked at some of the things that came out of those seminars as possible research problems.

By the way, Kurt [C.] Schreiber was also in the group. He is at Carnegie [Mellon University] now. He went to post-doc with Saul Winstein. And I shouldn't forget Sarah Jane Rhoads either; she was also in the group.

GORTLER: Were all of you housed in laboratories so there was continual communication between you, or were you spread out?

NOYCE: We were a little bit spread out. We were on two different floors, the sixth and seventh floors of Chandler, and then a little later some of us were in the refurbished top floor of Havemeyer. So we were sort of spread out, but we were in clusters of three, four or five people.

GORTLER: At that time, was Doering working mostly on synthetic problems or structure problems?

NOYCE: No. He was doing the synthetic problems more out of curiosity, I think. One of his college buddies was doing medical research in upstate New York somewhere, and had run into this metabolite and wanted to know some more about it. Doering was much more interested in the structural; the mechanistic. He wanted to explain things or use new concepts to predict what would happen.

GORTLER: So you were being exposed to that?

NOYCE: I was exposed to that all the time, which made me less than average in that sense. There was one other student, Richard Dreyfus, who worked on that structure problem for a while, but he was only there for a year. He was a foreign student from somewhere between France and Egypt.

As it turned out, Doering wasn't fundamentally interested in becoming a natural products chemist. But Ruth Alice was looking at vinylpyridine as a Michael acceptor. Lillian Levy was working on the acidity of bridgehead sulfones. Ruth Alice Weill was my lab partner. Ruth Haines was subsequently Mrs. Doering and now is Mrs. Zeiss.

Ruth Haines was working on the structure of the hydration product of dicyclopentadiene, and the molecular rearrangements of the norbornyl type that show up there. Marshall Beringer was working on concentrated sulfuric acid rearrangement of isophorone, I think. But it's an aromatizing rearrangement of a gem dimethyl group. And somebody was working on the structure of the dimer resulting from the acid-catalyzed dimerization of things like propenylbenzene, anethole, and so forth.

GORTLER: You were an NIH [National Institutes of Health] fellow in 1947 and 1948. Did you do anything different during that period?

NOYCE: I finished my Ph.D. in November 1947, formally. I applied for a postdoctoral fellowship, and since the mold metabolite did show biological properties, I was granted an NIH fellowship for that work and continued that research. It was sort of a pre-doc for three or four months and then it was post-doc the rest of the year.

GORTLER: So you spent that time working for Doering.

NOYCE: Right.

GORTLER: Had he started the Katonah Laboratory yet?

NOYCE: No. That didn't happen for another year or two. By the way, Ruth Alice Weill had a lab of her own.

GORTLER: Was she a student of his [Doering's] while you were there?

NOYCE: Yes. She was an older woman who came back to school after her children were out and off to college. She set-up her own little lab at home and came in, maybe a couple of days a week, after she'd finished her coursework and continued her research out there. They were just beginning to set it up as I was leaving, and I'm not sure that everything had been arranged clearly. And as far as the construction of the big new building—that was still in the future.

GORTLER: They eventually ended up building a building?

NOYCE: Yes. It was designed to house a small library and six to eight research chemists.

GORTLER: Can you tell me anything more about the general atmosphere at Columbia that you think might have been of value, in terms of the growth of physical organic chemistry? For example, how did Elderfield react to this new stuff?

NOYCE: Now that he's dead, it's all right to tell stories out of school. I don't think Elderfield ever got along terribly well with Bill Doering. I think Elderfield was threatened by Doering because he seemed to draw students better than Elderfield. And so there's this apocryphal story about Doering. Every year the faculty has their review-promotion committee meeting, and they start with all the [full] professors in the room and they talk about whether they're going to promote any associate professors. And then the associate professors come in after a while and they talk about whether they're going to promote any assistant professors. Later in the afternoon, the assistant professors come in and they talk about whether they're going to promote any of the instructors.

So on this one occasion, the professors started at one o'clock. At 1:30 pm, the associate professors came in, and the assistant professors paced the hall. They did so for the rest of the day, as they were never invited in. There had been a terrible argument that went on for hours about whether the faculty group should promote one of the assistant professors. Of course, there was only one assistant professor on the faculty at that time: Bill Doering.

The other amusing thing about that, historically, is that Elderfield, Curtin, and Doering all left Columbia the same year and went independently in three different directions. I suspect if any one of them had known the other two were leaving, one might have stayed.

GORTLER: How did Curtin get along with Doering?

NOYCE: Curtin got along fine with Doering. Curtin was a post-doc with Jack [John D.] Roberts at MIT, and so he and Doering were on the same wavelength. But there were a lot of the conceptual things Doering spoke about that Elderfield felt were supercilious and artificial.

GORTLER: Is there anything else about that period you think might be helpful?

NOYCE: Actually, I'm sure I had good contact with Hammett. It wasn't definitive in the long run, but the concepts he spoke about were valuable to keep in mind. He had just got back from the military when I took the course from him.

GORTLER: What course did you take with him?

NOYCE: The course using his text [*Physical Organic Chemistry*] (6).

GORTLER: So he did teach a course using his text?

NOYCE: Yes, right.

GORTLER: Because he said he never taught an organic course there.

NOYCE: He called it a physical chemistry course. He was always thought of there as a physical chemist.

GORTLER: But he did in fact use his textbook in physical chemistry.

NOYCE: That's right. Hammett's course that particular year was a disappointment because he hadn't had time to do anything but grab his 1941 notes, which were stale and almost exactly like the book. Having been in the academic life for thirty years, I fully excuse him from having that time pressure that one time.

GORTLER: Did you ever listen to him again?

NOYCE: He was not a dynamic lecturer. No, I didn't take that opportunity.

GORTLER: I see. How did you hear about the job at Berkeley?

NOYCE: I heard about the job because the Berkeley department wrote to the Columbia department, and Bill Doering wrote a letter of recommendation. At the same time, I put in applications for a National Research Council postdoctoral fellowship and for the Society of Fellows at Harvard. It turns out that Ken Pitzer, out here, received the National Research Council fellowship applications for review. So my dossier was already here, simultaneously with my name showing up as a fellowship applicant. I had written to Ken Pitzer the previous year regarding some questions of conformational analysis from a communication that he published in *JACS* [*Journal of the American Chemical Society*] in 1946 or 1947. So, he was quite aware of my name early on. I suspect that that set of coincidences probably helped. People didn't fly around the country interviewing in those days. Jobs were offered on the basis of the dossier, mostly. I'd had contact with Wright at the University of Toronto, and I'd also made an application to UCLA [University of California at Los Angeles]. But I was quite happy with the job at Berkeley.

GORTLER: So they just hired you sometime that year and you decided to come here?

NOYCE: Yes.

GORTLER: What did you find when you came here?

NOYCE: That old red brick building. [laughter]

GORTLER: Were there other organic chemists here at the time?

NOYCE: Yes. [James] Cason and Bill [William G.] Dauben came as young assistant professors in 1945, as did [Henry] Rapoport in 1946. Marshall Cronyn came here in 1947. And then of course, [Gerald E. K.] Branch and [Thomas Dale] Stewart were senior members of the department.

GORTLER: Was Branch an organic chemist?

NOYCE: Yes. He was about sixty-five. He taught the pre-medical organic lab. T. Dale Stewart taught the big sophomore organic course. [Charles W.] Porter had just retired a couple of years before, so I never really met him. All three of those people, Porter, Stewart, and Branch, were inclined mechanistically and were excellent at physical organic chemistry. Branch with his study of structure and spectra, and Dale Stewart worked variously on the mechanism of bisulfite addition to aldehydes. His research was sort of murky when I came here. He was working on the alkylation reaction; the isobutane-isobutylene-*t*-butyl alcohol alkylation reaction. Trying to carry out reactions of gases with concentrated acid solutions is a mess.

Porter had been interested in nitrogen rearrangements. He published the ACS [American Chemical Society] monograph on nitrogen rearrangements back in the late 1920s. I think maybe he was a Stieglitz student or something like that. I'm not sure.

GORTLER: Yet when you think of Berkeley just before the 1950s, you don't think of it for organic chemistry.

NOYCE: That's very true. Primarily because the structure of the department when I arrived here had essentially no place for natural products. These organic chemists had interests so they could talk to [Gilbert Newton] Lewis, because he was such a dominant personality. Now, I

never knew Lewis because he died in 1946, I guess, but Bill Dauben knew him and I've heard him talk about Lewis. He was an enthralling personality. He was naturally enthusiastic. He had a kind of insight that brought things to the surface very quickly. But his thought about organic chemistry was simply that organic compounds could serve as frameworks upon which to deal with physical principles. And so you get the phosphorescence, the fluorescence, the triplet studies that [Melvin] Calvin did with Lewis as a post-doc, and you get the development of things like the glass matrix for getting materials cold and not getting them to crystallize—the isopropyl ether-isopentane sort of thing.

And the department had a weekly, Tuesday evening seminar conference, where the situation was apparently this: Lewis told somebody in the afternoon, “Why don't you talk about your research work tonight?” And the faculty was always ready. Those seminars always had a fair amount of discussion, which was why the department was so unitary in its outlook; everyone was drawn toward that center of physical principle.

Wendell Latimer became dean when Lewis retired, around 1941, and he remained dean until 1949. Latimer hired the group I was in; from Cason, to me, to those students hired in 1948. He felt, quite clearly, that the time had come for Berkeley—it was a big enough university and the area had become prestigious enough—to be strong in more than one aspect of chemistry. So, not only did he do that, but he also hired a big group of people to strengthen applied chemistry and create the chemical engineering department. So there's a corresponding four or five people in chemical engineering that are in this same age group, from fifty-eight to sixty-two. Hence, soon there will be a substantial turnover in faculty. But Latimer made that change in the personnel, personality, and diversity of the department and carried through with the administration. His changes worked, and that's why there was a much more diverse organic chemistry group by 1950.

GORTLER: Even with those two or three so called “organic chemists” who had a very physical outlook, one never thinks of the early Berkeley years as being a home for physical organic chemists.

NOYCE: Certainly not.

GORTLER: They just didn't turn out graduates like that.

NOYCE: Some of the other stories are very pertinent. Cason came to Berkeley from Vanderbilt [University] to start graduate study, but he couldn't find any chemistry that was to his liking. So he left and went to Yale. Conversely, Fuson was here, apparently as a graduate student, some people say, and he didn't do very well in the thermo course and so they asked him to leave.

The other thing was that there weren't very many of that group of people that were, how shall I say, politically inclined. Now, [Joel] Hildebrand was politically inclined. He served as president of the American Chemical Society and was on councils and so forth. People were very well aware of Hildebrand. But Lewis was not a traveler or a speaker. He didn't go around and give lots of lectures. And the same is true of many of the others in that group. For instance, Branch would have been a disaster if he'd tried to be a traveling lecturer; he was a lousy lecturer. And so, I think that's the reason the early talent at Berkeley didn't have a well-known public reputation.

GORTLER: Of the influx of that group of new organic chemists, who were you closest to in terms of interests?

NOYCE: My most closely aligned relationship—I'm not sure it was interest, but in terms of outlook and background, essentially—was with Bill Dauben,

GORTLER: He'd been in Bartlett's group?

NOYCE: No, that's his brother, Hyp [J. Dauben, Jr.].

GORTLER: Where did Bill come from?

NOYCE: Both of the Dauben boys came from Ohio State [University]. Bill got his degree with [Louis F.] Fieser. But he was also—

GORTLER: —Harvard-oriented.

NOYCE: He'd had that same kind of course structure background that Doering passed over to me. And Cason was a post-doc with Fieser, but his graduate education was at Yale, so he didn't have the full flavor of the Harvard thing. So clearly, Dauben and I came closest to talking exactly the same language, and we talked over quite a few things of mutual interest. Also, we had one or two joint publications (7).

GORTLER: Hydride reductions?

NOYCE: Right.

GORTLER: What was the seminar situation like at that time? You didn't have a group yet, correct?

NOYCE: No. At that time, we had an organic division seminar. The group was small enough that most every graduate student got a chance to talk almost every year. Then, Dauben's group became large enough that he had a seminar discussing current research results, and so my first two or three students and I joined with him for the first three or four years.

Then, when Andy [Streitwieser] came to Berkeley, he and I put together a joint group, which lasted about six years. We put it back together again in the 1960, and so Andy and I have run a joint discussion group periodically for fifteen or twenty years.

GORTLER: What kinds of courses did you first teach when you came to Berkeley?

NOYCE: The first course I taught was an organic laboratory for pre-meds. It had two hundred students, although it only had space for about one hundred and twenty-five. It was the last wave of students that returned to school after 1945.

GORTLER: That was an exciting group of students.

NOYCE: Yes. I was younger than many of them, and they were extremely serious, diligent, and very well organized. One doesn't see that good an average class, except on very rare occasions. But by 1950 or 1951, that period was over and things had settled down much more. I taught that course for six semesters in a row. Then I started teaching the major organic lecture sequence.

GORTLER: Who was teaching advanced organic chemistry at that time?

NOYCE: I taught the advanced organic a couple of times after Branch died in 1953. Then Calvin taught it and then I took over for him a couple of times when he was on leave. Around 1954, we introduced a group of special-topics courses for graduate students. Dauben gave one on polycyclics, Rapoport on alkaloids, and I gave one on terpenes and rearrangements; it was a mechanistic course.

GORTLER: Calvin taught organic chemistry, but was he considered an organic chemist?

NOYCE: Yes. He was considered an organic chemist.

GORTLER: We never think of him as an organic chemist, I guess because of his biochemical work.

NOYCE: He taught the short organic course for quite a while. He periodically taught the majors organic course, and in the spring semester he taught this course from Branch and Calvin (8). It's an interesting book if you've never read it.

GORTLER: No, I haven't. It keeps coming up.

NOYCE: I'll tell you a couple of things about that book. If you read it, you'll find two or three of the chapters that are nice, clear, and clean, and others are so murky that you can't figure out what's going on. Those first two or three chapters are the ones that Calvin rewrote, and the other chapters are what Branch wrote.

Branch became so entwined in building a thorough generality, like [J.W.] Gibbs' *Thermodynamics*, that the reader couldn't see the forest for the ABCs and X's (9). It's all in general terms—no carbon atoms, no nitrogen atoms. He discusses what happens during a displacement reaction using A, B, and C variables—the entering, the center, and the leaving group—and describing them in terms of electronegativity and polarizability; as to how the reaction should be easier or more difficult, but he doesn't reduce it to any examples. So it's sterile.

But having studied from Hammett's book, and then later looking at Branch and Calvin, I saw the extra level of generalization that Branch was trying to include. It is interesting that the two books were published almost simultaneously.

There were many things in Branch and Calvin that I appreciated. Branch was a man of almost infinite insight, but he couldn't write so others could grasp the utility and validity of his extreme generalizations. It was very hard to find practicality in his writings.

[END OF TAPE, SIDE 3]

GORTLER: You were fairly young in the 1940s. Were you aware of the transition that was taking place in organic chemistry? I suppose you might be aware of it in retrospect.

NOYCE: Yes, in retrospect I see it very clearly. Although, I'm not sure I recognized some kind of "changing of the guard" or "palace revolution" when I was twenty-three or twenty-four years old. It was just that I was very satisfied with the sort of conceptual material that I saw because it solved exactly the things that I found most frustrating with using a traditional textbook. And so it kept me looking forward in the same direction.

Everything I've done in the way of research has been to pattern, generalize, and explore. It's not so much things that other people have taken and tried to do with it, which is to generate new reactions; though, that's a very useful way of going, too.

GORTLER: What did you think would be the most important problems? Not necessarily those that you worked on—although we'll talk about those in a couple of minutes—but what did you think were the most important problems in organic chemistry? That may be an unfair question this long after the fact.

NOYCE: I don't know. I suspect that that kind of question, in a way, has a psychological component. I never got the feeling out of graduate school—which is the kind of feeling that I've got from some of the young candidates we've interviewed over the decades—that I shouldn't do any research unless I could convince myself that it was the most important thing in the world to do. I've always found the questions were sufficient if I thought they would be interesting to answer. So I was looking at things from that point of view. I was pursuing my interests, and I don't think I ever paid any great attention to what people thought was the most important, hottest new thing.

GORTLER: Who were you reading? Who did you think were the influential people of the time?

NOYCE: That's another matter. That's easy. I was reading Roberts and Winstein and Bartlett and, if they ever appeared, papers by Doering. [Frank H.] Westheimer was another of my idols.

GORTLER: Were you reading papers by English chemists?

NOYCE: Yes, I was reading [Christopher] Ingold, and a little bit later I was reading [Clifford A.] Bunton, and [Vladimir] Prelog.

GORTLER: The problems you started to work on when you came here—you did a rearrangement problem, a terpene rearrangement problem, correct (10)?

NOYCE: Yes.

GORTLER: Had you seen that in the literature or had someone talked about it?

NOYCE: That was one of the problems that came up in one of the Doering's discussion groups. Marshall Beringer and I proposed different mechanistic solutions to it.

GORTLER: You'd made that proposal during a seminar?

NOYCE: Yes. Also, while I was a post-doc, I'd read the first couple of papers on lithium aluminum hydride and I did a couple of the experiments in the afternoons and on weekends on the stereochemistry of lithium hydride. I always had a lot of fun thinking about and experimenting with stereochemistry, so you'll notice a fair amount of stereochemical material in this. That lactone synthesis with John Weldon—I thought lactone was a structure in the natural product, and I turned out to be correct, but that was my major trip into synthesis for synthesis' sake (11).

The other major thing I began doing—and this is a long trail going from 1948 down into the 1960s—was looking at the acid-catalyzed counterparts of things like the aldol condensation.

GORTLER: Most of those had been base-catalyzed reactions, correct?

NOYCE: Yes, they were base-catalyzed reactions. So that's where we got into strong acid solutions. The acid-catalyzed counterparts were fundamentally slower than the base-catalyzed reactions by about a million-fold, so I had to use far more concentrated acidic solutions, which, coincidentally, brought me back into Hammett's realm of the acidity function. Fortunately, I was aware of the acidity function because I had listened to Hammett discuss it. And although I didn't appreciate or fully understand what he was talking about at the time, I developed an understanding of it later, by myself.

GORTLER: It's not an easy concept to understand on a single hearing. Your first graduate student was Don Denney, correct?

NOYCE: No. My first one was John Weldon, and Don Denney was my second.

GORTLER: But you did get graduate students fairly early, correct?

NOYCE: Yes, and that was a very good thing. Given a limited supply of graduate students, Rapoport, Dauben, and Cason felt that everybody should have something. Therefore, if there were five graduate students and four faculty members, every effort was made to assure that every faculty member had at least one graduate student. I've seen some situations around the country where that kind of philosophy is not promulgated or put into operation. If someone grabs all the graduate students, which are in limited supply, it leads to a very bitter feeling between departments.

GORTLER: Is that still true here, or are there enough graduate students?

NOYCE: We really have enough now to go around, so that the other limitation is more a matter of space. For instance, it's a little harder to convince somebody to have an accretion of space that will handle forty-five people. So basically it's become somewhat more *laissez-faire*, though I think if we were put to it, we would probably be able to convince the system here to operate that same way again.

GORTLER: When Andrew Streitwieser came a couple of years later, was there a conscious effort to hire another physical organic chemist, or were you just looking for a good chemist?

NOYCE: That is in the days when these things were done much more by the back room technique. I was not really involved. I was only in my third year here.

GORTLER: Apparently, Pitzer did that pretty much on his own.

NOYCE: Yes.

GORTLER: Was Pitzer the dean at that time? Who was chairman then?

NOYCE: Dean and chairman were one position at that time. There was a two to three year period—between 1949 and about 1952—that Hildebrand was dean, even though he was older than a dean was supposed to be. That was because Pitzer was in Washington as director of research for the AEC [Atomic Energy Commission]. Everybody knew that Pitzer was the one they wanted to be dean, but he wasn't coming to be back here until 1952. So the entire

department wrote a letter to the president saying that they'd like him to set aside the rule that the dean couldn't be over sixty-five and let Hildebrand be dean for a period of two or three years. Hildebrand was the one who would have hired Streitwieser officially. So I think it was probably Cason, Rapoport, and Dauben who said, "If Jack Roberts says he's a great student, we'd better take him."

GORTLER: Roberts was recognized already, correct?

NOYCE: Yes, he was recognized quite early on. But I wasn't really involved. I don't remember being involved in discussions about whether we should or shouldn't hire him.

GORTLER: If you look back, do you have any perceptions about the transition that was taking place? Can you see reasons for the development of physical organic chemistry after the late 1920s, the early 1930s, and what grew into what you saw as organic chemistry in the 1940s?

NOYCE: I think it's a matter of some of the advances in understanding that show up in various parts of physics finally permeating into the less theoretical sciences. And just as there are questions of the chemistry of sugars that have taken a decade or so to permeate into the far reaches of biology, there is electronic structure, the Schrödinger equation, and so forth, finally permeating all the way into organic chemistry. You would expect to see an increased rationality simultaneously in all parts of organic chemistry. If you have an increased rationality in spectra and molecular energies, as in aromatic systems and so on, you ought to see it in a lot of other ways. So I think that's the thing.

Now, why did it happen, where it did, or why didn't it happen some place else? For example, why didn't it happen more strenuously at the University of Chicago, where [George] Wheland was at the forefront of the theoretical side of this development as one of [Linus] Pauling's students? And what was it that made it something that came about so strikingly at University College London, Ingold's home, and at Harvard? I just don't understand.³

GORTLER: Maybe if we tell the story somebody else will fully understand. I can't give you reasons for that.

NOYCE: It may have been the outlook of people like [James B.] Conant and Arthur Michael who was also at Harvard. That outlook has a relationship between structure and kinetics. A structural outlook and a kinetic outlook to mechanism are more fertile grounds than one that's quantum-mechanically based. In other words, Branch's orientation here was toward the

³ See Addendum. Note written in July 2000.

quantum-mechanical side of things, and there's far more quantum mechanics in Branch's book than there is in Hammett's book. But there's less about down-to-earth organic reactions, like semi-carbazone formation and ester hydrolysis and so forth.

GORTLER: Right. Organic chemists, at that point, were not ready for the quantum-mechanical approach. In the 1930s, they weren't even ready for the kinetic structure-reactivity approach, I think. Apparently the chemists here, Dauben and Rapoport, were already willing to accept that kind of change.

NOYCE: That's not quite safe to say. They would be ready to have that as one package of one kind of organic chemistry but there's another. I'm talking now not so much about Dauben but about the others. Another package of organic chemistry, which is the traditional, is to know all the reactions and be sure you can make pyridine, picoline, quinoline, and so on.

They weren't willing to see this one [the traditional] evaporate to make room for this one [the mechanistic]. That took a little longer to come about out here. The basic undergraduate course, for example, should be mechanistically oriented in a fundamental way, but I think that took a long time to come around the country. The first real attempt at this was by [William E.] Bill McEwen. I should have mentioned Bill McEwen before because he was a Doering student; he was there at the same time as I was.

His book was one of the earliest attempts to incorporate a lot of these mechanistic things, except that it wasn't integrated very well. It [the mechanistic part] was put in a separate chapter and then not used regularly.

But there are two other books that have some of that flavor, and very much earlier. There's Conant's book (12) and [Howard J.] Lucas' book (13).

GORTLER: Yes, those books have come to my attention. Do you think that the War had an effect, one way or another, with respect to the development of physical organic chemistry? Or do you think physical organic chemistry was just ignored during the War years?

NOYCE: I believe it had an effect, but I don't think I'm in a position to justify that statement. I think that there's a group of people older than I am who might be able to. I think that question would be interesting to ask Jack Roberts, Gardiner Swain, and Bill Doering.

However, I can offer one example along that line. There was an extensive, intensive study about the hydrolysis of mustard gas, as against the horrible day when it might be used, and there—you see the mechanistic study basically proves that's really impossible to do effectively. That might convince some people that mechanistic organic chemistry has a place, as against simply trying another hundred different add-ins to the soap and the washing solution.

GORTLER: What do you think has happened to physical organic chemistry? Has it become completely absorbed into organic chemistry or is it still a distinct area?

NOYCE: To my mind, there are now two varieties of physical organic chemistry: the structurally-oriented physical organic chemistry that is my own inherent milieu, to which I may have contributed by research; as against another kind that is strongly oriented mathematically.

Now the former—the structural orientation of physical organic chemistry, mechanistic style, stereochemistry, and energy reactivity situations in a descriptive sense—I think that has been incorporated into the heart of all the places organic chemistry shows up. It shows up in the biological sciences when they talk chemically, as often as it does with natural products synthesizers.

Now, the other part of physical organic chemistry, which is the one that either leans on a big machine to measure spectroscopic constants, or leans on a big computer to calculate everything from scratch, is an entirely different area, and I don't know quite what to make of it. I'm not sure where it will end up. I know I'm the wrong age to consider becoming a part of it. So I watch it.

GORTLER: Anything else you can think of that might be of value to me, in doing an overview of the development of physical organic chemistry?

NOYCE: I've always been intrigued by how physical organic chemistry was almost started by [Arthur] Lapworth, in 1900, practically at the turn of the century. And then how it lay dormant for twenty or twenty-five years, until it was picked up by Ingold and [Robert] Robinson in the middle 1920s.

GORTLER: Right, Hammett mentioned that in a retrospective article, ten to fifteen years ago, and he had no answer for it (14). I'm not sure what the answer is, except what you suggested before; essentially, that there wasn't an appropriate theoretical overlay yet. You know, bonding theory hadn't reached the proper point and the picture of the atom hadn't yet been developed fully.

NOYCE: That's also perhaps pertinent to [Hans] Meerwein's early papers, right around 1920 to 1922, where he proposed that these rearrangements happen with cations (15). And you see, that followed so closely after Lewis' electron-pair publication that people weren't ready to think in those terms yet (16).

Another name that we've not mentioned that certainly deserves some attention along those lines is [Frank] Whitmore, at Penn State [University] and his contributions to carbonium ion theory.

GORTLER: Yes. Is there anyone else that you can think that I should talk to? I've talked to a number of people, but perhaps a new name might occur to you that I haven't thought of.

NOYCE: Have you talked to Bill Young at UCLA?

GORTLER: He died.

NOYCE: I was afraid I remembered that.

GORTLER: Someone had interviewed him up to the period I'm interested in, so that was good.

NOYCE: Have you talked to [Robert Hill] DeWolfe at [University of California at] Santa Barbara, who was one of his students?

GORTLER: No.

NOYCE: DeWolfe has been at Santa Barbara since—I'm tempted to say 1955, but I'm not sure of that. Have you talked to Erwin Schoenewaldt, who was one of Doering's students, just a year or two after me? He's at Celanese [Corporation] or maybe it's Merck [& Company, Inc.]. He's in the New Jersey area.

GORTLER: I haven't talked to Doering yet, so I suppose he'd be the one to start with. You say Schoenewaldt was a few years after you?

NOYCE: Yes, he was there before I finished, so he's not that much after me. But he has been active in industrial circles. I think that would be an interesting point of view.

Also, Tom Ashner was at Columbia when I was first there and is of that group that was there between 1945 and 1946. I don't know where he is now. He went to work for Smith Kline [Smith, Kline, and French Company], I think it was, in 1947, and the first thing that he did was to adapt his studies on the racemization of alcohols. He did so by oxidation-reduction to

ketones to taking the wrong isomer of a phenethyl amine, racemizing it, and then recovering half of the material they were throwing away at that point. In the process, of course, he saved them thousands of dollars. They got a double supply of starting material for free. So he did an aminoimine oxidation-reduction, coupled like the Meerwein-Ponndorf-Verley reduction.

Then he subsequently went back to school and got a degree in pharmacology, so I don't know where he is now, but it's possible that you could find out where he is. He'd be interesting to talk to.

GORTLER: Thank you very much. I really appreciate your taking the time to talk to me. Your remarks will be very helpful.

[END OF TAPE SIDE 4]

[END OF INTERVIEW]

ADDENDUM

Professor Noyce wrote the following in July 2000.

There are some severe overtones of British political infighting involved with this question.

Sir Robert Robinson, apparently to his dying day, thought that he was the inventor of modern mechanistic organic chemistry in England, Ingold's contributions notwithstanding. Add to this the fact that Ingold suffered from a severe interruption in his program due to WWII. The University College London department was largely evacuated and relocated in Wales (I think it was Aberystwyth). How severely this cut into the research productivity, I don't know. And I also don't know right off the top of my head exactly what kinds of war work each of these two centers was diverted to. I do recall that massive set of papers on "Aromatic Nitration" by Ingold about 1950, or shortly thereafter. That was probably his war work, if I can put it that way.

However, Ingold was much more successful in creating, and selling, a "catchy" pattern of mnemonics to deal with some of the various concepts. Fortunately both got their Nobel Prizes⁴, so their bitter antipathy toward each other never exploded. This bent of patterning showed up also in Ingold's participation in developing the rules for dealing with configuration,—Cahn, Ingold, Prelog.

I wish that I had a sharper picture of this part of the past. Bill Dauben probably did; he spent his Guggenheim in 1951 in Oxford with Robinson. But Bill died in 1997.

⁴ Ingold did not receive a Nobel Prize.

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