CHEMICAL HERITAGE FOUNDATION

PHILIP E. EATON

Transcript of an Interview
Conducted by

James G. Traynham

at

Chicago, Illinois

on

22 January 1997

(With Subsequent Corrections and Additions)

Philip Eaton

CHEMICAL HERITAGE FOUNDATION
Oral History Program
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Chemical Heritage Foundation
Oral History Program
315 Chestnut Street
Philadelphia, Pennsylvania 19106

PHILIP E. EATON

1936	Born in Brooklyn, New York, on 2 June

Education

1957	B.A., chemistry, Princeton University
1960	M.A., chemistry, Harvard University
1961	Ph.D., chemistry, Harvard University

Professional Experience

1960-1962	Assistant Professor, University of California at Berkeley
	The University of Chicago
1962-1965	Assistant Professor, Department of Chemistry
1965-1972	Associate Professor, Department of Chemistry
1972-present	Professor, Department of Chemistry
1963-1969	Research Fellow, Alfred P. Sloan Foundation
1983-present	President, Eaton Associates
1965-1977	Consultant, E. I. du Pont de Nemours & Co., Inc.
1968-1972	Consultant, National Institutes of Health
1983-1989	Consultant, Dow Chemical Company
1984-present	Consultant, U. S. Army ARDEC
1985-1994	Consultant, Enichem Synthesis
1986-1991 1996-present	Consultant, Fluorochem, Inc.
1986-1991	Consultant, SRI International
1988-present	Consultant, Geo-Centers, Inc.
1990-1991	Consultant, Displaytech Corporation

1992-1995	Consultant, Steroids, Ltd.
1996-1997	Consultant, DAS Group, Inc.
1998-present	Consultant, Eastman Chemical

Honors

1963	Alfred P. Sloan Foundation Fellow
1975	Research Award, Rohm & Haas Company
1985	Alexander von Humboldt Prize
1995	Alan Berman Research Publication Award, Naval Research Laboratory, U.S. Navy
1997	Arthur C. Cope Scholar Award, American Chemical Society

ABSTRACT

Philip Eaton begins the interview with a description of his childhood, parents, and early education in Brooklyn, New York. At age seven, Eaton and his family relocated to Budd Lake, New Jersey, where he attended Roxbury Grammar School and later Roxbury High School. Eaton displayed a great interest in science during his high-school years, and his parents' and teachers' encouragement strengthened his desire to major in chemistry. He attended Princeton University, receiving his B.A. in 1957. After graduating from Princeton, Eaton attended Harvard University for both his M.A. and Ph.D. degrees. While at Princeton and Harvard, Eaton worked during the summers at Allied Chemical, where his group leader, Everett Gilbert, had a profound effect on his career. There, he first became involved with cage chemistry, specifically Kepone. In his final years as a graduate student at Harvard, Eaton accepted a postdoctoral assistant professorship at the University of California, Berkeley. There he taught introductory organic chemistry with Melvin Calvin. In 1962, he joined the faculty of the University of Chicago, where he remains a professor today. Shortly after his arrival at Chicago, Eaton began researching chlorocarbon compounds, which led him to cubane synthesis. With the assistance of his postdocs, Eaton synthesized on several other cubane-based compounds. Other projects included photochemistry work and dodecahedrane synthesis. Eaton's students praised his teaching methods and his dedication to excellence in education. His research accomplishments have earned him several awards, including the Humboldt Award and the Arthur C. Cope Scholar Award. Eaton concludes the interview with a discussion on the future or scientific research, maintaining excellence in chemistry education and research, and thoughts on his wife, Phyllis.

INTERVIEWER

James G. Traynham is a Professor of Chemistry at Louisiana State University, Baton Rouge. He holds a Ph.D. in organic chemistry from Northwestern University. He joined Louisiana State University in 1963 and served as chemistry department chairperson from 1968 to 1973. He was chairman of the American Chemical Society's Division of the History of Chemistry in 1988 and is currently councilor of the Baton Rouge section of the American Chemical Society. He was a member of the American Chemical Society's Joint-Board Council on Chemistry and Public Affairs, as well as a member of the Society's Committees on Science, Chemical Education, and Organic Chemistry Nomenclature. He has written over ninety publications, including a book on organic nomenclature and a book on the history of organic chemistry.

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INTERVIEWEE: Philip E. Eaton

INTERVIEWER: James G. Traynham

LOCATION: University of Chicago
Chicago, Illinois

DATE: 22 January 1997

TRAYNHAM: Professor Eaton, I think I've read that you were born in Brooklyn, New York, on June 2, 1936. Can you tell me something about your parents and your early childhood?

EATON: I'm not one to remember things particularly well. I started elementary school in Brooklyn at P.S. 102, if I remember correctly. When I was seven, I believe, my parents decided that we should move permanently to a summer home, which we had in New Jersey in a small town called Budd Lake. We left Brooklyn. The driving force behind this was that my father had a small factory in Brooklyn that used a lithographic process he had invented for doing direct printing on cloth. It was a non-unionized factory. At the time, as I understand it, the unions in Brooklyn were very corrupt. Every time my father opened the factory each day, the employees would come in, and then they would then put a large steel door across the entrance and lock it so the union gangsters could not get in. One day someone forgot to lock the door; the gangsters got in and threatened my father. When he went home that night, he locked the door for the last time, and we moved to New Jersey.

My father was a very inventive man. He started his business at the depth of the Depression, and built it from scratch into something quite substantial. He was a very ingenious fellow. My mother was a loving housewife and a good mother, whom I enjoyed very much indeed. As I say, when I was about seven, we moved from New York to Budd Lake, a small town in New Jersey about 50 miles northwest of New York. My father opened his business in a town nearby.

TRAYNHAM: Did he continue in the same business?

EATON: Yes, and did reasonably well for the time. The area that we moved to was very much a rural area. In the summertime it was a vacation area for people from New York. In the winter it was quite deserted, and I had a ten-mile ride each way to school in a small town called Succasunna. Actually it was quite a nice school, and I enjoyed it very much. It was an interesting transition from New York—Brooklyn—to go to this small town.

I remember very distinctly having to take an examination to decide what grade I would be in. I did very well on this examination, except that I could not distinguish from pictures the difference between a mouse and a rat, having seen neither one of them in Brooklyn. [laughter] In any case, I skipped a couple of grades and got started in fourth grade right off the bat.

These schools had very dedicated teachers, I must say. It was to turn out that I was the first of the high school graduates from this area to go on to a major institution for further education, but I would not in any way want to put down the education that I got at Roxbury Grammar School and Roxbury High School. They were really quite good, and I have some very fond memories of teachers there. I was thinking about it this morning. I actually remember from fourth grade a Mrs. Eiloff, whose name I can't spell. Actually, she was Miss Eiloff. Also, there was Miss Congedo. Both were general science teachers.

Most particularly I remember a teacher in my freshman year of high school, a Mrs. [Celeste] Mendes, whom even now I keep in contact with. She introduced me into the outer world, took me to opera in New York. She taught an interesting combination of courses: Latin and Family, Home-Family, or something like that. She very much had a picture of civilization that was quite different from what one normally found in small towns in New Jersey. She was very important in my development.

That pretty much does it. I was one of the few in high school who was really stimulated by the courses in science. There were about a half a dozen others in a class of about one hundred and thirty people. We all banded together, enjoyed things. We went on to the National Honor Society and things of that sort. We were not appreciated by our fellow students, who had different things in mind, but I think that's typical of what happens.

TRAYNHAM: Did you know that you wanted to study chemistry when you left high school?

EATON: Oh, yes. I was very early on a tinkerer in the basement with chemicals. My parents were quite supportive of this, buying me chemistry sets and so on. I went perhaps a little bit too fast. I remember snitching some money from my father, and sort of "secretly" ordering various flasks and things of that sort, and ultimately being found out; being penalized by having my laboratory shut down for a year. It taught me a very good lesson about honesty—even if I was pursuing thievery in a higher interest.

My parents always encouraged me this way. I remember very distinctly my father taking me to New York to a small place that sold scientific instruments and things of this sort, and buying me my first microscope. I was always interested then in biology and chemistry; later, it became a focus in chemistry. Although my father was quite strict in terms of making me work around the house, mow the lawn, help him in his factory, and things of that sort, I was never under any pressure to take over the business, even though I was the only son. He always was quite supportive of my going on to get an education, even though he himself was self-educated. I'm not sure if he even had a high-school degree.

TRAYNHAM: What about your mother? What education level did she get?

EATON: Again, I'm not sure if she finished high school, but both my parents read. We had many magazines in the house, daily newspapers. This kind of thing was rather uncommon in that area of the country at the time. Fifty miles from New York in the 1940s was considered an enormous distance. The area now is a bedroom community—a commuter suburb of New York. When I went back there some years ago, I was dumbfounded by how it had changed.

TRAYNHAM: When you graduated from high school, did you have any hesitation about going on to the university, or was that definitely in the plan?

EATON: No, it was clear that I was going to do that. Some years earlier my parents had taken me to Princeton University. Actually, Princeton was on the way to Atlantic City from where we lived. We toured the campus. I remember, again quite distinctly, that at that time Firestone Library was being built at Princeton. The workmen there were hand-cutting the stones to do this, which impressed me tremendously. I still have a mental picture of that. Clearly, I wanted to go to Princeton. It was well located, it had a good reputation, and so on and so forth. Although I was admitted to numerous other places, Princeton was the one I wanted and the one I went to. I think it was a very good choice.

TRAYNHAM: Well, yes. What do you recall specifically, if anything, about your education at Princeton that is worth recording?

EATON: Yes. Well, there again, it was an interesting experience. I came into Princeton from this somewhat backwater country school at a time when the majority of the students at Princeton were prep school graduates—Exeter, Andover, and whatnot. It was a little disconcerting for me. I was helped in that when I was a junior in high school, my parents sent me to Europe on the Experiment in International Living. I lived for a summer in France. Then after my senior year in high school, I did the same thing in England. This gave me somewhat of a reasonable background of how to handle more cultured people, as the group that I traveled with was a very good group of people.

Still, Princeton was quite a shock. It was an interesting adjustment to make. I certainly had some trouble in the freshman year, because the chemistry course and things of that sort that I had had in high school were not competitive with what many of the prep school people had. I was admitted to the honors chemistry course, and in due course did quite well. I graduated summa cum laude.

It was interesting that at the time—and I think probably even nowadays—undergraduates were easily incorporated into research programs. I did some research with Dick [Richard] Hill. He was a delightful person who really helped me understand what science was about. I enjoyed that very much.

At the same time, in the summers I did work in industry at Allied Chemical. This came about in a strange way, I suppose. My mother broke her hip. The dog pulled her over. She went to the hospital and shared a room with the wife of the vice president of Allied Chemical. By the time she came home, I had a job at Allied Chemical—mothers being what mothers are. [laughter] This actually worked out beautifully, just beautifully. I worked every summer, from my sophomore year in college through my graduate work at Harvard, at Allied Chemical. They helped to send me to graduate school—provided equipment, and things of that sort.

That was a marvelous experience. The people there really understood industrial chemistry and taught me a <u>lot</u> in terms of technique. It has stood me in very good stead over the years. In particular, one person there, Everett Gilbert, really had a major impact on my career. Perhaps we can come back to that later.

One thing I should mention now about Gilbert. He was a group leader, and he had a very accurate picture of what was useful industrially. He used to say, "If you can't do the reaction on the steam bath in a two hundred fifty-milliliter Erlenmeyer, it'll never be useful." This taught me a lot about how to handle compounds. Nowadays, of course, we don't ever do reactions on a scale that's that large. Still, as an attitude, it was very important to me. The whole experience at Allied was very useful.

It coincided very nicely with my education. Indeed, the work I did at Allied Chemical concerned, for the most part, at the time a compound called Kepone, which was being developed by Allied as an insecticide against the fire ants. In fact, afterwards it was used extensively as an insecticide against the fire ant. It was banned a few years ago because as a chlorocarbon, it is not degraded easily. There are worries about it being a carcinogen, and so on and so forth. It's rather interesting that the fire ants now in fact are back to being a major problem, because there are no good chemicals to use against them.

More to the point, this compound Kepone is a cage compound. I got involved in cage chemistry very early. In a formal sense, Kepone is a bis-homocubane. I can probably trace everything I've done in chemistry back to the problems with Kepone. It's a long, tedious, tortuous story, but there really is an intellectual connection between these things.

This whole business with my mother breaking her hip and going to the hospital, and Allied Chemical, really worked out wonderfully. I very much understand the purpose of putting mothers up on pedestals.

TRAYNHAM: Good. When you were at Princeton, I believe you did a research project?

EATON: Yes. It was with Dick Hill. This actually concerned some cyclic compounds that one could get from dibenzyl acetone. I had occasion recently to go back and look at my senior thesis (1). I was reminded that in these compounds, solvent was encapsulated in the structure. At the time, we <u>knew</u> that. We knew that we had these solvates and that we could not get the solvent molecules out, and we left it at that. That was a very foolish decision because years later, [Donald J.] Cram won the Nobel Prize, with [Charles J.] Pedersen and Jean-Marie Lehn, of course, for these encapsulations and so on. I think really it was the same phenomenon. I wasn't ready to follow it up, and I don't think Dick Hill saw it in that same light. In retrospect, it would have been many years before its time, had we picked it up and gone further with it.

TRAYNHAM: The title of your senior thesis included reference to cyclopentadiene or dicyclopentadiene, didn't it?

EATON: In my senior thesis, no. It was in my Ph.D. thesis (2), but not in my senior thesis.

TRAYNHAM: Well, you were so productive in those days it's hard to pick out one from another.

EATON: Oh, here we go. Yes, the title of my senior thesis was "Giant Ring Di-Ether Kepones: Applications and Conformational Analysis" (1). As I said, I reread this recently. I understand the chemistry now much better than I did then, but I was truly distressed to see how poorly I wrote at that time. I hope it has improved.

TRAYNHAM: I have shared the same feeling as I look back at my dissertation now. I would not accept it from one of my students today.

When you graduated from Princeton, did you have any idea about graduate school other than Harvard? Or was your focus solely on that?

EATON: No, it was pretty much focused on Harvard. My interest at the time was in physical organic chemistry. It turned out, my definition of physical organic chemistry—or my thought as to what physical organic chemistry was—was very different from what it really was. I had always been fascinated by, literally, the physical properties of organic compounds: where they melted, how they melted, things of that sort. It seemed to me that physical organic chemistry would be something that considered that. I was dumbfounded when I got to Harvard and found that physical organic chemistry had to do with things like ionization and arrow-pushing and mechanisms, and things of that sort. Although in fact many of the people at Princeton had come from Harvard, my education at Princeton was exceedingly classical. Basically, it was lasso chemistry instead of mechanistic chemistry. You learned a lot of reactions and a lot of structures, but knew very little about how these things proceeded.

Harvard was a real shock. It was certainly the place to go. I think four of the Princeton chemistry majors went to Harvard. All of us had a shock, but ultimately, we did very well.

Oh, yes, I did at least apply to Yale. I remember that. I went and I visited at Yale, at Sterling Laboratory. This goes back almost forty years—Sterling Laboratory at Yale was awful. I think I once said I would not take a pee there, it was so dirty. I don't think it's changed much in the last forty years. The Yale people have really had a burden that way, so much so that I would not have considered going to Yale. I don't remember if I applied elsewhere—probably MIT. But the Harvard thing came through very quickly, so I took that. I was perfectly happy; I enjoyed it very much. I had to struggle with some of the people there who were much smarter than I was. But in fact, looking back, I did a good job, particularly in that I was already a superb experimentalist because of my experience in industry. That was the way to endear yourself to any professor: to be good in the laboratory and know how to handle various bits of apparatus. I have always been quite adventurous when it comes to laboratory work, so I had no problem getting along.

Because I had these research problems from industry—this background in industry—I was quite eager to continue some of that work. When I came to Harvard and talked to the various professors about doing this, most were very much against it. They wanted to give me new problems. I was quite interested at the time in working with Paul Bartlett, who was certainly the leader in physical organic chemistry in the world. I thought very highly of his work. When I talked to him, he agreed to allow me to work on my project for a maximum of six months. Then I would have to switch to one of his projects.

A younger man on the faculty, a non-tenured man, was Peter Yates. Peter—Professor Yates, I called him "Professor Yates" at the time—was much more amenable to taking on this problem that I had had in industry. He was quite interested in it, and so I decided to work with him. Many years later, I was having a long chat with Paul Bartlett, because we became friends over the years. He said to me, and it's a comment that I treasure, that one of the mistakes he had made in life was not allowing me to do my project with him. I always get a very rosy feeling when I think about it.

TRAYNHAM: I'm sure you do.

EATON: Yates was a very interesting advisor: very thoughtful, very helpful. I played bridge with him most every night, and I got a very interesting education with him. He was smart enough to recognize—in fact, quite brilliant—to recognize that an incidental observation that I had made in the laboratory, which came from all of this business with these chlorocarbons at Allied Chemical, related very much to the mechanism of the Diels-Alder reaction.

That was the reaction that at the time was being looked at very closely by Professor [Robert B.] Woodward and Tom [Thomas] Katz, who was a few years ahead of me. I had noticed the rearrangement reaction that was catalyzed by aluminum chloride. I remember again sitting in Peter's office and considering this observation, and then his saying, "If this is the case, then the Diels-Alder reaction itself should be catalyzed by aluminum chloride." Indeed I then proceeded into an investigation of this area. I demonstrated that the reaction, which was long thought to be un-catalyzed, was in fact catalyzed by Lewis acids, particularly the reaction with dienophiles containing oxygen. This in fact was a very important observation.

Over the years—although after the initial observation, I didn't follow it up—it has become extremely important in organic synthesis. It is a very significant reaction. Many Diels-Alder reactions in organic synthesis are now carried out with Lewis acid catalysis. Even enantio-specific syntheses can be done by replacing the non-chiral Lewis acid—for example, aluminum chloride—with a chiral Lewis acid, and so on. This has had really a major impact on organic chemistry. The initial discovery was mine. Well, the initial discovery was certainly Professor Yates' recognizing the importance of an observation. I guess that has always had a powerful influence on me, to think deeply about every observation—that it may have more significance than the trivial thing that's happening. That's been a helpful thing.

TRAYNHAM: Is there anything else specific about your graduate education that you recall or would like to record?

EATON: Well, some things are amusing. I took some of the—well, I had taken for me, I should say—some of the very early NMR spectra. Harvard had one of the early machines—not by any means the earliest. The Harvard organic group was always fairly primitive in terms of getting instrumentation. When they got an NMR machine, this was about 1959-1960. At that time, of course, air conditioning was very rare. I remember that you had to work in this room in your bathing suit, it was so hot from the magnet. My thesis, my Ph.D. thesis, has some of the very early NMR spectra in it (2). I remember very distinctly at my oral examination, Yates and Woodward arguing ferociously about the interpretation of the spectrum. As the years went by and NMR became more sophisticated, I realized that both of them were wrong. You see how techniques mature and become useful. Indeed, some of my colleagues went on for a few years to become NMR jocks and then suffered very much when simple NMR machines came out and everybody could, well, pick them up out on the street, which was a very fortunate happening.

Harvard was a good experience: a great group of people who really expected to produce and to argue with one another, and so on. The interactions between students in different disciplines, at that time, was very strong and very useful. I don't think it's quite like that now, but that's another matter.

TRAYNHAM: When you graduated from Harvard with your Ph.D. degree, did you go directly into academic employment?

EATON: Well, the situation then was very different than it is now. A few stories come to mind. Before I went home, the last summer before I got to write my thesis—it was typical that I would go home because I would work at Allied Chemical, which was near to my home. At that time support for students, of course, was nonexistent. When I went to Harvard for graduate school, I had a teaching assistantship, but I had to pay my tuition. Harvard insisted on a bond before they would take me. All the students had to do that—not like nowadays, when most graduate schools are panting for students and would do anything to get them. In any case, before I went home, Peter said to me, "You'll have to think about what it is you want to do." I mentioned something about maybe doing a postdoc in Europe. Peter's comment was, "Do you want to ski, or do you want to go to the music festivals?" He had absolutely no thought that I should do a postdoc in Europe.

He did think that I should go into the academic world, and said so. I was very surprised by this—dumbfounded, in fact. It was really not a common occurrence that people went into the academic world. Since I had these very close relationships with Allied Chemical, I had always assumed that I would go into the industrial world. It was a world I had a great deal of respect for, both because of my interactions with it and because I was raised by a father who was very much a businessman. I liked things like that. I said to Yates, "Well, I thought I was going to go into industry." Yates said to me, "You go home and think about it this summer. If you still want to go into industry, don't come back." It's an extraordinary statement. Of course I went home, and I thought about it. I came back and I said, "Okay, let's see what you're talking about."

TRAYNHAM: That was before you completed your doctoral requirements?

EATON: Yes, exactly. Of course, I needed to play both ends of the stick because you had to get a job of one sort or another. Industrial interviewers then were very much like industrial interviewers now. They came to Harvard, they looked around, and they invited you out if they were interested in you. One of these places was Hooker Chemical. Of course, they were very interested in me because Hooker was the manufacturer—still is—of hexachlorocyclopentadiene, which is the compound that goes into making Kepone. I was pretty much the world expert on the chemistry of molecules of this sort. A good part of my thesis involved these chlorocarbons, because of the connection at Allied Chemical.

Hooker asked me to come out for an interview. This was arranged. I flew from Boston supposedly to Buffalo, where Hooker was. This was on a Friday in the winter. Buffalo was snowed in, as it often is now. Instead of landing in Buffalo, we landed in Chicago. This was a very different time, and the airlines offered bus service from Chicago back to Buffalo. This was a Friday, and it was hopeless because I was not going to arrive in time for the interview.

Some months earlier Michael [J.S.] Dewar had been at the University of Chicago.

[END OF TAPE, SIDE 1]

EATON: Michael Dewar had spoken one time. I was absolutely enchanted by Michael. I thought he just gave this fabulous, fabulous speech. He was a fantastic character, and so on. I found myself in Chicago. I was one of these eager beaver young men. I called up from the airport because I had nothing else to do. I couldn't get out of Chicago. I called up his office here and got his secretary, who was a charmer. She used to, in fact, be a Ziegfeld Follies dancer. It turned out that Michael was at De Paul University. I was just, you know, taking a chance that I would be lucky enough to talk with him again. His secretary was sufficiently on the ball, and she called him at De Paul. He was absolutely delighted that he had an excuse to get out of spending the day at De Paul, which he had found terribly dull.

Well, he cancelled his plans there, came back to the university. I came in from the airport. I spent an afternoon with him and then the evening with Michael and his wife, Mary. I enjoyed this scientifically, fantastically. I remember they were living in one of the townhouses here on 57th or 55th Street, and going there for dinner. They were, well frankly, big drinkers, and I had a scotch. I was not much on drinking, but I had a scotch to be appropriately courteous. Every time I would take a sip of the scotch, Michael would fill it right back up to the top, like the Japanese do now. By about ten o'clock or something in the evening, when I had to go back to O'Hare [International Airport], I was just totally soused. It was embarrassing. Again, I have this picture of me stumbling away.

In any case, as part of the conversation, Michael enjoyed talking with me, and he offered me a postdoctoral position here. When I got back to Harvard, I came in. Peter Yates asked me how things were at Hooker. I said, "I never got there, but I got a postdoc with Dewar." It seemed that he would accept that.

As it was to turn out, Peter went to an American Chemical Society [ACS] meeting, I think on the West Coast, and clearly talked with some people at Berkeley. A few days after he got back from this meeting, I got a telegram out of the blue from Berkeley, offering me an assistant professorship at Berkeley—a very short telegram. It offered me an assistant professorship. It stated the salary, which was, I don't know, something like sixty-five hundred dollars a year—something like that—and gave me twenty-four hours to make up my mind. Very different from what goes on nowadays, where departments spend untold amounts of money and time interviewing people, and so on and so forth.

In any case, I decided to take this position, of course. I went directly from my Ph.D. to an assistant professorship at Berkeley. I had quite a good time at that. Assistant professors at Berkeley are quite different from assistant professors nowadays. [laughter] First of all, the assistant professor title was different from an instructorship only because assistant professors were allowed to get on the campus to hunt for a parking spot. Instructors were not even allowed to come on campus with their cars. I don't think there was any difference in salary or whatnot, but this was an important thing, to get on the campus.

The rules at Berkeley were fantastic; the rules were written by the state. For example, assistant professors were allowed to have desks that only had drawers on one side. Their visitor's chairs could not have arms. Extraordinary.

TRAYNHAM: Was this a status symbol or economy?

EATON: No, I think it was the state trying to keep down the costs, or something of this sort. Everybody thought these rules were ludicrous, but they were stuck to pretty carefully.

The experience at Berkeley was exceptional. I had a substantial teaching load. In fact, I taught elementary organic chemistry with Melvin Calvin (who just recently passed away). I really shouldn't say with Melvin Calvin. Basically, I taught the course, and Melvin was listed as the guy who was teaching it. He basically didn't do a damned thing, which was quite infuriating to me. On the other hand, Berkeley provided me everything I needed to do my research. After a big argument with the department secretary, I actually got a key to the stockroom, which was fantastic.

I had no problems with things of that sort. They were very generous—except that at the time at Berkeley, the young organic chemists were not permitted to have graduate students. The senior organic chemists—[Henry] Rappaport, [James] Caisson, and [William G.] Dauben, and Don Noyes—took all the graduate students. Of course I resented this, as did the other young assistant professors. All of us had two-year appointments, and it was very typical that nobody got renewal of the two-year appointment. As it was to turn out, I did very well at Berkeley, and I was the first of the young organic people in many years to get a reappointment. But that reappointment still came without graduate students, which was at that point ludicrous. As I said to them at the time, "You know, I've done the experiment in the first two years to see if I could do research by myself without students. Well, now it's time for me to do the next experiment, to do research with students," which I thought was quite a big challenge. But there was no change at renewal.

In any case, it wasn't that important to me, because Chicago was looking for people. Michael Dewar was still here. As it was to turn out, Michael left before I came—but Michael was still here. He remembered this whole incident in Chicago and thought I was terrific. He brought me here to Chicago, and they made me an offer. An offer at Chicago was very different from an offer at Berkeley. At Chicago, young people were really part of the faculty, not just people who were used. Of course if I came to Chicago, I was allowed to compete for graduate students, and so on. From my point of view, that was terrific. As I said at the time, Chicago was several thousand miles closer to civilization than San Francisco and Berkeley. Thirty-five years ago, I considered San Francisco uncivilized. I was still a New York boy. [laughter]

I chose to come to Chicago, to Berkeley's absolute amazement. I remember the chairman at Berkeley saying to me, "How could you possibly go to Chicago? It's on the wrong side of the tracks," referring to the location. I replied, "But at least it's on the right side of the Rockies." [laughter] I never regretted leaving Berkeley. Berkeley is an extraordinary school, but at the time it was really bound up in traditions and things that were ludicrous.

Again, looking at the differences, at the time at Berkeley, most of the assistant professors had come from major Ivy League schools—in particular, Harvard. Harvard, at the time when we were students, had cumulative examinations, as I think they still do. Berkeley was always debating, "Should we have cumulative exams?" There was a faculty meeting. It was very, very rare at Berkeley for there to be faculty meetings. The discussion came up about cumulative exams. A vote was taken. Since there were lots of assistant professors—all from Harvard—and all the assistant professors were for it, the vote was overwhelmingly for introduction of cumulative exams. At which point, the chairman stood up, dismissed the junior faculty, and another vote was taken. That was that. There were no cumulative exams.

That's <u>very</u> different from what happens at Chicago, and why I really love Chicago. At Chicago, the junior faculty have the same say as the senior faculty. Even nowadays, the senior faculty <u>look</u> to the junior faculty to change things and introduce new things. The only place here that a junior faculty man does not have the same vote as a senior faculty man is on internal promotions to ranks higher than their own. If we're trying to bring in a full professor from outside and trying to decide whether or not to make the offer, the junior man has the same vote—and it's a secret ballot—as the senior person. This I think is terrific. It really distinguishes Chicago from most universities in the world.

TRAYNHAM: Do you think your unexpectedly leaving Berkeley prompted them to make any changes in their treatment of assistant professors?

EATON: Oh, indeed it did. The next person in—I've forgotten if it was Kirby Scherer or Clay [Clayton] Hefcock—but things changed fantastically. They were allowed to have students. It really woke Berkeley up to the fact that times were changing, and you could not use people as they were at the time.

Again, things always look different in retrospect. The two years that I spent doing research on my own without students proved to be very, very important to me. I did a terrific job during the time. That was the time when we looked at the photodimerization of cyclopentenone and its mechanism, and things of that sort, and really put 2+2 photocycloaddition reactions on their feet. I did it myself. When it came time, when I came here, to work with graduate students, I was very confident about telling students what I thought they should do—very confident, when they would come and tell me it didn't work, to tell them they should go back and vary this or vary that. I could not be intimidated by the students.

I think now in many places, including here, the starting faculty members are immediately inundated with graduate students. They want lots of hands; they want to do this thing of interacting with graduate students; but I don't think they ever really get the chance to prove to themselves that they know what they're doing.

I think, in fact, the old Berkeley system—although certainly not put together for this reason, for helping these professors; it was put together for reasons of pure selfishness—but I think in fact, the net result of that system was far better. I would really prefer a situation in which first-year or second-year assistant professors were supported, didn't particularly have to worry about the grant problems—in fact, that's probably true nowadays—but also had to do it themselves. I think that they would find it would be very helpful to them as they moved ahead.

TRAYNHAM: Have you recommended that here?

EATON: No. But I've talked about it here. There is a problem in this country now, that if you do things differently—requiring more of students, requiring something different of assistant professors—unless everybody else is going to want to do it too, you can't get away with it. You can't win. It's a very unfortunate situation in that the competition for faculty members, the competition for graduate students, postdocs, and so on has risen to a level at which it's difficult to change things. It's difficult to be different, which is unfortunate. I always thought one of the advantages of Chicago was that it could afford to be different, but times change.

TRAYNHAM: After you came to the University of Chicago, what research program did you initiate or continue?

EATON: I have to go back a little bit to when I was at Berkeley and, for that matter, when I was doing my Ph.D. work. As I've mentioned, my Ph.D. work focused on compounds, which were heavily substituted by chlorine—the chlorocarbons. I had tried to address some of the structural problems that we met at the time instrumentally, going beyond IR, which was the tool at the time. As I've already said, NMR was used only rarely, and of course was useless at the time because these compounds were fully chlorinated; they didn't have any hydrogen. This was well before the time of carbon NMR.

There was something called nuclear quadrupole spectroscopy, which had the potential of being useful for chlorocarbons. It was a technique that Al [F. Albert] Cotton had done some things with at MIT. Of course I was adventurous with stuff like this, so I built a nuclear quadrupole spectrometer and tried to use it for the identification of the structure of these chlorocarbons. I remember this adventure into electronics. The only signal I ever managed to get from this nuclear quadrupole spectrometer was the Boston Police station. [laughter] It was really quite funny.

When I went to Berkeley, NMR was really starting to become this powerful technique. I realized that I didn't have a chance to compete if I stuck with chlorocarbons, because I could not use NMR. Then the question came, "Could we make cage compounds that had hydrogen instead of chlorine, using some of the methodology that had been developed in the chlorocarbon series?" At that time, what was known about the photochemistry of alpha-beta unsaturated ketones led me to the idea that, "Yes, you could use this method to do what is now called 2+2 photocycloadditions to make cyclobutanes." If you had set the compound up correctly, you could perhaps get cage compounds.

I started this work very innocently, just looking at the photodimerization of 2-cyclopentenone. That turned out to be a very good reaction. It gives what was unexpected at the time: an equal mixture of the head-to-head and the head-to-tail cyclobutane compounds. This was very much against theory at the time. Again, to set the stage, this was at the time that George Hammond was developing the sensitized reactions. There were many arguments as to what was really going on in photochemistry. Some reactions were thought to go through singlet states, some reactions were thought to go by triplet states. Maybe there were mixtures, and so on and so forth. The first investigation that we made was a physical organic investigation of this dimerization of cyclopentenone. There was the suspicion that part of it was happening by singlet state and part of it was happening by triplet state, because the reaction was very concentration dependent. You could make an argument, as Peter Leermakers did early on, that the concentration dependence had to do with the lifetime of the singlet state versus the lifetime of the triplet state, one being much shorter than the other.

Anyhow, we did some classical tests along these lines and disproved Leermakers' theory. We later found out that in fact the concentration effect was in reality a solvent effect. It was, as you changed the concentration of cyclopentenone, you were in essence changing the polarity of the solvent. We started then to understand more about the mechanism of cyclopentenone dimerization. You could conclude—at least I thought, from the results—that since the direction of the addition was not being controlled by the presence of a carbonyl on both parts of the molecule, that maybe you could do a cycloaddition between an alpha-beta unsaturated ketone and a simple olefin without a carbonyl. It turned out that was true. My addition of cyclopentenone to cyclopentene photochemically was, I believe, the first good demonstration of an intermolecular addition of an enone to an unactivated olefin.

At almost the same time, [Elias J.] Corey looked at photoaddition of cyclohexenone to unactivated olefins. There was also the critically important work by Paul Mayo on the beta acetoxy enones adding to unactivated olefins. I think this work—or these works together—really were the start of a very, very important synthetic technique of making cyclobutanes by intermolecular 2+2 photocycloadditions.

It was not just the dimerization of an enone. See, the dimerization of enones is essentially known from [Giacomo] Ciamician's work in the early 1900s in Italy on the cinnamic acids. That was all solid-state work. It was always considered to be very special and whatnot. The work that was done in the 1960s with cyclopentenone and cyclohexenone really put the 2+2 photocycloaddition reaction on the map—made it a staple of synthetic chemists. Of course, our work on it did a lot to establish the mechanism. We were able to show that in fact, it really did require a moderate-sized ring, you know—five-membered rings, six-membered rings—and that when you went to higher rings, you got into a quite different chemistry, photoisomerization into highly strained molecules: trans-cycloheptenone, trans-cyclooctenone. This fit nicely with my general interest in strained compounds: molecules that had geometry very unlike typical organic compounds. Of course in these cage compounds that we were working on, we had the same sort of thing: geometry unlike usual geometry. These things together helped define where I went in the future.

TRAYNHAM: That's what you started off with at Chicago, then.

EATON: When I came to Chicago, we wanted to understand more about the chemistry of cyclopentadienone, which was potentially a very important precursor of cage compounds with hydrogens on them. I knew a little bit about tetrachlorocyclopentadienone from my earlier work. I was driven to make this switch from the chlorocarbons to the hydrogen system; so much of the earlier work here was on cyclopentadienone—actually cyclopentadienone and ketals—how they might be used in synthesis. Cyclopentadienones dimerize very easily. This is a spontaneous Diels-Alder reaction. We investigated a little bit of that. We had heard that earlier, Chuck [Charles] DePuy had made some basic discoveries in that area.

The Diels-Alder dimers of these compounds, if exposed to ultraviolet light, undergo an internal photocyclization, which gives a cage compound. This is exactly the kind of reaction that we had discovered much earlier at Allied Chemical with chlorocarbons: this internal reaction to give cage compounds. The cage compounds are closely related to the Kepone insecticides. Now they have hydrogen instead of chlorine. You can really start looking at the NMR and understand more about the structure.

There was a reaction that was coming into vogue at the time, the Favorskii reaction, which dealt with ring contractions of alpha-halo-ketones. In fact, at a cumulative examination at Harvard—I never did very well on the cumulative exams at Harvard; I mean, they were hard for me. One of the tests had a reaction on it that had been concocted from the literature from some observations that [Arthur C.] Cope had made. It was a mechanism problem, and it dealt with what was fundamentally a Favorskii ring contraction. I remember when I had faced the problem on the cume, I had no idea what was going on. Later I found out the answer, and I was quite fascinated by it. It's the usual thing, you know—the ones you get wrong are the ones that you remember. Literally—it was three or four years later—I recognized that this thing from that Harvard cume could be combined with the information that we had from the halogenated cases and the information that we had from the non-halogenated cases into what could be a route into the cubane system, which of course at that time was an enormously fantastic goal. It was always being batted around. Everybody had proposals for how to make cubane, but nobody had done it!

I looked at this. I really think this is true—at least, it's the story that I presented at the time, and I think it is true. It really wasn't the question of, "Would this reaction work to get the cubane system?" Rather, "Did we finally know enough about cyclopentadienone chemistry, the halogen compound chemistry, the photocycloaddition chemistry, that we could actually make exactly the right intermediate to try the Favorskii reaction to get a cubane?" I thought that was the challenge. Two weeks later, we had done the whole thing, and it was fine. We had made the first cubane compound.

Interestingly, Kirby Scherer, who was at Berkeley and had been a graduate student at Harvard a year or two behind me, worked at Harvard with Woodward on some chlorocarbons—in fact, the very same ones that I had worked on at Allied Chemical. When he went to Berkeley, one of his ideas was to try to do this Favorskii contraction on the fully chlorinated material, which was very readily available. He called me here at Chicago, asking me if we had tried or were doing this reaction on the fully chlorinated material. I was a little bothered by this. This was the same time that we were doing the other stuff. I said no, we had left the chlorocarbon area. He went on to try this with the chlorocarbons. As it turned out—for reasons that we only now are coming to understand—it doesn't work on the chlorocarbon. You need to go onto the system that we had. He dropped it too soon, which was unfortunate. (Kirby is now very ill, which is quite awful.) Anyhow, that's how got we got into cubane chemistry. As I said, there is this line all the way back to my job at Allied Chemical.

TRAYNHAM: You had not deliberately chosen cubane as the target molecule, then.

EATON: No.

TRAYNHAM: It just happened that it looked like an intriguing target, associated with the chemistry you were already investigating.

EATON: Right. Cubane had a natural and obvious relationship to the kinds of compounds we were working with. We had also composed, I would say, a more classical chemical approach from where we were to the cubane system—a rather more step-wise approach through various intermediates that we were never able to get by our proposed route. It turned out that many, many years later we made those same intermediates, the ones that we had wanted, to make cubane—but the only way we could make them was from cubane itself. The reactions that we had proposed earlier to take these to cubane, in fact don't work. You get into much more complicated chemistry. It's this other very simple—I shouldn't really say simple, for it is not deeply understood—Favorskii closure, which provides a very, very special synthesis of compounds like cubane.

TRAYNHAM: I believe the graduate student who was associated with you on that cubane synthesis was Thomas [W.] Cole [Jr.].

EATON: Yes.

TRAYNHAM: Was he your first graduate student?

EATON: Yes, he was my first graduate student. He was in deep trouble here. He was working with Weldon Brown, who retired many, many years ago. He was actually a poor student at the time, and he was about to be let go. I thought I'd give him a chance. He was one of my first group of four. Tom actually made some of the precursor molecules. I was working, myself, very actively in the laboratory at the time. I don't want to take anything away from Tom, but the first round through and the first recognition that this was a cubane—a cubane diacid was the first compound that we made—was something that I was doing in the laboratory. Tom was a delightful person to have around, and he has gone on to do very well. We keep in contact.

It's a side story, certainly, but Tom is black. Chicago, of course, has never discriminated. But Tom and I had many interesting discussions about civil rights and the black and white relationship. This, of course, was in the early 1960s, and such things were really before us. I remember very strongly encouraging Tom to make use of the fact that he was black. He was sufficiently a realist to do this—not in the sense of affirmative action or any of that. Of course, Tom's a very competent person. He never made demands on people that, "You should do this for me because I am black."

He was smart enough to recognize that since he was black, certain opportunities would open up, and he should go with them. He went from here to Atlanta University in chemistry. Of course, Atlanta, at the time, was a black university. He rose up and did very well at Atlanta. If I remember correctly, he got up to being provost. Then he went to West Virginia, which was basically a white university, and worked up to, I believe, the presidency. I may have the exact things wrong. Now he's back at Atlanta University [Clark Atlanta University], which has expanded and become much bigger. He's president there and is doing a superb job as the president of an integrated university. I'm very proud of him, very proud indeed. He chose the right thing to do. I think Tom would say this himself. He enjoyed chemistry, but he wasn't driven along the academic road of chemistry.

It's interesting. If I go back to the original observation here, I was working in the older Jones Laboratory, which was a shambles—a terrible old building. I can remember getting the crude from this Favorskii reaction and making the methyl ester and then deciding that I should chromatograph it. I am surprised, in retrospect, that I actually decided to chromatograph it, but I did this chromatography. At that time we had an A-60 NMR spectrometer. I went downstairs to take the spectrum.

[END OF TAPE, SIDE 2]

EATON: There were just two lines in this spectrum. One line was for the six identical hydrogens of the two methyl groups of the cubane dimethyl ester. The other line was for the six hydrogens of the cubyl system. It was a fantastic spectrum; it was just these two lines. I was just realizing what the significance of this was, when Gerhard [L.] Closs, who was my colleague at the time, and Saul Winstein walked in. Winstein was the Kharasch Professor, a famous visitor. He had just arrived, and he was being shown around the department by Gerhard. They came over to the spectrometer, and they saw these two lines. They agreed that I had finally "done" a cubane. It was a very exciting time, very nice indeed.

Then I went back up to the lab because, being a good synthetic organic chemist, I wanted to see if I could improve the yield. I kept eluting stuff from the chromatography column, the silica gel column. The next thing that eluted turned out to be the dimethyl ester of terephthalic acid. Had I not done this chromatography, I would have had an NMR that was much more complicated. It would have had these aromatic protons in it, and I would have assumed that the whole molecule had fallen apart, because the mixture of the two things together would make no sense at all in the NMR. It was absolutely sheer luck that I did this chromatography. Interestingly, although we have done the same reaction hundreds of times since then, we have never, ever gotten this aromatic ester out again. To this day, I have no idea where it came from.

Phthalates, of course, are contaminants all over the place, but not as the methyl esters. The contaminants are long-chain esters of phthalates. This was a methyl ester. We've never seen that again, and we have all kinds of silly theories of what went on. It was a real bit of luck. I think God sometimes really smiles on chemists, and this was certainly a case. [laughter]

TRAYNHAM: What was present was probably some unsuspected and yet unknown promoter for this rearrangement.

EATON: Right. I'm delighted that we've never seen it again, but I don't understand it. I don't understand it at all. [laughter] I probably made an ass out of myself at many American Chemical Society meetings and things of that sort, with this emphasis on cubane. It was the first time—okay, maybe I'm making an ass out of myself again, but it really was the first time that a very highly strained molecule, a very contorted molecule with bonds that were very unusual—it was the first time that a molecule like this had been synthesized in a logical way. Admittedly there was an adventure in trying this last step, but there were good reasons to try it. The Favorskii reaction was known as a way of making cyclopropane compounds, cyclobutane compounds, and so on. Most of the cage compounds that had been made earlier—for example, the Kepone molecule—were discovered totally accidentally. It was a magic reaction.

Hexachlorocyclopentadiene was mixed with liquid SO_3, and out came this compound. To this day, no one knows how that reaction works.

All the other cage compounds—and very nice stuff by Peter Yates and Ellie [Smakula] Hand at Harvard, and [R.C.] Cookson in England—these were just made by photochemical irradiations of compounds off the shelf. Boom, sort of a "Shazam" reaction, you've got the cage compound. The cage was not an expected product. It was rather the kind of thing, "Let's see what happens if we irradiate compound X." These were totally fishing expeditions. For this cubane business, we really had to start way back and develop a systematic approach. This was pretty much at a time that synthetic methodology and logical synthesis were being developed for the natural products. Right? I mean Woodward, of course—where there was some logic to what was being done. I remember really getting up on soapboxes at various meetings and making this point. I'm sure—now in retrospect, I'm saying—"My God, I must have looked like a fool." I mean here was this twenty-four-year-old, twenty-six-year-old—whatever it was—making pronouncements.

We have tried ever since then, if I say so myself, to logically approach the synthesis of these very novel molecules that have a geometry far different from anything ordinary. We've always tried to make these molecules by a logical synthetic scheme, where it's not just, "Let's cook it, or press on it, or irradiate it." I mean, we use these techniques, but when we do there is reason to it. I'm really quite uninterested in what I call the "Shazam techniques," where with a bolt of lightning A becomes Z, and you have absolutely no idea how it happened or why it happened, and it could just as well have become W.

TRAYNHAM: In the cubane synthesis you actually have, in the several-step synthesis, two Favorskii steps. The first one was a very high yield. The last one, which is the last step to give you the dicarboxylic acid, was a modest yield. With what do you believe that to be associated?

EATON: It was a great mystery. It is a very great mystery. The difference between these two closures is really quite spectacular. The first one is as close to quantitative as you can get. It goes in a moderately strong base at room temperature very rapidly. It is essentially over immediately in a 10 percent aqueous base, and with beautifully high yield. The second one, under the best conditions, is 50 percent yield, and you have to go to a much, much stronger base—40 percent KOH. You have to do the reaction at reflux about 110°C, in order to get the cyclization to go. It's not at all obvious why. The amount of strain energy introduced in the two reactions is fairly similar, so the answer isn't there. Presumably it has some very subtle difference in geometry as the bond's closing. We have worked very hard over the years investigating this second closure and trying to improve the yield. We have never been successful with that.

We know a little bit about the side products in the reaction, but not much. One of the side products is a clue, and that is, the ketone gets reduced to an alcohol. Mind you, this is happening in aqueous base, so there's no obvious source of hydride. Yet the ketone is reduced, so somehow hydride comes in from somewhere. The reaction also turns pitch black. My guess is that when the hydride comes from somewhere, the "somewhere" turns black. We have been trying to outsmart this reaction for years, and we've been unable to do it. It certainly is very important now. In the early days we made a little bit of cubane diacid and converted it to cubane. This goes back many, many years. We achieved what we wanted to achieve, and it was a synthetic accomplishment—rather like the natural products people saying, "Oh, yes, I made this or I made that." Then, you went on to something else, which is what we did. We went on to other compounds, and cubane was left on the shelf.

In the mid-1970s there were some physical organic questions about the mechanism of metal-catalyzed ring opening reactions in strained systems. Jack Halpern and I, and Luigi Cassar from Enichem Montedison at the time, in Italy. We did a very nice project in which we used cubane to probe the mechanism of the reaction. Again, a small amount of cubane went a long way. There was no need to really work hard on the synthesis.

Years later, it became very important—and I'll tell you why in a moment—to be able to make large amounts of cubane diester. The synthesis was done on a pilot plant scale. Getting the second Favorskii closure up was a big deal. As I say, we put a huge amount of work, not only here, but in industry as well, trying to figure out how to improve that reaction. Yet it has always remained elusive. No idea what's going on. Now, although cubane diacid and its dimethyl ester are made in multi-pound lots, it's much more expensive than it would be if only we knew how to do that last step correctly. The rest of the reactions are really quite good.

Now, of course, I suppose your next question is, "Why? Why all of a sudden would cubane be made in large amounts?" This traces back to my experience with Allied Chemical—it was actually the General Chemical Division of Allied Chemical. Everett Gilbert was my group supervisor at the time. He did a lot to encourage me in chemistry. He took early retirement from Allied Chemical. As he was a very active experimentalist, he took a job with the—I think it was called, even at the time, ARDEC. It was the Army Research Development and Engineering Command, or something like that, at Picatinny Arsenal in Dover, New Jersey—now called Picatinny, New Jersey. He was very interested, as they were at Picatinny, in making new explosives. He came up with this idea that cubane could perhaps be the basis of a new explosive. He was always fascinated by cubane chemistry because he had followed my career, and knew that he was, in fact, a really important person in my career. He took great pleasure in following what I was doing.

One day he called me up. He said that they had done some calculations at the Army, and that they thought that octanitrocubane would be a terrific explosive. I was very amused by this, and I said, "Oh, yes, yes. Thank you for considering me, but we're doing other things." I suggested that he contact Tom Cole, my first Ph.D. Of course Tom was fussing around in the laboratory at Atlanta University in Georgia, but didn't have any support. They did indeed contact Tom. I don't know whether or not they actually did anything with him. Anyhow, nothing happened. Some years later, Everett again contacted me saying that the Army was really interested in octanitrocubane and so on and so forth. They had let a contract, apparently, to Gary Griffin, who was at Louisiana—Tulane, someplace like that.

TRAYNHAM: University of New Orleans.

EATON: New Orleans. New Orleans, at the time. Gary was trying to make dinitrocubane from cubane diacid. What they were trying to do was entice me into this project. They asked me to come to a meeting in Hilton Head, their workshop on high-energy compounds. I think this was in 1981. I knew, of course, that Ev Gilbert was going to be there, and he was very interested in this. We had plenty of cubane diacid around. Since I really felt a great love for this man, we took cubane diacid, and in a few weeks turned it into 1,4-dinitrocubane. It was done by very simple, straightforward chemistry, and it was no problem at all. 1,4-Dinitrocubane's quite a nice crystalline material, stable and shock-insensitive.

I arrived at the meeting, and I brought with me one hundred milligrams of 1,4-dinitrocubane as a gift to Gilbert. I gave this to him at the meeting, and there was this fantastic smile followed by the most hang-jawed look you can imagine. Apparently, they had been contracting with Gary for years to make 1,4-dinitrocubane, and Gary had been unable to make it. He was off on some other complicated path, and whatnot. Well, I won't speak badly of the dead. It <u>was</u> <u>very</u> embarrassing to people. Gary gave a talk at the same meeting about the impossibility of making this compound, and here I arrived with one hundred milligrams as a gift. [laughter] That was quite an experience.

In any case, there were further discussions at the time. The Army really wanted to pursue this. There were two things that got me interested in it as a real problem. I had thought initially that the conception that octanitrocubane would be a good explosive was based on the fact that cubane had a high amount of extra energy—the strain energy—and that the formula was nice to blow up to CO_2 and nitrogen. It turns out that was not the major part of the reasoning. This fascinated me. As was explained to me at this meeting, density is important in a variety of ways, to how effective an explosive is.

The first way is obvious: the more dense, the more moles you get into the shell, into the limited volume of a shell. The second is much less trivial. It turns out that the velocity of the shock wave—the detonation wave that goes through an explosive—increases as the square of the density. If the velocity is faster, you get the energy out faster. It's very important to get the energy out in the shortest period of time to get the maximum bang.

It turns out that cubane is a really dense hydrocarbon. It's 1.29 grams per cc, which is, of course, <u>much</u> heavier than any other hydrocarbon. Simple pentane is what, 0.8? Adamantane is 1.09. At the time, cubane was by far the most dense hydrocarbon known. Since then, there have been a few found that are slightly more dense—in the dodecahedrane series and in the polycubane series. But none of these are easily available.

It turns out that molecular volume is a group additive property. If you know the density of the compound, and you know what you're going to put on instead of hydrogen, you can do a fairly good job of calculating out what the density of the new compound would be. The Army people had done this. They had calculated out the density of octanitrocubane, and it's very high. Then they put two and two together. They probably got a little bit more than four, because they were very enthusiastic about this. They concluded that octanitrocubane, because of this density thing, would be 10, 15—depending on how enthusiastic they were—25, 35, 40 percent better than HMX, which is the explosive that's used all the time. That's an enormous improvement. That was the real push behind the Army wanting this compound and Ev Gilbert's conception of it.

I came back from this meeting and told my research group that the Army wanted us to make octanitrocubane. I was sort of laughing at this at the time, and my group was laughing at it. We were sitting there in the seminar, and then all of a sudden I got embarrassed. This really is true. I realized I didn't have the slightest idea how to make octanitrocubane. I knew how to make the dinitro compound because we had the diacid. That just took really trivial chemistry to do that.

To make octanitrocubane, you'd have to take the rest of the hydrogens off of the cube and put on nitro groups. There was no such chemistry at the time to remove hydrogens from cubane. It was hard enough to remove hydrogens from cyclohexane. But to remove hydrogens from something like polycyclobutane! There's just no way of doing anything like this in any sort of systematic way. I mean, the approach at the time was, if you wanted a highly-substituted ring system, you went all the way back to square one and you built it up from square one. In the cubane case, that was never going to happen. There was no way you were going to go back to the nitrocyclopentadienones—those compounds are still unknown—and try to do the chemistry there.

I realized I did not know how to make octanitrocubane. I did not know how to make octa-anything-cubane. We were supposed to be the world's experts on cubane chemistry, in fact—on the chemistry in general of these strange molecules. Yes, I made—now that I think about it, in retrospect—a brave decision that, by God, we could start taking these molecules, these cage structures, and really get to look at their chemistry. We would take cubane or cubane diester as a starting material, and we would go from there. We were, in due course, able to work out ways of doing systematic substitution—honest-to-God systematic substitution, of the kind that we could control and what we wanted predictably! We learned how to put groups on where we wanted them, and we could do this with high yield.

It didn't come easy. The clue to that was a visit that I made to Vic [Victor Snieckus] in Canada at Guelph Waterloo [University of Waterloo]. Vic mostly does natural product synthesis. He was one of the people—with [Peter J.] Beek and [Albert I.] Meyers and quite a list of others—who were involved in amide-activated orthometalation of aromatics. This is very important methodology that in fact I was totally innocent of—embarrassingly innocent of. I had never even heard of it. Although I lectured on synthetic chemistry here all the time—including natural product synthesis—this reaction, which is in effect critically important in the synthesis of aromatics, I just had never heard of it. Shameful! But it worked to my advantage.

There's a picture in my mind of Vic presenting me this reaction. I'm thinking, "Gee, this is really a wonderful reaction. It's fantastic, what he's doing with this thing." Then, as I was thinking about it, I realized that the hybridization of a carbon hydrogen bond in cubane is much like the hybridization of the carbon hydrogen in an aromatic. There are certain similarities in the geometry. Of course, we can easily make amides of the cubane system, because we enter the cubane system at the acid. I'm saying to Vic, "I wonder if this might work with the cubane amides." He said, "Well, why don't you give it a try?"

Well, I came back here. At the time I had a wonderful postdoc, Graziano Castaldi, from Italy. (Over the years, I've had many postdocs from Italy.) Graziano tried this reaction, the direct metalation of a cubanamide. It didn't quite work, but something was happening. We went around and around in circles for a long time with this, until we realized what was going on—and it's very pretty work, I must say. We came to realize that we were in fact doing the metalation, but only to a very small degree, because the cubane hydrogen is significantly less acidic than the benzene hydrogen. The metalation that we were doing was making a little bit of lithio cubane. We just could not get a decent concentration of it.

We had the idea that perhaps we could do a metal exchange and go from a lithium compound to an organo-mercury. The carbon-mercury bond, of course, is much stronger than the carbon-lithium bond—less ionic, and so on. Graziano put some mercuric chloride into the reaction mixture. Sure enough, we ended up with a new organometallic that we would use, an organo-mercury, and of course, such can be reacted with iodine to cleave them. Sure enough, reaction with iodine gave us iodocubanes.

Once we had this clue, we could really run with it. It took some years to develop the technique fully. But now it's a fantastic technique, fantastically good. For example, if you take the 1,4-diamide of cubane, it's easily made from the diacid, and instead of lithiation, you do direct ortho-magnesiation. You get the di-Grignard and then you carboxylate it, and you get 95 percent yield of the diamide diacid. Then of course, you can keep going, and you can make the octa-acid by this sort of methodology. It's really very beautiful. You can do this on cubane monoamide where the other acid group has been removed, and you can control what you're adding. You can also distinguish the two acid groups.

It turns out that the substitution problem on the cubane series has really been solved completely. We can probably make any cubane that one would want. I mean, you have eight positions. We could actually put on, if we wanted to, eight different groups. Of course, there are all sorts of orientational differences. I think we can probably make every one that you can imagine using this kind of methodology.

I'm fond of saying that I think that the substitution chemistry on cubane is about as well developed as the substitution chemistry on benzene. Benzene, you can make most anything you want, and I think that's true on cubane. We're a lot further in knowing how to substitute cubanes than people are in how to substitute cyclohexanes in a systematic way. The only thing, of course, we haven't been able to make yet is octanitrocubane. That still is a goal. We have gotten as far as hexanitrocubane by some very exciting chemistry—very nice chemistry that I'm very pleased with.

This project on the nitrocubanes has gone on for more than ten years now. I've sort of made a decision that either we get octanitrocubane within the next year, or that's it. If we can't, then we don't know how to do it.

We have developed, I think, some fabulous chemistry for getting first tetranitrocubane and then penta- and hexa-. We got them about a year and a half ago. These are very nice compounds, just like the Army people predicted. Their densities are very high. They are shock insensitive, which was something that was also predicted. When they take them and do tests about how effective an explosive are they, the results are terrific.

It looks very good. Because of this interest in these compounds, some companies in Italy—Enichem originally, and that's because of my having many postdocs from Italy over the years—made large quantities of cubane diester, ten-kilo quantities. Now it's being made in this country by a small firm here in Chicago called Steroids Limited. They have contracts with the Air Force and so on to make pound quantities of this material—actually multi-pound quantities. Aldrich Chemical sells it, and things of that sort. From a compound, which was first made in small amounts and put up on the mantelpiece just to be admired, things have changed tremendously. That pleases me very much. It also pleases me that we no longer have to make the compound—I've got it by the kilo—and we have plenty of stuff to use.

Now of course there's another side to the coin, and I think this is always true with organic compounds. You get going on something because you have maybe a particular use or a particular challenge in mind, whatever it happens to be—in this case to make the nitrocubanes for military purposes. In the process of learning that we could make all these substituted cubanes and how to do systematic substitution, it became quite reasonable to think about cubanes that are pharmaceutically active. It's an interesting point that the distance along the diagonal through the body of the cube is the same as the distance between the para positions of benzene. Many drugs, most drugs, are in fact based on substituted benzene. Of course, the substituents are in the plane of the benzene ring. On cubane you can do things in the plane, but you can do things above and below the plane. This is quite interesting.

Also, you can use the propensity of the cubane system to undergo ring-opening reactions under certain conditions. We've been working with a variety of people on this—with [Richard] Silverman at Northwestern to use ring-opening reactions of cubyl carbinyl radical in the treatment of Parkinson's Disease. We have some very interesting results now—tests that were run here against bone marrow cancer show some activity for some of these very special cubanes.

I think what's been particularly important for me is the support and enlightened encouragement that the military people have given us to make the nitrocubanes and other high-energy cubanes. There is now great interest in cubanes as fuels and propellants and things of that sort. That has allowed us really to probe the chemistry of the cubane system very, very thoroughly. We've ended up really changing the understanding of bonding in organic chemistry and the understanding of reactions in organic chemistry. We've been able to make such things as cubene, which is the most pyramidalized olefin known; 1,9- homocubene, which is the most twisted olefin known; the cubyl cation, which is the least likely cation. The cubyl cation forms 10^{15} times faster than originally predicted. This is the largest difference between theory and experiment that anyone knows of. [laughter] It's really changed the way people think.

TRAYNHAM: Have you arrived at an explanation for that?

EATON: Yes. We've worked very closely with the theoretical people, most particularly Wes [Wesley T.] Borden out in Seattle, who has really been a delight to work with. He has developed a very deep understanding of these compounds from the initial calculations as to what's going on. We've had a <u>very</u>, very fruitful interaction over the years. Wes is very good at explaining what the calculations mean, and predicting what would happen if you did something, some change in the experiment. We've been able to make some of those changes, in fact, and generate proof that—proof is too strong a word, but at least generate evidence of our understanding of the system. That's been a lot of fun—a lot of fun indeed.

Now, of course, cubane's not the only thing that we work on. I mean, cubane has just been a very popular compound. It's a compound that grabs people very easily. I mean, there's no problem remembering the structure of cubane. If you have to remember the structure of strychnine, you've got a problem on your hands. Cubane's easy. I always feel the military like to support cubane chemistry because they can always remember what it is. [laughter]

TRAYNHAM: Well, you did have a number of other strained-target molecules in between your early work on cubane and your return to the substituted cubane. Any particular comments about that segment of your research?

EATON: Well, after cubane, I don't remember exactly the ordering of things, but we were quite interested in propellane molecules. There are compounds in which two carbons are attached directly together. Then there are three bridges that connect two carbons, as well. The question is, "What happens when the bridges are small?" The hybridization of the carbon atoms has to change away from Sp^3 hybridization. The early questions were, you know, "What sort of ring sizes could you make? What would the chemistry be?" We did a nice job on that. Photochemistry was at the heart of it—<u>low</u>-temperature photochemistry. We developed nice techniques for doing preparative low-temperature photochemistry. We got down as far as the 2.2.2 propellane, which is a reactive molecule. It has a half-life of about twenty minutes at room temperature. We got a pure sample of it—my student was working in a parka in the cold room here at minus-forty to do the chromatography.

There have been numerous other highly-strained molecules we have made here. I think for the most part we have been the leaders in this area of synthesis. The small paddlanes, in which there are four bridges that connect two carbon atoms—the goal here is a molecule whose name is 2.2.2.2 paddlane. That's two carbons, which are connected by <u>four</u> two-carbon bridges. This is the kind of molecule that you can draw on the blackboard and everything looks fine. When you start to make a model of it, you realize that it is horrendously strained. Paul [R.] von Schleyer, years back, calculated some things about this compound and concluded that this was one of the real values of theoretical chemistry.

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EATON: It showed absolutely that this was something you shouldn't waste your time trying to make, that experimentally it had no chance of existence. Paul's probably right on this one. He's been wrong on many things, but he's right on this one. The thing is that you can very easily imagine compounds that have three of these two carbon bridges and then a much longer bridge. There's no question that molecules like that can be made. In fact, we've made them. The question is, what happens when you start to take this big long bridge and make it smaller and smaller and smaller? When the bridge is big, it's fine. When it's down to two carbons, it's impossible. What goes on in between? That's exactly the kind of question that I love to answer—basically, to apply the principle of the homologous series to strained compounds rather than to ethane, propane, or butane. We've been looking at this problem for quite some time. Buddy Leipzig—it's probably fifteen years ago now—made for his Ph.D. the smallest of these paddlanes to be known. That is three two-carbon bridges and a ten-carbon bridge. We ran out of material before we could contract the long bridge further. I finally found a graduate student whom I've been talking to about going further with this. We're trying now to get to even lower paddlanes to answer this fundamental question, "What happens?"

I would like to characterize my research as synthetic chemistry that's in search of fundamental answers to problems—in the cubane case, to make cubane and to probe what cubane is like. It's rather a different story than just making the compound and stopping. We've learned that's not enough, to do that. There really is reason to examine the compounds in more detail. Certainly, since the time that we made cubane, all of the projects that we have taken on have been taken on because we were really asking quite specific questions, like the one I've just said, "What happens as the ring gets smaller?" We realized that something has to happen, that you can't make a discontinuous transition. Something is going on, and nobody really knows what's going on. This 2.2.2.2 paddlane that I'm talking about, the theorists have two views of it: either the carbons at the central positions are flat, or they're pyramidal. Well, that's some difference. We need to answer the question, "What goes on as you approach this 'impossible' molecule?"

I find this is a very nice way of training Ph.D. students and postdocs. They have to be good in synthetic chemistry; they have to know what they are doing; they have to have good hands in the laboratory. <u>Nothing</u> replaces being able to do things in the laboratory and how to make and interpret observations. My people have never been just "pairs of hands." Pairs of hands are useless to me. They don't make observations. They don't know how to think; they don't know how to scale-up. My people do good synthetic chemistry: synthetic chemistry, which makes enough material that we can actually examine the chemistry of a product. Actually, it irritates the hell out of my natural products colleagues, but I always say to the natural products chemists that they're making something that's already known. They march up to the top of Mount Everest, and they just turn around and come back down. My kind of chemistry, we have to climb up to the top of Mount Everest and we have to bring enough stuff with us that we can have dinner. [laughter] That's a big difference.

We really want to examine the chemistry of the molecules, so we can't have syntheses that are too long or have too many poor steps in them. It goes back to the old saying of Ev Gilbert: "If you can't do it in a two hundred fifty-millimeter Erlenmeyer on the steam bath, it's is not worth doing." Well, I mean, obviously we don't do that anymore, but the philosophy is the same: that you really have to make enough to study. Otherwise, I can't do the kind of chemistry that I want to do. The kind of chemistry that I want to do is the physical organic chemistry that I originally thought I was going to find at Harvard, which I didn't quite find. [laughter]

Nowadays, oddly enough, we are looking at some of these questions: "What is melting all about? How do you change melting patterns?" This has become very important among the physical chemists looking at nucleation methods: you know, what goes on during melting? Some of the molecules that we make actually have important implications here. The high symmetry of cubane makes its phase changes very, very interesting. We just have a paper coming out with Peter Gehring and Tanner Yildirim at the National Institute of Standards just about this point of what's going on. It's taken a long time to get around to understanding melting as being a part of that.

Of course we have had many other adventures in synthetic chemistry, one of which is dodecahedrane. That's one that we did not win. [Leo A.] Paquette and [Horst] Prinzbach, in different ways, got there first. I think we really introduced the world to the problem of dodecahedrane. This is a very tough molecule, a very interesting molecule—not strained in the sense of cubane, but a very ordered compound, for which it was quite important to develop new synthetic methodology. We did that. At the time that we worked on dodecahedrane, there were very few natural products that were known that contained multiple fused five-membered rings. That changed dramatically. This was just fate, that the hirsutane compounds—and many other natural products, terpene-type compounds—were isolated that contained fused five-membered rings. Dodecahedrane is a molecule that contains many fused five-membered rings. We had developed a lot of new methodology for making quinanes, as they are called now. That methodology has been translated very nicely into the synthesis of these natural products—not by us, but by others who have used it.

We got pretty far on the dodecahedrane problem. Dodecahedrane has twenty carbon atoms with a very particular arrangement. We made the system called peristylane, which has fifteen of these atoms just where they are supposed to be as part of dodecahedrane. The idea was to roof over the peristylane with five more carbon atoms. In fact, we attached the five-carbon component. We got it into position to roof—but the roof always insisted on going on crooked. We can't get it to go on straight.

Again, sort of the way things go, Paquette made dodecahedrane first, employing at the early stages some of our reactions. His synthesis is an interesting synthesis. It received a lot of criticism for the wrong reason. At the very end, there's sort of a rearrangement that takes you into the dodecahedrane series. It's a rearrangement that is perfectly reasonable to propose. You should expect it to happen, and things of that sort do all the time. Yet, people tend to be very critical of one another, and they criticized Leo that the step was too important to be left to such a rearrangement. The criticism was very foolish, because the step was very logical from what we knew about physical organic chemistry. I think Leo deserves a lot of credit for having successfully made dodecahedron. I envy his success, although he could have given more credit to others who made relevant contributions.

Prinzbach, in Germany—who is a very good friend, as is Paquette—developed a different method that is, interestingly enough, based on some early chlorocarbon chemistry. The starting materials are chlorocarbons. The Prinzbach method is better than the Paquette method for making material, although neither method can make large amounts of dodecahedranes. It would be irrational to take either of those syntheses and try to make a kilogram of dodecahedrane. That's it. It's not a comment on the synthesis. It's a comment on the nature of synthetic chemistry.

This might be interesting. There are a couple of very highly specialized cubanes: the octatriflouromethyl compound and some bridged cubanes, which are made by methods different than our cubanes—the original cubanes. These compounds are all from the "Shazam" chemistry. They were made way after the cubane system was made originally by highly specialized routes that have no real application elsewhere. Our synthesis of the parent cubane system—its fundamental outline—remains the synthesis of cubane. There is no other choice, I mean, that has any conceptual difference from the key steps in ours. The same thing is true in dodecahedrane business. There's two syntheses: the Prinzbach one and the Paquette one. That's very different from the natural product world—where there are often twenty or even more syntheses of many compounds. There are innumerable syntheses of cortisone, for example—some good and some bad, and so on and so forth. The natural products people consider another synthesis of a natural product, presumably a better synthesis, a real goal worth pursuing. A worthy goal—and it probably is a worthy goal that is good for training students.

That doesn't happen at all in cubane—the non-natural product area. Here the molecule is made, and the challenge is considered answered. The chemistry of the molecule—that doesn't seem to matter. The chemistry is often not looked at. It bothers me very much that the younger generation doesn't look at these non-natural products as continuing synthetic challenges—and they certainly are. Better syntheses are needed. More needs to be known.

Cubane, for example, the hydrocarbon cubane—if the hydrocarbon cubane could be made cheaply, it could really change things tremendously. Cubane has the potential of being a fantastic fuel. Cubane is, in a formal sense, a tetramer of acetylene. In fact it's downhill from acetylene if you could find a way to tetramerize acetylene to cubane, an energetically favorable process, you could make cubane cheaply. By comparison to acetylene, cubane is stable. You can't work with acetylene, as you know. It detonates; it's shock sensitive. Cubane's a rock, up to temperatures over 200°. I mean, you could tank up on cubane. It would be fantastic.

In fact, the racing car people have looked at things like this. They've recently had to change the rules of the Formula One Race in Europe because they were adding things like quadracyclane, and they were about to add things like cubane to the fuel, to make the fuel better than the other guy's fuel. The old rules were that you could use any fuel you wanted, as long as it wasn't more than one hundred thirty liters. Now they all have to use the same fuel. That's sort of sad. [laughter] It's terrible. I don't like that at all. I hope this changes. I think there are real challenges in the synthesis of compounds like cubane, but it'll take a new generation to go after it.

TRAYNHAM: Last year in the magazine *Aldrichemica Acta*, two of your former students published an article summarizing your career, with major emphasis on research. However, the latter part of the article had very high praise for your teaching. Could you comment a bit on your approach or philosophy of teaching?

EATON: Well, I don't know exactly how to answer this question. I haven't really put things like that into words before.

TRAYNHAM: Well, certainly your interaction with your research students is teaching par excellence.

EATON: Well, I think if you're going to be at a university, your first job is teaching. Your first job is not research—even if you're at a research university, which is what the University of Chicago is about. I think the chemistry department here—some other departments as well—has always had the tradition that you live up to your responsibilities as a teacher, whether you're teaching an undergraduate class or graduate students. That's a very nice tradition. It's one that has been reasonably easy to stick with, because until recently the teaching loads have been quite light—the formal teaching loads. That's changing. That's another story that we can perhaps go into some other time. There's a warm feeling among the faculty group. There's quite a good collegiality among the chemistry faculty here, and there's a warm feeling about teaching. Some places I think if you overdo it with teaching, you're penalized. People look at you. They'll say you're not doing research first. Here it's recognized that research and teaching are intimately bound together. There is no great boundary or wall between these two things.

The graduate students at the university are a decent group. I can't say they're terrific. Now obviously, over the years, I've had some that are really outstanding. I've chosen fairly carefully, over the years, whom I will let into the group. That has put me in a position that, I think over thirty-something years here, I have only given twenty or twenty-one Ph.D. degrees. I have colleagues at other universities who, I think, give that every year. By <u>having</u> a small research group, I'm able to interact with them on a very intimate level. I think that's what a research advisor should do. I think it's very important in the early stages of a student's Ph.D. work—in the early stages of his career—that a research advisor really watches what's going on and is counting the drops with him, so to speak. I think it's very foolish to assume that a student in the laboratory, with the amount of training that we give them as undergraduates anywhere, really knows enough to do research or carry out laboratory work. You can't just say, "Here's the problem. Go do it." Or even worse, "Pick a problem and go do it." I don't allow my starting students to pick a problem. I tell them what I want them to do.

TRAYNHAM: Just as Paul Bartlett did with you to begin with?

EATON: Yes, exactly. Well, I mean, I haven't had a student ever come to me and say, "I have this outside problem that I want to do." That I would respond to right away. I really feel what's important is that a student should be given a problem that the research advisor is confident enough about that when a student comes in and says, "It doesn't work," or, "I can't reproduce this," that you know it does work and that it can be done. The problem now is the man's laboratory training. You get him over that hump, so that you're confident that when he goes on into the really exploratory world, that when he tells you, "It doesn't work," you can believe him that it doesn't work. At least, you don't have to ask him to repeat it <u>ten</u> times. A few times is enough.

To do this, I think you really have to be connected with what each student is doing. I don't think I'm as good at that now as I was years ago. As time goes on, as you know, you get other responsibilities, and they become overwhelming. I think it's interesting that for my sixtieth birthday celebration, the majority of the students who came back, the postdocs and so on, were from the old days—way back. It's very clear that then I interacted with the students on a <u>much</u> more personal, intimate level than what I'm able to do now. I really influenced much more strongly how they thought about science and what they were doing than I can do now. I still pay a lot of attention to my students, and they're in here all the time, but it's different from what it was before. Before, I was in the laboratory with them, and I was really looking over their shoulders. I enjoy watching students mature. The man who wrote the *Aldrichemica* article was really a kid when he came. He matured a lot while he was here, in the way that he thought about things and saw the world.

It's important to me also that students come and appreciate the <u>outside</u> world. The university is very good. It offers all kinds of opportunities for lectures outside of chemistry and stuff like that, but the city of Chicago is terrific. I say this as a New Yorker. It really is a great city. It <u>infuriates</u> me—absolutely, I go off the wall—when students come to the university and they just stay here, and they don't go down to the city. I've tried every year to take some of my consulting income and buy series tickets for the opera and the symphony and stuff like that, the theater—and I push my people to go. I raise hell if they don't. They think I should buy tickets for Bulls games. [laughter] I never do that. I mean, that they can do themselves. I think it's very important to appreciate the city.

This man Bob [Robert] Hormann, who wrote up that article—and in fact, I think arranged for this whole thing, this oral history—I sent him to his first opera. I remember to this day how stunned he was that it existed. Just an extraordinary experience for him. Of course, I took great delight in that. Mrs. Mendes, the teacher I mentioned from high school who taught me Latin and stuff like that, she took me to my first opera in New York. That was good.

I think it's very nice to work closely with students. I think Chicago is set up that way, because our graduate classes tend to be small. I don't think they're small because we want them to be small. We don't really find all the students whom we would like to have. The organic group over the years has been really quite small. It's always been the case that the number of students—the number of Ph.D. students—in each group has been small—three, four, five, something like this. There are a few exceptions to this, but they're never huge. They're never like, you know, thirty-five people or things of that sort.

I had an exchange years ago with a colleague, whom I'll leave unnamed, at another school, who asked me how many people I had in my research group. I think at that time I had, I don't know, four graduate students and a few postdocs. He said, "Phil, how do you get any research done?" I in turn asked, "Well, how many people are there in your group?" He said, "Thirty-five." I said, "How do you get any teaching done?" I got hell over that question, but I think it was appropriate.

I have a student now—a young man whom I think very highly of intellectually, but I can't get him to stop jumping from reaction to reaction to reaction. I had him in here today, and I finally laid down the law. I said, "You know, I'm failing in my duty to you. This has been going on for two years. You haven't changed at all. I keep yelling that you can't just try a reaction once. Then, because you think it doesn't work, try a different version, another totally different reaction, another totally different measurement. There are a thousand ways of doing this. Are you just going to try each one once? You're not getting down to the nitty-gritty of figuring out how to get it to work, or if it doesn't work, why it doesn't work."

I told him he's got until June or he's out. All right? He's got until June. All he has to do is take this one particular reaction. I said, "Just choose it. I don't care which one it is that you're proposing. I want you to choose that reaction and stay on that reaction until you can prove to me that you have enough experimental evidence to say that the reaction doesn't work, why it doesn't work, and so on." Well, I have one very shaken student, but it happens. I think it is important that I should do this. You don't just let the student drift along, and after five years give him a degree just because it's over. I think you have to set standards for students. The setting of standards, I think it's what all science is about—but certainly, it has to be what professors are about.

TRAYNHAM: You made reference a moment ago to having relatively modest classroom teaching assignments. That has been historically true; perhaps it's changing now. What is your principal classroom assignment?

EATON: I do both undergraduate and graduate teaching, depending on the year. At the University of Chicago, we don't have old-timers—I have now become one—assigned to do freshman chemistry or sophomore chemistry year in and year out. We try very hard to avoid that, so some years I'll do undergraduate organic, and some years I'll do an advanced synthesis course. This year, in fact, I'm doing one quarter of each. I normally, at the graduate level, teach things that involve synthesis.

The kind of teaching that we do here is teaching that introduces students, very much, to an early idea of independence. You know, I give students problem sets that are fundamentally impossible; I tell them they're impossible. You know, I don't want them to do the stuff that's easy. The undergraduate teaching at the University of Chicago is very nice indeed; there is a significant number of students who are really very, very good. I mean, they are a true challenge in the classroom to teach, and I enjoy that.

Next year—although I've never done it before, and this is in thirty-something years here; I don't think it ever has been done—I'm going to teach two quarters of organic chemistry sequentially. We have a quarter system here, so I'm going to do the fall and winter quarters sequentially. It's always been in the past—since undergraduate organic is quite a load on a person—that we've split it up three ways: three quarters, three ways. The kids hate the winter quarter. We get lots of reaction memorization and things of that sort. Of course the weather is depressing, and everything about it is bad.

I just sort of started to wonder, what would happen if they took the winter quarter from the same guy they did the fall quarter from, so that it really was a smooth development, rather than this fairly jerky thing that occurs. You never really know what the guy before you has taught—what flair, what flavor he's given everything. This was just a decision that I reached one day in the shower. My colleagues were fine—delighted. We'll see what happens. I'll only do that for a year. We'll do it as an experiment. The year after that, since this is a fairly heavy load, I'll probably just do one course or one and a half courses.

It's fun to teach here. It's taking more and more time, and it does leave one less time to teach at the graduate level. I don't mean the formal teaching; I mean the informal teaching. That's what graduate education is all about—having the informal teaching, the one-on-one interactions. There's the change in the amount that one has to teach, and there's the change in all the other—paperwork and stuff like that, that one has as one goes on. That's distressing. It's a standard complaint that everybody would make.

TRAYNHAM: Your research accomplishments through your career have been recognized with some awards.

EATON: Not very many. I mean, I could be very honest about that. That's one of the problems that I have with my colleagues, but this is rather typical of the University of Chicago. Although there's good interaction among people scientifically, there is not much selling of each other—quite different from other universities. As I get older, and I see others with many, many more awards, it annoys the hell out of me, in fact, to be quite honest. [laughter] I am pleased I've just gotten the one of the Cope Scholar Awards for 1997. Now, that pleases me no end. I must say, I think I should have had it a long time ago. I don't want to look a gift horse in the mouth, but we might as well be honest.

TRAYNHAM: You got acknowledgment from overseas at any rate, with the Humboldt Award.

EATON: Oh, yes, the Humboldt Award was very nice. I appreciated that very much. My interactions with German scientists have been very strong because of it. It's been very profitable from the intellectual point of view.

TRAYNHAM: You've continued that association since the time the award was given, in 1986?

EATON: Oh, yes. Right. When the award was initially given, the practice for the Humboldt people was that you would go over to Germany for nine months or a year. There was no way I could do that. I didn't want to do it; I thought it would be far too disruptive of my responsibilities here. Fortunately, my Humboldt Award was gotten as a collaborative effort from Prinzbach, [Emmanuel] Vogel, and [Waldmar] Adam in Germany, which is, you know, a fantastic combination of people. I told them I just couldn't do nine months at once. I wanted to break it up. I would come first for three months, and do it in three-month periods. This was unheard of. Fortunately I stuck to my guns, and these people were sufficiently powerful that the Humboldt Foundation changed the rules.

I did the first three months at Freiburg, and then three months at Cologne. Then I decided three months was too long. My wife, who accompanies me, couldn't take being away so long as well, so the next time we went for four weeks and then for four weeks again. I've still got four more weeks, or something like that, to go. This is why it's gone on over many years. I think, in fact, it has been very useful that way. It has become, now, a modus operandi for the Humboldt. You can go if you want for the nine months or the twelve months, but it's also possible to split things up. I think that's right; that's fair.

TRAYNHAM: You seem to be very effective at changing the rules of operation.

EATON: Well, if you're stubborn, you can. Some of these things—or some of these rules—are rules that got installed years ago when transportation was different, when your responsibilities were different, and so on. They just don't work nowadays. I think it's important to "just say no." [laughter] You'd be amazed how things change.

TRAYNHAM: I would imagine that would be useful to point out the needed corrections. What is your perspective on the future of organic chemistry?

EATON: It varies from day to day—from being in the dumps, to knowing that organic chemistry is central to—oh, how should I put it? Central to continuing. I once read long ago that if agriculture had to both continue feeding all the people on earth and clothing them as well, that it couldn't do it.

[END OF TAPE, SIDE 4]

EATON: I have no idea if this is really true. I think it probably is. That was basically without the kinds of things that organic chemistry has brought to agriculture, in terms of insecticides and so on, and more importantly, what organic chemistry has brought to clothing of people—the polymers and the textiles, and stuff like that. I think these are enormous changes, fantastic changes. I don't think they're changes that are really appreciated, as such. The physicists have been much better at selling the people on what they do. I admit television has changed things. The television is physics, but it certainly hasn't changed things the way organic chemistry has changed things. [laughter] I mean, people used to die all the time from simple things. Organic chemistry fixed that.

I think organic chemistry remains just as important as in the past. I mean, it really is the source of the things that allow a society to continue in some sort of a healthy fashion. I guess what I think organic chemistry ought to do right now is come along with a good pill for contraception. Otherwise, even with the best of intentions, organic chemistry's not going to be able to keep up with the population outrunning things.

I'm disturbed about the funding of organic chemistry in the country. The same complaints, I think, are true for most of science. We think things are really turning against invention and research. That's very sad. I don't think this is a matter of Republican versus Democrat. It has something to do with where the country is going, its lack of adventurous spirit, there being too many people, et cetera.

Organic chemistry, I think, is in danger—coming back to the scientific point of view—of becoming a tool, a service, for other areas of science: physics, surface studies, biology, molecular biology—things of that sort. I think it's very important for us not to get overwhelmed by these interfacial activities. Organic chemistry is a very strong discipline. It really is the core discipline at the heart of chemistry and at the heart of many other things. It shouldn't become a service industry.

One of my graduate students has just gotten a very good postdoctoral position with a group that deals with polymer chemistry. I keep telling him, "When you go, make sure that you don't become the synthesis chemist in the polymer group. You go into polymer chemistry so that you're an organic chemist who can both do synthesis chemistry and polymer chemistry—to have all of these things." The tendency nowadays is to get people buttonholed into little areas. This one does this kind of synthesis, this one does this kind of a gene manipulation, or whatever it happens to be. That's not a particularly coherent answer to your question.

TRAYNHAM: Oh, it's a very good answer. Let me pose it in a little different way. At <u>each</u> year-end, the editor of the *New York Times Book Review* publishes a list of the hundred most significant books published during the year. Among the list, published just over a month ago (3), was *The End of Science* by John Horgan (4). A capsule indication of the book's content read, "Embracing argument by a science journalist that the best and biggest discoveries are behind us."

EATON: Well, it's like this book, *The End of History,* which was written right before Russia collapsed—when the Soviet Union collapsed (5). It's absurd. I have not read the book, and perhaps it's wrong for me to be critical of it. I haven't read the Horgan book. Given its title, I know there's no chance I'm going to agree with the argument. We're nowhere <u>near</u> finishing the big discoveries. I take my little cubane, my little tiny project here. I told you before, you could change the way human beings fuel their cars and their trucks and everything else, if you could discover—I mean, we really have to be discovering; this is no minor thing—how you can make this stuff cheaply. Thermodynamically, it's out there. I mean, I know it can be done. To do it would be a fantastic discovery that would change everything.

Oh, this guy's nuts. I mean, there's an enormous future for science. The problem now is that first of all there's too many scientists—far too many. You take the end-of-the-year issue of *Journal of Organic Chemistry* that just came out. I mean, you could barely carry the damned thing. It's not that there's anything wrong with what's in there. It's just the question of, "Why bother with so much of this stuff?" That, I think, is the problem with science: not the end of science, but there's too <u>much</u> science. Too much of it is trivial; there's no purpose to it. You don't need to have the seven-hundred-twenty-eighth way of reducing a ketone. In fact, there's probably a thousand-fold more than that already. Such trivial things tend to get discovered over and over again. I think we have a problem in that we educate far too many people as scientists—<u>far</u> too many—or as semi-scientists. I think most of the Ph.D.'s whom we're producing in the country really aren't scientists, as we understand that. They're hands—technical people. It's even worse in Germany. I don't know if you know this, but Germany's producing more Ph.D.'s a year than the United States is. Their population is seventy million versus our, like, two hundred fifty. It's crazy.

TRAYNHAM: Well obviously, if science or organic chemistry is appealing to a larger number of persons than you think is appropriate to actually be in the work force, how does one go about telling them, "No, you can't enter your desired field?"

EATON: Raise the standards. That'll take care of it right away. I mean, my colleagues and my undergraduate students say that I grade at a ridiculously hard level. Yet I give exams now like I gave exams thirty years ago. It's a nice source of new exams, one that's thirty years old. Nobody has a copy in their file anymore. They can't answer the questions—again, except for the top people. The top people are superb. Even with a person like myself, who thinks that he's grading by the old standards, I very clearly have inflated my grades—going along, being co-opted. I haven't purposely gone along. I've been trying, I thought, to fight it; but I clearly know I haven't been fully successful. More and more people are going into these fields who think that they really come up to the standards of the field—well, what I think should be the standards of the field—and they're not.

You can see this is true if you take any journal now, be it the best journal or the poorest journal. You look at some articles and you must ask yourself, "Is there really some importance to doing this?" I don't mean important in the sense that it's solving a societal problem, but of scientific importance—or is it just an incidental observation that has been inflated far beyond its significance? An unusual amount of time and effort is wasted looking at things like this. Unfortunately, for most of this stuff, it really doesn't matter if it was done or not done.

I would see education of scientists changed very much. First of all, I would raise the standards. I would raise the standards at the undergraduate level—either by a change in the way that we're grading, or really dividing classes and having a true honors class, where the standards are very high.

Most of all, I would like to see a change in how graduate education is done. I think that the universities in general have done a disservice here with what has happened with master's degrees. At most places—at most of the major universities, like Chicago—students are not admitted to master's studies. They're admitted to a Ph.D. If you want a master's—just a master's—you can't come here. Students earn a master's degree incidentally here, on the way toward a Ph.D. Those students who don't make it through their first-year course work, and things of this sort, are fiddled into getting the master's degree because somehow we feel there's some responsibility to give them something. It's a booby prize—here. There are a few universities where this is not true. Lehigh, for example, offers, I think, a real master's degree. We don't. Most universities give these phony master's degrees. I think that's shameful—not because the degree is phony, but it could be used in a different way.

If I had my druthers—I don't, obviously—but if I had my druthers, I would admit, everywhere, qualified people to graduate school for a master's degree—a master's degree that has laboratory work associated with it, and I think perhaps two years of courses. We offer fewer courses here; I think that's becoming a mistake. Let's say, a real master's degree, not necessarily with a master's thesis, but at least the background. Then, you take a look at those people who are here in their second year and decide who "have it" for going on for a Ph.D. degree. You take a very small fraction. Actually, I'm sufficiently conservative that I'd set the fraction in advance—no more than 20 percent. It probably should be less than that, frankly. That should be a hard line, so that people don't slide in.

Now, the master's degree that you give the other 80 percent is an honest degree, where they're well trained and they can go out into industry. Industry is dying to hire master's degrees. If you have a master's degree now, you're snapped up by industry— a real master's degree. A Ph.D. can't get a job—but the master's degree can do fine. That's much of what's needed in industry. Educate the rest as Ph.D.'s. Those Ph.D.'s, I think, would raise the quality of science in the country—not only in the academic world, but in the industrial world. When they went into the industrial world, those Ph.D.'s would then have people who had master's degrees to work with—rather than putting a Ph.D., this top Ph.D., into industry all alone, competing with a whole flock of other Ph.D.'s and competing in a way that has to do with selling their research to management, rather than doing it. That's Eaton's solution to the problem.

TRAYNHAM: Well, the Eaton solution would certainly play havoc with your unnamed colleagues who launch thirty-five graduate students.

EATON: That's right. I think we should wipe that out. I would be delighted. [laughter] I mean, I know this guy—this guy I'm talking about—produced wonderful research, but not from thirty-five people. There are a few people among those thirty-five who have done that research. The rest have just been pairs of hands. He could have done all that research with half a dozen—maybe not all the research, but the stuff that was worth doing.

TRAYNHAM: One big difference between some European universities—in Germany, for example—and here, is that there are technicians to work in the university laboratories who are not degree students. Do you think that would be helpful here?

EATON: Well, I have a very practical example. Some years back—I guess it's five or six years ago—with the turmoil in Russia and whatnot, a man came to work with me as a postdoc from Moscow State University. He turned out to be fabulously good. At the same time, one of his colleagues who had an equal reputation went to Germany, where he was appointed initially for a postdoc.

That was a two-year appointment. It's possible here to get people into a third year of postdoctoral work with a little bureaucratic fiddling around, but not more. The university is concerned that one could keep postdocs on too long and take advantage of them. I think that's a very good concern. Now, this particular man was terrific; I managed to get him a third year. I raised his salary very substantially—more than I could afford, but he was terrific. Then, I also managed to get him a fourth-year job here. I could only do that through the university getting letters from outside as to how good he was and what his contributions were, and raising his salary again. I would not normally have done that, except it was at the depths of the unemployment problem with chemists. I wanted to make sure I had time to get this man a good job.

If I had had my druthers, I would have hired him onto the permanent staff—my nonexistent permanent staff—but there's no way to do that in the United States. The university obviously can't do it; you can't do it on soft money and be fair to the person. I mean, I'm an honorable man. I wasn't going to hire someone who, if my grant money disappeared, would have to disappear. I managed to get him a very good job at Abbott [Laboratories]. He's doing very well there, but of course he's not doing research with me—and he was doing fantastic research with me, fantastic.

This other man in Germany, who's from the same training and things of this sort, who went to Armand deMeirer—after two years Armand realized, "Oh, this is a gem." He was hired onto the permanent staff. Now, Armand is one of my competitors…

TRAYNHAM: Wow.

EATON: Yes. I like him very much. He didn't have any problem, you see, doing this thing. It's a fantastic thing for both of them—both for Armand and for this other person—and would have been for this man I'm talking about. I think it's very sad in this country that we cannot do things of this sort that way. We're not set up to do this. When it does happen in this country, when you look at the details, they're awkward. The person is being used. That I think is something we shouldn't do. There are significant numbers of people who will hire people on soft money, knowing that they're going to have to leave them go when the soft money disappears. They don't seem to give a damn about it. I think that's terrible.

TRAYNHAM: You made reference two or three times to your wife in the course of your comments.

EATON: Ah, yes. I'm glad you've given me the opportunity to talk about her.

TRAYNHAM: Do tell us something about your family.

EATON: I worked very hard for a long time; I was a shy young man. You won't believe that, but I was a shy young man. I was very bad with girls. It wasn't until late in life, when I was in my late thirties, that I met Phyllis, my wife-to-be. We struck it off very early on. In fact, I went to a brunch given by a friend. I have no idea why. It's again one of these things that God smiles upon because I normally don't go to brunches, particularly when I know that I am being set up with a blind date. That's not me. Well, I went to this place, I walked in, I saw her, and I fell in love with her. In fact, I said later on that afternoon, "We could save an awful lot of trouble if we just went and got married." [laughter] My father had said to me years ago, "One day the doorbell will ring. You'll open the door, and that'll be it." That's exactly what happened—exactly. It's an extraordinary thing. We're married now for about twenty years. In fact, it will be twenty years in March. We don't have a family. I think my wife would have liked that.

TRAYNHAM: You mean, you don't have children.

EATON: We don't have children.

TRAYNHAM: You have a family.

EATON: We have a family. We're a very close family between the two of us and a dog, a very big dog we both love. I was probably selfish that way: I didn't want to share her. I'm not big on sharing. Anyhow, Phyllis is a wonderful woman. I said at my sixtieth birthday, because I had to say something at the end—it was very difficult for me, because that's the kind of situation where I choke up very easily; I have a lot of trouble getting this out. I thanked all kinds of people, you know. I think I said something about thanking my colleagues for teaching me how to be a scientist, and thanking my students for teaching me how to be a teacher. I'm having trouble now. I thanked Phyllis for teaching me how to be a person. That's very much how I feel about it. She's a wonderful woman.

TRAYNHAM: What is her employment?

EATON: She is a psychotherapist. She has a private practice in psychotherapy. When I met her, her expertise was in child psychotherapy. It fit perfectly—I was prepared to move in. [laughter] Since then, we always travel together. It's too difficult to treat children under those circumstances; they can't understand things like this. She now does just adult psychotherapy; her practice is very small now. It's unnecessary; she just sort of really does it to keep her hand in. She's on the faculty of the Chicago Institute for Psychoanalysis and does some teaching. It's very important, very important. She's slowly learning things about cubane chemistry, and she actually enjoys it. I have come to respect psychotherapy from a position of substantial disrespect. I follow her patients—her chitchat about the patients—and it's very interesting to see how effective she is. I also see how much I've changed in twenty years. She's very effective—a very special person.

TRAYNHAM: Is there anything else that you would like to have to complete this interview?

EATON: Oh, I don't know. It's difficult to say. I have a lot of people to thank over the years, of course, ranging from Mrs. Mendes and early science teachers to Ev Gilbert—lots of people. Everybody has an impact on you in one way or another. Clearly, the adventure with students has covered the gamut of emotions—I mean, absolutely. From, "My God, what am I going to do with this one? How do I get him out of here?" to realizing that the student has the potential to be better than <u>you</u> are. It's been fun. I say good things about the University of Chicago and this department. I think things are changing in ways I don't like. I have a feeling part of that is just getting older, but part of it is real.

Science? Yes, I guess there's one thing I would say about science. Probably one of the biggest arguments that I have here with my colleagues, particularly those my age, is that they are not attuned at all to the practical aspects of their science. I am attuned to it—which I think again has to do with the early experience with Allied Chemical, and with being raised in a business family, and things of this sort. I probably have somewhat more concern about wanting to be wealthy. Wanting and being are two very different things. I've always thought that many of the people here—and I think many of the scientists elsewhere—don't understand that you can add to the intellectual adventure of research by trying to do things that are fundamental and <u>also</u> have practical implications. It's not an either-or situation. Too many of my colleagues and too many others feel that it is.

I'll take an example from natural products synthesis, because I'm always teasing the natural products chemists. I think the natural products people have proven that they can make most anything. They haven't proven that they can make most anything cheaply. Why not make part of the intellectual adventure the synthesis of a compound cheaply? It's very easy to come up with an example: things like quinine are yet to be made cheaply. Because of that, they can't be used in the Third World. Now, I'm not big on saving the Third World—I've got enough problems with population—but there's a reason for it if you need a reason.

I'm amazed that I got into this octanitrocubane business for a practical application. Again, I'm not big on blowing up the world, but that's where it came from. The support came from it. I think my colleagues, to a degree, look down on that kind of support because it's a type of military thing, which is already a no-no. It <u>has</u> a practical application. In fact, pursuing that practical application opened up the entire world of cubane chemistry in a way that <u>never</u> would have happened before.

I think it's important to see these two things, the intellectual challenge and the practical—the fundamental science and the practical science—as a unit. Science has moved to the point where it's not enough to just say, "It's interesting to do this," or, "It's fun to do this."

We've already proven that it's interesting to do it and that it's fun to do it. You can have the other thing as well.

Now, I don't want you to interpret this as support for what is happening now in the government, which is saying that mission orientation—the goal—is the only thing that's important and that the science that goes into the goal, they don't care about. They set a goal to cure AIDS, fine—or to cure cancer, or whatever it happens to be. I mean, these are all admirable goals. But they are certainly not excuses for funding crappy science. That's what's happening. That's why there are so many scientists, because there's so much of this money around to fund stuff which is, forgive the expression, just sheer crap. It's a rip-off on society; it's a rip-off on science. It's being fostered not only by the government, but by a lot of scientists who are covering their behinds.

Long ago, I established that I could be very irritating. [laughter] I suppose that has stayed with me. In the 1960s, I was asked to join an NIH [National Institutes of Health] study section. I was "integrating it" because I was the young person on it. I was young then. There was a threat of a cutback of money to the NIH. I was ruthless on this thing. I mean, I didn't think anything that was half-assed science should be funded. My colleagues didn't agree with that. When it came to this cutback that was going to happen, I thought this was just a marvelous opportunity. It was a ten-percent cutback, so you should just cut off the bottom 10 percent. It seemed to me totally obvious. [laughter] It was perfectly easy to identify the bottom 10 percent; there was no problem with that.

My colleagues—my own senior colleagues—said, "Oh, no. We'll all take a ten-percent cut. It's very important to maintain this bottom layer as an insulation, so they don't get to you." Now, I thought this was just horrible. I never got back on the panel, I can tell you. [laughter] I served my four years. Actually, the thing is still posted on my bulletin board. Miller beer, or one of the beers, came out with these little coasters. I think it was Miller beer. The expression on the coaster was, "Only you can prevent mediocrity." I left one of these at each of the seats at the NIH panel. That's something you're not forgiven for. I'll show you. It's still outside. The Miller beer part's gone, but the "Only you can prevent mediocrity" is out there. I believe that. I really believe that.

That's what's wrong with science, that there's not enough—but that's what's wrong with many things, not just science—politics, the whole shebang. People are unwilling to say, "There's no need for this mediocrity." It's perfectly fine to have average people doing average things. There's no reason to say that average people have to do the top things. All of this is what weakens it at the top. There are many different top things, and that's fine. No, I believe very much that the top carpenter deserves the same credit as the top scientist—the top craftsmen, and so on. The people who are out there working, who are paid for being lousy, that's what I object to. That puts it in a nutshell, I suppose.

TRAYNHAM: Well, thank you for being so generous with your time to tell us about your distinguished career.

EATON: Well, thank you for visiting me.

[END OF TAPE, SIDE 5]

[END OF INTERVIEW]

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