THE BECKMAN CENTER FOR THE HISTORY OF CHEMISTRY

MELVIN S. NEWMAN

Transcript of an Interview
Conducted by

John H. Wotiz and Milton Orchin

at

Ohio State University

on

3 and 4 March 1979

CENTER FOR HISTORY OF CHEMISTRY ORAL HISTORY PROJECT

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Melvin S. Newman, interview by John H. Wotiz and Milton Orchin at Ohio State University, 3 and 4 March 1979 (Philadelphia: Chemical Heritage Foundation, Oral History Transcript # 0004).

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

MELVIN S. NEWMAN

1908	Born in New Orleans, Louisiana, 10 March

Education

1929	B.S., chemistry, Yale University
1932	Ph.D., chemistry, Yale University
1933	National Tuberculosis Association Fellow, Yale University
1934	National Research Council Fellow, Columbia University
1934-1936	Research Fellow, Harvard University

Employment

	Department of Chemistry, Ohio State University
1936-1939	Instructor
1940-1944	Assistant Professor
1944-1965	Professor
1965-1978	Regents Professor
1978-	Emeritus Professor
1957&1967	Fulbright Professor, University of Glasgow
1944	Consultant to the United States Bureau of Mines
1945-1978	Consultant to the Upjohn Company

Honors

1939-1940	Howald Scholar, Ohio State University
1949&1951	Guggenheim Fellow
1956	Elected member of National Academy of Sciences
1961	American Chemical Society Award for Creative Work in Synthetic Organic Chemistry
1965	Honorary D.Sc. degree, University of New Orleans
1970	Wilbur Cross Medal, Yale University
1975	Joseph Sullivant Award, Ohio State University
1978	Honorary D.Sc. degree, Bowling Green State University
1979	Roger Adams Award of American Chemical Society
1979	Honorary D.Sc. degree, Ohio State University

ABSTRACT: This interview covers the education, teaching, and research of Melvin S. Newman, an eminent organic chemist. Initially, Newman discusses his family, childhood, and early education. He then elucidates his undergraduate and graduate activities at Yale and describes his initial experiences at Ohio State University, where he has spent most of his academic career. The interview continues with Newman's remarks about his early consulting and doctoral advising. The central portion of the interview contains Newman's reflections about his research at Ohio State and his approach to teaching in the classroom and in the laboratory. His publications, use of the innovative "Newman Projection," later consulting, patents, and awards are also discussed. The interview concludes with Newman's views about research funding, former students, and philosophies of teaching and administration.

INTERVIEWERS: John H. Wotiz is an organic chemist. Born in Czechoslovakia in 1919, he attended Furman University, the University of Richmond, and Ohio State University, where he received his Ph.D. degree in organic chemistry. He has since taught at six universities. Most recently, he has been professor of chemistry and chairman of the department of Chemistry and Biochemistry at Southern Illinois University. In 1982, he received the American Chemical Society's Dexter Award in the History of Chemistry.

Milton Orchin is also an organic chemist with an interest in the history of chemistry. He received his bachelor's degree in chemistry at Ohio State University. One of Newman's first graduate students, he earned the doctorate at Ohio State in 1939. Since then he has combined research in federal laboratories, especially for the United States Bureau of Mines, with university teaching both home and abroad.

NOTE: The following table correlates the tapes of the Newman interview with the pages of this transcript.

TABLE OF CONTENTS

INTERVIEW: Melvin S. Newman

INTERVIEWED BY: John H. Wotiz and Milton Orchin

DATE: March 3 and 4, 1979

PLACE: Ohio State University, Columbus, Ohio

Orchin: I'm Milt Orchin, professor of chemistry at the University of Cincinnati. I was the first graduate student to sign up with Mel Newman, probably one of the most fortunate experiences of my life. We have remained close friends and colleagues all these years.

Wotiz: I'm John Wotiz, professor of chemistry at Southern Illinois University. I'm also one of Professor Newman's former students. Mel, just for the record, how old are you?

Newman: Seventy years old, seventy years young.

Wotiz: Tell us a bit about your early youth.

Newman: I was the youngest of four children. I had two sisters and a brother. We lived in New Orleans, Louisiana. My father was a reorganizer of companies that had financial difficulties. They used to call him in to make the plans to reorganize and satisfy the different classes of stock and bond holders.

Orchin: I find your being the youngest child very interesting. I remember talking with Frank Westheimer not very long ago about chemists and their place in the family. I mentioned that I was the youngest in the family. He said, "You're lucky. The youngest in the family always does the best." Well, I don't know what happened to your sisters and brothers but we know what happened to you.

Wotiz: What was the sequence? Who is the oldest?

Newman: My brother, then two sisters, Sophie and Alice, and then myself. We were all about two years apart.

Orchin: So you were the fourth of four children.

Newman: As a kid, I was very much interested in athletics. I used to play every sport and was outside almost all the time. Unfortunately, in 1922, when I was about fourteen, the family moved to New York. This just about broke my heart. I didn't know a single person in New York and the children in my new neighborhood didn't play in the streets. I had nobody to play with. The family left New Orleans late enough in the year that I couldn't be enrolled in a private school. Furthermore, they didn't want me to go to a public school. So they advertised and got a tutor. This was a very fortunate thing because the tutor turned out to be a Ph.D. in organic chemistry. I started to have a little chemistry lab in our New York apartment.

Orchin: Was the tutor for all subjects?

Newman: All subjects. Chemistry wasn't being taught as a subject.

Orchin: What grade were you?

Newman: Let's see--about the first year of high school.

Orchin: What happened to your siblings? What schools did they attend? Were they still in school?

Newman: Well, my brother went to Cornell.

Orchin: Did they have tutors?

Newman: No, no. One of them went to a girls' school near Philadelphia and the other one never went to college.

Orchin: A residential school?

Newman: Yes. The other one was a musician. She took piano lessons in New York with the same teacher who taught Gershwin.

Orchin: But she wasn't through with high school by the time you moved, was she? Were the other children all through with high school?

Newman: Just about. Actually, I don't know if my youngest sister graduated from high school or not. What I remember about her most is her love for the piano. She used to practice on the piano practically all day long, and George Gershwin was in the same class. He used to come to the apartment every so often.

Orchin: George Gershwin did?

Newman: Yes, to play music in the evening, and I'd get out of bed to listen to him.

Wotiz: Is this where you developed your interest in jazz music?

Newman: That was a good part of it.

Wotiz: You certainly got it from a master. You mentioned that you missed athletic endeavors when you moved to New York. What was the particular sport that interested you more than anything?

Newman: Baseball.

Wotiz: Did you ever think of becoming a professional player or a semi-professional player?

Newman: No. As a matter of fact, I can't remember thinking as a child about what I was going to be. It never seemed to bother me. Since I was good in mathematics, my father said I should work with him because I would do very well in the reorganization business. He tried to pressure me to go into his firm, but my mother defended me. She said, "Let that boy do what he wants to do."

Wotiz: Did any of your brothers or sisters enter the family business?

Newman: My brother did. The older girl married a Philadelphia lawyer. The younger one married a musician, and today she writes and edits a column "USA," in New York City.

Orchin: Are you still in contact with your brother and sisters?

Newman: No, I'm not. My brother and older sister died some years ago, and my youngest sister and I had a parting of the ways many years ago. I haven't seen her for several years.

Wotiz: We got you to the middle twenties and in the hands of a tutor. What happened next?

Newman: I went to the Riverdale Country Day School for the next few years. I lived at the school for two years and graduated in 1925. Then I went to Yale and graduated from there in 1929. During my stay at Yale I never needed to worry about money. I rarely went out and was not extravagant about anything. If I needed a new set of golf clubs, however, I bought one. I played baseball and football. In those days I was very small. My nickname was Shrimp. When I went to Yale from high school, I went out for the 110 pound wrestling team to give you an idea of how small I was. During the next couple of years I gained about twenty-five pounds and grew three or four inches.

Wotiz: You mentioned that if you wanted a set of golf clubs you went and bought them. Is this about the time when you developed your interest in golf?

Newman: Well, I played golf on and off ever since I was about eight years old.

Wotiz: Golf is a sport at which you excelled.

Newman: It's been my favorite sport for many years. Actually, I was the seventh man on a six man Yale Golf Team. I played in the National Intercollegiate Tournament because one of the regular members had to serve a jail sentence!

Wotiz: For bootlegging?

Newman: I don't know what it was, but the judge said he had to spend a week in jail after he graduated. While he was in jail I played in the National Intercollegiates and actually qualified for match play. I won the first match against the second favorite in the tournament. On the second round I got beaten by Maurice McCarthy, one up on the 19th hole. He was the national amateur champion at the time. I was traveling in high society.

Wotiz: While you were at Yale, did you have to declare a major field of interest? Did you major in chemistry?

Newman: Yes, I majored in chemistry. I knew that I was going to do that from the beginning. Yale has such a wonderful golf course that all spring and fall when the weather permitted I would play golf every afternoon. Several professors called me into their offices and said, "Newman, you could be a good chemist if you'd stop playing golf."

Wotiz: Who was that? Anyone we would recognize?

Newman: I don't think so. Donleavy was the organic chemist and Saxton taught physical chemistry. They both said I should stop playing golf and devote more time to chemistry.

Orchin: I suppose the golf people told you "Don't spend so much time in the lab; you could become a good golfer." How large were the classes at Yale, say in freshman chemistry?

Newman: I never had a course in freshman chemistry because the chemistry teacher at Riverdale quit after two months and they never replaced him. So when I got to Yale, I just took the standard courses, but not general chemistry.

Wotiz: So you started with the traditional second year quantitative analysis?

Newman: I was always a rapid lab worker and I finished all the unknowns in quantitative analysis by the middle of February. The weather was still not satisfactory for golfing. The teacher of the course had some white material that he'd collected from stones in a building. It was leaching out. And he said he'd like me to do a quantitative analysis on that. I started to work on that and then the weather improved.

Wotiz: What did the white stuff turn out to be?

Newman: I played golf instead, and as a result he gave me a B

instead of an A. I said, "Why?"
He said, "Well, I don't like somebody starting something and not finishing it." So, I'd finished the course in half the time, but that didn't count.

Wotiz: How was it when you got into organic chemistry, which I think at that time was taught in third year. Did this create some different kind of response in your thinking?

Newman: Not particularly. I enjoyed it but I can't say that I was that highly enthused about it.

Wotiz: Where and when in your life did you start thinking about organic chemistry?

Newman: Well, when I was graduating my father made me an attractive offer to go into business with him. I turned it down. He said, "Well, what are you going to do?"

I said, "I think I want to go on for a Ph.D. in chemistry."

He said, "What are you going to do when you get that?"

I said, "I don't know. I'll wait and see."

This conversation took place before my father was hit by the Depression and experienced financial trouble. There wasn't any pressure on me at that time to earn a living. I just didn't think that way.

Orchin: Why do you think you liked chemisty, Mel? Did you enjoy the opportunity to manipulate nature?

Newman: That experience occurred during my work on my Ph.D. My thesis director, R.J. Anderson, wanted me to study the lipids of yeast. He had found that the fatty acids present in bacterial lipids were not all straight chains; some had branching methyl groups. He would do elaborate crystallizations and things like that, and of course he didn't have NMR. Nonetheless, he did some synthesis of a fatty acid and compared a synthetic compound. When he did, he was sure that there were methyl groups on the branch.

Orchin: I see. When you turned down your father's offer and decided instead to seek the Ph.D. in chemistry, you thought that you would enjoy doing chemistry more than working in the business world. What was it about chemistry that attracted you?

Newman: Well, it's really hard to say. I enjoyed almost every chemistry course I ever had, and it seemed to be what I would like to continue to do. Then, I remember in graduate school, Anderson got a fellowship from the Fleishman Yeast Company to investigate yeast. Since that was going to be my Ph.D. problem, he offered the fellowship to me.

I said, "Don't give it to me; give it to some student who needs the money."

He said, "Well, that's very nice of you, but you're the only one working on yeast."

I said, "Well, ask the Fleishman Company if they won't allow you to give it to someone else."

When I went to New York that weekend, my father called me to his room and said that he didn't know if he was going bankrupt or not, but that from then on I was on my own. That was a shock. I went right back and told Anderson, "I'll take that fellowship." Fortunately, he hadn't given it away yet.

Orchin: How much of a stipend was this?

Newman: I'm guessing. I think it was about eight hundred dollars.

Orchin: That was in 1929?

Newman: In 1929, or early 1930.

Wotiz: In other words, you got your undergraduate and graduate degrees at Yale. Did you know Anderson while you were an undergraduate?

Newman: I'd seen him but...

Wotiz: You never had a course with him?

Newman: He had just arrived in Yale at about that time. He went to Yale about 1928, I think.

Orchin: You know, it's the custom, Mel, that most universities encourage their graduates to go elsehwere and the students themselves try to go elsewhere.

Newman: I asked the chairman of the department if I could enroll in Yale's graduate school. He tried to discourage me. He said, "Why don't you go to another university?" I said, "Well, I like it here at Yale and there's no other place to which I particularly want to go." I didn't mention that none of the other universities had golf courses. Yale had a wonderful golf course. So why should I leave Yale?

Orchin: You never even applied elsewhere?

Newman: I never wanted to go any place else. Finally, I said, "Well, if I pay my own way, will you refuse me admittance?"

He said, "No, we won't."

Orchin: Was it common then for anyone to pay his own way in graduate school chemistry?

Newman: A lot of people paid for themselves. There were very few teaching assistantships. My best friend at Yale was being put through by the Texas Corporation. He knew he was going to work for the Texas Corporation when he finished.

Orchin: You mean Texaco?

Newman: Texaco. They opened a research laboratory at Beacon, New York, just about that time. He knew he was going to work there when he finished. All the others--and there were about eight people who got Ph.D.s when I did--did not get jobs. Fortunately I got a post-doc with Anderson to work on tuberculosis bacteria in June, '32. In July, I started to work with him on bacterial lipid materials.

Wotiz: So Anderson played a big role in your life.

Newman: At the beginning. He was the person who taught me to account for everything in a reaction. During most lab experiments, a sixty percent yield is satisfactory and the other forty percent is ignored.

Orchin: It depends upon what you're going to do with it, I suppose.

Newman: In lab courses you prepare a compound and forget about the rest of it. Anderson said that for everything you do, you want to account for one hundred percent of your material as closely as possible. He was very meticulous about technique and the quantitative handling of things.

Orchin: Was he a hard taskmaster? Did he keep after you?

Newman: On this post-doc fellowship I worked in his office.

Orchin: In your predoctoral work, you got your degree in three years. You started in '29, got your degree in '32?

Newman: Yes.

Orchin: What did you do in the summers?

Newman: I stayed there. But I used to take about two months off.

Wotiz: Was the lab closed or practically deserted during the summer?

Newman: Yale shut the lab at night during the Depression. The authorities turned off the heat and lights in order to save money. They were really hard-pressed financially.

Wotiz: Where did Anderson come from?

Newman: I think he was a Swede who his got Ph.D. at Cornell Medical School. He had spent some time in New Orleans but I never knew him when he was there. He was the chief chemist of the New York State Agricultural Bureau or Station at Geneva. He was supposed to find out which brand of New York grape was best for making different wines. In order to do that he had to categorize the wines; and he categorized, by ascertaining the pigments of grape skins. He used the rest of the grape to make wine. He did all of this during Prohibition and had a terrific collection of wines.

Orchin: In other words, he had a great following among the students?

Newman: Yes. He came to Yale right in the midst of Prohibition and his big question was, how was he going to get his wine supply from Geneva, New York, to New Haven, Connecticut? He filled his car with carboys of wine. He said that if he were ever arrested for a traffic offense, the headlines would read, "Yale Professor Arrested for Bootlegging."

Orchin: That wine wasn't fermented as yet, was it?

Newman: Oh, yes. He made champagne and brandy. He was a real expert at that. I would often go to his house for dinner, but often became drunk by the time dinner was served. Anderson's idea of a taste of wine was about two inches worth in a water glass.

Wotiz: So when he went from New York State to Yale, he took his research support with him?

Newman: No, he stopped working on wine. Then he started working on bacterial lipids. I don't know why, but he took things like tuberculosis bacteria, which were grown by some pharmaceutical company, and suspended them in alcohol. He then extracted various lipid fractions and analyzed them. Amazingly enough, even though you think of fats as glycerides, some of these bacterial lipids had no glycerin in them.

Wotiz: Listening to you, Mel, I get the impression that you were raised more along biochemical lines than along classical organic lines.

Newman: Well, in a sense, it was an analytical type of job. When I got through working with tuberculosis bacteria, I remember that I had to quantify once with an alkali. At that time, I noticed a little pinkish color, and remarked about this to Anderson.

He said, "Oh, you probably just left some phenolphthalein in there when you titrated the sample."

I said, "No, I haven't used phenolphthalein."

He said, "Well, see what you can find."

To make a long story short, I isolated one hundred eighty milligrams of a beautiful yellow crystalline pigment which he called phthiocol. One hundred eighty milligrams, that's a small amount, especially in those days when a lot of the micromethods were not available. This was during my post-doc. He was going to send the pigment to a biochemist at Rockefeller

Institute to see if it had any biological activity. I was talking with him in his office and I said, "Gee, you can't do that, you've got to determine the structure first."

He said, "How are you going to do that?"

I said, "Well, I wish you'd get on the phone and tell them not to put that in a lot of animals, but rather to send it back."

He did and he got the stuff back. We made an analysis of it, and I think that it contained C H O .

And he said, "Well, my lord, there are so many structures." You see, in those days, spectra couldn't tell you anything.

So I said, "Well, I'll fiddle around with it." I thought that if I could account for ten carbons as a naphthalene structure, then the eleventh carbon would probably be a methyl group. The oxygens would be either a phenone or an OH.

I said, "Why don't we oxidize it and see if we get phthalic acid?"

He did and it oxidized to phthalic acid. Well, that just about proves the structure in one step. And so it turned out to be 2-methyl, 3-hydroxy, 1,4-napthaquinone and we synthesized it and got large amounts of it. But to this day I don't know if they know how that thing arose in the lipids.

Wotiz: In retrospect how do you appraise Anderson?

Newman: Well, he was a real gentleman. He was one of the nicest persons I ever met. He was as honest as the day is long, which is a very refreshing trait.

Wotiz: You're talking about intellectual honesty?

Newman: Intellectual honesty. He would never think of ignoring somebody's work. He was very good in quantitative analysis. He had nothing to offer, however, in the way of synthesis. He was unable to make any synthetic applications.

Wotiz: In other words you taught him synthesis? Or did he learn from students?

Newman: He learned, I guess. But I was not reared in synthetic chemistry. I was isolating stuff from yeast. As a matter of fact, I got my Ph.D. degree by showing that when they grew yeast industrially, they put a lot of mineral oil in it. You might ask, how could that be a thesis? Well, he had me work on the lipids of yeast because he wanted to know something more about the unsaponifiable fraction of fat. He said that some reports in the literature said it was oil and others said it was ergosterol. He wanted me to find out. The Fleishman Yeast Company would send up yeast and I would extract. I got a lot of this unsaponifiable fraction that was oily. Now, I could isolate some ergosterol from it, but that was a very small fraction of it. I got rid of the ergosterol by shaking with strong sulfuric acid. The rest of it was completely impervious to strong sulfuric. I got it out. I distilled fractions on the best stills we had in those days, which were not very good, and I might say, in those days you had to do your own carbon-hydrogen analysis. There was no place to send it away.

Wotiz: (announcing) This is the second tape. The first tape was partly erased during an accident, and we will reconstruct it as best we can from notes taken during the period.

On the first tape we traced Professor Newman's life from his birth in New Orleans, to his family's move to New York City, to the completion of his high school education. While in New York, Professor Newman had a private tutor who helped him to complete his high school education. The private tutor was a Dr. Marcus, an organic chemist, who tutored him in all subjects. Professor Newman comes from a well-to-do family. His father was a businessman in New Orleans and then in New York City, who dealt with the liquidation of companies. Mel's family tried to induce him to enter the business. He refused. He found more satisfaction in studying chemistry.

Mel received his bachelor's degree and also his Ph.D. degree at Yale University. He worked with Professor Anderson, who was interested in the chemistry of lipids, especially as it related to yeast.

We also traced Professor Newman's work as a postdoctoral student with Professor Anderson and as a fellow at the Rockefeller Institute. Unemployed for three months, he was rescued by Dr. Anderson who helped him to gain a postdoctoral fellowship with Professor Fieser at Harvard University.

He was very much impressed with the quality of work at Harvard University. He considered it to be far superior to Yale's. Professor Fieser asked him to synthesize certain methyl-substituted phenanthrenes. During the synthesis, he developed an interest in stereochemistry. Some questions arose about certain cycloization reactions for making polynuclear hydrocarbons and the reaction of the Grignard reagent with the substituted aroylbenzoic acids. The formation of the pseudoesters was not understood at that time. He wanted to know what actually occurred. At the time he was also considering accepting a position at Ohio State University. He decided to focus his research upon understanding the formation and the properties of the so-called pseudoesters.

We are now going to continue with the interview. Professor Newman will trace his early years at Ohio State University.

Wotiz: In 1936, Professor Newman was in the process of moving from Harvard University to Ohio State University. Was it common at that time to have prospective faculty members present a seminar. What was the subject of your seminar?

Newman: It was probably in the field of cancer research because I was working with Fieser on it. I don't actually remember, however.

Wotiz: Were there any members of the Ohio State faculty at that time that impressed you one way or another?

Newman: Well, I remember liking Evans, but I can't say that I took to anyone else particularly either one way or the other.

Orchin: What attracted you to Evans?

Newman: Well, the way he talked about things. He was very forthright. I mean, he wouldn't just agree with you or not agree with you. There would always be a reason why he would say something. Additionally, he was a very cordial person.

Wotiz: Was this the only interview trip that you took during that period?

Newman: It was the only job offer I had. Period.

Wotiz: Were you actively seeking a position while at Harvard?

Newman: I must truthfully say, I think I would have accepted any job that was offered me. I wrote to several industries about a position but I never got an answer.

Wotiz: How long were you at Harvard?

Newman: A year and a half.

Wotiz: Is there any chance that the fellowship that you had with Fieser would have been renewed had you not obtained an offer of a position?

Newman: It would have been renewed, although I don't know for how many years. Fieser received money from the Eli Lilly Company for many years. Other people did also. Joshel worked with him. Didn't he?

Wotiz: Yes.

Newman: Do you know if it was Lilly money or not?

Orchin: No, I don't know if it was Lilly money. Joshel came there, I think, in '38.

Newman: Something like that. But Fieser really always had plenty of money to offer.

Wotiz: Was it customary at that time to have the applicant's expenses taken care of by the prospective employer? Or did you have to travel to Columbus at your own expense?

Newman: I think I travelled at my own expense.

Orchin: How about moving? Moving expenses were probably unheard of in those days?

Newman: I paid all my own moving expenses.

Orchin: What was your salary?

Newman: I think it was something like two thousand five hundred dollars.

Wotiz: For how many quarters?

Newman: Three quarters. You were expected to spend the fourth quarter doing research. The year's pay was always paid in twelve equal installments. In the early years, I often worked six quarters in a row so that I could take off for six months and still get paid. I could then do whatever I wanted with that six months. I could either spend it at Ohio State, or I could go some place else and earn some money. I remember that I spent six months at Yale, working on some kind of a fellowship. Getting money did not prevent me from getting some other money for that six months.

Wotiz: It wasn't considered sabbatical leave?

Newman: No. Ohio State has never had a sabbatical leave as far as I know.

Wotiz: You mean it doesn't have sabbatical leave now?

Newman: It's rare. You have three months a year essentially, and people have done what I've done. They take six months.

Orchin: You mean, you work six quarters and then take two quarters off?

Newman: Yes. For example, I had a Guggenheim Fellowship which extends for a year, but I had it broken up into two half years, two years apart.

Orchin: You could then get your full pay and the Guggenheim money as well. It's equivalent to a sabbatical.

Newman: It's equivalent to six months every two years which is better than a sabbatical.

Orchin: Well, that's an academic question we won't go into. What was your teaching load your first year?

Newman: Well, I taught general chemistry, freshman chemistry.

Orchin: How much?

Newman: Two quarters, three lectures a week. There was also a lab that went with it.

Orchin: You had some lab assistants, I trust.

Newman: Yes, but I gave lab lectures also.

Orchin: So your assignment was freshman chemistry, lecture and lab. You had three hours of lecture and one lab a week?

Newman: One lab lecture but I had two courses. I had two sections.

Orchin: I see, you taught the same thing twice.

Newman: Right.

Wotiz: When did you first teach an organic chemistry course?

Newman: About 1941, I think it was.

Wotiz: Undergraduate organic or graduate?

Newman: Undergraduate. I was transferred to the organic division after four years.

Orchin: I see. You didn't know, when you made the interview trip, that you were going to teach freshman chemistry?

Newman: Right.

Orchin: You'd never taught freshman chemistry.

Newman: I'd never had it!

Orchin: You never had freshman chemistry? You must have had to work on that.

Newman: Boy, I had to study up ahead of time.

Orchin: You probably were using McPherson and Henderson.*

Newman: I was.

Orchin: What was your rank?

Newman: I was an instructor.

Wotiz: After four years of postdoctoral work they hired you as instructor, and not as an assistant professor?

Newman: And I was tickled to death.

Orchin: Henderson was still in the department then, I remember.

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*William McPherson and William E. Henderson, An introduction to the Study of Organic Chemistry, 4th ed. (Boston: Ginn and Company, 1933).

Newman: Yes, and McPherson was in the graduate school.

Orchin: He was dean of the graduate school but Henderson was still teaching in the department.

Newman: I don't remember when he stopped teaching.

Orchin: Well I had him for a graduate course. Did you know him or McPherson well at all?

Newman: No.

Orchin: Who do you think were the most visible people in the department when you came? Who had you heard about at Ohio State? What was the image Ohio State had, looking at it from Cambridge?

Newman: None. As a matter of fact, when I told Bea there was a possiblility of a job at Ohio State, she said, "Where is it?" I said, "I don't know, let's look on the map." Both of us looked west of the Mississippi.

Orchin: Was Ohio State on the chemical map at that time?

Newman: It wasn't to me.

Orchin: Well, yes. When you came here and were here a year or so, who did you feel was...

Newman: Well, I thought Evans in his carbohydrate work.

Orchin: Wolfrom was here then wasn't he?

Newman: Yes. I had very little contact with Wolfrom. My contact was mainly with Evans and Foulk and Fernelius.

Orchin: Yes, and Werner.

Wotiz: You succeeded Bachman, that's how you got the job. But Bachman was more than an instructor when he left?

Newman: I don't know what he was. Was he a professor?

Orchin: No, I don't think he was a professor, but he was more senior than an instructor.

Newman: He went to industry.

Orchin: He went to Eastman Kodak Company?

Newman: To Eastman and then he went to Purdue.

Wotiz: When you were here on an interview trip, was there any discussion of what equipment you would need in the laboratory?

Newman: I don't recall it.

Wotiz: How long did it take from the time you were interviewed until you got a firm offer?

Newman: I don't remember, but I don't think it was more than two or three weeks.

Wotiz: Would you have any idea how many candidates there were?

Newman: No.

Wotiz: There was no commitment for you to teach any organic courses, graduate or undergraduate?

Newman: No. I was told I could have graduate students in the field of organic, but my teaching was inorganic.

Orchin: I'm curious as hell, Mel, about why Evans would have offered you the job. After all he wasn't in the same field. Carbohydrates was a special field; it had nothing to do with the kind of work you were doing. I wonder whether Evans relied upon Fieser's recommendation?

Newman: I think that Harvard had a terrific reputation. As a matter of fact, he told me after I was there, that over the years he paid more attention to recommendations from Harvard than from Yale. He said, that the chairman of the chemistry department at Yale had written a letter recommending somebody other than myself when asked to recommend me. Evans said, "The chairman of the Yale Department's letter would never have gotten you a job any place."

Let's talk a bit about honesty. After I got the job I passed through Yale, where the chairman called me into his office. He said that he wanted me to know how influential he had been in getting me the job here!

Wotiz: At that time who was the chairman at Yale?

Newman: J. A. Hill.

Orchin: Well, really, it's sort of a puzzle. I don't mean to detract from the personal impact that you could have made, but you know, it was a buyers' market.

Newman: Right. I think that Fieser's letter and Kohler's letter probably got me the job.

Orchin: Give some credit to Evans. He was probably looking out for you, maybe to build the image of the department.

Wotiz: Well, you did mention that Bachman was a physical organic chemist.

Newman: And he was a Yale man. Bachman was kind of an organic chemist.

Wotiz: Yes, but with an orientation towards physical chemistry.

Orchin: I did my senior problem with Bachman, and it was organic. I would have thought that you had a lot of strikes against you, knowing Evans. I did all my undergraduate work at Ohio State, and sometimes students know a good deal more about what's going on in a department than some of the faculty. You know its an old story, but I'm surprised that they would have brought in a Jewish faculty member at that time. Being Jewish, I had a lot of difficulty. You must have been very convincing.

Newman: Well, I don't know how it happened, but it did. I always felt, the letters...

Orchin: You must have given a damn good seminar.

Newman: You don't remember me giving a seminar, do you?

Orchin: No, I don't, but I was an undergraduate. When you entered the university I was a senior.

Newman: I think I talked with every faculty member. But my guess is that I didn't give a seminar. I don't remember making some slides up or anything like that. My feeling is that in those days the department head made the decision. He might consult other faculty members as a matter of courtesy, but it was strictly a matter of courtesy. In those days I don't think the faculty got together and voted on someone. I think Evans just talked with individual members and then made up his mind.

Orchin: Do you remember talking to McPherson, for example, in the graduate school?

Newman: I probably talked with him.

Orchin: He was an impressive person to talk to. He wore a stiff white collar and always dressed immaculately. He was a very impressive person and spoke with authority.

Wotiz: How many days did you spend in Columbus during your interview period?

Newman: About three days. As a matter of fact, I stayed with Evans in Evans' home. I remember playing ping-pong with one of his kids.

Wotiz: Evans was department chairman for how many years?

Newman: A long time. I know this, when the new chairman, Mack, was announced at some meeting, Fernelius said, "That means I'm leaving." So it must be that Fernelius was a candidate, or thought of himself as a candidate for chairman, and when he was passed over for Mack, he just said, "I'm leaving." And he did.

Wotiz: Do you recall when Mack became chairman?

Newman: I would guess it was in the early forties. We got into the war in '41. I would guess it might have been '40 because he went away during the war and Moyer became acting chairman.

Wotiz: I came in 1941 as a student and Mack was chairman at that time.

Newman: I guess that Mack came in 1940.

Orchin: I didn't realize it was that early. How long had Evans been head?

Newman: For fifteen or twenty years.

Orchin: Who was chairman of the department before him? Was it McPherson?

Newman: I think it was McPherson.

Orchin: Very likely, and McPherson dates back a long time.

Newman: Yes. I was promoted from assistant professor to professor in 1944. There's a story behind that. I'd had some contact with the Upjohn Company. Their research director had only a B.S. degree, and that was in biochemistry, not organic. I remember talking with him at Upjohn. He said, "The next research director we get here has to be an organic chemist." He foresaw the future of the pharmaceutical industry as being dominated by synthesis, whereas previously it had been a biochemical enterprise.

Wotiz: You mean, you had an offer from Upjohn to become...

Newman: Research director.

Wotiz: In 1944? Was that a firm offer?

Newman: Yes. It was a firm offer. I wasn't going to be the research director right away. I was first supposed to do organic research for a couple of years.

Orchin: That was in 1944.

Newman: 1944. And I told Moyer, who was acting chairman, that I was leaving. I didn't go to him to bargain. I just said that I was leaving. The end result of my statement was that I was offered a professorship.

Wotiz: But you did skop associate professorship, right?

Newman: Yes, and I accepted and stayed.

Orchin: When were you promoted to assistant professor?

Newman: After about four years.

Orchin: After four years as instructor, to assistant professor?

Newman: Maybe it was three years. I'd have to look back, you know.

Newman: Well, overall, it was a depression-dominated situation. There were no jobs.

Newman: They don't hire anybody as instructor any more. The market's different. But if we'd have another depression, you'd see a change pretty damn fast.

Wotiz: And had you gone to Upjohn, you would probably have tripled your salary?

Newman: Probably.

Wotiz: So you stayed here at considerable financial sacrifice.

Newman: Yes. Upjohn hired me as a consultant, however, and that helped my financial situation.

Wotiz: What attracted Upjohn to seek you as a consultant?

Newman: The talks with the chemists that I had while I was there. You see, I started on a Vitamin A synthesis that they were supporting. Helen Ginsberg was working on it, but she became allergic to the compounds she was working with and had to stop. I didn't put anybody else on that project and quit the Vitamin A work myself because I realized that Vitamin A synthesis was being done all over the world at a terrific rate. How was I going to compete with pharmaceutical companies that had whole teams? So I got out of it.

Wotiz: I'm a little bit confused here. This vitamin A work was financed through a fellowship. But, prior to that period, what particular aspect of your work did they find interesting: the carcinogenic synthesis, hydrocarbon synthesis, or what? Or were they impressed with your reputation?

Newman: No, it was just the general area and as I remember, when I spoke with them, I said that I did not want to be a consultant telling them what to do. They had to make the decisions about economics. I said I would only talk chemistry, which I always did. Helen Ginsberg also worked with their support. I think that she was the only other one. They had given me money over the years but never very much.

Orchin: What do you mean, they had given you money? How much money?

Newman: A post-doc for a year. No real program.

Orchin: Four or five thousand dollars occasionally when you asked them for it?

Newman: Yes.

Wotiz: I remember Fones, wasn't it, who also worked on Vitamin A precursors. Was he supported by Upjohn?

Newman: It might be. I'm not sure.

Wotiz: But work on Vitamin A involves some liquid ammonia syntheses. How did you become interested in this particular aspect?

Newman: That was mainly from my contacts with Fernelius. He did a lot of inorganic chemistry in liquid ammonia. As a matter of fact, I sat in on one of his thesis exams, and I stopped one of his people from getting a Ph.D. It was very simple. He had taken some kind of benzylamine derivative and studied the reactions with sodium and liquid ammonia. He explained the results by getting toluene and reducing it. He never isolated the toluene. During exam I asked, "How can you do work like this and not isolate the toluene?" Well, somehow or other it just never occurred to him.

Wotiz: You mean, he assumed the formulation.

Newman: He assumed the formulation. I realized that I couldn't sign his thesis unless he isolaed the toluene. So the exam was discontinued. He wasn't flunked, just delayed. The guy went and isolated toluene. It was eventually all OK.

Wotiz: Well, you did not deny him a degree.

Newman: No, he got his degree. It just delayed it a quarter.

Wotiz: But weren't Boord and Greenlee already working at that time with liquid ammonia as a medium for synthetic reactions.

Newman: They were doing large-scale Grignard reactions. I don't recall if they were doing large-scale ammonia or not. They might very well have. But I associated them more with large-scale Grignard reactions.

Wotiz: There is an organic synthesis by Greenlee and Boord whereby they make compounds in liquid ammonia. I just wonder if there was any close contact between the three groups of Fernelius, Greenlee and Boord, and you.

Orchin: Coming back to support of the work that Mel did, I dont't know if he'll remember this, but one summer you got some money from Parke-Davis. Do you remember Oliver Kamm?

Newman: Yes.

Orchin: You know, he was at Illinois. He wrote that famous book, the first quality organic book.* He then became director of research at Parke-Davis. They wanted to do some testing on cancer compounds and your laboratory was one of the few that could make them. So you arranged for me to prepare them for Parke-Davis. We agreed that I would make five hundred dollars for the summer which I thought was magnificent.

Newman: You were an undergraduate at that time?

Orchin: No, no I was working in the same kind of field. I thought I'd take this time off and make some money.

Newman: I don't recall this at all.

Orchin: Well, that just goes to show you. Of course, I was getting the money.

Newman: So you would remember.

Orchin: I think I made one hundred milligrams of three compounds in three months. I was working my head off to do all the conventional synthesis, starting with, you know, naphthalic anhydride.

Wotiz: What year was that, Milt?

Orchin: I'm guessing. I got my degree in 1939, so I would think this was the summer of '38.

Wotiz: But you were not the first student who got a degree from him.

Newman: No, Lloyd Joshel was the first student who got his Ph.D. with me.

Orchin: Lloyd was a year ahead of me and transferred with a master's degree from Illinois. He was ahead of me, so though he started work later that I had, he finished earlier.

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*Oliver Kamm, Qualitative Organic Analysis, 2nd ed. (New York: J. Wiley and Sons, Inc., 1932).

Wotiz: Well, you were then the first student who signed up to work with Mel? Tell us something of how this came about.

Orchin: Well, Mel probably has a different recollection of it because he's sitting in the office, but my problem was relatively simple. I got my undergraduate degree in June, 1936, at Ohio State. I had applied for a teaching assistantship at twenty-five other institutions and hadn't gotten anywhere. I went to see Billy Evans, as the students called him, about a teaching assistantship. I had done a senior problem with Bachman and I was working in his lab with Bachman.

Wotiz: Bryant Bachman?

Orchin: Bryant Bachman, because he was in charge of the undergraduate organic lab. He singled out one or two students and I worked with one of his doctoral students, Harry F. Miller.

Newman: Oh he went to Harvard.

Orchin: Harry F. Miller, yes, he was a character and a genius. Anyway, I worked in his lab and did a problem with him as a sophmore. Then as a senior I worked in Bachman's lab on the first floor. Bachman left in 1936 and the position opened up. I couldn't get a teaching assistantship anywhere so I was going to stay at Ohio State. Evans arranged for me to get what was called the Ohio State University Scholarship. It was a great honor. Evans told me straight out that there weren't going to be any Jewish boys who got assistantships there. So I didn't know who I was going to work with.

It was clear to me that the organic people there at that time were Cecil Boord, Brode, Evans, and Wolfrom. I didn't want to go into carbohydrate chemistry, which eliminated Wolfrom and Evans. And Brode, frankly had no interest in his students. Also, Boord was so classical. He was very well organized but he wasn't very stimulating. So I said to myself, "No matter who comes in, I'm going to work with that guy." So on practically the first day that Mell came into his lab, I went into his office and said, "Dr. Newman, I am Milt Orchin, a graduate student here. I'd like to work with you."

Newman: My recollection is, a student comes to the door and says, "I'd like to work with you."

My reaction is, "How do you know?" I hadn't even thought of a problem yet. It was kind of unflattering in a way.

Wotiz: So what problem did you suggest to him?

Newman: Making some of the substituted methyl-benzanthracenes.

Wotiz: Did you have laboratory space already assigned to you?

Newman: I was just cleaning out my own lab and beginning to work.

Orchin: My recollection is that I said, "Dr. Newman, I'm Milt Orchin and I'd like to work with you."

Mel's reaction was "Why sure."

I said, "Well, where would I work?"

He said, "Well, the first thing you can do is clean the glassware."

Wotiz: Whose laboratory was it?

Newman: My lab.

Wotiz: Prior to you?

Newman: Oh, Mary Renoll's. She worked for Dr. Henne. I went in there and said, "I'm told this is my lab."

She said, "Wait."

She went and talked to Dr. Henne and she started clearing out right away.

Wotiz: Was that a surprise to Henne that you pushed her out?

Newman: He wasn't a faculty member. He was with the Midgely Foundation.

Wotiz: Was Henne one of the people who was asked his opinion about who would be our new faculty member?

Newman: I don't think so. He wasn't a faculty member.

Wotiz: Was he consulted or anything like that?

Newman: I don't know. He was pretty independent. It was a strange relationship, but of course he was a pet of Midgley.

Orchin: And they were supporting his research.

Newman: Yes. It was a very special arrangement.

Orchin: I guess he came on the faculty, but I don't remember exactly what year that was.

Wotiz: Well, as an instructor were you consulted about additions to faculty or other personnel matters?

Newman: Not that I know of.

Wotiz: So Orhcin was your first student. How many students did you attract within the first year?

Newman: Wwll, Joshel and a fellow named Harold Vivian who got a master's degree. As I recall there were three.

Wotiz: So three students in your first year as instructor. That's a pretty good record.

Newman: I don't remember what ever happened to Vivian. I don't think he did any research. I think he just wrote a master's thesis.

Orchin: Yes, I remember he did some experimental work. Very soon you got some space across the hall, in B-14, where some of Boord's students were. When Joshel came in we didn't have any space, so you got some additional space over in B-14. When you got that space I moved with Joshel so Joshel and I then worked together.

Wotiz: Tell us something about your early years at Ohio State--the trials and tribulations.

Newman: Oh, I don't remember any tribulations. I asked for and got 8 o'clock to 9 o'clock lectures six days a week, so I had a Monday, Wednesday, Friday section and one on Tuesday, Thursday, and Saturday. All of my formal lecturing was over by 9 o'clock.

Wotiz: This was general chemistry.

Newman: General chemistry. Then I went and did research, which was only interrupted when I visited the labs and gave a lab lecture evey so often. I had the nurses and phy. ed. majors in one of the sections--which was quite a combination. I was doing some experiments on milk and they'd always drink half their experiments.

Wotiz: Did you have any experience in lecturing or teaching class in your postdoctoral work, as a teaching assistant, or anything like that?

Newman: Never one minute.

Wotiz: Well, that must have been difficult, coming into a big school with large lecture sections. How many were there?

Newman: In one section I had less than a hundred students; but another section sometimes had two hundred and fifty.

Wotiz: How did you cope with this, having had no experience at all.

Newman: I don't remember worrying about it too much because I did the best I could to explain whatever I was explaining.

Orchin: Did you think that was a good experience? Would you recommend that most entering faculty members have a large class to teach in order to develop certain kinds of teaching skills? How important is that?

Newman: Well, looking back over the years and seeing what'd happened in organic--and it certainly happens in every field--I think, in general, one of the biggest drawbacks or faults of teachers is that they try to teach too much. After all, you have only a certain number of hours of lectures,and I don't think that the human mind is capable of assimilating too much. When you lecture, you're not lecturing to the two or three geniuses who might be in the course. You're lecturing to the average student. I've heard some of my colleagues give lectures in organic chemistry, and in my opinion the lectures were not too good because they tried to cover too much ground. When I talked to them about it they said, "Look at the text-books now and compare themto what the textbooks used to be."

I said, "All right, look at the mind of the human now compared to what it was. The mind hasn't changed at all."

Orhcin: How do you resolve it?

Newman: I think it's better to teach a few things well than a lot of thing poorly. They'll forget it. I think one has to be more selective. I think that the teaching of organic should be to two types of students. One is the person who is going to be a serious scientist. The other is the person who is taking organic because he's requiered to do so. The latter are not going to remember a lot of the facts. So if you ask me what I want a student to remember ten or fifteen years later--after a lot of the facts are lost--I would say, think of how organic chemistry will satisfy the needs of the country.

To give an example, you start off teaching organic, and everybody starts with hydrocarbons. Then you get isomers, right to octane. Now, C H has eighteen isomers. I'll say, "I expect you to be able to write all eighteen isomers as quickly as you can make marks on paper. I don't expect you to take a half an hour to do it, although the first time you can take half an hour. But if you practice a little, you'll get a method in your madness which enables you to write down the eighteen octanes in two minutes or less." People wonder why I want a student to do that? Then I tell this story. The gasoline that you burn in your car is mainly octane, and years ago the American Petroleum Institute supported the work of Professor Boord here. They wanted him to make two gallons of each of the isomers of octane. Now, why should a petroleum institute spend that amount of money to get two gallons of each of these octanes? The answer is that they wanted to know which isomer made the best gasoline with the least tetraethyl. And after all that work was done, they designed their refining program to make as much as possible of these highly branched ones that turned out to be the best.

Naturally in gas and oil you get mainly straight chain things. Then you start talking about cracking, you see. All right, you get iso, I mean, butene form. Then you get the idea, well, you've got to isomerize these things. That gives you isobutene. And then if you recombine these things, you get the branched gasoline. So, I cover reactions of hydrocarbons, you see, with a practical aspect. The reason you want to do this is to make better gasoline; and that's the power of research.

Now, I think that's a much better way to teach them chemistry than to tell them, "Well, if you take magnesium and you put two halides together, you can make..." I cut out a lot of the synthetic work that isn't useful and show them the industrially important aspects, and impress upon them the importance of research.

Wotiz: You were talking about two types of students: the more serious scientist and the person who's taking chemistry as a service, pre-medical student, etc. Is what you've just outlined common to both?

Newman: I would say that it's common to both of them. Now, I would say to the people who are going to be serious scientists, "Don't expect that all you have to learn is what you get in a lecture. That's just a minimal amount. If you're a serious scientist, you're going to have to augment what you know by independent study."

I think it's wrong to educate students with the idea that all they have to do is to know what's given in the lectures. The number of people who are going to be serious about organic chemistry is very small, yet we now have five thousand students taking organic chemistry.

Wotiz: You have that many students?

Newman: Yes. We have roughly five thousand students who will take some organic chemistry. Now, of that number, what do you think would be a realistic number of those aspiring to be serious scientists? Maybe five or ten. You cannot direct all of your teaching just to these five or ten. I think that the sooner they realize that they have to do it on their own, the better they are. And the only way they'll recognize this is if you tell it to them. And in a way, industrially, I would say that the dollar sign is important. You can't spend a fortune for gasoline. But it's found out in research that these molecules rearrange and that you can go through a mechanism. It doesn't take too long to make a carbonium ion move. You can then combine this and show them how you can combine them to get branched things.

I would also tell my students that this sort of thing still costs money and the gasoline they buy at the tank still has only a small proportion of these branched things in them. But the organic chemist who's going to make a better drug, let's say, or a better plastic or anything like this, anything useful out of organic, has to realize that there's more than one way of doing things. I would continue by telling them that there may be thirty methods of making aliphatic hydrocarbons, but that I am not going to spend my time going over these thirty methods. If they're interested, they can get books, read them on your own, and see how it's done.

Wotiz: From a teaching point of view, I think that organic chemistry textbooks suffer from a disadvantage. They all traditionally start with hydrocarbon, whereas chemistry is relatively difficult to teach. I remember Conant's book. He started with alcohol.

Newman: Right. He's the only one.

Wotiz: The only one, and his book disappeared. I think it would be nice to come right back to his idea and start with alcohol because it's a functional group. You can really put it in the hands of a student and see what he can do with it.

Newman: You can get into the same thing pretty quickly if you take an olefin and say, "Now, if you treat this with sulfuric acid and water, you get secondary alcohol from it. But if you treat it with diborane and then an oxidizing agent, it goes on the end." This approach introduces them to the concept that you can take the same starting material and get different products.

Wotiz: What kind of approach did you use at that time? Was it any different from what you just explained to us?

Newman: I don't think so. My opinion of teaching has always been that it's better to teach a little bit well than to teach a lot poorly. The human mind can only grasp so much material in a certain period of time.

Wotiz: Well, I think you must have been a rather rare breed--a faculty member who came into a classroom with practically no teaching experience. I don't recall meeting anybody in my entire life who came as cold to a classroom as you did.

Newman: That's right. I didn't even do any tutoring.

Wotiz: In retrospect, was it an advantage or a disadvantage, having no experience in teaching?

Newman: Well, it's not a parallel experiment. You can't tell what I would have been like if I had done things differently.

Wotiz: Did you give any thought of preparing or making use of any existing lecture demonstrations that were available in the department?

Newman: There were a whole set of lecture demonstrations in general chemistry at Ohio State. I recall that when I saw demonstrations I always felt that too much time was being taken for the demonstration of a fairly simple point. I have always been against demonstrations in teaching. People talk about what kind of problems you should use on the blackboard. To me a blackboard is a plane surface and the best way to have students learn about problems in stereochemistry is to get a set of molecular models. I tell students that everyone should get a set of models.

Wotiz: But I do recall, when I was a student at Ohio State, that it was mandatory for students in organic chemistry to buy one of those Boord model sets. So obviously, it was already sold by that time. Was it mandatory in your earlier teaching days?

Newman: I think it was. Now it's optional. In other words, I tell the students, "I advise you to do it, but I'm not going to check up on you and insist on it."

Before I forget, I'd like to mention one thing that I've been doing for a number of years that everyone agrees is a good idea. I also have yet to find somebody who has copied it. It is the program I've had of interesting the gifted students in chemistry at an early age. When I started roughly thirty years ago, I would phone eight or ten high school teachers of chemistry in Columbus and tell them that if they had a really gifted student who would like a summer job, I would give it to him. I got the money for this from research grants. This item was never removed from any of my budgets, although other things were. The amount of money is not large. But I tell the chemistry teachers that what I want is the really gifted student, preferably sixteen years old, who thinks that he might like to be a chemist. And I give the youngster a job working with two or three people in my group, either post-docs or graduate students. His first job is to clean up. He has to learn how to clean apparatus in the lab, run errands to the stock room, and things like that. But in return for that duty, the post-docs and the students have to satisfy the student's curiosity about anything he asks. So it's a two-way deal. For example, if they want to teach a student how to take an IR spectrum, they have to explain the whole theory of spectra, why they do what they do, and how to interpret the results. But this aspect is entirely up to the student who's doing the work. Some people are interested in the spectra. Some people are more interested in learning how to make an intermediate.

Wotiz: And you have been doing this for the last thirty years?

Newman: Thirty years.

Wotiz: And it was always funded?

Newman: That item was never removed from a budget by national agencies.

Wotiz: Maybe I didn't quite understand. This was funded right from the start by an outside grant?

Newman: Well, specifically, for instance, I had a research grant for two students and a post-doc. Then in addition to that I put in, say five hundred dollars, and explained in a paragraph what it was to be used for--summer work for a gifted chemistry student. Some of these kids worked two or three weeks and quit. They said, "I'm not interested." But I think this was an advantage. The guy knew at a young age that he didn't want to be a chemist.

Wotiz: Are there any eminent chemists now who started out as high school students with you? Do you recall any?

Newman: Unfortuantely I haven't kept records. But I know there are quite a few who have gone on to get Ph.D.s in chemistry.

Wotiz: Could this program have been motivated, by any chance, by your experience with your tutor?

Newman: Consciously, no; although it might have been subconsciously. I've always had a feeling that not enough attention is paid in the teaching of chemistry to the really gifted students. These are the people who are going to make the contributions in the long run. If you take a high school kid who's very intelligent and he goes up against a class that doesn't move him, he gets into trouble.

Wotiz: When I was a teaching assistant for Art Campbell he advised me, "Don't waste your time with the poor student; give all your attention to the good student."

Newman: Well, I would disagree. By far the larger number of students are going to be the educated citizenry of the country.

Wotiz: And how many students went through your laboratory over the years?

Newman: About four students a year for roughly thirty years. That makes one hundred and twenty students.

Not all of them stayed all summer. But I told them when they started that I was not using the program to make them become chemists. I wanted them to see what being a chemist is really like. I've had girls in this program too. But it seems to me that it would not work very well in English, history, or something like that--although they might have different ideas on the subject.

I don't think there's enough attention paid to the development of the best students in the country at an early age. Since knowledge in every field has leaped so tremendously, it's even more important to start them early now than it was in the past.

Wotiz: I remember that you had an article in the Journal of Chemical Education.*

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*Melvin S. Newman, "Organic Laboratory for Graduate Students," Journal of Chemical Education, 46 (1969): 386-87.

Newman: A letter to the editor.

Wotiz: Would it have been worthwhile to follow up that letter in a more formal kind of publication?

Newman: I guess the NSF would have been the place to do that. But they have so many damn programs that I think the effort to push something almost unilaterally...As I said I don't know of anyone else who does this in our department. I'm not saying that some of them don't hire summer workers. I'm saying that I doubt whether they tell their graduate students and post-docs that this fellow is doing dirty work for you, and that you have to satisfy his curiosity and teach him how to do things. It's a two-way street.

Wotiz: Well, since the apprenticeship type of education is valuable...

Newman: That's what it is, yes. And we don't start kids younger than sixteen because of insurance laws. In other words, a kid has to be sixteen or more or he's not covered by university insurance. That's the only thing that stops that.

Orchin: When you first came to Ohio State, you had verbally agreed with Professor Fieser about what areas in the polycylic hydrocarbon work would not conflict with his research interests. I assume that there were two broad problems that you started working on. One dealt with the problem of opening rings and seeing what methyl groups do at the positions where the rings are attached and the general synthesis of benzanthracene derivatives. The other was pseudoesters and sterifications. Actually, I don't think you've ever abandoned either of those, have you?

Newman: No. Actually the keto acid thing is tied right in with the benzanthracene syntheses. What made it have theoretical interest for me was reading Hammett's book on physical organic chemistry* and learning about how Hanze had measured the melting point depressions of organic compounds in concentrated sulfuric acid.

Orchin: Yes, I think that's what I was leading up to, that in the study of these esterification procedures the next area in which you made major contributions was based on the work in concentrated sulfuric acid. You worked with Van't Hoff I-factors, learning what kind of species were present in concentrated sulfuric acid. You say that this then was the next major area of your work. It seems to me that in reading over your early work, this was really the first effort you made on a more theoretical basis. This was the first step you made in looking at detailed mechanisms and trying to trap or capture

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*Louis P. Hammett, Physical Organic Chemistry, (New York: McGraw-Hill Book Company, Inc., 1940).

them, and thus present evidence for intermediates which might lead to the variety of products which you got. Is that a correct assessment, do you think?

Newman: That's correct. The thing that has been always uppermost in my mind when I attacked things like mechanisms and so forth, was to deal with this in experiments that would mean something. I've read a lot of work on mechanism studies that leaves me cold. In other words, the work is done. It's well done. But then after you read it, you say, "Well, so what?"

Orchin: What do I have afterwards? After reading this beautiful work, how does it advance understanding?

Newman: Yes. And the thing that impressed me about that work in sulfuric acid was that for the first time you could measure the concentration of an ion accurately by this freezing point method. Once you could do that, you could draw a pretty valid conclusion as to what was present, just from the number of particles. And this was especially true in the benzoyl, benzoic acid type chemistry.

Now, at first you see, this was just a pure study in measuring the ions, measuring the lowering of the freezing point and deducing from that what must have been present. After that was done, you could say, "All right, if I have a certain ion present, what can I do with it?"

That then leads into chemistry. Here's the greater species, can I trap it by putting in a reagent or something like that? That led to the work of taking the sulfuric acid solution and doing something with it.

Orchin: Quenching for example.

Newman: Quenching. And that led, for example, to esterified mesitoic acid, which was the most significant thing at the time. Mesitoic acid was always taken as the classical example of an acid that you don't esterify by ordinary methods. Well, it turns out, if you use sulfuric acid, that goes rapidly whereas benzoic acid doesn't. And so it's a meshing together of the theory with a practical application.

Orchin: I wonder whether we don't do that sort of thing sub-sonsciously? In reviewing the work that you did in that whole area, it seemed to me that you were taking notions from physical chemistry and applying them to specific problems. It just seems to me that that expressed in many ways some of your best talents. For example, your selection of sulfuric acid. It was workable. The temperatures were in a more easily accessible range. The measurements were relatively easy. And you were ingeniously finding applications for the results--not simply recording the results, but recognizing that if the results were true, then they had implications. It seems to me that the problem and your talents were a perfect match.

Newman: Well, that might be, but as you say, I was always impressed by what you could do with something. Not just knowing facts, but how to use them. This also occurred with the benzoic acid type chemistry--in the so-called Hayashi rearrangement. Are you familiar with that?

Here essentially is the work we did. See, if you take a keto acid here, it's unsymmetrical. If you dissolve the starting acid with sulfuric acid, you go through these different ions. This is the cyclic ion. But if you heat it, it opens. Now, that opened ion gives what I call a spirocyclic mechanism. It goes on here and the other one opens. It just exchanges. When it opens, it gives the isomeric acid, number six.

Orchin: Yes, right, six and five.

Newman: They're isomeric and they rearrange through the medium of this spirocyclic ion. Now, this is a type of mechanism that forms a ring closure. Then, of course, if you heat this to a higher temperature, it forms a quinone--the opposite quinone from what you expect. So it's very useful. Now, in this synthesis, in these keto acids, remember that a big problem is to separate the isomers. So, take the mixture, dissolve it in sulfuric acid, heat it to a temperature which you have to determine by experiment, pour it on ice, and you will get the one isomer instead of the two.

Orchin: I think that's interesting because that article was published in 1972,* and we're talking about the sulfuric acid work you did in 1940.

Newman: Right.

Orchin: So I think it's fascinating that there's no end in chemistry.

Newman: There's no end. Unfortunately, this article, which I wrote for the Council of Chemical Research in 1972, has had almost no attention paid to it. This isn't my work. Most of this is the work of other people. The point is, this is a spirocyclic intermediate involving a carbonium ion of a keto type. The same type of thing has been done in indole chemistry with this compound number ten. If you cyclize that, you expect to get this hexahydroheterocyclic. Since it's symmetrical, you'd get only one compound. But if that's done with deuterium here--if it went this way--you'd expect to end up with the deuterium just here. Actually, the deuterium turns out fifty-fifty in both places, which means that you have to go through an akyl-type carbonium ion.

Orchin: Yes, just because of the spiral.

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*Melvin S. Newman, "Spirocyclic Intermediates in Organic Reactions," Accounts of Chemical Research, 5 (1972): 354-60.

Newman: If you treat this compound, you go through an heterocyclic intermediate G. Again, the numbers are involved. There's five atoms here, and three. I call that a 5-3 heterocyclic intermediate, in which this side is positively charged and that one negatively. It's still a carbonium ion rearrangement. But the point is that all of these reactions were discovered because people got different compounds than they expected. I claim that if you know there is such a thing as spirocyclic mechanism, that's what you'll expect. It's not the wrong thing.

Now, with carbene ions you have something called a Smiles rearrangement. Here, you take an orthohydroxy diarylsulfone. If you take that ion, that can go on this carbon, and you get a sulfinate ion. If it's unsymmetrical, you can get different products. So here's an example from the literature where you get a spirocyclic intermediate involving a carbene ion instead of a carbonium one, and there are quite a few examples of this. Here are some more examples of it. They are all taken from the literature. Then you go to free radicals. The same type of thing. I should say that the best example of spirocyclic carbonium is the Winstein phenyl ethyl thing.

Orchin: The Phenonium?

Newman: Yes. That's a spirocyclic. It's just one example. It's a 2,5 you see, two carbons. So that's already recognized. But it's just part of a generality.

With free radicals, you get the same type of thing. You get a radical spirocyclic intermediate. I have examples of that here. They always produce the unexpected product.

And then I have what we call no-mechanism reactions, where they're not any one of these three dipolar ionic intermediates. Here again are examples, all from the literature, where one side is a carbene ion and the other side is a carbonium ion. Again, these things always give unexpected compounds. I have forty-six references and my collection is not complete.

This is a rather short article. I noticed recently in an advanced organic book that there is no mention of this type of reaction. It seems to me, that this is the type of thing that should be pointed out in advanced organic chemistry. These things are possible.

Wotiz: And all this has its genesis in the work that started in the late thirties and early forties.

Newman: Right. That's where it started.

Wotiz: Professor Newman has been pointing out some reactions described in an article that he wrote, "Spirocyclic Intermediates in Organic Reactions." It appeared in 1972 in <u>Accounts of Chemical Research</u>, volume 5, pages 354 through 360.

Newman: That's what I mean about the teaching of chemistry. Instead of requiring that a student know umpteen methods of making a ketone, let the general principles be foremost.

Wotiz: But look, Mel, historically speaking, we went through that stage. What was once seen as unrelated reaction, which we now know goes through a carbonium ion intermediate, with all the rearrangement and all the side reaction--that was a forest that was impenetrable for part of the time.

Newman: It's a unifying concept, that's what I mean. Not just one or two examples.

Wotiz: Yes, it's something that obviously will work its way into the textbooks in due time. I think carbonium ion chemistry, which is very very elementary to us now, took about ten, fifteen years before it became widely accepted and was put into the textbooks.

Newman: That's right. When Whitmore first started talking about his work, he called them "hot carbons." He had slides made with a red carbon that we now call carbenium. He never would call them ions, you see, because in those days, ions were--not. But that was the beginning, really, of carbenium chemistry.

Wotiz: Wasn't there a German fellow who preceeded him and should be getting...

Orchin: Wagner?

Wotiz: No.

Orchin: Meerwein-Wagner requirements?

Wotiz: Meerwein, yes. They had the concept of electron deficient carbon atoms.

Newman: It's hard to really decide where an idea starts, because if you look in the older literature, everything's been said. The real credit comes to the person who points out that of all the things that have been said, this is right and those are wrong.

Wotiz: Here's a philosophical and tangential question. In your opinion, what is more important, meticulous laboratory work that can demonstrate something, or the work of a man who can relate what looks unrelatable based on somebody else's laboratory work?

Newman: They're both important. As an example, Barton was given the Nobel Prize mainly on his cyclohexy-type isomerium, chair and boat type form. He did his work in the late forties, early fifties. In 1922, in an article in Zeitschrift fur Physikalische Chemie, which no organic chemist would read because not only is it physical chemistry but it's in German, there was a man who studied some electrical conductivities in solutions of boric acid with different diols.* He took the cyclohexane cis and trans diols and one of them expanded the conductivity and the other resisted. Right in that article in 1922, he shows the conformation. In 1922! Right in black and white!

Furthermore, somebody else at that time wrote Newman Projections explaining another facet of this chemistry. I think it was with the 2-3 dimethyl butane diols, conductivity. He showed that if you looked at it from the end, the two methyl groups would be close to each other, and in one conformation and not in the other. In 1922!

Wotiz: Since you mentioned the Newman Projections, this might be a good time to pursue this a little bit more. How did you start thinking along these lines? Obviously, you were not influenced by that publication. That must have come to your attention at a later time.

Newman: From my interest in teaching. I would try to teach some of the principles that Barton had come out with, by drawing on the blackboard. And I found that no matter how I drew it, if I looked at it long enough, it turned itself inside out. In other words, it's inherently difficult to represent a three-dimensional object on a flat surface. So I started thinking, how could I look at this molecule from a different angle and make it clearer?

I realized that the bond you don't care about is the bond between the two atoms. That doesn't change. Why not look at it right along that axis? So when I do it that way, and take models, it only takes a moment to see how clear it is. When I used the models with classes I'd get points over in ten minutes time that I'd previously taken a whole hour to do.

Wotiz: When was this done? When did you introduce it in your classwork?

Newman: In the early fifties.

Wotiz: Were you discussing this with some of your colleagues at the time, as an improvement in teaching methodology?

Newman: I think so. I remember, I gave a talk on it at--was it at Wayne State University where they used to have...?

Wotiz: Yes, "Frontiers in Chemistry."

Newman: That's the first time I talked about it in public,

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*P.H. Hermanns, "Uber die Reaktion einiger Glykole mit Aceton" Zeitschrift fur Physikalische Chemie, 113 (1924): 337-84.

at one of those lectures.* I had been teaching it in classes for a couple of years before then.

Wotiz: Well, two years is a pretty long period. Weren't you impressed well enough to rush off to publication? Why the delay of two years?

Newman: I can't say if it were two years or two months. I know I tried it out in classes first to see how it would go over, and that takes some time. The students reacted very favorably to it. As a matter of fact, I published a paper in the Journal of Chemical Education in which I even wrote a steroid by Newman Projection.**

You've never seen a steroid look anything like this. If you take napthalene, hydrogenated naphthalene, you have a cis-trans ring, and as you can see, one is flat and one is bent. But you really see the bending much better if you look at it by Newman Projection. And yet people don't use this for napthalene compounds or steroids. They only use it for cyclohexanes and aliphatic things. The reason is that the person who gets expert in this field of research gets so used to doing it in a certain way that he doesn't want to change it. You usually don't teach all these fine points of steroid chemistry in a course.

Wotiz: Newman projections were used in your book on steric effects.***

Newman: Yes. That appeared in 1956. So all this stuff was stewing in the early fifties.

Wotiz: I see. How was it established, through your publication in the Journal of Chemical Education, or did it require the book?

Newman: It took time, because more and more people used it to explain the results of physical organic stuff--rate measurements and things like that. I was asked at one time, didn't I want to put out a more recent edition? My answer was no. Frankly, I just didn't want to do it any more. That book contained the principles involved. When applied to more and more fields, it just provides examples of the same thing. I

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*The lecture occurred at Wayne State University on March 17, 1952. Using his notes, Newman prepared an article, "A Useful Notation for Visualizing Certain Stereospecific Reactions," Record for Chemical Progress, 13 (1952): 111-16.

**Melvin S. Newman, "A Notation for the Study fo Certain Stereochemical Problems," Journal of Chemical Education, 32 (1955): 344-47.

***Melvin S. Newman, ed., Steric Effects in Organic Chemistry, (New York: John Wiley and Sons, 1956).

said, "If anybody wants to produce a more recent edition he has my blessing." No one has.

Wotiz: Again, a question which is offered in retrospect. Does it elate you or disappoint you to be known for the Newman Projections more than anything else.

Newman: I think it's a miscarriage, because I myself do nothing with this. I only use it as a teaching device.

Wotiz: But you don't want to be known just for the Newman Projections, do you?

Newman: Frankly I don't care what I'm known for. I'm always looking for something new, rather than at the past. I get more of a kick out of the new reaction I was talking to you about than the sulfuric acid work. It's the present that matters, not years ago.

Wotiz: Who coined the phrase "Newman Projections"? I know you did not use it in your writings.

Newman: It was in the book, wasn't it? Weren't they called Newman Projections? Eliel wrote the chapter, didn't he?

Wotiz: Dauben and Pitzer use the term in the first chapter. So I guess they must have been the first ones to use it.

Orchin: You know, John, I gave a paper at the symposium that you talked about--the one where that citation was first used. In my paper I showed that the Newman Projection displays the symmetry of ethane and its derivatives. I spent about thirty minutes explaining why I thought that this had acquired such great popularity. And I think that I mentioned that Mel had created a situation analogous to that found in the graphic art of Escher and its relationship to crystallography. Escher was a great graphic artist who conveyed the dimension of depth on a plane surface. I think that he probably used all the crystallographic space groups that are known. Crystallographers have analyzed his drawings in terms of what space groups they represent.

I think that sometimes people can see things in a way and use them without quite recognizing that that method of representation is an expression of some kind of fundamental representative art, and in this case, that key to me was a very simple concept. Two dimensional drawings are useless for chemical representations without a convention for knowing what groups are in front and what groups are behind. What Mel did was to use a plane projection that allowed the viewer to know which groups were in front and which groups were behind. That unlocked the stereochemistry on a plane surface of a projection formula. And that's the trick to doing a representation on a two dimensional plane. We have all kinds of devices for doing that, but those are perspective drawings. You get the plane projection formula and still retain the knowledge of what groups are in front and what groups are in back.

Wotiz: I have a related question. I always admire a chemist, a stereochemist in particular, who can take any molecule and just write it three dimensionally on the blackboard. Can you take it and write it really just as it exists?

Newman: It depends upon how complicated it is. I have difficulty doing it. Something in my education that might have led to this. When I was in high school I had a course in mechanical drawing. Have you ever had a course in mechanical drawing?

Wotiz: Yes.

Newman: They teach you how to do a three dimensional working drawing of a piece of metal with a hole bored at an angle. You look at it at one angle, then at a right angle, and then from the top. In other words, you look at it three ways.

Wotiz: It's strictly geometry.

Newman: That was what I did. I looked at this molecule from one of those directions, and it turned out that the preferred direction from which to look at it was along the axis of the two atoms.

Wotiz: If you were to write an updated edition of your book, would you retain all of the chapters and just bring it up to date or would you delete certain chapters and add new ones

Newman: I haven't thought about it that much because I just decided I wasn't going to do it. I've written two books in my life. One is this thing, because I thought there was a need for it, and the other one is a lab manual, which is absolutely unique but not used at all.

Wotiz: I'm not even familiar with the lab manual. Which one is that?

Newman: Well, it has what I call my philosophy of lab teaching. We've been doing that at Ohio State now for eleven or twelve years. Sophomores can take the lab course. We have what we call an optional course, and they use my text more or less, although a lot of the experiments have been changed. The students are given lectures on laboratory techniques, that is, different ways of doing things, and the advantages and disadvantages of each way. They are then given objectives and have to fulfill the objectives in their own way. In other words, there isn't an experiment in this course where they do cookbook stuff. Furthermore, rather than getting the usual seventy percent yield, they have to account for a minimum of ninety percent of the products in every experiment. This is research.

Wotiz: How long has this been going on?

Newman: Oh, seven or eight years. It's an advanced organic laboratory text.* I originally wrote it for the graduate students.

Wotiz: Who's the publisher?

Newman: Macmillan.

Newman: The beauty of this approach, I think, is that right from the beginning, it's done with research philosophy. A guy has to ask, "Well, how long should I heat this? What solvent should I use?" We never give him the answer. So, he has to ask, "What's the purpose of the solvent?" He has to realize that the solvent affects concentration and temperature, and that polarity may affect an intermediate.

Wotiz: Why do you think this approach has not become popular?

Newman: It takes too much time. A student here has his own lab space. And he's told, if you take this course, which is optional, you must spend a minimum of fifteen hours a week on it. An ordinary lab course takes only nine hours. Furthermore, a student gets no extra credit for the extra time spent. Well, a lot of students spend more than fifteen hours in the lab. Our students also work in pairs. In other words, we encourage students to compare their results.

Wotiz: In other words, both are doing the same...

Newman: The same experiment.

Wotiz: Do both have to agree on the procedure of the experiment, or do they have free choice?

Newman: They do the same experiment except for one variable. They'll do the experiment and they'll find out what the effect of that variable is. This is what I call a research atmosphere. If you give them directions, I don't care how good they are, you're training them to be like technicians.

Orchin: We would like to discuss in some detail the book that you edited. Did you write one chapter in it?

Newman: Two chapters.

Orchin: Two chapters in that book Steric Effects in Organic Chemistry, as it was called.** I think recognizing the

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*Melvin S. Newman, An Advanced Organic Laboratory Course, (New York: Macmillan Co., 1972).

**See note on page 3.

importance of steric effects in all kinds of reactions was one of its major contributions.

Now, when we get to the work that you did in your own laboratory, with respect to steric effects, certainly some of the most interesting work was what you did on what you called overcrowded molecules. I think we touched on that a little bit when we spoke about the synthesis of the dimethylbenzanthracenes and in the benzpyrines. I wonder if you'd want to say a little bit about bent rings, bent methyl groups, and the whole introduction of a new kind of optical activity in hydrocarbons.

Newman: Well, that arose from the argument about where the methyl groups were, how they were bent. I thought, well, instead of having methyl groups, why not have another fused ring? And I did so because the carbon next to the previous ring is the spatial equivalent of a methyl group, where the carbon-hydrogen bond is in the plane of the other ring. In other words, a methyl group has free rotation, but another fused ring doesn't have free rotation. It's frozen in that conformation, in which the CH bond is in the same plane as the other ring. And if you make a molecule like that, you're talking about where the methyl groups are, because the whole ring is involved.

It turns out, if you do that with dimethylphenanthrene, you get to a five metal ring. But, from work we had done on resolution of such compounds, it turns out that at room temperature the racemization is so rapid that you really can't study the thing. So the answer is to put another ring on. Instead of five rings, you have six.

Orchin: How did you know you were getting racemization?

Newman: Because we did compounds with two methyl groups, and if we worked quickly we could see rotation. Twenty minutes later the rotation was gone; so obviously it was racemizing in the machine.

Wotiz: But this was a hydrocarbon, wasn't it? Why would it racemize? What would be the mechanism of racemization?

Newman: Well, consider 4,5-dimethylphenanthrene and an acid group someplace else on it for resolution purposes. Now, when you resolve it, let's say this methyl group is up and that one is down, they flop at room temperature. You have to work quickly, see, and if you work quickly enough you can see a rotation.

Wotiz: The energy barrier was there.

Newman: Well, instead of methyl groups, if you have a ring, that's five rings, it turns out. I said, well, to make it better, let's put six rings, and that led to the concept of hexahelicene, and when that compound was made that turned out to be optically stable. As a matter of fact, when we heated that compound to racemize it, the molecule decomposed.

Orchin: Say a little bit about the method of racemization. You had to worry about that too because there weren't too many hydrocarbons that could be derivatized in a way that would allow separation of the diastereomers.

Newman: The argument there was that we couldn't have an acid group or an alcohol group attach to an optically active hydrocarbon where the concept was to make an optically active complexing agent. That is derived from your studies. You were the first to make tetranitro-fluorenone, a good complexing agent. But that's symmetrical, so we made a compound with a bromine which is unsymmetrical, and we put a side chain on it to get it resolved. So we had an optically active resolving agent, which owed its optical activity to an asymmetrical carbon, so that it wouldn't racemize. Now, this thing has a different rate of formation of a complex with a D and L isomer, and so, by using that concept, we could resolve it.

Orchin: You crystallized it through its diastereomers.

Newman: In that case, no. We couldn't get a complex. But what we could do was to arrange the concentration of the medium so that the complex, even though it wasn't crystalline, could be separated. This complex, which wasn't good, was filtered off, and what was left had an unequal amount of the D and L.

Orchin: I see, you get partial...

Newman: Partial separation. Then you could crystallize them. And they were stable enough, with benzene added, so you wouldn't get racemization. To racemize these things, you have to heat them up to about three hundred degrees.

Orchin: You're talking about racemization of the hydrocarbons.

Newman: Yes, the hydrocarbons.

Orchin: Not the complexes.

Newman: Not the complexes.

Orchin: This work that you just described, was this a Ph.D. thesis?

Newman: Yes.

Orchin: Whose work?

Newman: Lutz.* And for that, for example, we took napthyl-secondary-butyl ether, which has an asymmetrical carbon. You try to resolve that by some method. You treat it with this reagent, and you could get it in half an hour, because it so happens that a solid complex crystallizes out immediately. You filter it off, decompose it, and get naphthyl ether with a good high rotation.

Wotiz: Did you eventually fish out both modifications of hexahelicene or just one?

Newman: We fished out just one.

Orchin: You never had both pure enantiomers.

Newman: I think we did. I think we got them both, because we'd take the optical isomer of the resolving agent to get the other one. I'm pretty sure we got them both out. One was something like plus three thousand five hundred; the other one was minus three thousand six hundred--something like that. They had a very high rotation. That surprised me at the time. I knew so little about rotation. The reason why they had a very high rotation is interesting. With most asymmetric compounds, say secondary butyl acetate, you have all kinds of conformations in solution. What you're measuring is the average of all the rotations. Suppose, however, that you froze it in one conformation. The rotation would probably be very much higher, because you're not averaging it out. Now, with these polycyclic hydrocarbons...

Orchin: The average.

Newman: Yes, yes.

Orchin: Because some of them may rotate the other way.

Newman: Yes. And if you had it in one form, you might have one form that had a rotation of plus forty, and the other one of minus thirty-six. What then is your average. Two. With the hydrocarbons, however, they're so fixed that they only have one conformation, and that's part of the reason the rotation is so high. You're only measuring one conformation. You see, they come as close as they can approach each other. They don't go further because that would take more energy. So you really have just one conformation. A little vibration.

Orchin: When you take the optical rotation, what solvent do you use? They're not soluble, are they?

Newman: They were not very soluble.

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*Wilson B. Lutz, "A Reagent for the Optical Resolution of Aromatic Compounds by Means of Molecular Complex Formation," (Ph.D. dissertation, The Ohio State University, 1955.)

Orchin: The rotation was so high.

Newman: That's right, you had to have a dilute solution, the rotation was so high. But the point is, in solution you really essentially have only one conformation.

Orchin: You may have one conformation but there are many orientations. The light is striking in a multitude of orientations.

Newman: That might not be, that's right.

Orchin: I am thinking again of the term "overcrowded molecules" which at one time was more popular than it is now. I don't see the term used in today's literature.

Newman: People have a short memory. But I'll tell you one thing that we're doing now that's very interesting. I thought about it for years, but I never could think of a way to accomplish it, so I never did anything about it. And that's this. If you take a molecule like benzene, and you want to measure the rate of reaction at any one position, you find, because it's symmetrical, that you get the same result at each spot.

Well, a man like Dewar was interested in phenanthrene. He wondered, "What are the relative reactivities at the five different spots of phenanthrene?" What he did was to brominate phenanthrene and to isolate the five compounds--an extremely difficult task. And from the amounts of each he said, "That's the relative rate of reaction at each position." Well, that's pretty crude.

I happened also to read some papers by an Englishman, Taylor, who was doing the most beautiful work on aromatic electrophilic substitution that's ever been done.* He took a compound like 1-bromonapthalene and 2-bromonaphthalene and replaced the bromine with tritium via an organometallic. He then had pure 1-tritio and pure 2-tritio. He found that every time you treated this with trifluoroacetic acid, a proton went on a carbon with a tritium. That's the intermediate. The tritium then went off, so that when he measured the rate of loss of tritium, he was measuring the rate of electrophilic substitution at that position, and at that position only. So he took the two compounds and got the relative rate, alpha and beta, in naphthalene. And he did this very accurately.

Orchin: What did he treat it with, sulfuric acid?

Newman: No, trifluoroacetic acid, at a certain temperature.

Orchin: Oh that's right, so it's just tritium-proton exchange.

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*See, for example, Roger Taylor and Jean LeGuen, "Electrophilic Aromatic Substitution, XV," Journal of the Chemical Society: Perkins Transactions II, (1974): 1274-77.

Newman: Furthermore, the intermediate is as symmetrical as you can get and is subject to as little steric effect as you can get. You take bromine. You can argue that the rate of disposition is affected by the size of bromine. You can't do that so well. You can't get anything smaller than hydrogen.

Well I read his work and I immediately wrote him a letter. I said, "We would like to make some bromo compounds of, let's take the first example, 4,5-dimethylphenanthrene, and put a bromine in the nine position and then take 2,7-dimethylphenanthrene, which has the methyl groups in equivalent positions, except that there's no steric factor, and see the difference in rate of reaction at the 9 positions of these 2 things."

He said, "Fine."

We did. You'd be surprised how much more reactive the hindered compound is than the nonhindered. What we want to do now is take benzanthracene, which has twelve positions.

Orchin: Why is it such a big surprise, Mel?

Newman: It was to me.

Orchin: The electrophile has to come in, you know, perpendicular to the plane of the ring.

Newman: Yes, but the ring is flat in one case, not flat in the other. So I thought it would be faster because the ring isn't flat.

Well, I should say it was more surprising to him than it was to me. I expected it to be faster, but I couldn't tell you if it was five or a thousand. It turned out to be about four or five times faster.

Well, we've measured several compounds like that. But I want to go into the benzanthracene series now and use the 10-methylbenzanthracene and make all eleven monobromos and get their rates. Then I want to take the 9-methyl--which is methyl and yet the whole thing is warped--and see what effect the warping has at each position. Nothing like this has ever been done.

Wotiz: This is work that is presently going on in your laboratory?

Newman: I just got an NSF grant to do it.

Wotiz: This will be done with graduate students or postdoctoral students?

Newman: I don't have any graduate students, only postdoctoral. Making these bromo compounds is much tougher than making methyl or chlorine or fluorine compounds, but we'll have to try to do it.

Orchin: Why choose bromo?

Newman: He wants the bromine because of his organometallic methods of replacement. In other words, this is cooperative research.

Orchin: I see, you're just making it and sending it to him.

Newman: Yes.

Wotiz: Where in England is he located?

Newman: Sussex. He must have published twenty papers on this. It's so interesting because it's easy to do the work. All somebody has to do is make the compound. And the beauty of it is, it gives you the rate at only that one position so that there's no complication. He gets the financing and he gets all these rates much better than Dewar did by just brominizing. All he does is measure the rates.

Orchin: Let's pick up some more on these steric effects. Mel, you were about to tell us a story about how facts interplay and about how, frequently, the total facts add up to much more than the actual sum.

Newman: Well, the sequence of events is very interesting to me. I'll start by saying that I was always interested in reading work on steric effects. H. C. Brown in this country has done a lot. One of the things he did was to make 2,6-di-t-butylpyridine. The t-butyl groups swept out a lot of area adjacent to the nitrogen, and he showed that it would form a hydrochloride but wouldn't form a boron trifluoride complex. The t-butyl groups were so large that they kept the BF from approaching the nitrogen. So I wanted to look into this a little closer. I first contacted him to find out that he wasn't doing it.

Wotiz: What year are you talking about now?

Newman: Later fifties, I would say. I wanted to see whether 4,5-dimethylacridine, using the same concept--you see, the ring is equivalent to a methyl group, ortho to pyridine, and then a methyl group in the ortho position--would make it the equivalent of a t-butyl group. So if you have 4,5-dimethylacridine, with methyl groups on either side of the nitrogen, that would be the equivalent of di-t-butylpyridine, except that the methyl group would be held in the plane and therefore would be smaller than the t-butyl group which can rotate.

I had a doctoral student named Warren Powell make this compound. It formed a hydrochloride, but it did not form BF. He mentioned that it was very surprising that when he took the melting point of this dimethylacridine, it wasn't acrid. You can tell that acridine is acrid if you take the melting point. With this dimethylacridine, however, there was absolutely no physiological effect. At the time I considered this to be a steric effect on a physiological problem.

Some years later, I served on an antimalarial committee at Bethesda, for the army. There I became interested in antimalarials. I looked into books about it. It turns out that there's an acridine that was used about twenty-five or thirty years ago because it was very effective against malaria that had to be discontinued because it produced toxic side effects.

Then these facts started coming together. I said, "Well, suppose I make the antimalarial that they had, and put two methyl groups ortho to the nitrogen. Would it leave the antimalarial feature intact and cut out the toxic effect?" That's what I'm working on in the lab now. I'm trying to synthesize this compound.

Now, we had this dimethylacridine tested for toxicity. It's not toxic. Acridine has its toxic level. Dimethylacridine is toxically inert. So it means at least as far as that toxic test goes, that dimethylacridine is inactive. Now, if this dimethylacridine that's used for malaria is nontoxic, this will be revolutionary.

Wotiz: Well, it will not be the first example where a particular steric isomer or particular compound was active or not active just because of its stereochemistry.

Newman: It shows again the possible utility of a finding in pure chemistry. After all, this di-t-butylpyridine, BF certainly has no practical application that you can think of. It's a point of theoretical interest. Boron fluoride won't form a salt and HCl will. Yet, in connotation with something else, it might lead to the answer of toxicity.

Wotiz: I thought Brown's strain, S strain and I strain, had some practical connotations and that this would get him the Nobel Prize; but it never did.

Orchin: Would you tell us about your success in getting funding for research.

Newman: Certainly. Well, I've had a pretty good record. I would guess that I've had eighty percent of my proposals accepted, although in recent years the rate has not been that high for a number of reasons.

Wotiz: I think that's an outstanding record. I mean, over the years, eighty percent. Would you know how many proposals you have written?

Newman: Say, between fifty and one hundred.

Wotiz: And eighty percent accepted. That's certainly worth recognizing. Well, I think that one out of three is the norm.

Newman: The relative numbers are occasioned, I think, by a large number of people who just don't know how to write a decent proposal.

Wotiz: What, in your opinion, makes a proposal successful?

Newman: Well, a lot depends upon the individual. I might, for example, make quite clear just what it is I'm trying to do and how I propose to do it, but be remiss in saying why I think it's important that it should be done, largely because it's never been required. In the past, the referees who have evaluated proposals have been more or less satisfied with a clear statement of what you want to do and how you propose to do it.

Wotiz: Do you also include proposals or requests for funding from industry in your eighty percent?

Newman: No. I have almost never applied to industry, except in the early days at Upjohn. The department chairmen have gotten grants from Du Pont, but these grants never went to an individual. During the war we did some cooperative work with Goodrich. When the war was over, they asked me if I would do some work on a topic of interest to them. I said no. But I asked if they would consider funding a proposal for something I wanted to do? I sent them the proposal and they turned it down. There were some personal factors involved there. The guy I had been dealing with had become angry when I said I didn't want to do their work. After that I couldn't get a penny from him.

Wotiz: Were you ever a consultant to various industries and government agencies? Do you recall all the consultantships that you had?

Newman: Well, let's see. I consulted with Continental Oil Company.

Wotiz: The first one was Upjohn.

Newman: But now in terms of time, I can't be sure. Continental Oil, then Diamond Alkali and International Flavors and Fragrances. I think that's all.

Wotiz: How about some panels of the National Academy of Sciences?

Newman: I'm on no panel of the National Academy. I have been on panels of the Research Corporation and the army and navy and NSF.

Wotiz: But you are a member of the National Academy of Sciences?

Newman: I am a member of it.

Wotiz: Since when, do you recall?

Newman: 1956.

Wotiz: And these other panels, the army, navy, and the Research Corporation; were these for specific jobs?

Newman: For a number of years, largely after the war. Actually, I found out when the war was over that I'd worked on the A-bomb.

Wotiz: Making some solid extraction chelating agent or something?

Newman: Well, I worked for Henne making some fluorinated trifluoroacetoacetic ester. We made quantitites of that. He never knew why. I said, "What do they want it for?"

He said, "I don't know. The contract calls for it."

After the war was over, we figured out that they were making things like uranium chelates to see if they could separate it that way.

Orchin: Did you ever know how toxic those were?

Newman: No.

Orchin: The trifluoroacetates are terribly, terribly potent.

Newman: I'm still alive.

Wotiz: When I was a student I worked on an army project making 2,4-D and 2,4,5-T. I was chlorinating the phenol, making phenoxyacetic acid. I must have made a lot of dioxin while doing that. I'm still alive, but I don't know for how long!

Newman: People who work in labs take precautions, not to get stuff all over them. They have enough sense.

Wotiz: Yes, but I remember when I was chlorinating that I had the odor of chlorinated phenols all around.

Newman: While working on one of the war projects, we got a big can full of these mixed DDT-type things. The army was going to take a big contract. Four different companies had to submit their crude material. Four different labs had to analyze the material--one for each company. We had that stuff all over the place. But we were damn careful about it.

Orchin: One of the things that probably distinguishes your work from the work of a lot of your peers, over your lifetime, is the variety of problems that you've worked at. True, these interests are retained, but nevertheless I think one would have a hard time, just looking at the titles of your works, trying to fit it into three or four or five categories.

Newman: Yes, I've been amazed at that too. I think the reason is that once I feel like I know fairly much about a reaction I don't continue to look into finer and finer details, which is a thing many people do. You know, you'll never know everything about any reaction. There is no such thing as a simple reaction, but there is a degree of approach towards the end. Rather than take ninety percent of my time trying to get the last ten percent, I'd rather go on to something that I know less about and push it up to the ninety percent point.

Wotiz: I'm very much interested in your saying this because I've heard evaluations of your work over the years by different people on different occasions. They seem to agree that it depends upon what interests you at the time, because you have so many different interests. You go from one area to another. There may be something tying it together, but to the uninitiated it seems as if you are hopping from one project to another.

Newman: Well, I think saying "hopping" is...

Wotiz: But if you look over the sequence of your publications, there very seldom is continuity. There are so many areas.

Orchin: How does that happen, Mel? You certainly don't sit down and say, "What am I going to think of that's new today?"

Newman: No. I think that when I've done a certain area and can't think of any experiment that looks like it's going to lead to real results, I quit.

Actually I put it out of my mind. Take the question of unsaturated carbonium ions. If you take a beta-hydroxy ester and make a hydrazide from that, and then you convert to an azide and heat it, it goes to an isocyanate. Now, if there's a beta-OH in there, it cyclizes and forms a cyclic urethane. These reactions go together with a very high yield. Now if you hydrolyze that cyclic urethane, you get an amino alcohol. That's a well known method of making amino alcohol.

Rather than hydrolyzing I took the urethane and put on an NO. Now, if you treat that with base, that's one of the ways of making diazomethane. I wanted to see what happened to these compounds. It turned out that the results at first were best explained by getting an unsaturated carbonium ion because the beta-hydroxy group is eliminated, and from your diazonium thing you get an unsaturated carbonium ion. This does various things, depending on what the R groups are originally. A whole raft of compounds are made, and as far as I know, this is really the first work on unsaturated carbonium ions other than adding an acid to an acetylene. After all, if you add a proton to an acetylene you have an unsaturated carbonium ion. But this is an entirely different route because it gives you these in an alkaline medium.

I published maybe six or eight papers on this,* until Hine wrote his book on physical organic.** Do you know Jack Hine's book?

Wotiz: Yes, a follow-up on Hammett's.***

Newman: Yes. In one of his chapters he talked about this reaction and he said, "You know, maybe these things don't go through unsaturated carbonium ions. Maybe they go through unsaturated carbenes."

Well, that's something I had never considered. In the first place, when I first started this work, nobody was working on carbenes. Of course Hine's work on carbenes came in beautifully. So I waited a couple of years. I didn't know Hine. He wasn't here. I figured, well, this guy must be doing something. I waited a couple of years and he never did anything with it.

So I thought, well, I'll get a student to work on it. Well, it turns out they can go by unsaturated carbenes and it's very closely related. Depending on the experimental conditions, it sometimes gives you the unsaturated carbene and at other times it gives you the unsaturated carbonium ion. There are a lot of useful syntheses that can be done from these things.

But once I found out the patterns, I stopped working in the field. I could have gotten more groups, but I could do that ad infinitum. I thought the work was up to the ninety percent stage. If somebody else gets interested in the last ten percent, let him do it; but I'm going to do something else. So I stopped working on it entirely.

Wotiz: How many groups, how many areas of research interest can you identify? Have you ever tried to summarize them?

Newman: I suppose quite a few.

Wotiz: Would it be more than a dozen well defined areas?

Newman: I had a lot of interest in vinylene carbonate. Do you remember anything about vinylene carbonate? You know what ethylene carbonate is?

Wotiz: Yes.

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*The original paper was, Melvin S. Newman and Roger Addor, "Vinylene Carbonate," Journal of the American Chemical Society, 75 (1953): 1263-64.

**Jack S. Hine, Physical Organic Chemistry, (New York: McGraw-Hill Book Company, Inc., 1956).

***Louis P. Hammett, Physical Organic Chemistry, (New York: McGraw-Hill Book Company, Inc., 1940).

Newman: This just has a double bond. When I first announced a paper on that, I gave a talk at an ACS meeting.* The place was absolutely jammed. You never saw so many people. The first thing I said was, "I'm not going to talk about polymerization because I haven't done any work with it." Immediately after I said that, about half of the people left. They rushed for the exit. Funniest thing I ever saw.

It's a very interesting compound because it does Diels-Alder reactions. I got into it because I was making a new aromatic ring. If you take tetralone and do a reduction on it, you get a pinacol type thing. If you dehydrate, you get a diene, right? And if you use vinylene carbonate on that diene, and hydrolyze off the carbonate, you have one double bond and two OHs. It splits off two waters and makes an aromatic ring. So, this was the answer to the question: "What dienophile could you choose that would lead to an aromatic substance?"

At first I looked in the literature to see how it was made. Well, it hadn't been made; so we made it.

Wotiz: Incidentally, how did you make it eventually?

Newman: We chlorinated the carbonate. Now here's a funny one. We took out a patent on how to make this compound and we were confronted by DuPont. They'd spend quite some time trying to do the same thing and hadn't made it. They went at it the same way we did. You chlorinate it you see.

Wotiz: You mean ethyl carbonate?

Newman: Ethylene carbonate.

Wotiz: You chlorinate ethyl?

Newman: No, not ethyl. Ethylene carbonate.

Wotiz: But I thought you are making ethylene carbonate?

Newman: No. No. We start out from CH CH . Take ethylene glycol and a saturated carbonate. Now you chlorinate that, just by passing chlorine in it.

Orchin: Irradiation?

Newman: Yes, you irradiate it and it chlorinates. You get monochlorethylene carbonate, which you distill. Now, to dehydrohalogenate that, we used triethyl. It worked pretty well. I think we got about sixty, seventy percent yield. We went up against Du Pont, and what did they use but trimethylamine?

Wotiz: And you used triethyl.

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*The meeting occurred in Chicago on September 8, 1953.

Newman: We used triethyl and got it. They claimed that they had fractionated their product. Everything was analyzed they said, except for a little forerun, a few drops. They claimed that that was the compound. It was the compound, but they never showed it. We got the patent. Isn't that amazing? Trimethylamine wouldn't do it?

Orchin: What was the major product that they got?

Newman: I don't know. I don't remember now what they got, but they didn't get vinylene carbonate.

Orchin: What do you mean by saying "We got a patent?" Who is "we"?

Newman: The OSU Research Foundation. Incidentally, this was a precedent-making thing, because I was the first to ask, "How does a person like myself get a patent in a university?"

They said, "You go through the Research Foundation." I went to them and said I'd like to patent.

After a while they reconsidered "We don't want to do it. We don't think it's worthwhile."

So then I said, "Well, will you sign a release to let me do it?" It turned out that there was no state law allowing this to be done.

Orchin: You mean to get the release?

Newman: The release, yes. The president of the university himself said, "I would be glad to sign it but it would have no legal validity."

I said, "Well, how about getting it legalized?"

They finally did. I think they put a measure into the legislature so that a person could get a patent at Ohio State.

Wotiz: This work was done at Ohio State with Ohio State money?

Newman: Strictly departmental money.

Wotiz: Who was the student?

Newman: Roger Addor. He went to work for American Cyanamid.

Wotiz: OK, go ahead. I just wanted to be sure.

Newman: OSU finally took out the patent to avoid this problem. After we got the patent we had quite a few letters about licensing; but it never amounted to anything.

If you want to be amused some time, look at <u>Chem Abstracts</u> under "Vinylene Carbonate" in certain years. There are actually pages and pages of references to this thing. There must have been eight companies that produced quantities of that stuff to find out its use in polymers. It's unusual because it is a polymer with oxygens on both carbons. The companies used these as copolymers with everything you could think of, and all kinds of patents were taken out for these polymers. But I don't think that any of the polymers achieved any industrial importance.

Wotiz: Maybe I'm missing something here. You got the patent on preparation of the monomer?

Newman: Yes.

Orchin: Was it very stable?

Newman: Well, yes and no. If it's really pure and you keep it in the ice box, it will last quite a while. But if it's not quite so pure and you leave it out in the room it will go to pot.

Orchin: What you mean by stable? Did you ever try to hydrolyze that off? What would you get if you hydrolyzed that like an ester?

Newman: Well, it would be an dienol. We never tried to do it.

Orchin: You know that compound has tremendous commercial interest.

Newman: We've used it for an anthracene, you see. We did it on benzanthracene too. Then if you cleave it you get a dialdehyde. We reduced it after we opened it and got a di CH OH. It's not nearly as good a maleic anhydride as a dieneophile. Yet, it has some properties.

Wotiz: But what about its properties as a monomer or as a polymer?

Newman: I never studied them.

Orchin: Apparently, some companies tried it and they didn't find anything unusual about it.

Newman: They found out all kinds of things, but nothing commercially useful. I claim that I probably cost American industry more money than any other person did. Everybody wanted to make some of it and examine its uses.

Wotiz: Ohio State has a policy as far as patents are concerned?

Newman: Yes. I don't know what the policy is, but they have a policy that's written into the law.

Wotiz: Is this the only example of any of your works that found it way into a patent? Can you think of anything else?

Newman: There were some others, but I'd have to look them up.

Wotiz: But you do have other patents?

Newman: I have four or five.

Wotiz: Didn't you patent the famous Newman Stopcock?

Newman: No. I talked to a lawyer and he didn't think it was patentable.

Wotiz: What have you patented besides a process for making a specific compound? What other patentable work can you recall? Or maybe we should ask that question later when you have a chance to look through your reprints? Are there any patents in this bunch?

Newman: Nothing important. I think the only thing I had any chance of financial success with is vinylene carbonate. At the time it was made you couldn't say that these polymers wouldn't be valuable.

Wotiz: What year are we talking about?

Newman: I guess it's the early fifties.

Wotiz: Was the student named as coauthor of the patent?

Newman: No. We sent it in that way originally, but by the time they started questioning, they said, "Whose idea was this?" It was my idea; all he did was execute it.

Wotiz: Following instructions.

Orchin: Where was the record of reduction-to-practice, in his notebook?

Newman: Yes.

Wotiz: Well, reduction-to-practice means a method of preparation.

Orchin: Yes, the actual record of the work. If it was in his notebook...

Newman: Well, anyway, that's the way it was done. He didn't object and I would have done anything they said. I didn't know enough to argue about it.

Newman: You were talking about teaching the beginning classes. Everybody talks about cis and trans olefins. I used to do it with butene. The last couple of years, however, I changed my approach completely. What a difference it made with the students! I asked them, "What do chemists do?" during one of the first three or four lectures in organic. I helped them to answer that question by saying that years ago people saw a certain plant in the East that exuded a milky-like liquid when it was cut. The liquid solidified into a rubbery-type substance. I then described natural rubber and said, "The chemists were interested in natural products." I asked, "What did they do?" And I answered, "Some heated it and got isoprene out of it." They realized that the substance must be a form of isoprene because when they heated it they got a low boiling material.

Some other chemists then said, "Well, suppose we start with this isoprene, can we get back to rubber?" They tried it and they got two different results. Some got a hard substance, gutta-percha. Others got a rubbery-like substance. So, they both started with the same substance and they produced two different polymers.

I then wrote them on the board and showed that the difference was cis and trans. So, the first time I mentioned cis and trans therefore, the students related it to rubber-like or gutta percha-like.

Wotiz: You end up with it, but you started with it.

Newman: I started with it as a teaching device. They'd sit on the edge of their seats to listen to this.

My point is that almost everything in chemistry has something you can relate it to that will interest a student who's not necessarily going to be a chemist. This is an interesting phenomenon.

Wotiz: You always had a reputation as an excellent man in the laboratory. Do you recall any student who was not properly influenced by your insistence on good laboratory technique and material balance and everything that went with it?

Newman: Well, I'd have to look over the list, but certainly there were some students that I never thought too highly of. I would say what I had to say, up to a certain point, and then I wouldn't say anymore. If they weren't doing it; they weren't doing it.

Wotiz: But as you know you have some colleges where students are usually sloppy because the man for whom they work does not point out their deficiencies.

Newman: Well, sloppiness and results are different things. One of the most productive students I ever had was also one of the sloppiest, a man named Harold Karnes. He works at Upjohn. You'd go to look at his lab, and there would be stuff all over the place. Yet, he got more work done than anyone else, and it was well done. When he got his final compounds, there they were, beautifully pure things with high yields.

Another fellow, named George, an Indian, came here as a post-doc from Gilman. He was the neatest person I ever saw. You could eat food off every lab desk. Yet he too was a highly productive guy. Karnes and George were two extremes.

So I think it's an individual thing.

Wotiz: Citations usually accompany honors or medals. Do you recall the wording in these citations, that is, what other people or what a committee considered your most valuable contribution?

Newman: No.

Wotiz: Well, how about the one awarded by the Division of Organic Chemistry in 1963. What does it say?

Newman: It doesn't say anything.

Wotiz: Let's see if we can find that citation. Yes, here it is.

Newman: "In recognition of his outstanding contributions to chemistry, in particular, his brilliant syntheses of a series of unusual polycyclic hydrocarbons, thereby demonstrating optical activity in overcrowded molecules." That was presented at St. Louis in '61.

Wotiz: Well, obviously a committee in 1961 reviewed your work up to that point, and I think it summarizes well your principal activity. Now, more than fifteen years later, would you add anything to this citation? What other fields of chemistry would you add to it now?

Newman: Well, I don't know, I may be unique, but personally I think all awards should be abolished. The personal satisfaction of doing good work is enough for me, and the fact that other people come around and present a medal after dinner is really of almost no consequence. My family likes it but I can't say that I do.

Wotiz: Aren't you unduly modest?

Newman: I don't call it modesty. It's a question of personal taste. Some people like things and some people don't, and the less attention paid to me for an achievement the better. It's enough for someone to say to me, "That's a nice reaction you did." I don't need to be given a medal. The everyday use of whatever I do is plenty reward.

Wotiz: Well, I still think that you are unduly modest. Being a university professor for so many years in organic chemistry in particular, you produced compounds. You also produced people. We've talked about compounds. Who in your opinion was the most satisfying student, both as a good chemist and a good human being?

Newman: Each person is an individual and trying to compare two different people is like an unduplicated experiment.

Wotiz: I understand, but which student gave you the greatest satisfaction? You mentioned that right now you're most interested in difluorination. You must find the challenges and the unexpected occurrences satisfying. Similarly, you must have had like reactions dealing with people.

Newman: Well, if I have, I haven't thought about it. Each person is an individual. I don't try to compare them.

Wotiz: How about the chemical contributions of some of your students? That must bring you some special satisfaction. Do you recall work that Harry Walborsky, for example, did with cyclopropane. Did you get great satisfaction from that.

Newman: Well, I can't say this is something that I get much satisfaction from. I don't think that way. If I see a nice piece of work, I think that it's great no matter who did it. If I have any influence on any people, it's fine, but I don't think about it. I feel pleasure. Pride is the wrong adjective for me. I can't truthfully say that I feel proud about anything. I can't think of anything I've ever done that made me feel proud. I don't mean I don't enjoy things and appreciate things but I'm just not proud about them. Maybe I don't know what "proud" means.

Wotiz: I recall you were telling me about John Schaefken, who went on a tangent for a year or so. Was this typical of you, giving so much rein to a student who wanted to pursue what looked to you like a dead end.

Newman: Well, he wanted to study the change of a pseudoester, and he was going to make it from benzyl benzoic acid, the l-menthol ester. Talking with him, I said, "Now, you should get two compounds because you're forming a new asymmetric center." He worked with it for a while and he got one pure compound but he couldn't get the other. I said, "Well, for your purpose, you don't need both; one is perfectly OK. We just want to get an idea of where the rate is."

"No," he said, "I want to get the other one."

I said, "Well, how much time do you want to spend doing it?"

He said, "I want to do it."

I said, "Well, I think you're making a mistake but I'm not going to forbid you to do it."

So he spent several months before he finally gave up on it. When he finally wrote his thesis he berated me for not stopping him. He said, "Why did you let me waste so much time?"

Wotiz: Looking back, he was one of your early students.

Newman: He was smart. They called him The Brain.

Wotiz: Did you change your attitude since that time?

Newman: No. It didn't bother me at all.

Wotiz: I know, but did it influence you because a student berated you in retrospect? It just might have been a friendly beratement. I know John.

Newman: It's the kind of thing that comes up all the time. I've talked with many students. I've said, "Look, you usually write an approach to a problem." Any chemist knows you can write more than one approach. I've said, "What you normally do is to write as many approaches as you can, and then you evaluate the approaches. You ask yourself, Which one am I going to try first? And you do this, because you have to start somewhere." I said, "You're very fortunate indeed if the first thing that you choose turns out to be the best. The odds are against it."

As a matter of fact, I talked about this with Woodward years later. I said, "You're one of the world's greatest synthetic chemists. When you want to make a compound, you think of every way you can, don't you, before you start?"

He said, "Yes, that's right. I think of as many things as I can."

I said, "Then you choose what you think has the best chance of working."

He said, "Yes."

I asked him, "Did you ever have the first thing you tried work?"

Without any hesitation he said, "Never."

Wotiz: Is doing this sort of thing something that experience has taught you to suggest to students? Or is it something that Anderson or some associate of yours, say Fieser, may have used?

Newman: I don't recall anybody ever saying that to me. It came as a result of my own experience.

Wotiz: Talking about influence, I'm quite sure that many colleagues will start a student right where a former one left off. No changes. Continue the work. The challenge is gone.

Newman: Right. The thing that disturbs me a lot about modern professors in universities is that you'll see an article by a star--you know, one professor, with six names associated with it--and you can tell from the nature of the work that the six were all graduate students or post-docs or some of both. This sort of thing is done because the professor wants to get his results out quickly. He's not thinking of the education of the people involved. He's thinking about how quickly he can get his results out. If you have six people working on something, you have to organize it, don't you? You say, "You do this. You do that. You do this." You can't leave it to six people to work independently.

I think this is a mistake in chemical education because they're being more or less told what to do. To me it seems that the best education is to leave the problem with the student.

You may tell him to think about it and to talk about it with you as much as you want; but he is going to have to make a decision about what he is going to do. I'm not going to tell him what to do.

Wotiz: Well, we can rationalize. We've all heard about "publish or perish," that is, produce lab results or perish. And one needs to do it in order to get funded. Obviously, it's wrong.

Newman: It's wrong as hell. Just think of what the guy is doing, parceling out work, saying: "Here, you work on this. Here you work on that."

Wotiz: Did you ever have any difficulty, having received a grant, where the final report was not exactly along the line that you outlined?

Newman: Never, because my reports were always very brief. I looked upon these reports as that which somebody, somewhere, had to make a bookkeeping entry about. I doubted if anybody ever read them. Now that I think about it, there was one time when I did encounter some difficulty. About six or eight years ago I was told that I wasn't going to get continued funding on a cancer grant. The grant involved two or three post-docs as well as myself. I phoned the appropriate person and said, "What's the problem here?"

He replied: "Well, the committee said, you didn't have enough publications on the grant."

Upon hearing that I really hit the ceiling. I said, "In the first place, a lot of this stuff is going to be published in the _Journal of Medicinal Chemistry_. It won't accept a paper without the test results. It sometimes takes over two years to get the test results. As for this particular endeavor, we will write two or three papers as soon as we get the results."

Well, to make a long story short I contacted some of the people who were doing the testing. They wrote letters and the grant was finally made. But that just shows you how numbers can be used mistakenly.

Wotiz: Well, this is one of those things. The granting officers are being supervised by a committee of Congress and they do count the number of publications. It's a foolish and shortsighted sort of a policy, but trying to eliminate it is a chicken and an egg type of problem because it keeps propagating itself.

Newman: Common sense is misleading, isn't it? The more the federal bureaucracy has control, the worse it's going to get.

Orchin: Mel, one of the things that you might talk about is your advocacy of the idea of the Rule of Six. Maybe you can say a few words about its utility. I mention this rule because I think it's the kind of thing that is characteristic of your work. You found a lot of interesting facts, tried to put them together in a way that's useful to other people, helped them to understand, and then predicted what they might get if they used these simple approaches to organic synthesis.

Newman: Well, your mention of the Rule of Six is interesting because it brings to mind the letters and telephone calls I received when I first had it published. In the original publication I stated that the Rule of Six is only useful when you want to make a rapid estimate.* The way to find out whether steric effects will be important is to make a set of models and to look at them. That includes not only the substance you're talking about but also the reagents that you're using.

The Rule of Six came about because we had the greatest number of relative rate constants for a number of aliphatic acids, and I asembled them all in a paper. I think there were twenty-five or thirty aliphatic acids whose rates had been measured. Early in the steric effects everybody was talking about alpha substitutions. If you had a substitution on the alpha carbon, that slowed the rate down. But in this paper, I pointed out that beta substitutions could be as important or even more important, depending on the proof. The way to estimate this was to count the carbonyl oxygen as one and then count around the chain. The hydrogen was in the sixth position. The more atoms there were in the sixth position, the more hindered the compound would be. I said that it was only a qualitative concept, and that when the sixth number became as high as nine or twelve, then you were in serious, serious trouble.

Well, I received letters from people saying that this is a very poor rule. They objected that I could not explain why this or that acid, which has the same six number, has so much difference in the rate.

My only answer was that this is a qualitative rule, and that when you get nine and twelve you're in trouble. Of course, for anything more than that you're in serious trouble. I always thought that it was a useful qualitative concept that enabled one to look at a formula and count when he didn't have models. Anybody who tries to apply it quantitatively is just not using it properly. It was never meant to be a quantitative tool.

The use of ketones has never been studied. Acids have only one chain that can be branched on, but nobody's ever done a systematic study of ketones. For example, is it best to have all your branching on one side or on both sides? This is the kind of thing that could be done, but nobody's seen fit to do it.

Wotiz: Are you game to investigate ketones now? Did you ever seriously consider investigating this topic?

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*Melvin Newman, "Some Observations Concerning Steric Factors," Journal of the American Chemical Society, 72 (1950): 4783-86

Newman: I never considered it because I didn't see the utility of doing so.

Wotiz: In other words, you proved the point and this would be belaboring it.

Newman: Yes. Some people would make fifteen or twenty ketones and measure the rates. I saw other things that were more important to do.

Wotiz: Even under adverse criticism, did you ever feel strong enough about this point to justify spending the time?

Newman: I thought there were more important things to do.

Wotiz: I think it's almost typical of you.

Newman: Look at some of the physical organic work that's published. The people who do it seem more interested in getting straight lines than in deciding what good it is. I'll give you a perfect example of that. You know this Hammett sigma-rho relationship is a beautiful concept. But if you're trying to determine the rho, you can do it with three or four compounds. You don't need twelve compounds. If you investigate twelve compounds, the curve fits better. But so what? The rho doesn't change much. And what does the rho mean anyway? What is the difference if it's 3.9 or 4.1?

Wotiz: You did some work on the Van't Hoff I factors, I believe. You had a postdoctoral student, Bob Taft, who went into the field and pursued it much further with sulfuric acids.

Newman: That's right.

Wotiz: How would you evaluate this particular aspect? He did come up with some additional information.

Newman: Well, he got some useful stuff out of it. I can't say that I remember now just what it was.

Wotiz: Neither do I.

Newman: He had a point, and it was useful--not on highly hindered compounds, but in the intermediate range. Since the highly hindered compounds wouldn't react, he never got to them. There was no point in doing so.

Wotiz: You wouldn't think however, that he was belaboring it.

Newman: He had a different aspect of it.

Wotiz: Are there any other publications you would like to comment on?

Newman: Here's one little point. There's not many publications on it, but it's a question of monoalkalation of a hydroquinone. Now, a hydroquinone has two OH groups on it. Suppose you want to make a monomethyl ether of it, which we wanted to do at one time.

I looked up the literature on it and was amazed to find out that nobody had a good preparation for the monomethyl ether. If you make the sodium salt, sodium hydroxide and methyl iodine, you get as much dimethyl as you do mono, even with one mole. The answer is that once you've gotten methoxy the other one can do it. As you start building up concentrations of mono, you get di, until you reach a point where you're destroying it as fast as you're building it. So nobody had a good preparation.

Well, I thought about it and realized that here's a situation where Hauser's principle of dianions ought to be tried. Nobody had done it. You know Hauser's dianions. He showed that you can make a second ion on anything that would make a mono ion, if you treated it with a strong enough base. So if you take hydroquinone and just add two equivalents of sodium hydroxide, you get bisanions. They are always more reactive than the mono.

Wotiz: The dianion is more reactive than the first substitution?

Newman: Yes, because it's a much stronger base. And so he did this. It turned out that the solubilities had something to do with it. But when you allow for that, you could get fine yields of monoalkylation products of a variety of things.

Wotiz: So, you used two equivalents rather than one equivalent?

Newman: Right. It's perfectly simple. And yet it hadn't been done.

Wotiz: Professor Newman is referring to a paper written by him and James Cella that was published in 1974 in the Journal of Organic Chemistry, volume 39, page 214-215.*

Newman: Science was fairly well along in 1970, and yet, up to that date, there was no decent method of making a monomethyl ether. Isn't that almost unbelievable?

Wotiz: All right, let's continue along the same line of thought. The same problem exists if you try to esterify a dibasic acid. Would you recommend going through the dianion instead of esterification, using alkali halide and the salt?

Newman: It depends upon how close the ions are to each other. You get complications with a disalt of an acid-cyclization

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*"Monoalkylation of Hydroquinone"

type thing. One has no influence on the other. Offhand, it doesn't look as good.

Wotiz: I see. But still in any dibasic acid, there is ten times the fourth or fifth power difference between the ionization constant of the mono and di. Or is it true only in the lower ones?

Newman: I don't know how it works out with the higher ones. But the principle is there, whether they're di or not. It wouldn't take that long to find out.

Wotiz: This paper I'm looking over, is this a Ph.D. thesis?

Newman: Yes.

Wotiz: A tangential sort of thing?

Newman: It was done while the student worked upon his dissertation. He had other problems he was working on.

Wotiz: This was not the main problem.

Newman: Not the only one, no. It didn't take him long to do it. For example, in the first experiment he got a sixty-two percent yield of the monomethoxic, which is far above what anybody else had ever done. And he only did one experiment with methyl iodide because he had other things to work on. We were more interested in bromo esters, for a certain reason. Nonetheless, he found out that he could get quite good yields of monoalkylation products by using this principle.

Wotiz: You published mostly in the Journal of the American Chemical Society and the Journal of Organic Chemistry. Did you ever have any papers that were rejected by the referees which eventually were published in some other journal and turned out to be outstanding?

Newman: No.

Wotiz: You had no problem with referees over the years?

Newman: Oh I've had problems with referees, but usually they were worked out.

Wotiz: Who did most of the writing of papers based on dissertations?

Newman: I've written all.

Wotiz: You've written all of them?

Newman: All of the papers. The reason is that I usually go through quite a job trying to get a good thesis written. It very often drives me to the limit of my patience. Then when it comes to writing a paper on it, I just arbitrarily decide that I am going to write it. I'm not going to go through the same thing again. I tell the student that if he does anything any place else he should write the paper, but that while it is fresh in my mind I want to get it done.

Wotiz: Are there any dissertations that never found their way into print?

Newman: A few. Almost without exception, however, there's been at least one publication for every person who got a Ph.D. And I think I've had something like maybe one hundred and twenty Ph.D.s. I don't think there are as many as five who did not publish.

Wotiz: One hundred and twenty students who got a doctorate. How about master's students?

Newman: Forty. Some of them got Ph.D.s.

Wotiz: How many students got one or more degrees from you?

Newman: That would add up to about one hundred and sixty.

Wotiz: Say one hundred and fifty students, but obviously there were many more students who started work but never finished. How many would be in this category?

Newman: I never counted.

Wotiz: That started and changed for one reason or another.

Newman: I gave some of them very good advice for which they thanked me later. It was something like this. They'd start out working for a master's degree and after a certain period of time I would evaluate them, also taking into account the current employment situation. When jobs were scarce I would say to myself that here is a person who has not performed in an outstanding manner. If he spends two or three more years pursuing graduate work and gets a doctoral degree, he's going to have a hell of a time getting a job. I'm going to advise him to stop now. I did that, face to face, exactly as I'm telling you.

The student would be a little bit miffed, but after a day or two we'd talk it over again. He would say, "Well, what advice would you give?"

Well, the advice that I gave was twofold, that he look into business administration and law. If you go into these areas with a background in chemistry you will be a valuable person.

Quite a few of these people did that and wrote letters afterwards saying, "I have a wonderful job and thank you for the advice."

Wotiz: How many started working with you and then dropped out?

Newman: Maybe twenty or thirty. Not very many.

Wotiz: Your record is far superior to any that I know. We all see so many students who start and then just disappear a while later. They sign up but never do any work. How many postdoctorates have worked with you?

Newman: I'd say roughly a hundred.

Wotiz: A hundred postdoctorates. Would you be able to identify the source of your postdoctorate students? Was there something of a pipeline?

Newman: I have a list at the lab. It indicates from where the funds for most of them came.

Wotiz: Yes, I hope to get this. You can mail it to us. Was there any particular school or individual more than anybody else who was sending postdoctoral students to you?

Newman: No.

Wotiz: How did postdoctoral students reach you? Recognition? Recommendation?

Newman: By writing a letter.

Wotiz: Yes, but usually it's by recommendation of somebody. They never said, "I'm writing to you at the recommendation of Professor So And So"?

Newman: What I'm saying is, I can't remember any one particular person sending a series of students.

Wotiz: This means that you were widely recognized for your contributions. You must have influenced people and then they made recommendations. About how many foreign students did you have over the years?

Newman: I'd have to look over the list. I would say that lately I've had a large group of Indians. They started some years ago. I've had excellent people from different places in India. They kept on recommending very good students. The Indian post-docs that I've had were characterized by their excellence as lab workers and their very hard work. They have very little originality, however. Very few have really gone into the chemistry. They want to do the work, but they haven't originated ideas. There have been a few exceptions.

Wotiz: Indians were mostly interested in natural products?

Newman: They were interested in coming to this country and getting a job. A lot of them want to stay here. We can understand that, when we hear what they would go back to in India.

Wotiz: There's a Swami who is a senior professor at Madras. Was he sending his own students?

Newman: He's sent some. Not a great number, but some. This fellow George I mentioned.

Wotiz: In comparison to American students, how well were foreign students prepared, psychologically as well as professionally?

Newman: I had two Germans, one from Wittig and one from Huisgen, who came to work with me in the same year. Both were extremely highly recommended. I was very disappointed in both of them, however, because they didn't seem to have much originality. Technically they were expert.

Wotiz: These were postdoctoral students?

Newman: People who had Ph.D.s and wanted to come to this country. Each of them stayed for only one year. I don't know why they lacked originality. I think that it was because both of these people worked under men who had large numbers of people working for them, and who, I think, more or less organized their work for them. Huisgen had something like forty people working with him. Now, how can a person take care of forty people? He doesn't have time to sit down and talk with each one for any length of time.

Wotiz: This sounds a little contradictory. If he didn't have time then a student would have more chance to show his initiative.

Newman: But on the other hand, he could be the type that wrote out what he wanted this guy to do and what he wanted that guy to do, which I think is the way Huisgen operates.

Wotiz: Well, the relationship of student to professor in Europe in particular, is very different from here. It has been changing in recent years, but the professor is still God Almighty. When he says something, it goes.

Newman: At one time, Huisgen had almost all forty people working on the same project. This is not the best way to operate. I think Wittig may have done a lot of the same thing when he was working on carbenes.

Wotiz: You mentioned two German postdoctoral students.

Newman: I also had a number of English postdoctoral students.

Wotiz: That's what I'm leading up to. How do they compare?

Newman: Well, they both developed beautifully. Eglington's a prof now at Bristol, and Ian Scott is now in Texas. He's been all over the world. He was in Canada, at Yale, and now he's in Texas. Ian Scott is probably the one with the bigger reputation. He's done some magnificent work.

Wotiz: I forgot that Jeff Eglington was a post-doc.

Newman: Eglington and Scott both came from Glasgow.

Wotiz: And there's Sir Jones, E. R. H. Jones.

Newman: No, he was not at Glasgow. Rafiel and Barton were from Glasgow.

Wotiz: From Manchester. Jones was a student at Manchester and then he went to Oxford. How about your experience with Oriental students?

Newman: I've had some Taiwanese.

Wotiz: Did they have a language barrier?

Newman: Not particularly, no.

Wotiz: They received their degree in proportional times.

Newman: There was a fellow named Bill Hung who got his Ph.D. at the University of Massachusetts. He came to work with me and had a terrific stuttering problem. If I wanted to talk with him, I'd just have to wait until he'd get the words out. Before very long I said, "You ought to try and see if you can have something done about the stuttering."

To make a long story short, he went to the English department where they had specialists. He took some treatment there for about six months and improved terrifically. He could carry on a conversation. He was an outstanding chemist but his language was just terrible.

Wotiz: Was Hung Korean?

Newman: I don't know. I think he was from Taiwan.

Wotiz: Any Japanese students?

Newman: Yes. Two or three.

Wotiz: East Europeans?

Newman: I had one Hungarian. He never took a bath. You wouldn't want to work in the same lab with him. I think he lived in a house with twenty-four people. He had some horror stories.

Wotiz: He's back in Hungary? Did he come on some sort of official exchange agreement through the Academy of Sciences?

Newman: No, he just wrote for a postdoctoral fellowship and I'd never had anybody from there before. He had good recommendations. He was a good chemist.

Wotiz: Do you know his capacity back in Hungary?

Newman: No, I haven't heard from him since he left. His English wasn't very good, and as I say I wouldn't dare go home with him!

Wotiz: I want to be sure that I ask you--how's your health these days?

Newman: Well, it's been lousy this past week. I had a head cold, but other than that it's been fine.

Wotiz: Are there no ill effects from the accident that you had in 1978?

Newman: No. I lost my hearing in my right ear and that upset my sense of balance. I still feel dizzy when I walk. You know, sometimes when you stumble, you almost fall? Well, I do that. I have to watch it when I walk.

Wotiz: You lost your equilibrium because of damage in the inner ear.

Newman: Right.

Wotiz: But was this a result of the accident or was it the cause of your fall? It could have very well been, you know.

Newman: With all the examinations the doctors made, they couldn't find anything the matter with me, except that I had a concussion. I was resting after lunch in my office, lying down in an easy chair. Next thing I know, I'm in the hospital. The doctor said that the telephone probably rang, that I got up suddenly and the rush of blood to my head caused me to lose consciousness, and then I fell and struck my head. The doctor couldn't say for sure how this happened because nobody saw it. All we know is that they saw me lying on the floor.

Wotiz: But other than that you look to be in good health, vigorous and energetic.

Newman: But you know, I've lost my ability to gauge distances, and therefore my putting is just atrocious. Everything else is all right.

Wotiz: This is March 4, 1979. I'm in Professor Newman's office at Ohio State University. Professor Orchin who accompanied me previously is not with me.

Last evening when Milt Orchin and I were having dinner, we were rehashing the day's activities. We realized that there were a few questions that we should have asked you. One was suggested by your mentioning that you had the opportunity to become a research director for the Upjohn Company and that you declined this offer. We were wondering whether you had opportunities to become an administrator in a university, such as a department chairman or dean? And if so, what was your reaction? After all, you expressed frustration about the inability of bureaucrats to see that your work was properly taken care of. Do you think you would have done differently had you been an administrator and a researcher?

Newman: Well, I have some very definite ideas about department chairmen, deans, and presidents of universities, which I don't think will ever come to pass.

Wotiz: Would you mind telling us?

Newman: I'll say a word about it. From the earliest, when I was first approached to be considered as a department chairman, I thought about it and decided, no. I like research and teaching too much. People have often said, "Well, you don't have to give up teaching and research when you're chairman." But that runs counter to my thinking about the function of a chairman. I think if a chairman is going to be a chairman, he should be a chairman and nothing else.

Wotiz: Not delegate his responsibilities to an executive officer?

Newman: No, or to committees. I think committees kill everything. In my opinion, an executive--whether in a university or in business or any place--should execute. He can talk with people--and by talking with a sufficient number of people can get the pros and cons of any question--but he then has to make a decision.

Wotiz: Do you mean that anybody can become chairman? Would a chairman have to have a background in research to become an executive chairman?

Newman: Well, preferably I think a chairman should have a background in research and teaching, because these are the two things that a chemistry department does. I'm greatly against the present trend in American universities that permits a man to serve as department chairman for only four years. He might serve for four years, or sometimes for only three years, but the chairmanship rotates. We all have the same idea, "Well, I'm going to do this only for three years. Even though I don't really want to do it, I'll do it because it's my duty. Then I'll get back to teaching and research." That way, nothing gets done. The person wants to serve with as little bother as possible in his three or four years and then let the next guy shoulder the burden. I think it would be much better if we went back to the old system, where you get someone to be a chairman for twenty years.

Wotiz: Well, even in a rotating chairmanship arrangement, one can be considered for another term. That's not excluded.

Newman: But he usually doesn't want it. He usually says, "I'll do my four years and that's it." But I think that you can really make changes under a department chairman. I can truthfully say that I've noticed considerable differences between the way things are done at Ohio State now and the way they were done when I came here in '36. The times have certainly changed in chemistry and things should be done differently. But I don't like the way tenure is decided at Ohio State and at some other places I know of. To me, tenure is one of the most important decisions that can be made in the chemistry department, because the people you get determine the quality of the department--not the rules and regulations, the people. And if a faculty wants to approach a certain level of excellence, there is no way to improve itself except by getting better people. The present tenure system just seems to promote mediocrity.

Wotiz: I don't quite understand. You have probationary periods?

Newman: It's supposed to be five or seven years. But somebody has to make a decision that this man is not going to get tenure.

Wotiz: Well, isn't there a committee that looks into it?

Newman: I think it should be in the hands of the department chairman. He can talk to as many of the faculty as he wants to, and then act. Furthermore, his recommendation to the dean should not be rubber stamped, as it presently is. The dean should appoint perhaps three people who would review things so that the chairman would know that what he did would be subject to review. Now I won't mention any names, but I know of at least three cases at Ohio State where the chairman recommended tenure for a faculty member who was obviously mediocre, and the dean's committee never found this out. In other words, it never went beyond what the chairman said.

Wotiz: I'll play the role of devil's advocate here and ask this question. You say that the department chairman made a recommendation to the dean, but wasn't this done at the recommendation of some sort of tenure committee?

Newman: Let me tell you, the tenure committee--in the case I'm talking about--reported that there was something like an eleven to ten vote in favor of giving this particular man tenure. Now, when there are ten people in a department who don't think a man is qualified for tenure, you shouldn't even take a vote. Furthermore, in getting eleven people to vote for him, his backers sought out and secured the votes of three or four people who never voted on anything, that is, people who took no interest in the running of the department. They said, "We need your vote, give it to us." And that's the way this particular person was given tenure.

Wotiz: Don't you have a departmental operating paper where ground rules are spelled out? Here you go by majority rule. But if you had an operating paper you could very well say that a two-thirds vote was necessary for a favorable tenure vote. I don't think that putting tenure in the hands of a dean is wise.

Newman: I didn't say to put tenure in the hands of a dean. I said that the dean's office should let the chairman know that his opinion will not be the only thing the dean's office looks for. Currently, the department chairman need not tell the dean that there's any opposition. How then, under the present system, is the dean going to know this?

Wotiz: Well, that's the purpose of an operating paper. In my university all documents are passed on to the next review level. Our dean therefore knows how we arrived at our decision.

Newman: Some of these things don't come out in documents.

Wotiz: True. True.

Newman: I'm saying that if there is an opinion to the contrary the dean should know about it. I'm talking about a valid academic reason. For example, that the man is not at the top of his profession. I'm not talking about some trivial matter like the suits he wears or his language.

Wotiz: Then you favor a benevolent dictator in the department, a man who above all can feel out his faculty and come to some kind of consensus?

Newman: Yes, and I'd like a faculty member to get tenure if he has scientific honesty. In other words, get some individuals that you feel are honest even if you don't agree with them all the time. You feel that they approach problems honestly.

Wotiz: But again, people do change when they change positions. What guarantee do you have that a fellow won't turn out to be exactly contrary to what you thought he stood for?

Newman: I'll tell you why nothing gets done. Consider the field of general chemistry. I feel safe to say that general chemistry at Ohio State University is dominated by physical chemists, mainly because all the organic chemists are busy with the organic areas. Only one or two of the latter spend a certain amount of time in general chemistry, whereas eight or nine physical chemists do.

Now, I think that the type of general chemistry that they promote is just disgraceful. I think we could do a much better job. I have talked to chairmen and they agree with me. But they don't do anything about it, because they say, "Oh, it's going to be a big hassle, and I'm only in this for four years." So, they won't do a thing.

Wotiz: The point is however, that organic chemistry starts on a second year level. There are far more students taking organic chemistry than physical chemistry. Consequently, to keep busy, the physical chemists have to teach freshmen. The same is probably true for inorganic chemists.

Newman: The real problem is that we have too many physical chemists. There are as many of them as organic chemists. Their graduate load is nothing like that. The position load is nothing like that. And the physical chemists don't even publish in the Journal of the Americal Chemical Society. They publish in Physics and Chemical Physics. In other words, the field of physical chemistry has gotten so far away from chemistry that I think it's a crime that they have equal representation.

Wotiz: You don't have biochemistry in this department?

Newman: We have. It started about seven or eight years ago.

Wotiz: How many biochemists are there?

Newman: We have five.

Wotiz: You have very strong feelings, but has it ever reached a point where you said, "Now I will be a candidate and that's the way I will run it?"

Newman: No. As I say, pure selfishness. I'm not willing to give up my teaching and research in order to serve as chairman. I have my opinions, but I won't do it myself. I've been approached by other universities, but I don't encourage them the least bit. I say, "You're wasting your time."

Wotiz: You mention other universities. Were you approached to assume a position as professor or chairman or both?

Newman: Well, I had phone calls. "Would you be interested in being considered?" And my answer was "No." That stopped it right there.

Wotiz: In other words, once you crossed the Alleghenies, you found your home. You never were tempted to assume a position in some other university?

Newman: I was happy enough here doing what I wanted to do. My biggest temptation occurred when I was offered an endowed professorship in Texas.

Wotiz: You mean at Rice or at the University of Texas?

Newman: No, in the state of Texas, the Welsh professorship, which was very attractive not only because of the salary but also because of the research money. But I decided, I didn't want to move to Texas. I was perfectly happy here.

Wotiz: Was it any particular university in Texas?

Newman: I'd rather not mention it. It was confidential.

Wotiz: I see. Well, that was one of the items we were uncertain of--what your reaction would have been. Now I think we know it pretty well. We were also wondering about your main interests outside of chemistry and golf. Were you a member of any societies or clubs? What did you stand for?

Newman: Nothing. I paid attention to raising the children while they were still at home and I like music. Other than these things, however, I spent all my waking hours reading and doing chemistry.

Wotiz: We thought that maybe there's something about which we don't know. But, in other words, you lived chemistry.

Newman: Yes. At one time my wife wanted us to socialize more and I was agreeable. But after a year or two of going to cocktail parties, it didn't amount to much. You drink liquor and you talk, and everybody's screeching at the top of his lungs, and nobody's talking sense about anything. You get more and more under the influence of liquor and then go home. To me this was a complete waste of time.

Wotiz: Current events obviously make an impression, and with this present shortage of petroleum there are things that chemists can do. You must have an opinion of how you as a chemist could have educated the public better about alternatives. Did you do anything about it?

Newman: Well, my opinions about educating the public are very long range and won't help in the next few years. I think that the biggest fault in the teaching of chemistry in the universities in this country is that people are being taught to be university professors. In other words, this career is presented as being the most wonderful thing that could happen to one. If you analyze the contents of most courses, ninety-five percent of them are academically oriented.

I really don't hear anybody talking about any industrial processes in chemistry. I don't say that sort of activity is nonexistent. I say it's almost nonexistent. And I think that when a faculty member lectures to the students who are going to become professional chemists, he should stress the importance of research to the country and the world now. There are plenty of good scientific examples that illustrate scientific principles that are not put in our courses. A faculty should show the importance of research in every area of chemistry by the way it teaches.

Wotiz: Mel, I'm sure that there are chemistry courses that are more like general science courses, that is that are oriented to the nonscience majors. Did you ever teach a course like that?

Newman: No, but I talk to people who teach them. In such a course, teaching the importance of research is not a major objective. Let's put it another way. In my opinion, the best teaching for any student occurs when he is taught what isn't known, rather than what is known. That sounds a little bit funny.

Wotiz: In other words, you want to challenge the student.

Newman: I want to show the students that what they're learning is not the final word, but rather that there's a lot to be learned yet that can only be discovered by further research. At present, if you talk about organic and you talk about ten methods of making ketones you imply that these are the best methods there are in the world and that they're good for everything.

Wotiz: You know Bob West, at the University of Wisconsin?

Newman: Slightly.

Wotiz: Well, he has left research in order to popularize chemistry. He now teaches on the first year level. I wonder if you had any chance to get his ideas.

Newman: I haven't talked with him about that. What we're doing is teaching what we call the educated public. These are the people who ten, thirty, forty years from now are going to have opinions.

Wotiz: I certainly agree with almost everything that you say. Only one thing still bothers me though. It is that you never came to a point where you said, "By golly, if nobody else is going to do it, I'm going to do it." You had the opportunity.

Newman: I told you what I did personally about gifted students.

Wotiz: Yes. OK.

Newman: I did something about it. I didn't wait for a committee to say "We'll get you some money." I just did it. And I think that if people want to do something, they should just do it and complain about it only if they're stopped from doing what they want to do.

Wotiz: It's difficult to evaluate in a relatively short period of time how effective you may have been teaching this course. Obviously this would have been done at the expense of research work.

Newman: Well, you take the lab courses. By the way, I can show you the lab manual.*

Wotiz: Yes.

Newman: My whole philosophy, as I mentioned before, has been that in every experiment it is the student who decides what reagents to use, how long to heat them, at what temperature, and what solvent to use. The only thing I require of an experimenter is that he account in each experiment for a minimum of ninety percent of his material, not just sixty. So many organic courses say, "Well, it's an organic synthesis prep, we'll have them do this, and if the organic synthesis is seventy percent and they get seventy percent it's an A experiment." To me, that's teaching a person to be a technician.

I would ask, "What happens to the other thirty percent? Why is the yield only seventy percent? Can I do something to improve it?" That's the research aspect. But that's not in any lab course of which I know.

So I developed this lab course and I gave it first to entering graduate students. Now we also give it in the summer time to entering graduate students here. We say, "You come in the summer and take this advanced lab course, two quarters work in one quarter." In other words, they work in the lab all day long every day, and they take the lab course which gets them ready to do research.

After I'd done that for a number of years, I thought, why shouldn't such a course be given to undergraduates as their first organic course. Well, the answer is, we can't do it with too many students because there aren't that many who are interested. So we started this course on an optional basis. I got a grant from the NSF for one year because I got a post-doc who would have his lab space right in the lab so he was there all day long. Four students volunteered to take this course. That's the way it started.

We have a lab that has twenty-five lab benches in it. We need this much space because a guy needs to be able to come in at any time during the day, have his space readily available, and not have to put away all of his equipment because somebody else is going to use the same space. That's unrealistic as far as research goes.

Well, for the last three years there have been over forty people who indicated that they wanted to take the course. The person who's supervising the course has to

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*See note on page 38.

interview the applicants and decide which twenty-five may take the course. That shows you that it can be done.

Wotiz: What are the criteria for selection?

Newman: Well, If a guy is eager to take the course for one thing. And of course, the question arises, whether we should let an A student take it, even if he's not enthusiastic, or a C student who is.

Actually, one year I took, I think it was seven A students, seven B students, and seven C students. They got these grades in organic chemistry. The lab course is only given during the second quarter. The first quarter is a time for lecturing. Of the students who received grades of C, two of them got A's in the lab course and were among the best laboratory workers.

Wotiz: What's the Ohio State course for credit called, "Undergraduate Research?"

Newman: That can be taken with anybody. The only trouble with it is that a person is given work in a very narrow field and doesn't become expert in others. The whole point of this lab course is that they do photochemical and electrochemical experiments. They do experiments in liquid ammonia. They do distillation, crystallization, extraction, and chromatography with all the techniques described, and the good and weak points in each one are pointed out. Compounds are characterized by spectra. Now a person who goes through that has quite a background. If he does undergraduate research, he usually gets just one narrow field.

Wotiz: True, unless he is inquisitive.

Newman: Now, this is designed to be a cushion as far as time goes.

Wotiz: I must congratulate you on being able to convince your administration to provide the space and lab benches. That must have been a story in itself, I would think.

Newman: Right. Well, it was done and fortunately even though we've moved a couple of times, we're in a place where we have twenty-five benches, plus places for the graduate teaching assistants who inhabit the same lab. They're there all the time.

Wotiz: Is there anything else that you'd like to elaborate on?

Newman: The thing I really feel most strongly about is the teaching of laboratory chemistry. In this whole country, the teaching of chemistry is not geared enough to laboratories. It overstresses lectures. The interesting things intellectually are mechanisms and things like that, but when you come right down to it, when a person works on research he works in the laboratory. Chemistry is a laboratory thing, and therefore I think that our teaching overemphasizes its intellectual aspect and does not emphasis enough its physical aspect. A person needs manual dexterity to work in the lab. This is never mentioned. Why are some people better lab workers than others? Why are some people better tennis players than others? Because they have physical coordination.

Wotiz: Unfortunately we are living in a period where space is at a premium and the cost of laboratory work goes out of sight.

Newman: Let me tell you what I think about this. Why should everybody who takes chemistry take a laboratory course? If you go through the laboratories with less people, and you just ask most of the students what they're doing, they'll respond, "Experiment 23."

You ask, "What is that?"

They respond, "I don't know."

They're doing cookbook stuff and therefore not really benefitting from their activity. Look at the space used and the time and money that is expended for a lot of people who are only doing lab work because it's required. My solution to this unfortunate situation is very simple: make all laboratory work optional.

Right away a person will say, "Well, the college of pharmacology says that we have to have so many labs." Yet the only reason why so much lab work is required is because doing so has become a tradition. Now, if pharmacology says, we want to keep that tradition, that's all right with me. Consider, however, the following situation.

I once requested a meeting with about eight department chairmen, in the biological sciences, in agriculture, and things like that. I asked them, "Would you object if we made the laboratory part of chemistry optional, rather than required?"

At first everybody said, "No, it has to remain as it is." We then talked for about an hour, and at the end of that hour, the chairmen said, "We think you're right."

Changing course requirements in a university takes a lot of work though, doesn't it? And so nothing ever came of my proposal. I spoke to a dean about it, but it died there. Administrators have too many other things to do.

My point is that universities expend significant resources funding laboratory work for people who will not benefit from it or who will benefit only marginally. When you abolish a requirement it does not mean that you're abolishing laboratory work.

Wotiz: Are your courses structured so that laboratory work accounts for part of the course's credit?

Newman: Yes. In other words, the whole structure would have to be modified.

Wotiz: You would assign the lecture and the laboratory different course numbers.

Newman: In most courses students get credit for laboratory work and credits for attending lectures. One reason why I got everyone of the department chairmen over to my side was because I asked, "Are you satisfied with your present requirements?"

They said, "We don't have enough time to do what we want."

I said, "Well, suppose you took some of the lab work out."

Then they saw the light. They realized that if we did what I suggested, they could do more of the things that many of them thought were more important than organic lab work.

Wotiz: Speaking about the requirements of chemistry, you know that the American Chemical Society has a committee that approves certain programs in universities. They don't accredit the programs but they approve them. The ACS very clearly specifies how many hours ought to be spent in a laboratory.

Newman: These specifications refer to chem majors.

Wotiz: Yes, so that they may get an ACS degree.

Newman: I understand. I'm not saying to abolish lab work. I'm saying make it optional.

Wotiz: But then the student might not qualify for the ACS degree and as a consequence suffer when he applies to graduate school.

Newman: But you don't want a student of chemistry who doesn't do lab work in graduate school, do you?

Wotiz: What about the case of the late bloomer?

Newman: The late bloomer in any system always creates problems. He can always take a lab course a little bit later. If he's a late bloomer, he's going to take more time.

But I'm talking about the big schools. Do you realize that we have to hire about seventy new students each year at Ohio State. We don't turn out seventy Ph.D.s because we hire magina people because we need their bodies. Why do we need the bodies? Because lab teachers are needed, see? So we perpetuate two abuses. We spend too much money on almost worthless lab courses, and we award graduate teaching assistantships that encourage students--who shouldn't be encouraged--to do graduate work. That's fundamentally wrong because we cannot get seventy really qualified graduate teaching

teaching assistants a year.

Wotiz: Well, I hope that you can devote more time and attention to it and convince people. Are you aware of any school that presently operates in a way that you favor. Have you ever tried to make a survey?

Newman: I have communicated with people, but my memory is such that once I write a letter and answer it and put it in the files, it's out of my mind.

Wotiz: It's frequently easier to bring about a change by pointing out that it already works without detriment somewhere else, you know.

Newman: I know. I would guess that there are six or eight universities that have lab programs that operate as I would prefer ours to operate. They involve a small number of students who are really interested. At the undergraduate level, lab work is optional; but if students take lab, they have to agree to spend at least fifteen hours a week in the lab. They get no extra credit for the extra time. That means that only interested people take it.

Wotiz: Well, I think that the tutorial system of education is obviously far superior. But Ohio State is the epitome of the big state university where you're dealing with bodies.

Newman: That's right, but I just want to show you that it can be done.

Wotiz: Even at State.

Newman: It's no big thing. There's no reason why you can't get a small group of people to do anything you want to do. You just have to do it. The only thing I got from NSF was the one year grant that required that one post-doc be in the lab. After that, the department funded the program by simply requiring that one or two of of the graduate teaching assistants be assigned.

Wotiz: Doesn't Ohio State have a program--Honors Program or President's Scholars?

Newman: They have the Honors Program.

Wotiz: That would obviously be the vehicle to use. Students who qualify for an honors curriculum have the freedom to choose and structure their courses and their laboratory work.

Newman: The Honors Program here in chemistry has been almost dead for a number of years. People don't like to go through the extra paperwork. In other words, the students get their credits and they take exams, and if they want to do research, they do research. Nobody has enthusiastically supported the Honors Program in the chemistry department.

Wotiz: But the forty students who signed up for this lab course, aren't they the logical candidates for an Honors Program?

Newman: Well, a lot of them are premeds, surprisingly.

Wotiz: That doesn't exclude them from honors.

Newman: They say they want to take it because they think it's a great course, not because they're going to be chemists. We have a number of people who do undergraduate research, but they don't need to be in the Honors Program to do this. They just do it. With respect to laboratory work in other areas, people in physical chemistry said they were going to try this, and it was a complete flop. Nobody wanted to take it. This happened because nobody paid enough attention to the lab. They had a lab with poor equipment. They wouldn't repair it. The word got around. It has to be well done or it shouldn't be done at all.

Wotiz: Since you are now officially retired and you cannot take graduate students, would you be willing to make yourself available to supervise an undergraduate program, as you have described it?

Newman: I would be willing to talk with any department that was seriously thinking of instituting such a course. I would not be willing however, to spend two or three months elsewhere supervising it. My wife would not be interested in going somewhere else for two or three months and I wouldn't want to leave her for that length of time. Chemistry departments are much too slow. After all, look at the rate at which science is changing every five years. Yet, a lot of the courses that are given are almost the same as they were forty years ago.

Wotiz: That really surprises me. I always thought that a big university such as yours would be in the forefront of monitoring the current status and adapting to it.

Newman: No. It comes right down to the personnel involved. I don't care what the name of the university is, you've got to have the right people to do it.

Wotiz: True. The bureaucracy of handling so many people, however, is a challenge in its own right.

Newman: Let me tell you about an experience that I had about four or five years ago. I went to about seven universities in California, giving a talk on something I was doing in chemistry. I also said, "I would like to meet with the organic chemistry faculty and department chairmen to talk about laboratory instruction." We did in each place. I outlined to them what I've outlined to you, about the theory of giving a different course for a small number of people. At every place, except one, they agreed with me one hundred percent. There were some questions but there was no antagonism. I then asked each chairman privately if he would do anything about my proposal. The answer at every place was, no.

Why? The answer was, "Well it would take too much time from the research activities of the young people who were trying to get tenure."

I said, "Well, that's because you pressure them to publish. If you said to a fellow, You develop this course and that's going to count, he would do it because the young people are enthusiastic about this."

Wotiz: This is an awfully difficult decision, however, because we work within a system, and to change a system you really have to shake it up. In other words, you would have to change the requirements for tenure in order to give the young man a longer period of time.

Newman: In the first place, I think this five year and seven year stuff is for the birds. You can tell within two years whether a guy has the qualities you want. I think that requiring someone to publish six articles is a false measure of his abilities. It encourages the man to stress the number of his publications rather than their quality. Talk with a man day in, day out for two years, and there will be only one question that you really have to ask when pondering about whether to grant him tenure. It is, if some other university came and offered to hire him, would you fight to keep him? If the answer is no, I would say, don't give him tenure. If you say, "We sure will, we're going to try to keep that guy here," then you want to give him tenure. You don't need five or seven years therefore to decide that. So I think all this committee work and stuff is for the birds.

There's one joke I'd like to tell that is very illustrative. Two professors were arguing about giving exams, and one said, "You know, the most unpleasant task I have in teaching is to make exams. It takes me so much time and I don't like it."

The other fellow said, "Nonsense, I give them the same exam every year."

So the first professor asked, "Don't the students get onto you?"

The other professor responded, "No, I change the answers."

Wotiz: That's a good way to finish our discussion. Thank you so much.

INDEX

Zeitschrift fur Physikalische Chemie 33