

CHEMICAL HERITAGE FOUNDATION

HENRY TAUBE

Transcript of an Interview
Conducted by

Leon Gortler

On the way to Grand Central Terminal
New York, New York

on

19 March 1986

(With Subsequent Corrections and Additions)

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



HENRY TAUBE

1915 Born in Neudorf, Saskatchewan, Canada, on 30 November
1942 Became naturalized United States citizen
2005 Died in Stanford, California, on 16 November

Education

1935 B.S., University of Saskatchewan
1937 M.S., University of Saskatchewan
1940 Ph.D., chemistry, University of California, Berkeley

Professional Experience

1940 University of California, Berkeley
Instructor

1941-1944 Cornell University
Instructor

1944-1946 Assistant Professor

1946-1948 University of Chicago
Assistant Professor

1948-1953 Associate Professor

1953-1961 Professor

1956-1959 Chairman

1961-1986 Stanford University
Professor

1972-1974 Chairman

1978-1979 Chairman

1986 Professor Emeritus

Honors and Awards

1949 Guggenheim Fellow
1955 Guggenheim Fellow
1955 American Chemical Society Award for Nuclear Applications in
Chemistry

1960 Harrison Howe Award, Rochester Section, American Chemical Society
1964 Chandler Medal, Columbia University
1966 John Gamble Kirkwood Award, New Haven Section, American Chemical

Society

- 1967 American Chemical Society Award for Distinguished Service in the Advancement of Inorganic Chemistry
- 1971 Nichols Medal, New York, American Chemical Society
- 1971 Willard Gibbs Medal, Chicago Section, American Chemical Society
- 1973 F.P. Dwyer Medal, University of New South Wales, Australia
- 1973 Honorary Doctorate, (L.L.D.) University of Saskatchewan
- 1976 Marguerite Blake Wilbur Endowed Professorship
- 1977 National Medal of Science, Washington, D.C.
- 1979 Allied Chemical Award for Excellence in Graduate Teaching & Innovative Science
- 1979 Degree of Ph.D. *Honoris Causa* of the Hebrew University of Jerusalem
- 1980 T.W. Richards Medal of the Northeastern Section, American Chemical Society
- 1981 American Chemical Society Award in Inorganic Chemistry of the Monsanto Company
- 1981 The Linus Pauling Award, Puget Sound Section, American Chemical Society
- 1983 National Academy of Sciences Award in Chemical Sciences
- 1983 Bailar Medal, University of Illinois
- 1983 Doctor of Science, University of Chicago
- 1983 Robert A. Welch Foundation Award in Chemistry
- 1983 Nobel Prize in Chemistry
- 1984 Doctor of Science, Polytechnic Institute, New York
- 1984 Honorary Member, College of Chemists of Catalonia and Belears
- 1985 Priestly Medal, American Chemical Society
- 1985 Doctor of Science, State University of New York
- 1985 Corresponding Member, Academy of Arts and Science of Puerto Rico
- 1986 Honorary Member, Canadian Society for Chemistry
- 1986 Distinguished Achievement Award, International Precious Metals Institute
- 1986 The Oesper Award, The Cincinnati Section of the American Chemical Society
- 1987 Doctor of Science, University of Guelph
- 1988 Honorary Member, Hungarian Academy of Sciences
- 1988 Doctor of Science, *honoris causa*, Seton Hall University
- 1988 Doctor of Science, Lajos Kossuth University of Debrecen, Hungary
- 1989 Honorary Fellowship, Royal Society of Chemistry
- 1989 Honorary Fellowship, Indian Chemical Society
- 1990 G. M. Kosolapoff Award, Auburn Section, American Chemical Society
- 1990 Doctor of Science, Northwestern University

ABSTRACT

Henry Taube begins his interview with a description of his early career at Cornell University and the University of California, Berkeley, and cites some of the reasons for his decision to accept a position at the University of Chicago. While at Cornell, Taube felt suffocated by the authoritarian style of compartmentalized departments. Taube also felt some level of separation at the University of Chicago between department members and members of the Institute for the Study of Metals and the Institute for Nuclear Studies. In 1956, he became chairman of the chemistry department at the University of Chicago. Taube then discusses his relationship with Warren Johnson, the dean of the physical sciences, who he felt helped the department survive in terms of balancing the budget and finding financial support. Taube then reflects on the history of the chemistry department and the various members of the faculty who ran the department in its early years. Next, Taube discusses his means of funding his research during his early years at the University of Chicago and his work with mass spectrometry. While at the University of Chicago, Taube worked with Frank H. Westheimer, a man he greatly respected, as well as Willard H. Libby, who was a personal friend. As a member of the chemistry faculty, Taube enjoyed numerous discussions with his colleagues and enjoyed the friendly atmosphere where faculty felt encouraged to share their research, which greatly contrasted with the atmosphere at Cornell. Taube also describes a confrontation with Morris S. Kharasch, which he felt greatly affected the early part of his term as chairman. Taube concludes his interview by discussing the ways in which his career as an instructor at Cornell and the research he was involved with negatively affected his first marriage and how he learned later to delegate authority and find balance between his professional and personal life.

INTERVIEWER

Leon Gortler is professor of chemistry at Brooklyn College of the City University of New York. He holds A.B. and M.S. degrees from the University of Chicago and a Ph.D. from Harvard University where he worked with Paul Bartlett. He has long been interested in the history of chemistry, in particular the development of physical organic chemistry, and has conducted over fifty oral and videotaped interviews with major American chemists.

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INTERVIEWEE: Henry Taube

INTERVIEWER: Leon Gortler

LOCATION: On the way to Grand Central Terminal
New York, New York

DATE: 19 March 1986

Professor Taube delivered the H. Martin Friedman Lecture at Brooklyn College prior to interview.

GORTLER: You already answered the question of why you left Cornell [University] for [University of] Chicago. "It just wasn't stimulating enough."

TAUBE: I don't know yet whether I would have gotten tenure there if I had continued there. I think my chances were good. But the fact was that I did not enjoy the professional environment and I really wanted out. I was attracted to other places where I thought it could easily be more fun. I should also mention that Cornell also was run under the German system. I didn't spend much time on Cornell in my talk because if I were to choose any of the places that I've been to as the nadir of my career, Cornell was it. One of the things that I didn't like at all was that, in contrast to [University of California] Berkeley, it was highly compartmentalized. There was an inorganic, an analytical, an organic, and a physical chemistry division. My particular division, inorganic, was run in rather strict authoritarian style. I didn't really get along with the man, Professor [Albert W.] Laubengayer, who ran it.

GORTLER: Wilder [D.] Bancroft, the physical chemist from Cornell who built this minor empire, was the fellow who started the *Journal of Physical Chemistry*.

TAUBE: He was an emeritus by the time I got there. He died at an advanced age during the time that I was there.

GORTLER: He was always at loggerheads with the Berkeley school of physical chemists. They really saw things quite differently. He was more of a classicist.

TAUBE: I haven't thought of him for many years. In fact, I know that one of the phonograph records in my collection came out of his estate. So I do have this little connection with him.

I don't want to give the impression that I was socially unhappy. I was not. I have close friends who are there now. [James] Lynn Hoard and Florence [F.] Hoard are very dear friends of mine. Si [Simon H.] Bauer was among the few who would ask me a question about what I was doing. In fact, I would say that apart from my friend Thor Rubin, whom I mentioned in my talk, Si Bauer was the only one on the faculty who ever came up to me and said, "What are you doing today?"

GORTLER: I see. That was so different from what you had at Berkeley.

TAUBE: In a sense, there could not have been a greater contrast. I obviously believe the Berkeley system worked better in practice.

GORTLER: I am concerned with why Berkeley, where [Gilbert N.] Lewis had developed the new bonding theory, was always rather weak in organic chemistry. Some people say that Lewis hated organic chemists. The organic chemists on the staff were people who had invariably gotten degrees with him until [Melvin] Calvin arrived in the 1940s and they hired some real organic chemists. I wondered if you had any feeling about that.

TAUBE: I think there was the feeling that he did not really appreciate synthetic organic chemists. He held the view that all chemistry was physical chemistry and synthetic organic chemists would not appreciate thermodynamics, for example, in a way that he thought that every person who called himself a chemist should have done. He probably did have an unreasonable mindset but nobody would ever accuse Lewis of being entirely reasonable. He was opinionated. He could be arrogant, but he had the respect of all of us.

GORTLER: When you went to Chicago and joined the chemistry department, did you also have an appointment at one of Institutes [The Institute for the Study of Metals or the Institute for Nuclear Studies] (now the James Franck Institute and the Enrico Fermi Institute, respectively)?

TAUBE: No. I was brought in solely for the [chemistry] department. Having two classes of citizens caused a bit of a problem. We, who were not members of the Institutes, were treated as second-class citizens; I did not really learn how badly until I was chosen by my colleagues to be the next chairman.

GORTLER: You followed Warren Johnson.

TAUBE: That's correct. And it was rather an embarrassment for Warren. Warren was a kind friend and good supporter, and before he turned over the reins of office to me he said, "Henry, I'm in sort of an embarrassing position. As chairman, you're going to look at the books and you're going to see that your salary does not commensurate with that of other people of your experience who are in the Institutes." But I adjusted to that because I know it is difficult to keep things fair and even, and an administrator does what he needs to do to keep his institution going. And, of course, I received a big boost in salary when I became chairman. I thought that, on whole, I was treated well enough. I never went in and asked for a raise.

GORTLER: Did the chairman of the chemistry department "wheel and deal" for the people in the Institutes or was that a totally separate unit?

TAUBE: I should remember how that was done because I was chairman for a while. I think the Institutes had separate budgets and, if that is true, he (the chairman of chemistry) had no real responsibility. I just don't remember. But I know that this point that I've raised helped me enormously as chairman. To use a vulgar expression, I sort of had Warren "by the short hairs." He made it up to me. He was very generous during my term of chairmanship in providing support for the department. He was very good to us. He found money when I thought none existed.

GORTLER: He was dean?

TAUBE: Yes. He became dean. I should have mentioned that.

GORTLER: I knew that but I couldn't remember quite what dean he was.

TAUBE: I suppose it was dean of the physical sciences. I was really a very poor chairman. I did not understand budgets. I had had no administrative experience before, but at least the department did not fall apart while I was chairman. I think the credit for surviving probably belongs to Warren because I think in a real sense he did part of my job.

GORTLER: That must have been a hard position to handle. Was [Robert Maynard] Hutchins gone already by the time you became chairman?

TAUBE: That was probably true, yes.

GORTLER: I think [Lawrence A.] Kimpton was probably president at that time. There was a very long period from [Julius O.] Stieglitz to Johnson when the department at Chicago had no chairman at all.

TAUBE: That was during the war years.

GORTLER: No, no. From approximately 1933 to 1945 or 1946. He absolutely refused to appoint a chairman from within the department.

TAUBE: I didn't know that.

GORTLER: There was an enormous search. In the archives I found lists of people who were considered for the position—a number of Europeans.

TAUBE: Hutchins did not trust his own faculty.

GORTLER: It may have been that he didn't care for [Morris S.] Kharasch. Kharasch was a logical choice. [Hermann] Schlesinger essentially ran the department during that whole period. There was a *troika* running the department for a number of years of Kharasch, [William D.] Harkins, and Schlesinger. Schlesinger was essentially the chairman of the committee so he ran the department. But he never had the title. Also, the way Grant Urry tells it, Hutchins absolutely refused to appoint Schlesinger chairman too. I just can't remember quite what it was that he had against him.

TAUBE: When I first interviewed at Chicago, Schlesinger was acting in place of the chairman.

GORTLER: That's right. So there was no chairman. Johnson had not been appointed yet.

TAUBE: By the time I got there, Johnson was chairman. In the interim, between being invited and being appointed, I actually got a raise in salary. I think Schlesinger tended to follow the Stieglitz tradition trying to save the administration money. That was the way he was brought up. It was nothing against him. He believed we should not be spendthrifts and they could buy me at that figure and I was happy to come at that figure. Actually, it was Bill [Willard F.] Libby who had a look at the books, and I guess Libby actually compared my salary with Clyde Hutchinson's and others who came at the same time and decided that they weren't paying me

enough. I have no bitterness towards Chicago about salary questions. During my entire time there, even though I mentioned it, it was not one of the things I worried about.

GORTLER: What were the conditions like at Chicago when you got there, in terms of facilities, laboratories, and things like that?

TAUBE: My requirements at the beginning were very modest. I had done experiments often following rates by simple titration. Spectrophotometers were not yet universally available. But I did acquire a Beckman [Instruments, Inc.] spectrophotometer rather soon after arriving there. I probably got that through the ONR [Office of Naval Research] because my first research support came from them. That was during the good old days when they came to the universities and they asked people to accept support because they would like to support some of their research. Then a little bit later the A.E.C. [Atomic Energy Commission] came into the picture and I was given a mass spectrometer by them. In that respect, again, I have no complaints against Chicago whatsoever. My needs continued to be modest. In fact, I had a very difficult time adjusting to the new ways because I thought we were begging for support while other people who should have been supported, weren't getting any. It took me a little while to get used to the new ways. And the mass spectrometer stood me in very good stead because it allowed us to do a number of stable isotope tracer and isotope fractionation experiments.

GORTLER: Was it one that Harold Urey built?

TAUBE: No. It was a commercial instrument and it was very good. I think that its precision was only three times lower than the refined machine that Urey's group put together, which I understand gave accuracy at the level of electronics in those days. They had really taken everything out of it; you just couldn't do anything better with electronics. Urey had a magnificent mass spectrometer. He needed very precise isotope ratios. As I remember, he determined one part in ten to the fourth for O-18 [oxygen-18] at natural levels. I think our mass spectrometer gave results quite a bit short of that. However, it was certainly useful for the first measurement of the number of water molecules that are bound by a cation in aqueous solution.

GORTLER: How in the world did you do that by the use of mass spectrometry?

TAUBE: In the course of giving an advance course, I had deduced that water molecules would stick to chromium ions so that they would exchange slowly, and of all of the ones that were accessible, I chose that as the one most likely to yield an interesting result. Now people ask, "Why was that a significant experiment?" What many may not appreciate is that in those days many physical chemists thought that all reactions of inorganic ions proceeded instantly, i.e., they would be very, very rapid. Now, when John Hunt, who undertook the measurements,

started to work with me, we didn't yet have a mass spectrometer. The idea was that we would take a solution containing hexa-aqua chromic ion dissolved in ordinary water and then would spike it with a given amount of water enriched with O-18. Of course, you would get instantaneous dilution of the O-18 isotope by the free water. But you wouldn't get dilution by the water that was bound firmly to the chromic ion. If no water were held back by the chromic ion, you'd get a certain ratio of O-18 to O-16 [oxygen-16]. If, on the other hand, the chromic ion held back six waters, the isotopic enrichment would remain higher. So by looking at that difference you could then determine the number of water molecules bound by the cation.

GORTLER: That was a very clever experiment.

TAUBE: It was an important experiment and I've heard physical chemists of my age publicly acknowledge that they were astonished by the fact that an inorganic ion would be so slow to undergo substitution. Of course, the study of hydration of ions continues to this day. We did some of the very easy ones. They're doing the hard ones now.

GORTLER: Who were the people you interacted with most at Chicago? Would you say something about the people on the staff?

TAUBE: At that period, I interacted most with Frank [H.] Westheimer because each of us understood what the other was trying to do but it was different enough so that it had a sort of fresh flavor. In fact, if you look at Frank's review article on the oxidation of chromium (VI), you will see that he acknowledges me in the appropriate section. I participated in the discussions that led to that paper. I learned an awful lot from him. I learned especially to appreciate Frank's precision of thinking. He did not allow any sloppiness in his thinking.

Libby and I had known each other since my Berkeley days. In fact, my first wife and his then wife, Lorelie, were very good friends. I was always at ease in Libby's presence. Bill Libby was interested in electron transfer reactions. He claimed he so fully understood the subject, there was nothing more to say about it. [laughter] But I know that he respected the work that I was doing. He was a stimulating colleague. He asked me challenging questions but he would do it in such a manner that you knew he wasn't trying to put you down. He wanted to get a discussion going. And, in fact, he did make an important basic contribution to the field of electron transfer reactions in chemistry because he was really the person who publicized the relevance of the Franck-Condon restriction to the thermal electron transfer process (1). Now, [James] Franck himself appreciated the relevance of the restriction, as did [Robert L.] Platzman, who was a student of Franck's, but Franck never pushed the idea, whereas Libby got up publicly and talked about it.

And then he made another very important contribution. I think he was instrumental in organizing a symposium held in 1951 at [the University of] Notre Dame on electron transfer

processes. It had to be somebody with Libby's sort of broad view of things because it dealt with electron transfer in the gas phase, theoretical and experimental, and then dealt also with theoretical aspects, to the extent that one could theorize, for the processes in the condensed phase (he, himself gave a paper), down to rather detailed reports on what are now called self-exchange processes on some relatively simple oxidation-reduction reactions. Now I didn't attend that symposium but it had an enormous impact on the field, not so much, I would say, for the papers themselves, as for the discussion that ensued. It was well accepted that some electron transfer reactions take place with coordination shells of the partners remaining intact. It is rather interesting that the idea which we had all discussed among ourselves—that some inorganic redox reactions might proceed like organic ones, that what we called electron transfers might really involve atom transfers—was actually documented for the first time in the literature in some comments that Herbert C. Brown made following a paper on the self-exchange rate between Iron (II) and Iron (III) (2). [R. W.] Dodson and [J.] Silverman had measured the Iron (II)—Iron (III) self-exchange for two of the halides and found that the reaction was more efficient when bromide mediates the reaction than when the chloride is used (3). But at that time, there had been no experimental proof of the validity of this mechanism. And then, a third mechanism, the so-called hydrogen atom transfer mechanism was proposed. I think nowadays we would say electron motion is coupled to proton motion, because you have to move charges in solutions. That also, for the first time, was enunciated as a model. That was a very important meeting. Even though I wasn't there, it still was important to my own work.

GORTLER: Were there a lot of new people who came into the department at that time?

TAUBE: Oh, yes. Tremendous influx. You have to count the people in the Institutes when I say that. I may have been the only one that came into the department proper, but there were about five or six people of my age at the Institutes: Norman [H.] Nachtrieb, Clyde Hutchinson, Tony [Anthony L.] Turkevich, Nate Sugarman, Norman Elliot, who then went to Brookhaven National Laboratory, and Jake Bigeleisen. I think there were two categories. There was one category, sort of a high level post-doc or fellow. Jake Bigeleisen was in the second category. They were all very, very good people.

GORTLER: That's quite a cast.

TAUBE: It turned out that Kharasch and I had many interests in common. During my years at Cornell, I worked on inorganic free radical reactions, so I knew quite a bit about Kharasch's work. Kharasch, Bill [W. H.] Urry and I, never together, but separately, had many interesting discussions on problems in chemistry. Schlesinger never had any feeling for my work. He was deeply involved in boron chemistry but I knew that Schlesinger was a very strong supporter of mine. I would guess that Frank Westheimer would have influenced him and maybe Bill Libby. I know that Frank thought very well of my review paper in which I correlated rates of

substitution with electronic structures (4). He never said anything to me directly, but somebody passed on a remark that he had made.

GORTLER: If that's true, then you passed a harsh judge. He really is a very quality conscious person.

TAUBE: It was a paper that I continue to be proud of. It has to be read in the context of its time. I was later asked to give a paper at a [Linus C.] Pauling birthday celebration. I thought that I might talk about Pauling and his great contributions to inorganic chemistry. However, I decided that would be difficult for me to do because of the success that I had in that paper depended on my putting aside some implications of Pauling's views on coordination chemistry. I realized that descriptions of inorganic complexes as ionic and covalent did not really go beyond the magnetic criterion. It soon became apparent to me that the ionic complexes could actually be more covalent than the ones he called covalent; that he was really talking about low-spin complexes on the one hand, where all of the non-bonding orbitals are occupied—this is modern language—and then complexes, on the other hand, where some of the non-bonding orbitals were empty or you had occupied anti-bonding orbitals. And I saw that he was, in effect, confusing covalency with rate of substitution. Chromic ion is slow but there is no reason to believe that the complexes are more covalent than those of ferric ion. The correlation survived the change from valence bond theory to ligand field theory. It has a valid experimental basis.

As a member of the department, I was invited to give seminars in front of my colleagues. I think that was a point that may not have been mention to you. It [the "Institute Seminar"] was a very important function in the early history of the postwar chemistry department with respect to interactions with chemists from the Institutes. Joe [Joseph E.] Mayer had the idea of bringing all the chemists together on a Wednesday afternoon. Coffee, cookies, and doughnuts would be furnished and then afterwards people had a chance to chat with each other. Joe would look around the room, pick out one of his colleagues, and say something like, "Henry, we haven't heard from you before. Would you get up and tell us what you are doing in your research?" It was wonderful. It was informal and all of us, of course, were deeply interested in research. I remember starting to talk and as time went on, I was asked to continue the next time. I felt very flattered. This was a tremendous contrast to Cornell, where I was never invited during my entire time there to give a seminar to my colleagues.

GORTLER: If the system at Cornell was really Germanic, you were fairly low down on the totem pole.

TAUBE: I was way down because I had very little contact with [Peter J. W.] Debye. Debye would get in touch with Laubengayer. He would tell him what to do and think. I did not admire Debye as chairman. It was impossible to, because he did not fit my idea of what a chairman should be based on my experience in the department at Berkeley. But I guess Cornell organized

its department in its own way. Debye did not, I must say, show much interest in his younger colleagues, unless, of course, they were working close to his own research. He felt no responsibility for them. I think that when he was named chairman they pretty well understood what they would be getting and I think it worked out well for them because he was a highly respected and extremely good scientist. I think he did them a lot of good.

GORTLER: Did you get the impression at Chicago that there was a philosophy the organic chemists followed? Chicago's is the one department that I've come across where there were no classical organic chemists.

TAUBE: It might have been the Stieglitz influence, I suppose. We were what they would call physical organic chemists now, wouldn't we? Organic chemistry was pretty full when I came to Chicago. As far as I can remember, we saw no need to appoint any organic chemists.

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TAUBE: Frank Westheimer was cement in the department. He interacted with people who were physical chemists, inorganic chemists, and analytical chemists.

GORTLER: That started very early on. One of his early important papers was with [John Gamble] Kirkwood, who was there for only a short period (5). And, he did work with Joe Mayer.

TAUBE: That work with Kirkwood was a very important paper. He also did one on the hindered rotation of biphenyls (6). I think that was with a physical chemist. I didn't know he had a paper with Mayer.

GORTLER: That was the paper with Joe Mayer. The Westheimers were very close to the Mayers.

TAUBE: Yes, they were. I suppose if Frank had stayed at Chicago I would still be there. When he left, it made a big difference to me. I guess my social relationships with my colleagues remained warm and cordial, but, in terms of our work, we began to take each other for granted. Cross-fertilization of ideas was not as noticeable towards the end of my stay there as it was early on. This wonderful seminar program that I talked about only lasted about two years and I still don't remember why it was given up.

GORTLER: It sounds very familiar, having been in other departments where this has happened. Our newest faculty member [at Brooklyn College], Lesley Davenport, has started a literature seminar each week. That's what young people do, but after a while, you get off on your own thing.

TAUBE: It's strange that I don't remember how or when that seminar was discontinued. I guess it died with a whimper not with a bang.

GORTLER: Did you have any particular philosophy when you were chairman?

TAUBE: I didn't. I really was not ready for the chairmanship, I have to say. I'm still puzzled as to why my colleagues selected me. I think I was the least objectionable of a number of alternatives. I would cause less trouble than anyone else.

Early on, I got involved in a way that was very unpleasant for me. That's no quite the right word. I'll tell you the story. It spoiled my life as a chairman for at least a year and a half. I had not held the chairman seat long when Morris Kharasch came in and said, "You know that I am proposing Walter Nudenberg for a tenured position in the department." I pointed out to Morris that I thought there would be difficulty with that in the department, because I felt that my colleagues had the view that every faculty member must be independent in research. Kharasch said, "Yes, I'd like to argue the case and would you as chairman introduce it for discussion?" I said, "Of course, Morris, I will." He said, "Would you as chairman support it?" If I were a tactful young man, I could have avoided the issue but that was never my way. So I said, "No, Morris, I can't." He didn't expect that from me because he probably felt I was a bit more pliable or something. He turned white in the face and paced back and forth in my office and said, "Well, in that case I have no alternative but to resign." I didn't really know how to handle it. I didn't really handle it. I did, I think, do what was the right thing. I didn't do anything.

Then a formal letter of resignation came from him. I responded to that in some way. Then other events began to take place. Morris published the news that he was leaving the University of Chicago and he was looking for a place where he could be more comfortable. I'm not sure that he got any offers, but there were a couple of phone calls to me and people inquired into the circumstance. I didn't bad-mouth Morris, because I had respect for him and I liked him as a person. But he could be difficult.

GORTLER: Yes. That's the impression I've gotten from talking with others.

TAUBE: He'd get stubborn. You know, he made a mistake in resigning in my presence like that. If I had been more his age, I could have gone in and said, "Come on, Morris, you don't really want to resign. What can we do to keep you here?" But we weren't on that basis and I don't think I could have done that then. So what we did then was talk to the administration. We asked them to make an arrangement for Morris so he could stay and be comfortable with his resignation from the department. We persuaded the administration to create an institute for him. The question of his relations to our department proper were never defined because it was better not to go into any details. I don't think any articles of agreement were drawn up. It essentially meant that Morris operated as he had before, which was sort of independent of the rest of the department.

GORTLER: So it didn't really change a hell of a lot. [laughter]

TAUBE: No, it didn't change a hell of a lot. But the resignation created very bad feelings between Mrs. Kharasch and me and other members of the Kharasch family and me, and also former students of Kharasch who did not know what went on. My wife, Mary, in fact, invented a new participle called "Kharasching." I was "Kharasching" for a period of almost two years. The reason it was tragic is that Morris died shortly after he had his institute and I can't help but feel that the tension had its effect. It was not an easy time for him. I remember sitting behind Morris once at a seminar. He was older than I, and I was looking at the back of his gray head, just feeling like, "What a shame."

GORTLER: Nudenberg must have left before the formation of the institute. I guess he took that attractive offer.

TAUBE: I talked to Walter and I explained the department's position and, you know, Walter and I were personal friends. Walter was very helpful to me at times when I needed a piece of apparatus or when I needed access to a vac [vacuum] line. He was always cooperative and always one that we wouldn't have violated for anyone. I think by then Walter was so sick of the whole situation that he wouldn't have stayed on in any case. He's a very nice man. I don't know if you know him. He went into industry. I think he did extremely well.

GORTLER: Kharasch had a habit of using people in much that way. Frank Mayo left as a result of that. I guess H. C. Brown wasn't really used.

TAUBE: It would be hard to use H. C. Brown. He was tougher than most.

GORTLER: There was a letter in the archives that I was almost embarrassed to read. It was a letter, concerning Bill Urry—I can't remember who wrote the letter—from the administration suggesting that Bill Urry might be better off leaving the university because they were afraid that he was too linked to Kharasch and it would be far better for him and his career if he went elsewhere.

TAUBE: He cut himself off from Kharasch but that was not without difficulty and anguish. I'm not sure they remained good friends after that. It's quite true. Morris seemed to have this way of dealing with people.

GORTLER: Clyde Dillard told me to talk to Nudenberg some day. Clyde thought Nudenberg was used by Kharasch.

TAUBE: Oh, he did? Walter never said that to me because he had tremendous respect for Morris. But we thought he was being used because he never published an independent paper. It always had Morris' name on it. There was nothing wrong with Walter's brain. Morris bucked at the stipulation that Walter would continue to work for him. Walter would have been a serious contender for a position if one had existed. It was another point to show that we needed another organic chemist.

GORTLER: That's interesting. N. C. Yang was there and did finally end up as a faculty member.

TAUBE: I guess. I think he did in a way.

GORTLER: Because he came in with a fellow named Rudy to take over Nudenberg's position. Then shortly thereafter Kharasch died.

TAUBE: And Tom [Thomas] Rudy went to Shell development.

GORTLER: That's right. He went back into industry.

TAUBE: I heard from Tom a couple of years ago. I've seen him since. He's not at Shell development now. Shell closed when they went to Houston but Tom stayed in the San Francisco area. I knew that Tom never had any bad feelings towards me on account of my dealings with Morris.

GORTLER: You remained chairman until when?

TAUBE: I think the term of office was three years.

GORTLER: So you didn't stay chairman until you left?

TAUBE: Oh, no. I had had my fill of it. I was simply no good at it. I couldn't keep books. I really took advantage of Johnson and you can't run a department in the red every year. The department was treated very poorly by the administration during that period because it was a continuation of the tradition. It had come from Stieglitz and we had never been able to break that tradition. In contrast to the way the Institutes were treated.

GORTLER: I wanted to ask you about your philosophy on handling the high pressures of working in a major university. You mentioned today that you worked so hard at Cornell that it disrupted your family life. How do you think that affected you in general?

TAUBE: I didn't think of it in those terms. I wanted to do what I was doing and I don't think I could have done anything else. It was as simple as that. It was only one factor in the break-up of the marriage. I clearly did not spend enough time with my family. I didn't have enough time for them. I didn't really get to know my wife as a person.

GORTLER: How did your second wife handle it?

TAUBE: By then, circumstances had changed. The trouble was that at Cornell all the research that I did was done with my own hands. But when I got graduate students, the relationship changed. I still worked in the laboratory and sometimes in the evenings. But I wouldn't work all night any more like I did at Cornell. So it was a much more normal married life. I learned something in the interim. I was actually too green. I was too green for a lot of things that happened to me. But I grew up.

GORTLER: It's fortunate that we do grow up and we learn from our mistakes.

[END OF TAPE, SIDE 2]

[END OF INTERVIEW]

POSTSCRIPT

By Leon Gortler

Previously I had asked Henry Taube about George [W.] Wheland. I told him that Frank Westheimer had said that George Wheland was one of the brightest people he had ever met but that, for some reason, he did not have the energy to produce the products that his mind might have been capable of producing. Henry indicated that he disagreed with that analysis in part. He agrees that Wheland was very bright but he felt that Wheland had a rather narrow view of chemical problems and that his answer to most problems was resonance and that he could not see the other effects that might be important in the explanation of certain observations.

During Dr. Taube's talk today, he spoke of a memoir of G. N. Lewis that had been written by Joel Hildebrand (7). I assume that was a memoir for the National Academy of Sciences. In that memoir, Hildebrand indicated that during the Lewis years at Berkeley, the department was never divided up into sub-disciplines. Everyone essentially practiced chemistry and everyone worked on all kinds of problems

*The last two comments were added on the evening of 19 March 1986,
after Leon Gortler dropped Henry Taube at Grand Central Terminal.*

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