An Interview with Hans Jaffé

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Hans H. Jaffé was born in 1919 in Marburg Germany. He began his chemical training at the Berlin Technische Hochschule in 1937 but was forced to flee Germany the next year with his family – first to Caracas, Venezuela, and then to the United States, where he completed his B.S in Chemistry at the University of Iowa in 1941 and his M.S. at Purdue University in 1942. After serving two years in the United States Army, Jaffé was employed as a physical chemist with the United States Public Health Service, while simultaneously completing a Ph.D. under Oscar Rice at the University of North Carolina in Chapel Hill. In 1954 he joined the faculty of the Department of Chemistry of the University of Cincinnati, where he remained until his retirement in 1989. During this time he also served as Director of Graduate Studies (1962-1966) and as Departmental Chair (1966-1971). In the course of his career Jaffé supervised the theses of 45 graduate students, coauthored five books, and published more than 165 articles. He passed away in the fall of 1989, a few months after completion of the following interview, which is based on a series of conversations which took place between Dr. Jensen and Dr. Jaffé on 10 and 24 January 1989. Editorial clarifications are in square brackets.

Hans H. Jaffé (1919-1989).

Tell me about your parents and childhood.

I am a third generation Ph.D. and my eldest son makes the fourth generation. My father's father was the developer of Lanolin and founded the Pfeilring Werks of Germany. He was a very rich man and lost it all after the First World War. He was a Ph.D. chemist. My father was a zoologist who never really practiced. He was wounded early in the First World War, hit in the hip somewhere so he couldn't sit over a microscope any more, which is what a zoologist did at that point. So he never continued with his zoology and did various other things over his lifetime. My grandfather's wife had a brother who was a geologist at Heidelberg and eventually at the University of Ankara. My father had a brother who was a pathologist at the University of Berlin and then at the University of Caracas. So there was a lot of academia in my father's family background. My mother's family were bankers. My great great grandfather – my mother's great grandfather founded what became a private bank. Basically, he was a banker to one of the little local Counts outside of Breslau.

So I would assume that, almost from childhood, you were interested in becoming a chemist?

Well, there is a key moment, though I can't tell you the exact date – it must have been in the early thirties – 1934 maybe. My mother came up with a mineral collection which she had as a child. Marvelous stuff, beautiful pieces, bought in the best shops for lots of money. But, of course, all the labels were lost. She said, "You can't have this collection if you can't put an order into it." So hardness and HCl tests got me into chemistry.

I understand that you began your studies at the Technische Hochschule in Berlin.

Actually, I almost went to Zurich to study physics instead because at that point my interest was already on that borderline. If I could have gone to the ETH in Zurich, I would have been a physicist. The only reason I am a chemist today is when I came to this country and the Registrar at the University of Iowa evaluated me, he found that within a year's work I could easily get a B.S. in chemistry, but it would take two years for a B.S. in physics. That's what made the decision.

During your brief stay at the Technische Hochschule, did you have contact with any famous chemists or physicists?

Well, Hans Geiger gave the big physics lecture and Westphal of the Westphal balance gave the physics lab. I don't know that the chemists were particularly well known people. [This is the wrong Westphal. The balance actually dates from the first half of the the 19th century].

What were your courses like?

It was very interesting, all kinds of qualitative analysis – real systematic goods – plus, I don't remember how many preps were required. The preps I was assigned were not particularly easy ones for a beginner: aluminum chloride and titanium trichloride. The laboratory was open from 8:00 am to 6:00 pm Monday through Friday. I'm not sure whether Saturday was also a full day or a half day. You went in and did what you wanted. You were supposed to take this book and do all of the preliminary experiments and then demonstrate to the Assistant that you understood them. Then you were given your unknowns and you worked on them. During my year there, I got my qual done but I didn't get most of the quant or the preps done. I still think qualitative analysis is the most fun and that it teaches you the most about chemistry. If I were to teach Freshman lab today, there would be at least one full quarter of qualitative analysis.

Rabbit hunting as a child in Germany. Hans wrote on the back of this photo, "Mein erstes Kaninchen," which may mean, "My first reviewer."

A slightly out of focus Hans at age five. Xmas 1924.

Given your developing interest in theory, did you have a preference for the theoretical physics courses versus the laboratory chemistry courses?

No, I wouldn't say that. Where my physics interest came was from reading on radioactivity. It wasn't really theoretical physics; it was atomic structure versus chemistry.

Was there much atomic structure integrated into the chemistry courses or were they largely descriptive?

No. It was outside reading mostly.

Tell me about leaving Germany. I understand that you and your parents originally went to South America.

Hans as a teenager with his “Motorrad.”

My uncle, who had been a pathologist at the University of Berlin, got fired in 1936 and got a “Ruf”' from the University of Caracas in Venezuela. He became well established in Caracas as the first and only pathologist at the university and got the rest of his family in there at a time when it was virtually impossible to get anywhere. I remember the day that we had somehow managed to get an appointment at the U.S. Embassy in Berlin. When we arrived, there were lines around the block. I remember one guy pushing through the lines waving his American passport above him. And, of course, I got pulled in. But I mean, there were millions of people wanting to leave Germany and they couldn’t have cared less where they went. I don't care what embassy it was, you would find lines like that – American worse than others. My cousin, who at that time was a Ph.D. candidate in Zurich, managed to buy (I don’t know what he had to pay for it) a visa to Paraguay with a transit visa through Uruguay. On that visa you were supposed to be able to get into Switzerland. Everyone was using these fake visas. Unfortunately, they caught on to that before I left. That was in late 1938. How could you get out? You could only take with you ten marks or $7.50 – at that point probably worth $2.50.

You stayed in Caracas a year and a half before coming to the United States. When you came here, had you already made arrangements to go to the University of Iowa?

Yes, I came as a student. Somebody got me a scholarship and a place to stay rather cheaply at a co-op house. So that was my first year over here. Interestingly, the Registrar at Iowa said he would count my high school leaving exam as two years of college. He said, “Write down the number of hours that you spent in every course in those last two years in high school and your year in the TH and we will translate them into our credit equivalents.” When he finished, he said, “You are shy three credits of high school English, three semester hours of college English, six semester hours of college social science, and a certain amount of chemistry.” So I went to the Head of the English Department, and he said, “You won’t need your German literature and your high school English can wait.” So that left me with a course in English – any course in English. I ended up taking English Drama from the beginning to 1642. Can you imagine me, with my English still being very, very shaky, competing against seniors and graduate students in English and Dramatic Arts in a course dealing with pre-Shakespearean English? We read one play a week. I got a C because the guy felt I got a raw deal. On the other hand, in the social sciences, I took a course in European culture from a South African – a Boer – who had been educated in Amsterdam, Berlin and London. A beautiful course. Of course, there I had more background than anybody else. It was nice, it was lovely. I enjoyed it. And then I took chemistry, lots of chemistry.

Did anyone in chemistry at Iowa influence you or was there really no opportunity, given your brief stay?

Influence, no. Nobody had much influence on me. I did my best to get through. The guy I remember best was my organic professor. I went to a few of his lectures and I found that he just stood there and read the book to us and did some bad demonstrations, so I stopped going. So he called me in and he said, “If you don’t come to class, I'm going to have to do something.” And I said, “What’s my grade?” He said that it was a good B, so I told him, “That’s good enough for me, thank you.” And I never went to another class.

Did he give you the B in the end anyway?

Oh sure. What could he do?

What determined your choice to go on to Purdue for the Masters?

I got a TA and then a fellowship.

You applied to a number of schools?

I applied to a whole bunch of schools and that was the only thing I could get – a half-time TA at $37.50 a month. By the time I got there, they didn't want to put me in the lab. Antisemitism in this country was still pretty bad. So they gave me a fellowship for $60 a month. The Chair at Purdue, Henry Bohn Hass, was a SOB and lousy teacher.

Whom did you work for at Purdue?

The project was Henry Hass, but my direct supervisor was Tommy DeVries. Tommy DeVries was a nice guy. He may still be alive, I'm not sure. I saw him three or four years ago, an old man still sitting in an office doing something.

Did you interact with any other faculty?

M. G. Mellon and I had an interesting interaction. I took the chemical literature course with him. He assigned a bibliography. So I asked if I could I bring in my tray of cards. He agreed and I got an A. It was a tray of cards dealing with papers that interested me.

So you had put it together for your own interest independently of the course?

Sure.

What was your Masters project on?

Separation of enantiomorphs by some process of chromatographic adsorption. It doesn't work worth a damn. As a matter of fact, the best way to do it is to adsorb the pair of optical isomers directly onto an optically active absorbent. That's the way it will work – it's been made to work that way. We made it work too. But I have two left hands, I can't do good lab work.

I assume the reason that you didn't go on directly to the PhD. was that both the army and marriage occurred at this juncture.

As a matter of fact, Henry Hass had me drafted. Henry Hass and I didn't get along very well, so he called the draft board and had me drafted. Haas was a dictator from the word go and his right hand man was E. T. McBee who, for 20 years after Haas left, continued that dictatorship.

Tell me about your experiences with Uncle Sam.

I shipped around the country for two years or so, and I got to Tulelake, California, right on the northern border, where they had one of the Japanese National internment camps. I was a so-called medic and was assigned to the infirmary. The Captain there was intelligent and he saw that he could use me. So he said, "Do you want to become a lab technician?" I said sure. I went down to San Francisco and bought myself a couple of books and for six months was his lab technician.

This was followed by a lot of short-term assignments but, wherever I was, I managed to find myself something to do. In Medford, Oregon, I went to the infirmary thinking I would be a lab technician again and, indeed, they needed one. They were just setting up a mass screening and I was the lab technician replacement. Each patient would give me a urine sample and I had 13 minutes per patient to run the necessary tests. It wasn't that difficult. I stayed there for three or four weeks and I set things up for them. It was trivial. They gave me Master Sergeants to work with. I was their boss, though just a buck ass Private.

In Camp Ellis, Illinois, I couldn't find a good job like that, so some of us went out nights and worked for Caterpillar Tractor. I don't remember what they were making for the government, but they hired us and we laid around and did very little for eight hours each night and then went back to camp, stood for reveille and then found a quiet corner and went to sleep. Nothing to do all day. That's the way the army went.

I finally got to Saipan. I found out later that I was shipped there as a replacement for a company clerk. I had somehow received a secondary specialty as a clerk. I guess they found out I could write. I didn't know I was being sent there as a clerk, and the first night I got there I went to the lab and introduced myself to the Lieutenant Colonel in charge. He picked up the phone and I was a clerk no more. He needed me to replace one of his people in the lab. So I became the chemist in the lab. They made me a Private First Class as soon as they could, and then they made me a three striper. I was in for the fourth stripe by the time I left. So there I got treated properly.

They had a Coleman Junior Spectrophotometer, but unfortunately without any directions or methods, so I put together a manual for us outlining the 20 or 30 tests that we used. The most interesting part of it was when I found out about the blood sugar determinations that the army was doing. Blood sugar was done using the old Folin test and a Duboscq colorimeter, and you were told to use one of two reference solutions: a 100 or a 200. So one day I made up a 150 and compared it. Reading it against the 100 reference, it read 180 and reading it against 200 reference, it read 120. It didn't follow the Beer-Lambert law. But nobody in the army had found out. I made a calibration curve so we could tell what we had. One day there came a Captain – a biochemist. I needed either a standard hemoglobin or a bilirubin solution, and asked him if he could make it for me. I needed it to calibrate something. So I had the Captain working, making my standards for me. It was fun.

But God! – the day that the first hospital ship came over from Iwo Jima. We all knew they were coming so we had set up. We had a 2000 bed general hospital and we emptied the place out by that morning. That afternoon the ship came and unloaded 1000 casualties and they were in bad shape. We were doing blood tests on them all, the simplest blood test you can make in order to do them as fast as possible. You could assume, of course, that these were reasonably normal bloods no microorganisms or anemias or anything like that. So all we had was this long series of copper sulfate bottles and you dropped the blood into the bottles and read the specific gravity. We did 1000 of those in two or three hours, and then I went out to collect blood from specific patients for specific tests. That is when I lost every bit of respect for nurses. The doctors, the officers, everybody was working on the patients. The nurses were sitting at the desk writing letters home and filing their fingernails. I haven't been able to really talk to a nurse politely since. Here were these kids and they were in bad shape. The real decision for the Colonel and for the medical officers was, "Do we take the worst case first? He is going to die anyway? Should we save the third one and let the others go?" Those were the kind of decisions they were making. That is the kind of decision I've often talked about here. Why do we offer a job to the best graduate student, who we know is going to go to Chicago or Harvard anyway, and in the process lose the second and the third? But it is very difficult to do – just as it was there.

Well, anyway, I was in the army and in the army they give you those heavy boots. And, of course, they have no idea how to fit you for them, so for all three years in the army I had the wrong size shoes – 9EEE instead of 10 1/2D. I didn't realize it either. I didn't know my shoe size. You mustn't forget that I was relatively recent in the country and American descriptions of sizes are different from European. Well, as a result, I had awfully sweaty feet and they were raw and hurt all the time. One day I decided I'd go to sick call and see whether they could find me some low cut shoes. That's all I was asking. The man looked at those feet and said, "Do you think you can get away from the lab for a day or two? I'm sending you to the ward." That afternoon the ward officer came and said, "I don't like that. I'm sending you home." For sweaty feet? You've got to be kidding! So three or four days before Christmas, 1945, I arrived in San Francisco on a hospital ship. You see, I was one of those poor patients and had to be evacuated! San Francisco harbor was full of troop ships which they didn't have the shore facilities to unload. Douglas MacArthur had said he would get the troops home for Christmas, and he dumped them into San Francisco and San Francisco didn't know what to do with them. As for us poor patients, we were immediately taken ashore to hospital, and the next morning we were put on planes and taken to the general hospital nearest our home. Well, at 11:00 on the 23rd of December, I checked into Cambridge General Hospital, Cambridge, Ohio. My home was listed as Indianapolis, Indiana. At 2:00 I said, "How about some shoes?"

They sent me home on Christmas leave instead and by 2:30 I was on the road hitchhiking. But it was so cold and slick out that I turned around and hitchhiked back. When I got back to the base, I asked about the low cut shoes again. They said, "You won't be here long enough." Three days later I was sent to Camp Atterbury, Indiana, for reassignment. I hitchhiked as usual and let them pay me three cents a mile.

The army would reimburse for that?

Oh yes – for travel by privately owned automobile. Anyway, by then I had enough points for discharge – in those days discharge was a matter of points. So I immediately got shipped from the reassignment to the discharge center, which was only across the street. I went through the entire discharge procedure but, a half hour before the final ceremony, comes a runner from headquarters saying, "You come with me. You can't be discharged. Medical lab technicians are essential." So I went back to the reassignment center, where they said, "Because you have been overseas for a year, you are entitled to 60 days of rest and recuperation." So they made out my papers and I went and spent 60 days in Indianapolis having a good time.

When I reported back, my orders now read, "Report to Camp Crowder, Missouri." Again, at three cents a mile in my pocket, I hitchhiked down to Missouri. Hitchhiking in those days in uniform was the easiest thing in the world. Everybody stopped. I got to Camp Crowder and they said, "You're not essential anymore." They changed their classification! So they shipped me across the street and they said that Monday I would be sent to Leavenworth for discharge. Since this was my last weekend in the army and I had never been down in this neighborhood, I took a little trip through the adjoining states of Arkansas, Oklahoma, Kansas and Missouri. When I got back, the Lieutenant asked me, "Didn't you know that you were supposed to be on KP?" I said no, though I knew damn well that I had been. He threatened to send me to the stockade for six months and to give me a dishonorable discharge, but he didn't, and I finally got discharged. The last day of my army career was after the discharge, I think. They asked me if I would like to sign up with the reserves. I said, "For three years I had a contract with the army in which the army held me to the letter, and the army changed its obligations as it wished. No thank you, I don't want any more contracts with this army!"

At what point did you join the Center for Venereal Disease?

That summer, after my discharge, I decided I would go back to school, but I needed money too since I had gotten married in March. I went to the University of Chicago for a summer, where I took a marvelous course on physical organic chemistry from Fred Westheimer – absolutely marvelous – and worked for E.S. Guzman Barron in the hospital at the medical school. But I was not overly happy. After three years away from everything, it was not easy to settle back in. One day Barron comes in and tells me he has a friend, Harry Eagle, who is looking for somebody like myself. Would I be interested? Eagle's right-hand man, George Doak, is going to be here in Chicago at the ACS meeting next week and I should go and see him. I went to see George Doak. Doak, at that point, was working as an arsenic-antimony chemist for the public health service – the VD service. He was looking for somebody who had done some reading on physical organic chemistry. I had just taken that course with Westheimer and read Wheland's book on resonance theory. So he hired me as a technician. I had a Masters from Purdue at that point, nothing more.

What types of projects did you work on?

The first thing Harry Eagle wanted me to do was find out about "Bacitracin." Bacitracin was a new antibiotic at that point. I believe it's still used. He didn't tell me what it was or anything. He had a new counter current liquid-liquid extractor and I was supposed to fractionate it. Well the damn thing turned out to be a polypeptide and, basically, I didn't know anything about what I was doing. I wasn't given enough information. I was hopeless. Then George got me to work on his project – the thing he had hired me for – and we ran kinetics on a solid-state reaction out of which we got a paper or two. It wasn't great stuff. But we did incubate these things at 100 degrees, or whatever it was, and then quench them and analyze them. It was kind of accurate for kinetics in the solid state, as you can imagine.

How did you become a student of Rice at Chapel Hill?

Harry Eagle got transferred to the NIH and we got a chance to go to the laboratory down in Chapel Hill. About this time I was ready to go back to school. I had planned to work with a biochemist – a big name in pH – at Johns Hopkins (W. M. Clark, I think) or to go back to Chicago. However, I went to Chapel Hill instead to see Oscar Rice and asked if I could work with him on kinetics. At that point I had an interesting kinetic problem that I never made work – the decomposition of diazonium compounds in solution. It could have worked, but my curves never made much sense to me and I never got good results.

How did you get interested in the Hammett equation?

George Doak got me interested in the Hammett equation. I was being paid by the public health service, though I was working part time in order to take more courses, and I spent much of that time just writing a bibliography on the equation. That ultimately led to the famous review paper and also got me interested in asking myself, "How does this work – why does this work?" I came across Louis Hammett's statement that it was undoubtedly a matter of electron density. But that was said in the prehistoric days, so I decided to go and look up the quantum mechanical calculations of electron density for these compounds. And what did I find? There weren't any in the literature! So I said, "I'll use a calculator." I found that there wasn't a method of calculation. There were fragmentary pieces. So I did various calculations and what I would today call a job of numerology. That is, I demonstrated that you could find MO parameters to make the electron densities correlate with the sigma values. I wrote this up one day before the 75th Anniversary of the ACS at the meeting in New York. I took it to Oscar and asked if I could use it as a paper. He looked at it and said yes and then told me to also write it up as a Ph.D thesis. I gave it to him in June or July. I took two weeks off from work and wrote the thesis in ten days. I always tell these kids today, who spend six months writing a masters thesis, that they are crazy. Of course, you have to know what you are going to write. But the writing doesn't take any time. It shouldn't. You've got to sit down and do it and not worry about it.

What was the sequencing of the thesis versus the famous review article?

Hans as a young professor at UC, circa 1959.

I wrote the thesis and gave it to Oscar in the summer of that year. I borrowed it back when the paper by H. S. Gutowski came out, which sounded like it was doing basically the same thing. Luckily, it didn't. I wrote up a paper so as not to be scooped by him. It got published, and I gave the thesis back to Oscar, who was mostly in the hospital in those days. In March he called me in and said that he had a few changes he wanted to make. I said that I had done much more work and asked if he wanted me to include more. He said no, it would just mean more for him to read.

That Fall, while I was waiting for Oscar's word, I went to a mechanism conference at Bryn Mawr, where C. Gardner Swain gave a big paper on substituent effects. He said, "If I just had 3000 pieces of data, I would do a refitting of the Hammett equation." Then I stuck up my finger and said, "I have never counted how much data I have, but it's a pretty good sized collection." After the meeting closed, Herb Brown came over and said, "Hans, if you have such a collection, you have to make it available to others. That would be extremely valuable." The next morning at breakfast I sat down next to one of the members of the board of *Chemical Reviews* and told him what Brown had said, and asked him if he could get me an invitation to write a review. Two weeks later I had a letter of invitation from the editor and that is where the review paper came from.

It sounds pretty much like you were on your own for the PhD., that you came up with your own problem and worked independently of Rice.

At the oral, Oscar Rice said, "This is Hans Jaffe, who has directed his own research. He ought to be the chairman of his own committee."

I assume that you were totally self-taught as far as Hückel MO calculations went, that you taught yourself by reading the primary literature?

Who could have taught you? I looked at Wheland's papers and others.

At what point did you begin to look for an academic job?

Not right away. After all, I was in Civil Service and I had a job in which I had a lot of freedom and was doing pretty much what I wanted. You can see that from the publications list. I finally had a fight with the boss, not with Doak, but with the M.D. in charge of the entire laboratory. I said I would come in by 8:15 am each day unless I worked after midnight at home. I like to work at night. I didn't like to come in at 8:15 in the morning. It also gradually became obvious that the utopia wasn't going to last. The last six months that I was there, you had to justify every project that took over 10% of your time in terms of the laboratory's overall mission. That was in the days of Oveta Culp Hobby, the "Secretary of Not Too Much Health, Education, and Welfare."

Did you attempt to apply the Hammett equation to drug design?

No we didn't, though others have done this since.

I understand there is an interesting story behind your request for a reprint of Cotton's ferrocene paper while you were at the Center for Venereal Disease.

I was sending for a lot of reprints, including some on ferrocene. When Cotton saw the return address, he thought he had made his fortune. He thought that we had found a medical use for it in treating you-know-what, and that he would be able to sell tons of it. I think I was one of the first to propose a structure for ferrocene. I don't think Moffit was any more correct than I was.

How did you end up at the University of Cincinnati?

Well, the handwriting was on the wall that the freedom

I had enjoyed was not going to continue. Moreover, I was now having children and I didn't like the South for obvious reasons. I had seen enough of race problems. So I decided it was time to get out. I applied to about 150 schools and got two interviews and one offer, and I took it.

What was your impression of the Department when you arrived in 1954?

That's hard to say – not much. There wasn't much to brag about. Hoke Greene was Head but he also worked as Dean of the Graduate School at the same time. He wasn't happy unless he had two heads – at least in those days. He and Ian MacGregor were organic inbred. They hired Hans Zimmer at the same time as me. I don't know if there were any other organic chemists or whether those two were the only ones at that point. Wayland Burgess was approaching retirement in physical, Joe Sausville was physical, Glenn Brown was analytical and Tom Cameron was inorganic. That's about what the Department was. There wasn't much going on here. Glenn Brown was giving out theses that were not doable. They were not theses, they were life-long occupations. He gave one kid the topic "The Effect of Electric Fields on Reaction Rates." It was tough.

Were you hired primarily to teach or was it expected that you would do research as well?

It was assumed that we were going to be doing research. Joe Sausville was working on contract, but was doing research. Ian MacGregor was doing a little. Glenn Brown was doing research, at least nominally. Tom Cameron also had students. I think there were 10-20 graduate students doing research and, of course, there was always the Applied Science Lab across the road. No, I think research was expected, though I don't think anybody knew anything about evaluating it.

I assume, however, that it wasn't the sink and swim situation it is now, where research performance is the key to whether you stay or not.

No, I suppose not. Hoke Greene had published three papers in his life, I believe, and he was the Head. Tom Cameron, who was the senior freshman man, had only published one since his Ph.D. These people couldn't really be evaluators, though Tom Cameron had more depth than anybody was willing to give him.

At what point did you become aware of the existence of Milt Orchin over in Applied Science? As I understand it, he actively attended departmental seminars and other functions before officially transferring to chemistry.

Oh, yes. Milt was always a "member" of the Department. The first month or so that I was here, he invited me out to dinner at his home.

Did Milt have a similar impression about the lack of properly evaluated research that was going on?

That I couldn't tell. It was obvious that Hoke Greene, Dean of the Graduate School, was not being the kind of department head one should have. A year after I came, we got a new President, and a year after that, we got a new head. Milt was the obvious internal/external choice for a head. He was the best chemist around.

Both the Hammett equation and your work on the basicity of organic molecules are essentially research themes that you brought with you to UC. Did your interaction with Milt in writing the books on spectroscopy and symmetry have much of an impact in sparking your interest in the theory of electronic spectra?

Yes, I guess it did. It pushed me more toward spectroscopy. I think that's probably true.

What is your feeling about the impact of those books? They were very influential in making the Department well known throughout the country.

That's probably true. I was most pleased with our green book – the first one – *Theory and Applications of Ultraviolet Spectroscopy*. One day I was talking to

Hans without his bow tie, circa 1960.

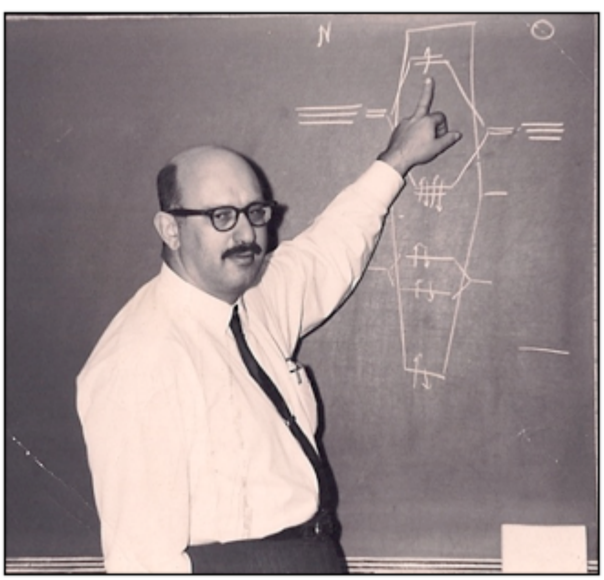
O'Connell, one of the spectroscopists out of Chicago, and he said, "Now, you know that book of yours? Of course it isn't a spectroscopist's book, but when a new student comes to work with me, that's the first thing I give him to read." Now that was a spectroscopist, a serious spectroscopist's spectroscopist. That made me very happy. That comment, by the way, was made not so awfully many years ago and probably 10-15 years after the book came out.

That was probably the best book we wrote and the writing was an interesting process. Each one wrote a chapter, then we switched. And many, many times we threw away the other guy's chapter and wrote our version. After two or three passages like that, you came to something that you both could live with. In other words, we came to the point where I said that it was no longer wrong and Milt said it was understandable. Basically, I was the physical chemist making sure that things were correct mathematically and physical chemistry wise, and Milt made sure they were intelligible.

I recall Dr. Orchin telling a story about finally having to force the completion of that book by going to a hotel out on Reading Road, which was just outside the city limits, so that you could legitimately claim that you were out of town.

We went away for five days – neither home nor office. I'm not sure that anybody knew where we were, and we basically dotted the i's crossed the t's. We never did that on any of the other books. It was a useful process.

You indicated at one time that you thought the best way of characterizing the Department during the Headships of Dr. Orchin, yourself and Tom Cameron was as a triumvirate of some sort in which the official headship rotated.



Hans explaining MO theory, circa 1964.

Well, not quite. Milt used Tom and myself in strong advisory positions – Tom at the undergraduate level and myself at the graduate level mostly. He made me Secretary for Graduate Admissions right off the bat, so I was involved in recruiting students and such. I don't know what Tom had as a title, probably something similar. When Tom became Head, that same kind of division remained; Tom made me Director of Graduate Studies and he basically concerned himself with the undergraduate level, which was more his interest. In that sense, there is a continuity through those years. As the former Head, Milt was the consultant all along.

Did the three of you have an explicit agenda for changing the emphasis and direction of the Department?

No, I don't know that it was that simple or straightforward. Don't forget, until 1955, we had a University President whose great contribution was that he was the official keeper of the statistics of enrollment for the University. I don't know that Raymond Walters was ever interested in graduate work or building up research. Then, in 1955, they brought in Walter Langsam and he initiated the directive for change from the top. Walter Langsam (I disliked the man and always have) is dead now, but he was more in the right direction than Walters. He made this school a reasonable place. I doubt that I would have stayed here very long under Raymond Walters and his appointed Department Head. Luckily, after a year, Langsam (or "Slowly," as the students called him) brought over Milt.

So there was a perception that there was a mandate from above to upgrade the quality of graduate education and research at Cincinnati?

I would think so. It is very difficult for me to say because I never really worry about politics; I never did, but I think that must have been the way it went.

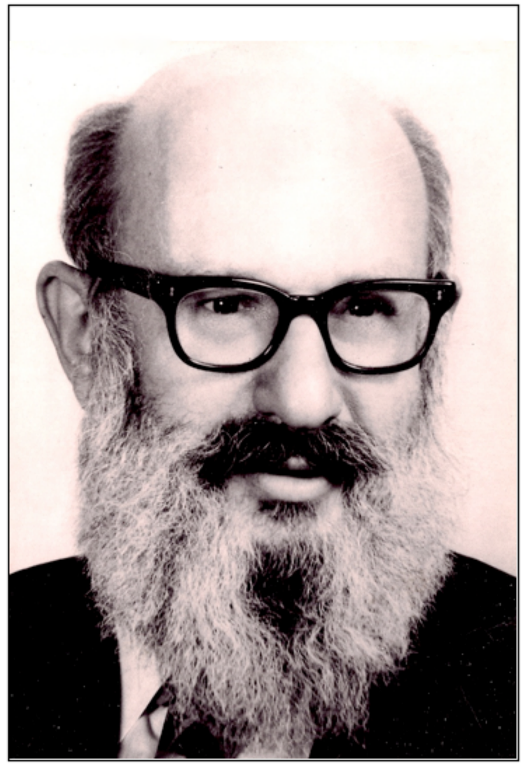
How was the quality of the graduate students? Were you able to find the kind of students you wanted?

No and yes. There are always bad ones and there are always better ones. There is never one that is as good as you wished he was until, all of a sudden, you run into one that is even better than you can keep up with. Mostly they were all over the place. Heavens, did I have peculiar graduate students! There was H. Lloyd

Jones. He was among my "potboilers." You see, I had the "potboilers" and the "button pushers" in those days. He never completed an experiment. He had dicyclopropylmercury sitting on the bench for months but never could make up his mind how to work it up. He was looking for tricyclopropylaluminum.

So you actually conducted an experimental program to confirm calculations or to get data for calculations?

I came out of an experimental background. I was doing experimental work at UC from the start, but in some cases we got into a mess. Gradually the calculations came to work with the experiment. Jones' work was an interesting project. I maintained that the dimerization of aluminum trialkyls was to a large extent a matter of hyperconjugation and cited as evidence the methyl, ethyl, isopropyl trialkylaluminum sequence, along which dimerization decreases. I predicted that, with tricyclopropylaluminum, the sequence should shift back toward increased dimerization. So I had "Jonesy" try to make it. He was the second student to work on the project and I think maybe there was another one after him. The same sequence has since been done by Holmes at Detroit and it was exactly the way I saw it was going to be. But we never did it.



Hans as department head, circa late 1960s.

At what point did the Department develop its present divisional structure?

Milt established the divisional structure. That was strictly Milt's doing and that would have been somewhere around 1956-1958. There wasn't any such structure before that.

Bill Gilbert mentioned that in the early 1960s there was a movement in many chemistry departments, including Cincinnati, to dispose of analytical chemistry and he credits you and Tom Cameron with having successfully opposed it.

That's me. I take credit, but I take it for the wrong reasons. I did not see the handwriting on the wall relative to the coming instrumentation revolution. In 1969-1970 I maneuvered the department a new position at the full professor level. Of course, the Organic Division immediately screamed for another organic chemist. I had to get a vote, but I basically forced it toward analytical and we got Harry Mark. My logic was very simple and straightforward. We were getting 10-20 students who wanted to pursue careers in local industry in our programs every year. Half of these were in organic and the other half were analytical. But we didn't have a significant analytical program for them. They wanted to come and do analytical, so we had to provide them with it. That this became the national trend was pure luck. I fell into it. That was the logic.

You were basically hired to fill a teaching need rather than a research specialty need. At what point did the Department begin to look at research specialties as a criterion for hiring new faculty?

It slowly developed in that direction. I don't think you can say at what point we decided to go that way. Yes, of course, at one stage (5-15 years ago, it's hard to say) we sat down and asked the question, "Should this Department try to hire people in the fields that are not represented, or should this Department be satisfied to be one of the strongest departments in such and such fields." Our answer was the latter. We thought, "Let's not spread ourselves so thin that we have only one man in a field and no depth." But this conscious planning didn't work too well. For example, we hired people like Bruce Ault and Estel Sprague because we really needed spectroscopists. They both run excellent research programs but the programs have nothing to do with being spectroscopists. As in these cases, I think we should always hire the best person and not insist that they do this or that. That's not the way good research is done.

Tell me about becoming Department Head.

I had no ambition to become Head. Milt came to me (he was the chairman of the search committee) and asked me to write down what it would take for me to agree to become Head. I said I didn't want to be Head. Nevertheless I sat down and wrote out a set of demands which I thought would be so far beyond anything the administration would possibly agree to, that I would be safe. There were ten points, the tenth one of which was that I wanted the agreement on the other nine in writing. I made only one mistake. In my request for the TA budget, I forgot that I needed two increments: one to increase the stipend and one to get the number of stipends up. So that one I missed. But basically, the departmental budget, other than faculty salaries, was doubled. I think administratively that was probably my biggest coup. There was no negotiation. The President called me in and said, "Sold."

You initiated the position of Assistant Department Head. My impression is that you have always been careful to guard your creative freedom. be it from the encroachment of routine laboratory work at the Center for Venereal Diseases or petty administrative work.

No, no. It happens to all of us. For instance, through the years that I was Department Head, I couldn't read any of the literature, and I've never caught up – never started again. But, I will say this, when I have gone around interviewing for deanships, as I have occasionally, my description of a proper deanship is the following: the dean's functions are fourfold: 1) to needle the administration for more money for his show; 2) to needle his department heads to run their individual show properly; 3) to needle his faculty as a whole to be productive; and 4) to be available to a few faculty and undergrad students to worry about their concerns. All of this is at best a half-time job. The other half time he should be in the laboratory working as a scientist. The counting of the paper clips can be done by assistant deans. Mechanical administration can be done by somebody else. And this is exactly why an Assistant Head was hired.

Tell me about your experiences with the new building.

I became Director of Graduate Studies in 1962. As a matter of fact, I was in Paris in 1961-62 and they offered me the job when I was over there. This was when Tom became Head. Shortly after, we were told that we could have a new building, that they were going to build a set of four towers in a square and we would be occupying the first, together with Chemical Engineering. We were told to submit our design specifications. So we went and asked the administration for its growth projections for the next ten years and they said, "Who knows!" So Tom and I sat down and we said, "The College of Engineering is pretty stable and does not grow much, give it a 1.5 growth rate for ten years. Arts and Sciences grows faster, give it 2.0. The University College is the fastest growing, give it 3.0. Use these fractions to weight our student population in each of these areas and that will give us the 'official' university growth projections."

Breaking ground for the new chemistry building in 1964. From left to right: Tom Cameron, Fred Kaplan, Hans Jaffé, and Milt Orchin.

It very soon developed that E. M. Kinney was going to be the architectural engineering firm to build this building. Kinney said, "Now you feed us your needs and we will go away for six months and, at the end of six months, we will come back and let you see some tentative plans, and then you can tell us whether you like it or not." We (myself, Milt and Tom) replied that we couldn't do that. Then we got Fred Kaplan largely relieved from teaching so he could run interference for us. Over the next two years, we fought the battle. They said, "You feed us what you need," and we said, "Sorry, we can't do that. The needs depend on the interrelation of what you are building." Finally they assigned us Mattie (Marvin E. Mathewson). Mattie was a good man. Mattie was patient with us and patient with Kinney. I don't think we abdicated on anything.

Two big decisions loom in my mind. One of the first questions that Mattie asked was do we need a head's "head" and the second one was how do you want the "johns" distributed over the tower. My answer to the second question was very simple. Everybody should be able to go to the john within one floor of where they are, but there are lots more men than women in the Department. He retired for two weeks and worked on it. Two weeks later he said, “In that case, what I suggest to you is the floor sequence: men, women, men, men, women, men, men, women,” and I agreed. Well, you know what happened to that. Years later, on the first of April, we found half the men’s restrooms relabeled. The Equal Opportunities Office, or whatever, told us there had to be an equal number for each. I replied that I didn't think they built ladies johns with urinals and, of course, there was a lot of confusion because every one thought the new signs were an April Fool's joke.

Do the women’s rest rooms still have the urinals?

No. I insisted that they be rebuilt (I was Acting Head at the time) and that they pay for it, not me.

That’s one story. The other has to do with the inspection of the building in March, shortly before its final October completion, and coming upon those great floor to ceiling windows in the hallway of A-3. A friend of mine said, “Isn’t it amazing what they can do with safety glass these days!” I was with the architectural inspector and the university inspector, and they said, “Safety glass; what safety glass? They’re regular glass.” I asked if there would be a railing and they said no. I said, “There will be a railing.” When I came back through in September, just two weeks before we were supposed to open, there were still no railings. I asked where the railings were and they said they would get to it someday. I said, “If they are not there the day we open, I, as Head, am going to close this building. I’m not going to have a student fall out!” That’s the kind of language you had to use. The middle building (A-3) they planned with only the fourth floor for the stockroom and the Business Office. They had two 16-foot high floors on the other side for the stockroom and library. I had one hell of a battle convincing them that a 16-foot high library is useless and a 16-foot high stockroom is even more useless and they could, at very little cost, put in another floor. The seminar room was also an afterthought and we had to fight like mad to get it. What would we do without 502 Rieveschl today!

Hans was well known for his blustery argumentative style which largely consisted of yelling as loud as possible at his opponent, as aptly commemorated by this caricature presented to him by his graduate students.

How do you account for the lack of proper lecture rooms in the building? Were you told only to plan the laboratories?

We were told to plan for undergraduate and graduate laboratories and offices. We have some classrooms throughout the Tower because we insisted that we needed seminar rooms where we could meet with our graduate students. They were not supposed to be classrooms. They were not supposed to be assigned to anybody but us, but they took them away from us. Rooms 506A and B were expansion space. We had an agreement from the University, absolute and binding – it was in the NIH and NSF proposals – that floors 10-12, and maybe l3, were expansion space for us. The same is true of the space now occupied by Fashion Design and Art History. Nothing new here, the growth slowed down and we lost the space.

Actually you should never build a tower. A tower is horrid. Communication is horizontal not vertical. But I guess I am not unhappy with the building. We planned it in the mid 1960s. We completed it in 1970 and we didn’t have to make major alterations until 1987. I think most of the things we designed worked, and most of the things the architect designed, didn’t. Like, for instance, I haven’t heard anybody complaining, or only one recently complaining, that we didn’t have enough electricity in the labs. I put color coded dual circuits everywhere.

You became active in molecular orbital calculations at the point when it was becoming a widespread and active area of research. Can you comment on the state of the field at that juncture?

In the 1930s and 1940s, there were the people like Hückel and Wheland. They did calculations that were really, from the point of view of physics, indefensible. And there were the physicists, who would calculate hydrogen atoms, helium atoms, and maybe hydrogen molecule ions. Their calculations were physically correct but chemically useless. The bridge between those two was built in the 1940s and early 1950s. I took a course – it must have been 1948 or 1949 – in quantum mechanics with Oscar Rice, who was a marvelous teacher. I told him afterwards, "Oscar, all this is fine and is interesting – the tunneling of alpha particles out of the nucleus and such but what has all that got to do with chemistry?" At the same time I was starting to do my calculations, via Hückel, on large molecules. But these two things were connected only in principle, not in practice. It was largely John Pople who made the connection.

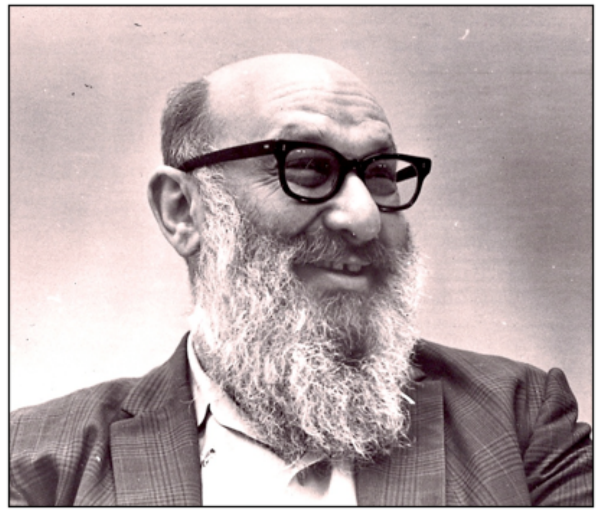
What members of the quantum chemical community did you interact with the most?

I have always been an isolationist.

Next to your work on the Hammett equation, your development of the orbital electronegativity concept is considered as your most important piece of work – indeed one that has filtered down to the textbook level – and your third major contribution is the extension of the CNDO method to the treatment of n - π transitions. Can you comment?*

Those are the three contributions I have made: the Hammett stuff, the electronegativity stuff, and the CNDO stuff. That is also their chronological order. Where did the electronegativity stuff start? Well, it started with the wrong paper. It was a paper that I wrote in the early 1950s, in which I tried to explain why transition metals didn't form carbon-metal bonds, which they didn't at that time (this is before the discovery of ferrocene). What we did was to calculate overlap integrals and, if you go through the periodic system, carbon has small overlap with the alkali metals and alkaline earth metals, medium overlap through the transition metals and a large increase at copper and on through the end of the table. I interpreted the sodium and calcium bonds with carbon as having a very strong ionic component, the transition metals as having insufficient overlap with carbon to form good bonds. Copper and carbon? – explosive! Silver? – nasty! In the *p*-block the bonds are stable. It was a beautiful little paper. It came out just about the time that the carbon-metal bonds started to arrive. I still maintain that I was basically right and that the carbon-metal bonds you do see are all because of some funny thing you are doing to the carbon, either pulling or pushing the electrons in some special manner, but I'm not sure. I haven't followed it lately.

Then I did what one should never do. When you have something that looks neat like this, you should say, "Now let's do something else, rather than see whether we can dot the i's and cross the t's." I had about six theses wasted trying to dot i's and cross t's on that, and part of that was electronegativity. It stood up by itself and worked all right for itself because I gave it to Jürgen Hinze, who knew what he was doing. As a matter of fact, another student, a Vietnamese priest named Joe Zung, had done basically the same job before, but had done it wrong. It never got published because there was too much trouble with it.



Looking more like himself, circa late 1970s.

Before that there was Ivan Goldfarb – I believe my first Ph.D. He was really the start of it. I had gone to a Gordon Conference and Les Orgel had asked me what we were doing. I said that we were calculating valence-state ionization potentials. He told me it had already been done, though it wasn't published yet. Skinner had done it. I reviewed Skinner's paper and I held it up for a year until it was rewritten and redone. Now that the Skinner paper was in press, I pulled out Ivan's work on carbon valence states and quickly wrote it up as a special case study. After this I went to a meeting in Montreal, where everybody told me that Skinner was giving essentially the same paper. I talked to Skinner and it turned out that he had almost the same paper as ours. There was one minor difference in the technique, in that we had a larger paper because we did our calculations using a computer and he had done his by hand. So I asked the chair to combine the discussions for the two papers. There was no sense in discussing the same paper twice. Skinner got up and gave his paper, as he had intended, and I got up and said, "I might as well sit down. We have done exactly the same work, but with the difference that we have explored things more extensively." I also said a bit about what it all meant –

including my concept of the "adiabatic pliers." As you know, a valence state is a state of an atom in a molecule like methane, rather than the ground state atom itself. To get a valence-state carbon, you take hold of the carbon atom in methane with the adiabatic pliers and pull off the protons, and the pliers hold the carbon in exactly the condition it was in the molecule.

As for CNDO, when I came to Cincinnati, one of my first students was Bob Gardner, and he and I (and possibly Lloyd Jones) went and took much of the spectroscopic material that we had accumulated and tried to see whether we could make sense out of it. We did some Hückel calculations and made sense out of the entire sequence of compounds that has two benzene rings connected by a bridge which may be CH=CH or N=N or other variants. We published this and John Platt referred to it as the "definitive" paper on the subject That made me happy. John Platt was the dean of spectroscopists at the time. So what was I going to do after a definitive paper? Do you start somewhere else or get scared into a more definitive paper? Then Dave Beveridge came and he started to do more sophisticated calculations, that is, calculations including electron repulsion. For π-π^* transitions that worked beautifully. But he couldn't handle the n-π^* transitions. I tell this story often at seminars. What does a professor do when a student comes and says that to him. It's obvious – wait until the next student comes and say, "Now you do that." That was Janet Del Bene. Just about that time Pople came to UC and gave a seminar on his not yet published CNDO method. Janet felt that was the kind of calculation we should do and she developed on her own from there. That's how we got into the CNDO.

I note that one of your early research grants on tautomerism was funded by the American Cancer Society. I know that in the 1950s Pullman and others were attempting to correlate the carcinogenic properties of molecules with various quantum mechanical measures of unsaturation. Did you attempt to cash in on this trend as well?

No, I was just fascinated by the game of "proton, proton, Of course the reason it was sent to the cancer people was because the molecule we were studying also happened to be a carcinogen. Milt wrote some bullshit at the front of the proposal. He was always a good BS writer when it came to grant proposals. I think there were also previous publications from the Department in that same area [the work of Francis Earl Ray in the 1940s].

What are your feelings relative to current trends in funding? Sometimes I get the impression that people are doing research to get the funds rather than getting funds to do research. In other words, putting the cart before the horse.

That is what I have always worried about as being the ultimate effect of making funding too important. It happens that way.

What, in your own opinion, is your favorite piece of work? What do you consider to be your most significant research contribution?

Oh, I don't know. That's hard to say. The three things we talked about are the most important. The second one, the electronegativity stuff, is really Jiirgen Hinze's and could never have been done without him, though it's mine too. It was really a cooperative thing. The CNDO? Well, Janet did the basic program, but most of the rest I did and redid. It's not really that wild. Ultimately, the review paper on the Hammett equation which I wrote in graduate school is my favorite. It has stood up and I was still getting reprint requests 20 years later. That paper also single-handedly introduced certain statistical methods into organic chemistry.

Publication History

This is a slight revision of an interview first published in *Chem. Bond,* **1989**, *22*, 4-16. The original publication also contains a complete list of Dr. Jaffé's graduate students and his papers and books.