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Coordination Chemistry Reviews 249 (2005) 275–279

COORDINATION CHEMISTRY REVIEWS

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Celebration of inorganic lives: interview with Henry Taube☆

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Available online 2 November 2004

When I first floated the idea several years ago of preparing the present interview with Henry Taube, I remembered our conversations when I was a postdoctoral fellow in the late 1960s. In those he reminisced fondly about his student days at the University of California, Berkeley, in the late 1930s and early 1940s. Given the spectacular accomplishments of the individuals then at Berkeley, such as G.N. Lewis, Willard Libby, Glen Seaborg, Robert Connick, Taube himself and many others, the Berkeley School of Chemistry was without question 1 of the great collections of chemical talent during the 20th century. I would have been pleased to hear more. However, the interview took a somewhat different course but one that is typically Henry Taube. Instead of reminiscing, much of Henry's focus that day was on the importance of scientific integrity and on various individuals that influenced his views in that regard.

The discussion during the interview was fairly free ranging, and even though I recorded it electronically, there is no way to convey accurately the tone or life that it had. There was a lot of laughing as well as some exchange of some personal information, since we had not had a real conversation in several years. For example, at one point he asked me about my sons, one of whom was born during my stay at Stanford, and exclaimed that he could not believe that they were already grown. (My rejoinder was that I could not believe it either.) However, this interview was not about me or even my relationship with this special person. So I posed a series of questions to him that are summarized below with his responses. These underwent very limited editing to remove some parts of the conversation that were tangential to the interview.

First, some background information that I have drawn in part from comments prepared by Mike Clarke [1].

Henry Taube was born in Neudorf, Saskatchewan, Canada, in 1915. His parents were farmers of German stock who had immigrated from Russia. As a child, Henry remembers his father's plow turning the virgin prairie and uncovering arrowheads of which he had a bucketful. His first language was Low German.

Henry attended the University of Saskatchewan where he received his BS and MS degrees. At the University he was at first timid and would often spend long hours reading as an escape and to improve his English. It is said that he thought to enroll as an English major; however, the registration process disconcerted him, so when he spied a friend, he asked him if he had registered. When the friend replied that he had registered in chemistry, Henry said "Show me how." And so he became a chemist.

After finishing his MS studies, Henry moved to Berkeley where he earned his PhD in 1940. After faculty positions at Cornell and Chicago, he accepted a position at Stanford in 1961. Only recently, he closed his experimental laboratory at Stanford.

Henry has always been interested in oxidation–reduction processes and his very earliest work includes publications on electron-transfer reactions involving ozone and hydrogen peroxide. His published efforts throughout the 1940s centered on oxidation by small molecules containing main-group elements. The use of transition metal complexes in redox reactions began to appear in his work around 1950. In 1952 he published an insightful and comprehensive review that pro-

☆ With some background information provided by Michael J. Clarke.

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0010-8545/$ – see front matter
doi:10.1016/j.ccr.2004.09.010

vided a rational framework for understanding the different rates of ligand substitutions on transition metal complexes. This understanding of metal ion ligand lability led directly to the conception of experiments that definitively showed that electrons could be passed between reductant and oxidant through an intervening ligand. I understand that, since he had a hard time interesting a student in doing so, he carried out the initial experimental studies on inner-sphere electron-transfer studies himself.

While Henry Taube is most widely known for studying inorganic electron-transfer mechanisms, his research has encompassed much more. Early in his career he was the first to determine coordination numbers, geometries and stabilities of solvated metal ions — parameters fundamental to the solution chemistry of the transition metals. He was also the first to apply paramagnetic metal complexes as NMR shift-reagents and used these properties to investigate the coordination spheres of metal ions in aqueous solution. Henry was one of the first to point out the importance of ion-pairing in the reactivity of metal complexes, and carried out pioneering studies in photochemistry. He was among the earliest to use isotopes in delving into reaction mechanisms. In particular, he employed ^{18}O to establish that oxidation–reduction could occur by oxygen-atom transfer. Work done in the late 1960s on the "Creutz-Taube" ion, a ruthenium dimer, precipitated considerable interest on mixed-valence compounds. More recent studies were concerned with the reactions between osmium amines and various organic ligands and represent an interface between traditional coordination chemistry and organometallic chemistry. There is simply no way to give full justice to the influence his work has had on modern chemistry. In 1983 he was awarded the Nobel Prize in Chemistry and in 1985 the Priestly Medal, the highest award of the American Chemical Society. His Nobel citation concludes with the comment "There is no question that Henry Taube has been one of the most creative research workers of our age in the field of coordination chemistry throughout its extent." [2]

Henry Taube has won nearly *every* major award in chemistry, yet those who worked with him as students or postdoctoral fellows value him not only for his scientific achievements but also as a really genuine person. Henry had a remarkable style as a research mentor. He would make his morning and afternoon rounds through the laboratory to chat with students, frequently beginning a conversation with the question "what's new?". This could easily be answered by a brief discussion of research results and ideas or even by commenting on politics or the latest baseball standings. He also had a sometimes disconcerting habit of quickly turning on his heel and walking away, sometimes when one was midsentence.

Chemical and Engineering News once quoted a colleague of his as follows: "Taube always tells me that his students think of their own problems. However, all of the problems come out to look very much like Taube." He motivates through insightful questions and suggestions, indicating possibilities, and drawing out the potentialities of a particular line of thought, yet urging the student do his own thinking on the problem and leaving it to the researcher as to what to do next. Often an intellectual discussion would be accompanied by a wager on a particular result, a bottle of wine being a common currency. He also urged seminar speakers and visitors to spend some time in the laboratory discussing research with group members. His students did not work *for* Henry, they worked *with* him. Some years ago, when I spoke in an ACS symposium honoring Henry Taube, I reminisced in my introductory remarks that the real talent of this man was making chemistry not only challenging and stimulating, but a lot of fun as well.

Henry's impact upon the field of inorganic chemistry is extended by his several hundred former students, postdoctoral fellows, faculty visitors to his laboratory and other coworkers. Since many of these are in university positions themselves, there are also countless scientific grandchildren and great grandchildren.

While he is certainly a chemist's chemist, Henry is equally at home talking about gardening, opera, politics, mystery novels, baseball and even tennis. Variously he has been a collector of all types of items, at one time having a world class collection of old recordings, and a visit to him is likely to leave one better educated on an subject such as expresso coffee makers, antique blow torches or old bottles. He once gave me an expresso pot that he had rescued from a thrift shop and restored. It is a prized possession.

He still loves a good martini.

Interview at Stanford University on 11 August 2003

PCF: Henry, I don't know how this is going to go: this is my initial attempt at journalism. Over the years *CCR* has published interviews of several prominent Inorganic Chemists. I thought it would be fun to try to put something together regarding you.

HT: I'll try to take it seriously.

PCF: Oh I don't think you have to take it too seriously. As preparation for this interview, I have read several short biographies of you, the most recent being an article in the *Journal of Chemical Education*. I can glean information from those, so this should be more about your thoughts and points of view. First, I assume that you've read some of these articles; do you think there is anything that ought to be corrected?

HT: You mean about myself? No, although I don't think they were very interesting.

PCF: Well some of the stories I've heard about you are a bit more colorful, but perhaps you'd rather not see some of those in print.

HT: Probably not.

PCF: In talking about someone's career, one of the important topics that comes up concerns the individuals that influenced your development. Would you like to comment on some of the people that had that kind of impact on you during your formative years?

HT: Well, the beginning is very easy, it was my father. He was of German extraction from the Ukraine where he was of the peasant class who immigrated to Saskatchewan. He was self-taught and was known to nearly every one as "Honest Sam Taube". He always kept his word and I felt that had a large influence on my life and science. Although I've often been disappointed by ideas I had that didn't work out, I learned not to fool myself, not to see things that weren't there, not to disregard the science.

PCF: I saw something along the same lines in one of the articles I read about your learning from your PhD advisor William Bray that it was important to see what was there was to be observed, as opposed to seeing what you wanted to see.

HT: Yes, he was influential. However, I had developed a backbone of my own during my Master's work at the University of Saskatchewan. I had worked with a very ambitious young professor John W. Spinks who wasn't very interested in the chemistry, only his career. That was a difficult period for me. He was very authoritarian and we did not get along at all, but I survived that. I chose to work with him because he was doing photochemistry. The idea of shining light on something and making reactive species and observing what they might do appealed to me. I was following up on some work Spinks had done in England previously, and he suggested that I look at the photoreaction of chlorine dioxide in carbon tetrachloride solution as sensitized by bromine. I was concerned about the purity of the materials so spent several months addressing this. He came one day and said "why are you wasting your time on that, just measure some quantum yields and let's get a paper published." This was a project I did on my own since he was absolutely no help. But in a sense that was a good beginning for me, being on my own.

I was a shy kid but I wanted to do good science and I stuck to my guns even though my mentor disapproved. My father's influence helped me survive this.

It turned out that I had the opportunity to spend the next stage of my career at Berkeley. That was arranged by the Department Chair at Saskatchewan, who took an interest in me. It turns out that Professor Thorvaldsen knew G.N. Lewis from their being students together at MIT. When they parted, Lewis had told him that any student he recommended would be accepted for graduate study at Berkeley. So this is what happened in my case; he took it upon himself to recommend me to Berkeley, I guess because my grades were good. I remember the day when he called me into the office. I was frightened, since I knew that he really had a fear of mercury poisoning and I was using mercury in the laboratory. When he called me into the office, actually his secretary came to me and said "Professor Thorvaldsen wants to see you", I was quaking in my boots. It didn't help when he looked at me rather sternly and said "Mr. Taube, please sit down." Then he said "I've heard from Professor Lewis and you are going to Berkeley." I myself had been thinking about going to Columbia. I had given a seminar on newer developments in physical organic chemistry and I had wanted to do my PhD with Louis Hammett. (*HT* aside: Louis was a very nice man for whom it has been said that he was born three martinis below par (laughs). *PF*: Well I guess three martinis below par sounds like your kind of guy-right? (more laughter)). I had great respect for Hammett. What we learned from him is still very applicable to inorganic chemistry.

PCF: At any rate you headed to Berkeley, but you didn't go to work for Lewis?

HT: No, he was very selective. I don't think it would have done any good to go to him and say, "I want to work with you." He didn't ever have many coworkers. I didn't interview at all widely. I met Bray fairly early, and I decided to work for him. This was a wonderful choice. He didn't have any other coworkers initially. The only other coworker during my graduate studies was Connick, and I was a year or two earlier than Bob. Bray left me to my own devices. He met with me once in a while to offer suggestions, but he made no effort to direct the research. The fact was that he had published a paper interpreting some work in the literature on a reaction of hydrogen peroxide plus ozone. But his analysis did not fit my data for this system; it was a chain reaction. It was a difficult problem. And it turns out that I measured some properties of hydroxyl radicals very early. The reactions in different acids, sulfuric, nitric and perchloric all gave different results, and I guessed correctly that the hydroxyl radical was reacting with the sulfate and nitrate but apparently did not react with the perchlorate. I had a very good time with that, but I was afraid that Bray would be disappointed that his interpretations didn't really apply. However, he was a very honest person; he learned from the facts.

PCF: You were at Berkeley at a very exciting time, there were a number of very good people there at that time. I remember your telling numerous stories about Berkeley, usually after a few drinks.

HT: (Laughs) I don't remember those conversations too well. Yes, Berkeley was very exciting. It couldn't have been better for me; I can't imagine a better place. It was just such a tremendous change from Saskatchewan. At Berkeley, when people met in the hall, they immediately began discussing the seminar they had just heard or some work they didn't understand.

PCF: You went on to Cornell after Berkeley.

HT: Yes, I stayed on at Berkeley as an instructor for a year after graduating. One day a secretary entered, with a message from GN Lewis "you have a job at Cornell", even though I didn't apply there. Actually I didn't have any job but I wasn't really concerned — I guess I was too caught up in the chemistry I was doing. However, it's clear that I really benefited from the "old boys" network. Lewis and Debye were in contact.

PCF: Peter Debye was Chair at Cornell at that time. What was that like?

HT: I didn't have much of a relationship with him. He was really a scientist not an administrator. When my wife and I first arrived at Cornell I went to his office to introduce myself. But I was dismissed rather abruptly; since it apparently didn't register with him that he had hired me. However, I guess it soon dawned on him who I was (I think his secretary told him). As we were walking down the hall, he came running after us "wait, wait, I want to talk to you." I liked him very much. He was a scientist first, but he ran the Department and, what he wanted, he got.

The theoretician John Kirkwood was also there. He had a superior mind and was a nice person.

I worked really long hours in the lab and didn't have any graduate students. I was working under someone else's supervision, but he didn't have the faintest idea about what I was doing.

PCF: Did you have anything to do with the Manhattan Project at this time.

HT: Not really, well that's not quite right. Hoard had a project that had to do with nitrocellulose explosives, and I was on that project. I solved the problem upon being introduced to it, so he put my name on the patent, but he put my name last. I didn't find out until years later that I was even on a patent.

PCF: So at the end of the war in 1946, you moved on to Chicago.

HT: Yes, I had offers from both Chicago and UCLA. I had personal reasons for going to the West Coast, since I had a daughter living there and I wanted to be closer to her. But Kirkwood really encouraged me to go to Chicago. Well, I'm sure that was the best choice I could have made. Finally I was on my own.

PCF: Your early training was in main-group chemistry. What led you to focus on transition metal complexes?

HT: At Chicago I was teaching a graduate course on inorganic chemistry and decided to incorporate the chemistry of transition metals into it. There really was little descriptive chemistry of transition metals in the course. I was teaching myself the chemistry of transition metal elements as I taught the material. The students enjoyed that and helped me learn the material. It was quite a popular course. The fact was that I used teaching as a way to learn, and this is what I was doing when I taught Physical Inorganic Chemistry and started studying transition metals.

It struck me very early on that there were enormous differences in the rates of substitution reactions of metal ion complexes. So when the chance came up to take a sabbatical (sometime in the late forties). I opted to go to Berkeley in part to be closer to my daughter. I spent most of the time in the library. The fruit of that effort was this article that I sent to the *Chemical Reviews* on the connection between electronic configuration and substitution rates. However, I didn't realize that you had to be invited to publish in *Chemical Reviews*. But I just wrote the article and sent it in. The editors at *Chemical Reviews* realized that this wasn't just a summary of past work but that it presented some novel ideas as a conclusion. It was sent out for review and accepted. It was a totally different idea.

PCF: So now we have the combination of your new interests in metal complex substitution chemistry and your continuing interest in redox chemistry going back to your days at Berkeley. That led to your demonstration of inner-sphere/outer sphere electron-transfer reactions, or I am putting words in your mouth?

HT: Yes. The turning point in my career was choosing to look at metal ion chemistry. I owe a great deal to Frank Westheimer. He was the only one at Chicago interested in what I was doing.

PCF: At this time, the field of mechanistic inorganic chemistry was getting exciting and other people began to look at transition metal complex mechanisms. I was wondering if you have any comments about people working in this area?

HT: I always really respected Fred Basolo. He welcomed my contributions. He listened. I really liked Bob Connick. He had no pretensions and really liked to understand what he was doing. Also I don't think there is anyone I like better than Alan Sargeson. So thoughtful about chemistry and very insightful. He is so very generous in his opinions of others.

PCF: What led you to Stanford in 1961?

HT: They needed someone in Inorganic Chemistry, and Ed. King suggested my name to Bill Johnson, who had been put in charge of finding someone. I was invited to visit here for a quarter and I taught the course "Advanced Inorganic Chemistry". I think it worked well for both Stanford and for me. Jim Collman came to Stanford a few years later.

PCF: What is the most interesting referee report you have received?

HT: They came very early in my career. I remember some that rankled, but I don't remember the details.

PCF: What is your opinion of new developments in Inorganic Chemistry. Would you like to look into the future. Where is the field going?

HT: I do like what they are doing in nanotechnology. They are finally beginning to believe in atoms and are putting things together atom by atom. I think if I were to continue in chemistry, I might choose that area to do research.

PCF: One of my first experiences in the Taube Laboratory was the sherry party. This one was to celebrate your receiving an ACS Award. Too much sherry was my result. How did that tradition begin?

HT: I don't remember who started it. It didn't last that long. It went on for several years with the group we had at Stanford then.

PCF: What do you feel your most important contributions have been or will be?

HT: I'm not sure about the "will be". I'm beginning to become interested in small things. I'm collecting bottles now.

PF: The last time we talked about collecting we talked about blowtorches.*HT*: (Laughter) Well that didn't last very long. There wasn't much depth to it. Bottles are different. They come in many different shapes and sizes and they age. What happens to them on aging I find extremely interesting. Old bottles adopt layered structures and diffract light. They look different in different light. This takes me back to my original interest in photochemistry. I'm even doing some experiments with these, it's extremely interesting. The processes are autocatalytic. My son Karl, who is an archeologist, inspired me to collect bottles.

PCF: I must admit I'm not knowledgeable on the topic myself, but I will look forward to your next paper on the subject. OK. That's the future, what about the past? What do you think the are your most important contributions.

HT: Well the turning point was choosing to work in metal ion chemistry. Everything else led from that decision and metal ion reactions became my real passion. Frank Westheimer encouraged me in this endeavor and I owe him a tremendous debt of gratitude.

PCF: What is your secret for inspiring students, for getting them to give the most?

HT: Well I didn't know that I had any secrets. I think students feel that I really want to understand. They like to participate in the generation of new knowledge. I tried to introduce them to questions not answers. I used to bet bottles of wine with them and often lost. I was willing to learn from the facts.

PCF: In science, many people aspire to the Nobel Prize, but few have that experience. How did that event change your life?

HT: It didn't change my work or affect my interest in my work. I really don't know how to answer that. It was very gratifying and nice to get the recognition. But it didn't affect my interest or stop me from working. In one sense I felt I had to justify the award.

PCF: Is there anything else you would like to talk about. Do you have any advice for us young people (don't laugh too hard).

HT: My advice is to keep your eye on the ball and don't be persuaded by your wishes. My negative experiences with my mentor at Saskatchewan taught me a great deal. I learned that I really wanted to know what was going on rather than just getting papers published. We all have ideas that we would like to prove to be correct, but we have to learn that we are limited. There are honest mistakes, but . . . .

What I really enjoyed was doing the research itself. It is hard to stay very interested if I'm not involved, although if I were still teaching, I'd be more interested. I enjoy music, novels, gardening and a number of other things. I'm not trying to keep up with the field of chemistry anymore, but I certainly enjoyed my research career and choosing to work on ruthenium certainly was advantageous.

Some concluding remarks

In looking over the above introduction and interview, it is easy to think of other questions that I might have asked Henry or other things that one might offer about his impact as one of the outstanding chemists of the twentieth century. His contributions to understanding the nature of metal ions in solution, and their substitution and electron-transfer reactions together with their effects on ligand reactivity are fundamental building blocks of modern inorganic and bioinorganic chemistry. Furthermore, there is an obvious connection between his insight into electron-transfer reactions between oxidants and reductants through extended ligands and the continuing effort to understand the nature of such processes through biological macromolecules such as proteins or nucleic acids or through conducting polymers or nanosized materials. However, my own viewpoint is much more personal and focuses on his mentorship. As a young scientist at the beginning of an academic career, I learned from Henry to keep my eye on the facts, not in a way that was deflating or dull, but with excitement and wonder at what was being revealed. That's what made, and continues to make, research in chemistry stimulating and fun.

References

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