

An Interview with Bill Gilbert

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Theodore William Gilbert Jr, or Bill, as he was known to his colleagues, was born in 1929 in Attleboro, MA. He received his B.S. degree in chemistry from MIT in 1951 and his Ph.D. from the University of Minnesota in 1956 for work done under the supervision of Ernest Sandell. This was followed by a year at Oak Ridge National Laboratories and three years as an Assistant Professor at the University of Pennsylvania. In 1960 Gilbert joined the faculty of the University of Cincinnati in the Department of Chemical Engineering, where he taught quantitative analysis to engineering students enrolled in the College of Engineering Coop Program. In 1963 he transferred to the Department of Chemistry, where he remained until taking early retirement in 1987. Dr. Gilbert passed away at his retirement home in Nantucket, MA, in March of 2007. The following interview is based on a series of conversations which took place between Dr. Jensen and Dr. Gilbert on 20 May and 03 June 1987, shortly after Dr. Gilbert announced his retirement.



Theodore William Gilbert Jr. (1929-2007).

Tell me about your parents and childhood.

My father was non-professional. In fact, he never graduated from high school, but he was a bright man. You couldn't stump him on word meanings. He devoured newspapers – really a self-educated man. He went into the furniture business and was a furniture merchant from the time he married my mother until the time he died, and in the shoe business before that. I was born in Attleboro, MA and grew up there, which was the town my father had his business in. Then, after going through Attleboro High School, I went on to MIT to earn a Bachelor's Degree. I went to MIT thinking I would go into metallurgy as a profession and took some metallurgy and chemistry courses, but decided I would prefer chemistry. I did a senior research project with a professor there by the name of David Hume, who just recently retired. Hume was originally a Canadian, but went to work on the Manhattan Project during the war and went from there to MIT. He was a young man on the rise and very much interested in analytical chemistry.

He had been a student of Kolthoff at the University of Minnesota, and I consulted with him at the end of my Bachelor's Degree experience about what graduate school to go to, just like most students do. He recommended that I apply at the University of Minnesota. In fact, he had invited Kolthoff to come to MIT to give a lecture and introduced me to him. We talked and I thought Minnesota might be a good place to do graduate work. I made an application there, along with several other schools, but got an immediate reply from Minnesota accepting me and giving me an assistantship, so I didn't follow up any of the other offers that I got. Although I didn't work with Kolthoff after I got there, I did work with his very first student in the United States, Ernest Sandell, who was also a professor in the chemistry department at the University of Minnesota. I did my work on solvent extraction and trace metal analysis, finishing up my degree in 1956.

Going back, were you an only child?

Yes.

Were there any influences in your family, in terms of relatives, that got you interested in science or was that through a high school teacher?

What interest I did get was through my high school chemistry teacher. Of course, MIT was a very well known institution in Massachusetts and several of my friends were applying there. So I also applied and went with a close friend of mine named Harold MacKay. We shared a room most of the time I was at MIT. He was in mechanical engineering. We were buddies throughout our college days. Both of us were very much interested in science.

Did you actually start in metallurgy and then transfer to chemistry?

Well, at MIT the program is a common program for all freshman. In the first year the curriculum is exactly the same, regardless of what you go into. Being an engineering-science curriculum, they felt at that time all students had to take a common year, so there was no decision to be made really until after I had a year of physics, chemistry, mathematics, engineering drawing and things like that – as well as English.

How did you get into a situation where Hume was able to influence you to the extent that you decided to go into analytical chemistry? Was it a result of a project or a course you took under him?

The chemistry curriculum at MIT was a little different than most in that they had a required summer term for all chemistry majors – not optional – but required. You had to come and take a full summer term of qualitative analysis based on the old hydrogen sulfide scheme. That was between your freshman and your sophomore year. Then in the sophomore year we had two full semesters of quantitative analysis, so there was a heavy emphasis on analytical chemistry. The professors in these courses were David Hume and another professor, Lockhart Rogers – though he was universally known as Buck Rogers. They were young, dynamic men, very active in research, very productive and, of course, they went on to very distinguished careers.

The object of taking the summer term was to free up your senior year from a heavy load of courses and to have a full year of research experience. So I worked with Professor Hume that senior year on a research project which happened to be electrochemistry, which was something I didn't go into later. The project involved the polarography of cyclooctatetraene, which



Izaak Kolthoff (1894-1993).

happened to be a compound that Arthur Cope, who was head of the department and a distinguished organic chemist, had been working on. Hume, being a young analytical chemist, told me that he was going to convince Cope that polarography was good for something. So we studied the polarography of cyclooctatetraene. It was an interesting project.

And the solvent extraction that you got into at Minnesota, was that one of Sandell's interests?

Yes, he was world renowned as an expert in trace metal analysis. He liked to elucidate empirically developed analytical methods. For example, someone would discover a color reaction between a metal and some organic reagent and a recipe would be made. You would dissolve the sample and you mix so many milliliters of reagent with it, wait ten minutes, and read the color on a colorimeter – that sort of thing. But in many cases the interferences were complicated and the mode of action, the stoichiometry of the reaction, and even the oxidation state of the metal in the product were often unknown. So he had many students working on the fundamental chemistry of various analytical methods that were already pretty much established but whose chemistry was not known.

The one I had was the colorimetric determination of molybdenum using toluene-3,4-dithiol as a reagent. We isolated the compound and determined its stoichiometry and its oxidation state. Unfortunately, however, we

didn't determine its structure. We assumed that it would be octahedral because the stoichiometry was three dithiols to one molybdenum and there are two coordinating sulfurs on each and every dithiol, so the six sulfurs would give an octahedral arrangement. It turned out later that Harry Gray discovered, by making the same compound in an anhydrous way (whereas we had worked with aqueous solution, which is a much dirtier way of making the compound) that the compound was an example of the very rare trigonal prismatic structure. The six sulfurs make a little distortion and they switch from octahedral to trigonal prismatic. It would have been nice if we had even suspected such a thing.

Kolthoff is really the founder of a fundamental school of analytical chemistry, and it sounds like Sandell was heavily influenced by his teacher. I recently examined an English translation of Kolthoff's book on volumetric analysis from the 1920s. If you compare that book with previous books on volumetric analysis, the earlier books are exactly as you said, "how-to-do" manuals, whereas Kolthoff's book is resplendent with physical chemistry, theoretical deductions of titration curves, etc. It represents a school concerned with the elucidation of the physical chemistry underlying the analytical processes.

Yes. In fact, Kolthoff really started that in this country. It may be very unfair to single these out – but at the time I think there were three principle centers for analytical chemistry in the nation. One was at Minnesota, the other one was at Princeton, under the direction of N. Howell Furman, and the third one was at the University of Michigan with Hobart Willard. They were the top men, at least in training analytical chemists thoroughly in chemical principles with emphasis on the fundamentals – the idea that you have to understand the fundamentals in order to properly design analytical procedures. So I consider I was fortunate to go through Minnesota at the time.

It strikes me that you went through the PhD. process at what might be called the zenith of classical analytical chemistry, really before the instrumentation revolution, which in many ways has led – if you believe some of the dissenting opinions – to the training of analytical chemists who know a lot of solid state electronics but very little wet chemistry. You appear to have come into the field at the point when the classical approach was peaking in terms of its sophistication. Well, after competing your PhD., you went on to two short-term positions, first at Oak Ridge and then at Penn State. Could you tell us about them?

I took a position at Oak Ridge after graduation and enjoyed it very much. I worked on a little known project, largely because it was highly classified at the time. I suppose I can talk about it now, though I don't know if it's still classified. The project was the nuclear aircraft. They spent huge amounts of money to develop engines for an airplane using an efficient nuclear reactor, which is fantastic. I mean, today we think of this and we're horrified that they even conceived this thing. First, when you think about putting a nuclear reactor in an airplane and taking off, you have to provide shielding for the crew and lead is not exactly the best thing to make an airplane out of. Not only that, but the concept of the project was to use molten sodium as a heat transfer agent operating at about 1000-1200°C, which is white hot, circulate this molten sodium in tubes out the wings of the aircraft to ramjet engines. The air being compressed in the ramjets would encounter this temperature and burst out the rear of the jet and propel the aircraft. Well, even on the ground they were constantly plagued with leaks of this white hot sodium metal. It turns out that the metallurgy of the project was really the thing that sunk it. They could not control the corrosion of the tubes under these tremendous thermal stresses. But think, if the plane was in warfare and just one little bullet hit one of those tubes, why you have white hot metallic sodium flying around. Not only that, but a nuclear reactor smashing into the earth and spreading contamination all over the place.

What use did they have for an analytical chemist on a project like that, or didn't you function as an analytical chemist?

My job was to analyze these alkali metals for the corrosion products that they leached out of the stainless steel tubes – metallic zirconium, metallic titanium, etc.

Was there a differential extraction via the sodium of one of the components?

Yes. They wanted to make the materials out of stainless steel. The problem was that the hot sodium would dissolve the chromium out of the stainless steel in the hot zone and then go to the cool zone in the engines, where it would cool down and promptly crystallize out the chromium again in the flow loop, causing all sorts of resistance to flow and eventual blockage.

So you had a thermal transport process going on?

Right. They believed this was caused by oxygen in the sodium. They also used other alkali metals as well. Lithium was one that I worked on extensively as a heat

transport agent. NaK, sodium potassium alloy, was also used. Well, they believed the corrosion was due to oxygen or nitrogen (as lithium nitride), so we had to determine oxygen, nitrogen, carbon, as well as all of the component metals of whatever alloy was being used at the time for the transport tubes.

Did that involve designing new sampling and detection methods? I can't imagine that there were fixed procedures available.

There were no procedures at all for any of these analyses, so there was a very large effort in analytical chemistry to develop whole new procedures. No one had ever measured parts per billion of nitrogen in solid lithium metal. Why would anyone want to do that? But they had a need for it there, so a whole new technology evolved.

What prompted you to apply for the position at Penn State? Was the Oak Ridge project closing up, or did you consider it to be a short-term appointment from the beginning and not intend to stay there?

No. I intended to stay there, but Oak Ridge is an unusual town, especially for a bachelor. I wasn't satisfied with my social life. A friend of mine at the University of Minnesota named Wallis Lloyd, who had gotten his Ph.D. in chemical engineering, became a professor of chemical engineering at Penn State and said that they were hiring people in the chemistry department. He put me in touch with some people in the department, and I applied for a position there. There was also another contact, a professor by the name of Joe Jordan, an analytical chemist who was trained in Israel – I believe – but did a postdoc with Kolthoff and whom I had met at Minnesota. So we had some contact and he knew me, and I was offered the job at Penn State and stayed there for three years.

At the present analytical chemists are worth their weight in gold in academia. Smaller colleges have to strenuously compete for analytical chemists because they can't offer the same wages that are available in industry. Right now many graduates in analytical chemistry can walk directly into industry with high paying jobs. Were analytical chemists in great demand at that time as well?

Very much so. At the time when I was looking for my first job, following my Ph.D., we happened to have the ACS meeting in Minneapolis. So I signed up for interviews and, knowing that other people were having no trouble getting them, I became choosy. I simply put on

my resume that I was not interested in talking to any petroleum companies and that, of course, knocked out about half of the potential jobs. Nevertheless, I got 60 interviews; 60 companies wanted to talk to me about a job and one of them was for Oak Ridge National Laboratory. So it was a beautiful market in which to seek a job.

Did you find the position at Penn State satisfying?

It was my first academic job and I enjoyed it a great deal. I loved the location in rural central Pennsylvania. However, a serious political situation developed at Penn State. The head of the department, Conard Fernelius, had been head for a long time and believed very strongly in a certain mode of education, namely, that no graduate student should do any classroom freshman teaching. This was a principle upon which he was unmoveable. So all freshman recitations had to be held by a permanent faculty member with the result that the chemistry department, when I joined it, had about 55 faculty. It was an enormous department.

Well, the political situation at Penn State deteriorated and some of the senior faculty were in open rebellion with Fernelius about this. The faculty was too damn big, we didn't get enough graduate students, the faculty couldn't prosper, you have to use graduate students for recitations, etc. It was a long battle. Fernelius left the university and the next step was to weed down the faculty. The year prior to my resignation, they had brought in eight new faculty at the assistant professor level and, though it wasn't told to me, those eight were called in by the new department head and told that there would be only one left at the end of the year and, of course, I was also untenured. The situation was not good to even try to continue.

So when I heard about the incident, I looked around quickly for a new job and went to the ACS meeting in New York. I talked to Tom Cameron about the possibility of a job at Cincinnati. He said that at that time he didn't have a position available, but he thought the Chemical Engineering Department might and that he would contact them and, if they did, he would suggest that I talk with them. And so that came about, and I made a trip to Cincinnati and interviewed at the Chemical Engineering Department. The head of the department at that time was Bill Licht, but he was on sabbatical leave that year and the acting man in charge was a professor by the name of McDuffy, who was a metallurgist. At the time chemical engineering was a part of the Chemical and Metallurgical Engineering Department and McDuffy was acting head, so all negotiations were done with him. I didn't meet Bill Licht until I actually joined the department.

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Did the situation at Cincinnati, with the chemistry faculty divided between the Chemistry Department and the Chemical Engineering Department, strike you as odd?

Yes it did, but it was explained on the basis of the calendar and, on this basis, it certainly made sense. They could not operate a single department with some of the faculty performing on a semester basis, which is the way Arts & Sciences was operating at that time, and others on a seven-week basis, which is what the Engineering College was using – a turn around every seven weeks with a final examination. The calendars were so totally different that there would be no way to function.

They explained that to you while you were interviewing?

Yes. I did think that it was unusual that they had a full staff of chemists in the Chemical Engineering Department for the sole purpose of providing chemical education for engineering students.

How were your duties – your responsibilities – presented to you in terms of teaching versus research?

Well, primarily I was hired to teach engineering students quantitative analysis. It was presented to me that some of the chemistry people in the Engineering College did do research some of it with chemical engineering students, if the projects were properly chemical engineering. Some people did research with chemistry students, but this was more or less at the sufferance of the Chemistry Department, if they wanted to accept a thesis from faculty outside the Department. It was really their prerogative to say yes or no with regard to a proposed Ph.D. thesis. That opportunity was made available to me and my first Ph.D. student was from the Chemistry Department while I was still in engineering, though I think I may have switched over to Chemistry while she was still here – this was Sally Vonderbrink.

Can you describe how both your transfer to chemistry and the University calendar change occurred in 1963?

There had been, for a number of years, a movement to get the entire University onto a common calendar basis. It was eventually accomplished by switching off of the seven-week co-op calendar and the semester system. The compromise was the quarter system used today. It wasn't too long, so you could run a coop system and also have regular instruction. And with that movement to a common calendar, people were saying



Dr. Gilbert as he appeared in the early 1960s about the time he transferred from Engineering to Chemistry.

that now we can have a Department of Chemical Engineering and a Department of Chemistry, and the chemists can be in Chemistry where they belong. However, in the Chemical Engineering Department we had tenured faculty members who had been there a number of years and who definitely did not want to move over to Chemistry, so they were asked if they wanted to move. The only one who considered it was myself. The others were adamantly opposed, for reasons I didn't quite understand in some cases.

Before we move on to the situation in Chemistry, there is a final question I want to ask about teaching in the engineering program. I have taught on both the semester and the quarter system and, quite frankly, find the quarter system much more exhausting. I was wondering what it was like to teach on the seven-week schedule that went with the co-op system. It must have been incredibly hectic.

It was horrible. I didn't like it at all because I had a position at Penn State before I came to UC and they were on a semester system there. I was also used to the quarter system because the University of Minnesota, where I did my graduate work, was on that system, so I was quite used to that length of time for a course. But the seven weeks was really just too chopped up for good education. Its justification, of course, was that the employers wanted the students who were cooping to alternate frequently – it seemed to work out with their jobs. Students didn't get tired of a routine job in seven

weeks, but in a longer period they began to get restless. I think that was the primary reason.

Did you have seven weeks on and seven weeks off, or did you just go from one seven-week group of students to another?

You taught your seven weeks and then the section shifted and you immediately taught over again the course which you had just finished. Then you advanced to another level for two seven week terms, then advanced to another level, repeated it, and then around again to the beginning to fill up the year.

Coming back to Chemistry, what was the situation with respect to instrumentation after you transferred?

It was abominable, to say the least. The instrumentation was shared between Engineering and Chemistry because Engineering taught a course – “Chemical Analysis” – as did the Chemistry Department. So we shared all instrumentation. I don’t think anybody had anything that wasn’t used by the other. There was no private stuff except possibly some equipment that was in the chemical engineering unit operations laboratory. I was allowed to borrow things, like potentiometers, which weren’t accessible in the Chemistry Department, though we had things like an Orsat gas analysis apparatus. You know, glass things filled with concentrated potassium hydroxide solution, sulfuric acid, etc. and bulbs that you raised and lowered.

Like Hempel gas analysis?

Yes. The same sort of thing. We also had an experiment using colorimetry which was done with a Duboscq colorimeter in which you visually match the colors by raising and lowering a plunger that goes up and down in a little cup. So in Beer’s law, instead of measuring the absorbance, we made the absorbances equal by varying the path length, which you read on a little vernier scale.

Were there pH meters?

Yes. We had a Beckman Model G and a Model H or something like that.

What about bench top spectrophotometers?

As I said, colorimetry was done with Duboscq colorimeters, so we didn’t have any electronic instruments.

No IR either?

No, not for teaching purposes. Come to think of it, I don’t know what we did with a lot of the stuff. I do remember that I set up a polarograph with DC circuitry, using just batteries and a galvanometer.

With the exception of chromatography, which we will talk about later, and which came in through your own research, was the drive for large scale instrumentation, like NMR and IR, more from the people in organic interested in the latest means of characterization than from the people in analytical? I get the impression that Dr. Orchin kept hiring faculty who could do NMR, ESR, etc. or even build their own equipment, but that it was largely with the goal of upgrading characterization techniques for people who were doing synthesis than from the standpoint of building up the analytical division.

Yes, I think that’s correct. The major change in my own career here was when we got money for moving the Department in 1970. We got some money prior to the move into Crosley Tower and actually moved into A-1 with the teaching laboratories before the tower was completely finished, so we kept our offices over in the old building and had our teaching labs over here. With that move into A-1 came a nice chunk of money for upgrading the teaching equipment. It was a quantum jump to better physical facilities as far as laboratories were concerned and the equipment. Some of the money was available to us before we moved, so we were already buying the equipment before we moved into the building.

I was told that at one point the Department decided to dispose of analytical chemistry all together, but then discovered it wasn’t as disposable as they had thought. I’m unsure of the timing of this relative to your transfer. Can you comment?

I wasn’t privy to all of the arguments that went on in the Chemistry Department prior to my transfer, which would be the critical arguments. I sensed when I did come over that Tom Cameron was convinced that analytical chemistry should be taught by professional analytical chemists. I think he felt strongly about that, and I think he felt strongly that it should be part of the undergraduate curriculum. Some experimentation was going on at the time, such as putting more quant in with freshman chemistry and trying to phase out analytical as a discipline. There may have been some sympathy in the Department at that time to do that.

Was this a nationwide trend?

Yes. It didn't succeed fortunately, at least from my point of view, but it was a trend that was happening in quite a few places across the country. The Midwest is sort of a bastion of analytical chemistry, whereas I think the prestigious Eastern schools were the ones that sort of began to drop it. My old *alma mater* at MIT had Hume and Rogers, and some younger faculty – David Hercules was at MIT later – and these people were very strong on analytical chemistry. Yet Rogers left and went to Purdue and then Hercules went various places. Now he's at Pittsburgh, and the department de-emphasized analytical chemistry. I guess they still have some people they call analytical chemists, but it's not a department that does analytical chemistry as such any more. I think they have 18 physical chemists – no one that's really an analytical chemist.

So, in summary, to the extent that this trend existed in the early 1960s in the Department, it was really a reflection of people trying to emulate what they saw elsewhere. It wasn't a totally internal thing that arose spontaneously at Cincinnati?

Oh no. This was argued across the country in various departments, as to whether analytical chemistry was just too applied for a pure science department.

Let's move on to your research. I was told that you built what was probably the first gas chromatograph on campus and that you not only built it, but actually paid for some parts out of your own pocket.

I don't think I had the first gas chromatograph. There was a professor here who was in charge of the freshman program in the Chemistry Department by the name of Bill Rellahan, who was an organic chemist by training, and he bought one of those little Varian Aero-graph gas chromatographs at about the same time I was building mine. He wanted to use it for his research; it wasn't intended as a teaching tool. Gas chromatography in the early 1960s was a new thing, and it was a hot new thing. It was an exploding field, and I personally felt that it was something, particularly in chemical engineering, that students should know about as it was revolutionizing the petroleum industry. But it was the same old story – no money around. I wanted to learn about it. Since I had no training in chromatography, I applied for a co-op fellowship with the Union Carbide Corporation, which was offering competitive fellowships to take college professors, bring them to their research laboratories at Charleston, West Virginia, and let them gain some experience in whatever they wanted to work on. So I wrote them a proposal saying I would like to learn something about gas chromatography, that

I knew that Carbide was heavily involved in this field, and that I wanted to come to work for a summer in their laboratories and learn the technique.

So I did that and got the experience, or at least an understanding of what was going on in the field, and came back to Cincinnati. Carbide also gave me a few pieces of equipment. One was a thermistor bridge, which I could use for a detector, but it was electronically too noisy. They said, "Well it works, but it doesn't work perfectly. Take it and use it." So I brought it back with me and I set to work to build a chromatograph and did it very simply – actually in a number of different ways. I bought metal tubing from the Williams Metal Company and the packing materials, and I packed my own columns, coiled them, and put them in the chromatograph. I had a heater, which was just a cone wire-wound type, and I rigged up a little relay circuit with a light bulb. So there was a main heater, which kept the oven hot, and a light bulb that flashed on and off to provide the regulating heat to keep the temperature constant. Then I bought a few fittings and put it all together and that was my gas chromatograph, and it worked. And Rellahan said – this sounds very self-serving – that it worked better than the one that he had bought, at least with regard to sensitivity. We used that chromatograph for a few years. In fact, the remains are still down in my laboratory – I throw nothing away! We used it for quite a few years, five or six, I believe.

I take it that from there your interests branched out into other types of chromatography. How, in particular, did you get into liquid chromatography, and what led to your development of the membrane ion detector?

My graduate work at Minnesota was in the field of chemical separations with Sandell, who was at the time particularly interested in ion exchange. So I had gotten some experience in this area. It was a topic that was talked about a great deal in my graduate days. It also seemed like instrumentation was becoming more and more popular and if you wanted to get research money, if you wanted to get graduate students, and so on – well, they were attracted to instrumentation techniques rather than problems involving purely wet chemistry or classical analytical problems. So it just seemed to me that chromatography was booming. I enjoyed it. I thought it was going to go somewhere and that I would like to get involved. There weren't too many starter grants in those days the way there are now, but the Research Corporation gave out small grants to younger faculty trying to move into new research areas, and the membrane detector was my first successful grant application.



Dr. Gilbert working with a group of graduate students.

The idea just occurred to me that volume change in an ion exchange process is a completely general thing. If you take an ion exchanger in the sodium form and you convert it to the potassium form, that is, exchange the sodium ions which are the counter ions to the sulfonic acid fixed to the resin, the hydrated sodium ion being larger than the hydrated potassium ion, you will get a volume change. In other words, any two ions will generally be slightly different in size and so any time you have an ion exchange reaction, you are going to have a corresponding change in the volume of the resin. It's a general thing that always happens and it happens regardless of what the eluant is, so I thought we could use it as a detector for ions as they are eluting from an ion exchange column. Then the problem becomes one of how to use this change, that is, how can you detect this tiny change in volume? Well, the idea grew and I thought of using a membrane. They did make commercial ion exchange membranes at that time and we tried them out and were able to develop an instrument that would measure the change in volume. But unfortunately, the manufacturers of ion exchange membranes made them so as to minimize the volume changes as much as possible. They cross-linked them as strongly as they could because they wanted to use them in batteries and in applications where the dimensional changes would be deleterious.

Well, we did use commercial membranes for a while successfully, but later on, when I wanted to pursue the matter further, the membranes that we were using had gone out of production and we couldn't get them anymore. Then an interesting coincidence happened. The Engineering Department got a new cobalt-60 source for irradiation, so we began making the membranes ourselves. What we did was to use ordinary Kroger-style Gladwrap and graft the polystyrene on the surface of polyethylene using the irradiation source to make membranes that turned out to be much

more satisfactory than the previous ones we were using. The project continued through several Ph.D. candidates, but unfortunately never caught on elsewhere.

So the detector was never used commercially?

No. We had an arrangement with the Research Corporation with regard to patent rights and so forth. The University was interested in exploring with the Research Corporation the patentability of the detector, but when they analyzed the market, they felt it didn't even warrant a patent, though it should have been patented eventually.

Ironically, a scientist's personal assessment of his own work – his or her own favorite project – doesn't always coincide with the assessment of one's fellow scientists or with that of history. Of all the things you have published, which is your favorite? What do you consider to be your best piece of work?

That's a beaut! Well, I will have to choose one and then qualify it. The project I enjoyed the most was a piece of work I did while on a sabbatical leave at Brookhaven National Laboratories back in 1967. I got into this project because I was doing abstracting for *Chemical Abstracts* and I was given a report from the Hanford Works in Washington in which they had developed a separation technique for removing fission product cerium from their waste stream. That is, they had these radioactive fission products or low-level wastes that they had to clean up before dumping in the Columbia River and they did this using solvent extraction. A problem developed with their ligating agent – citric acid. The system would work fine and then it would slowly fade and the cerium no longer would extract. Interesting! The guys that wrote the paper said, "We think there is a complex formed involving cerium, citric acid and some metal ion that is corroding out of our process equipment." That, for some reason, fascinated me because I hadn't heard of any complexes between two different metal ions and the same ligand.

So when I went to Brookhaven that was what I wanted to work on. I found a very good man to work with – Leonard Newman. I knew he was there before I went, and he became interested in the problem too. We started out seeing if we could discover what we called "mixed metal complexes" – two metals and the same ligand – and we found a whole bunch. We studied in detail the mixed metal complex formation of chromium, indium and citric acid. That was the publication I got the most satisfaction from. He and his staff went on and made mixed complexes of americium, curium and whatever. Anyway, I sort of got him into the field

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and he followed it up at Brookhaven with a series of publications on these kinds of complexes.

And to wind up the story, the very last paper I published with one of my students here at UC – Gabe Abbay – was related to this as well. I wanted to see if I could demonstrate the existence of these complexes through the use of chromatography. To start, we worked with chromium and citric acid, and I told him, “Let’s look at chromium and citric acid alone before we mix in any other metals to see what we can do chromatographically.” We mixed chromium perchlorate, citric acid and sodium perchlorate – that’s all – just three things, and reacted them at 45°C to get the reaction to go and separated them chromatographically by weak ion exchange on a bonded phase column. I expected to get maybe three peaks out. We got 15 peaks – 15 resolved peaks! I just couldn’t believe it, but he repeated it and repeated it. There was a regular pattern for these hydrolytic products, which would go up and down with varying pH and, of course, chromium is sufficiently inert in its chemistry that, on the chromatographic time scale, you can pull them apart. They don’t go back to equilibrium fast enough so you

can get them separated. Then, just to prove it all, I wanted to use another separation technique. We got into isotactophoresis and got the same distribution of species that we got chromatographically. That was an enjoyable paper.

In looking back on your career at the University, how would you rate the relative satisfaction you've gotten from teaching versus research?

I guess I’m going to give you an answer that’s middle of the road – I enjoy both. I know my research career has certainly not been an illustrious one, but nevertheless, I enjoyed it immensely. On the other hand, I enjoy teaching – I really do. I enjoy the interaction with the students. But there is teaching and doing research, and probably the most fun is working with a new graduate student, which combines both.

Publication History

This is a revised version of an interview first published in *Chem. Bond*, **1987**, *21*, 12-19.