

## **LIFE OF A CHEMIST**

### **PART II. A CHEMISTRY CAREER**

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My professional life as a chemist began in 1962 as a postdoctoral biochemist at the University of California-Berkeley. It ended in 2000 when I retired as Research Professor Emeritus (chemistry) at the University of Washington-Seattle – thirty-eight years! My career didn't follow a straight line, but involved positions in five different institutions, three sabbatical leaves, and one year as a visiting program director at the National Science Foundation (NSF) in Arlington, VA.

As a chemist, with a B.A. in chemistry from Pomona College, and a Ph.D. in biochemistry from the University of Illinois at Urbana-Champaign, I spent a postdoctoral year in the Biochemistry Department at UC-Berkeley. My first real job as a chemist, in 1963, was at the U. S. Department of Agriculture Western Regional Lab in Albany, CA, next to Berkeley.

At the USDA Lab, I was able to continue my research along the lines of my work at Berkeley. My supervisor didn't understand what I was doing but allowed me to go on. After a year or so, it was apparent that the job was a dead end. I could continue, but it wasn't very challenging. I then started to look for another position.

Jobs in science at that time were not hard to get. I interviewed for two academic positions, one at Arizona State University and another at the University of New Mexico. I was offered a position at New Mexico but was turned down at Arizona. I declined the position at New Mexico because it wasn't a very good department, and continued to look elsewhere. A physiology professor at the University of Texas Southwestern Medical School in Dallas wanted me to come and collaborate with him on a project. I first told him I wasn't interested, but he persisted and convinced me to come for an interview. In 1965, after offering me a dual position as Assistant Professor in the Departments of Biochemistry and Physiology, I accepted. Interestingly, two other chemists who came to the USDA Lab the same time I did, also were not satisfied and left at the same time for other positions.

The collaboration at Southwestern did not develop into anything interesting, but I was able to get my own NSF research grant and work on my own projects. That position was dependent on at least half my salary coming from my grant – soft money, not a secure situation. When I came, the Biochemistry Department was without a chairman. A couple of years later a well-known professor from

Pennsylvania was recruited as chairman. It was evident that he would bring his people with him. Because of the politics – no academic institution is without politics – I started looking for another position.

I quickly found an opening for a biochemistry professor in the Chemistry Department at St. Olaf College, in Northfield, MN. I applied and was offered a position there as an Assistant Professor. It had a good Chemistry Department, which also offered an opportunity for research as well as teaching – similar to what I had experienced as a student at Pomona College. I accepted and spent twenty-eight years there.

I came to St. Olaf in 1968 with about ten other new faculty members. I was eligible to be considered for tenure after only three years, due to my prior teaching experience at Southwestern. The others had to wait the usual six years. When they came up for consideration, the college decided it was over-tenured. Times had changed – the economy was going bad, and science was not as well-supported. Most of the new faculty who came with me were denied tenure – not because they were no good (most were very good) – and that was a tragedy. Timing is everything, and mine was excellent.

I started at St. Olaf with a new NSF grant. That, and my prior experience, allowed me to resume my research immediately. One of my students asked to do research in my lab during the January Interim – he turned out to be my best student ever. I gave him a project, and by the end of the month, he had produced most of the results. During the Spring semester, we wrote a research paper based on his work, which was published in a major biochemistry journal – not a bad start. After that, I was able to support, from my grant, two or three summer research students each year. That was at a time when science support was waning. I was promoted to Associate Professor in 1973

During my sixth year, I began to think about a sabbatical leave for 1974-75. My research grant, then from NIH (National Institutes of Health), was up for renewal. My graduate advisor, Professor Carter, suggested I write to Professor vanDeenen, at the University of Utrecht in The Netherlands. He invited me to work in his lab. My grant renewal was verbally accepted, and I made plans to move our family abroad for the coming year. At the last minute, before our departure, I called my program director at NIH to verify the details. He said I shouldn't leave until I received the final acceptance letter. We left anyway – a matter of faith – and I received the letter about a month later, after a few sleepless nights. If it wasn't funded, we had barely enough money to live abroad, and no money to return. I learned that at times, one has to decide on faith, otherwise we would not have gone. My research at the University of Utrecht was successful and resulted in two research papers published in a major journal.

When I returned to St. Olaf, my wife, Betty, worked as a lab instructor for a year. After that, she decided to join me as a research technician – our kids were in high school then. This was the start of a collaboration that extended through the rest

of my research career. Her graduate training as a synthetic organic chemist prepared her well. We continued to have several summer research students in our lab each year, supported by my grants. Research during the academic year, when I taught, continued with Betty in the lab.

In 1980 I was up for promotion to full Professor. A final part of the process was an interview with the Tenure and Promotions Committee. One of the questions asked by a sanctimonious, holier-than-thou, religion professor was, "what church do you go to?" I was incensed, but kept my composure, and answered, "I belong to a Unitarian Fellowship in Northfield." Such a question about religion is strictly forbidden by the Equal Employment Opportunity Act. After a subtle hint about hiring a lawyer, I was promoted. These are the perils of working at a religious college. But since I had tenure, they couldn't fire me.

My second sabbatical came up in 1982-83 – all of our kids would be in college then. I decided to spend a year with Professor Ed Dennis in the chemistry department at UC-San Diego, in La Jolla, CA. His was one of the leading labs working in the same area as my research. At the same time, Betty worked as a synthetic organic chemist at Scripps Research Institute in La Jolla. It was a productive year and resulted in two research publications in a major biochemistry journal.

In 1987, a delegation from Lanzhou University in China visited St. Olaf College. I spoke to two of the visitors, from the Chemistry Department, about my research at St. Olaf. A month later I had a phone call from the president of St. Olaf asking me if would I be interested in visiting Lanzhou if I was invited. "Yes, of course," I replied. I then received a letter inviting me to visit – they wanted to be sure I would accept, and not "lose face" if I didn't. They paid for my airfare and all expenses in China. China was opening up at that time, and they wanted to bring in Western experts to "pick their brains." I was happy to oblige. At the University of Lanzhou – Gansu Province, northcentral China on the Yellow River – I lectured for six days on the biochemistry of lipids. The other two weeks, Betty and I toured Beijing, Xian, and Shanghai, staying at each city in a university guest house with student guides.

It was time to think about a third sabbatical leave in 1989-90. I decided to work with Professor Hays Griffith at the University of Oregon in Eugene. My NIH grant was up for competitive renewal in 1989, and I wrote proposals to both NIH and NSF with a change of direction: I proposed to study a bacterial enzyme, which the Griffith lab had begun to research several years prior. My support for that and the following years turned out to come from NSF. Again, it was not until I started work in Eugene that I received written funding approval of my grant – another act of faith! In the Griffith lab, I learned recombinant DNA techniques, which enabled the cloning of a gene for the enzyme, and allowed us to prepare large amounts of the enzyme for our research. Betty got a job as an organic chemist, synthesizing phospholipids for Molecular Probes, a small specialty chemical company also in Eugene. After our return to St. Olaf, we began an

entrepreneurial venture in which we synthesized and marketed some of our research compounds through Molecular Probes.

The year 1993-94 was traumatic for me at St. Olaf. In the Fall I gave a disastrous first exam in my biochemistry course. Although it was not unlike previous first exams, many students received low grades. They complained to the Dean that I was too hard. The Dean caved in and supported the students – "these are good students after all." He allowed students to drop the course or change to pass/no credit, even though it was past the deadline. Several students dropped the course but came to my lectures as auditors. Others opted for pass/no credit and studied minimally to just get a pass. These were pre-meds who would not accept any grade lower A or A-. Otherwise, they thought they would never get into medical school, and their lives would be ruined. At the end of the semester, they proved that they were plain lazy, and didn't want to do the work. The Dean, a real terror, harassed me for the rest of the year. In the politics of academia, Deans are mostly the bad guys.

At the end of that year, I was in Washington, D.C. for a chemistry meeting, and visited NSF to talk to my program director. He offered me a one-year (1994-95) position as a visiting Program Director in the Molecular Biophysics Program. I quickly accepted and asked my Dean for a leave of absence. As program director, I received grant proposals, sent proposals out for review, set up review panels, and approved funding as much as my budget would allow. It was good to get away from the politics at St. Olaf. I vowed that when I returned, I would only stay one more year, not knowing what I would do then.

In the Spring of the year I returned, the College announced a one-time retirement buyout. I jumped at the offer and arranged for a research appointment at the University of Washington-Seattle, where I had a collaborator in the Chemistry Department. In 1996 UW offered me an appointment as Research Professor and gladly accepted half of the research money that I brought with me, for indirect costs. That's a normal condition with research grants at major universities.

Betty worked with me in the lab at UW. As I was no longer teaching, we cranked out more research papers. We managed to stretch out the grant to four years – for the last year Betty worked as a volunteer to save grant money. At that point, the choice was to apply for a new grant or retire. I had lost the competitive drive to compete for another grant and decided to retire. I was 63 years old and accepted that my life as a chemist was over. The last four years at UW were fortuitous, since I then retired in 2000 as Professor Emeritus, with benefits from the university such as free parking and Washington State retiree health insurance. No longer a chemist, my next occupation was as an "unemployed, by choice" folk musician. Retirement is an opportunity to do something you always wanted to do but never had the time. Now I have the time.

I had thirty-eight years as a professional chemist, with fifty-four research publications. I left out a lot of boring details, such as collaborations with

pharmaceutical companies like Hoechst and Hoffman La Roche, international travel to chemistry meetings, and meeting colleagues here and in foreign countries – I was even a driver for Sir Hans Krebs (Krebs cycle) at Southwestern Medical School when he came there for a series of lectures. For an unabridged, technical, boring account of my research in the Chemistry Department at St. Olaf College, see [Lipids and Lipases: A History of Biochemistry Research at St. Olaf College](#). My chemistry web page with my CV and publications list [is here](#).

*Pictures: Stewart, 1959 (Pomona College graduation), and 2015*

**Recommended Reading:** [The Bourbaki Gambit](#) by Carl Djerassi. A prize-winning chemist and literate author, he developed a new genre, *science in fiction*, as opposed to *science fiction*. He described science from the viewpoint of a working scientist – how science is really done, competition in science, and the politics of academia.

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