

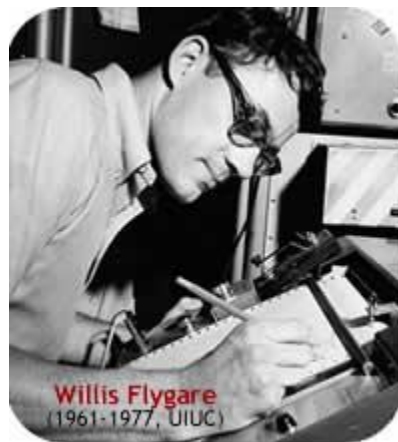
A Remembrance of Graduate School in the 1970's

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I began graduate school in the fall of 1973. I had expected to be drafted into the army and possibly continue my studies at "Saigon U" in Vietnam, but the draft unexpectedly ended and by the summer of 1973 I found myself needing to find a good graduate school rather quickly. Fortunately, the University of Illinois still had room for another graduate student as well as an available TA position. So, by very good fortune, I ended up as a graduate student in the U of I School of Chemical Sciences. Since I had never visited the Midwest, attended high school in the mountains of western Pennsylvania and college in the rolling hills of north Georgia, my initial impression of Champaign-Urbana was "How can any place be this flat?" Upon arrival on campus, I was surprised to find that I would not just help with labs, make solutions, etc. but would actually be teaching chemistry to undergraduates. This became for me one of the most enjoyable aspects of graduate school.

Initially I worked in Professor Willis Flygare's group doing laser light scattering of lipid vesicles. Prof. Flygare had a large group and all of them, except the European post-docs, seemed to be avid rough and tumble basketball players. Flygare himself was quite athletic and a very good player. Once I joined the group, I found that for some reason, several members had nicknamed him Frog or Froggie. He knew this, and must have had a good sense of humor, since it didn't seem to bother him. Among my many pleasant memories are the numerous lunches he had with his group as well as a party or two at his home. About 1974, he decided that his home driveway needed to be repaved with concrete and his group quickly told him that they were going to come out in the middle of the night and draw a frog in the wet concrete. This idea gradually evolved into painting a frog on the driveway. Later, since most of the group really did want to graduate someday, the plan was changed to painting a clear plastic drop cloth and spreading it on the driveway. This was, in fact, done using a large plastic sheet that had a wonderfully well-painted 9x9 foot green frog on it. I believe he enjoyed the prank or perhaps was just relieved that a more permanent image had not been left. I gradually became more interested in NMR than in light scattering, so in 1975 I left his group to begin doing NMR. Although I had only known Prof. Flygare for a short time, I had found him to be a wonderful person. He had enormous energy, and it was clear to even the newest students that he was a gifted scientist. His untimely death from Lou Gerig's disease (ALS) in the early 1980's was a tragedy not only for his family and friends but also for the University and for science.



I first visited the molecular spectroscopy lab to record some proton NMR spectra for Prof. Flygare's book *Molecular Structure and Dynamics*. Although I had used a small spectrometer as an undergrad, this was my first experience with FT NMR and I found it quite fascinating. When Professor Eric Oldfield arrived in 1975, I became his first and, for a short time his only, graduate student. At that time, it was possible to build an NMR spectrometer that was on a par with, or better than, what was available commercially. Although the computer was purchased, within a

few weeks we built the pulsed RF equipment and probe necessary to do solid state deuterium powder pattern NMR, dragged it through the tunnel from Noyes Lab to RAL, and attached it to the 220 MHz Varian superconducting magnet that was the premier instrument in the molecular spectroscopy lab. Using inexperienced graduate students to build NMR equipment that contained high voltages was not without hazard. I recall accidentally wiring a fuse on a 600 Volt (DC) power supply such that it resulted in a particularly unpleasant surprise for my major professor. (Note to current students: electrocuting your professor is, as Martha Stewart might say, not a good thing). I also recall building a kilowatt Heathkit RF amplifier and turning it on for the first time using a very long screwdriver. Our spectrometer, however, worked quite well and quickly yielded at least one publication. By the time one graduated from Eric's group, one had a fair amount of electronics and machine shop experience along with an exceptional understanding of NMR and how an NMR spectrometer works.

The computers of the early 1970's were quite primitive by today's standards. Core memory in our first computer consisted of tiny ferrite beads strung on very fine wires. 4K of memory occupied about 12 cubic inches. I believe our computer had 4 of these modules to yield a massive 16 K of memory. The NMR program occupied 4K so a whopping 12K was left available for data. Initially, there were no disk drives of any kind. If the computer crashed or was turned off, the entire NMR program usually had to be read back in using a paper tape reader. A series of commands (the bootstrap) first had to be entered one-by-one using toggle switches. These commands gave the computer enough information to enable it to read the paper tape that contained the rest of the program. Using what looked like a teletype machine, it took about 20 minutes to read the program in, and about 10 percent of the time, it didn't read properly and had to be started over.

Dr. David Rice (now at Varian Inc.) joined the group a few months after I did. For at least a year, we worked intensively on the synthesis of dimyristoyl phosphatidyl choline lipids deuterated at specific locations. The synthetic work made possible the studies of protein-lipid interactions that formed the bulk of both our dissertations. One particular step in the synthesis used rattlesnake venom to convert lecithin to lysolecithin. The reaction of the venom on the lipid is quite visible over the course of 15 or so minutes. As a result, I've had a particularly healthy respect for those reptiles ever since. Sometime during this period, the lab acquired a nice wide-bore Nalorac 150 MHz superconducting magnet and, much to the relief of the lab manager, we no longer had to tie up the spectroscopy lab magnet.

About 1976 or '77, a second superconducting magnet was brought into our lab. This magnet operated at about 220 MHz, but used a lot of liquid helium. Since the liquifier was located in the physics building we made many, many trips using a small hand cart to haul the 50 liter "space capsule" dewars. This was great fun when the temperature was near zero and there was several inches of snow on the ground. It seems to me now that there were some exceptionally cold winter days during the 70's. I recall one particular day when the high was ≈ 9 and the low was ≈ 23 . These were actual temperatures, not wind-chill factors. Fortunately a post-doc and another graduate student joined the lab and the helium hauling duties could be shared.

I have now worked in NMR almost continuously for 27 years, and I suspect that I am among a very few who actually manage to have a career that is so closely related to their graduate school

work. Being an NMR spectroscopist is still fascinating and definitely more pleasant now. There are really BIG magnets, magnetic resonance imaging has developed into its own field, and there are all kinds of new gadgets and techniques. Computers are much more powerful, but no less aggravating. Fortunately for my coworkers, and humanity in general, I am not called on to do any high voltage wiring, Perhaps best of all, even though I still fill magnets with liquid helium, I no longer have to haul the dewars through the snow of Midwest winters.