Paul C. Lauterbur

The Nobel Prize in Physiology or Medicine 2003

Autobiography

My ancestors apparently emigrated from Europe in the middle of the 19th century; the Lauterburs probably from Luxembourg, and my mother's people, Wagners and Weingartners, from Baden-Baden or nearby. They settled in northern Ohio, where my mother's father, Hans Christian Wagner, married Margaret (Maggie) Weingartner. They lived in Tiffin, Ohio when I was a child, where they had raised my mother, Gertrude Frieda Wagner, her twin brother Joseph, and their youngest child, who became a nun with the name Mary Monica. Nearby lived my grandfather Paul Lauterbur who married a woman of Irish descent, Margaret Hillan. They eventually moved south to Sidney, Ohio and had a number of children, of whom my father, Edward Joseph Lauterbur, was the youngest. He later married Gertrude Wagner (the families seem always to have been acquainted) and they had four children, Thomas who died shortly after birth, me, my younger brother Edward Joseph Lauterbur II (Joe) and my sister Margaret.

We grew up in a house in Sidney complete with a series of dogs, and as the years went by, birds, turtles, newts, fish, snakes, and other animals, and with interesting yards full of trees, bushes and flowers, as well as a nearby park, open spaces and neighbors, some of whom did not resent children trespassing on their property. It was, in memory, an idyllic time. My father worked in the town, as an engineer and part-owner of the Peerless Bread Machinery Company, and my mother kept house with help of a young woman who did some domestic chores and sometimes cared for the children. Although I attended a parochial school, Holy Angels School, I recall little of it except that the nuns who taught there seemed to value order and discipline over all else, which made it especially desirable to evade their control. More influential in my later interests was, I believe, my aunt Anna Lauterbur, who taught in the demonstration school at Ball State Teachers College (now Ball State University) in Muncie, Indiana, just west of the Ohio-Indiana border. She was fascinated by natural history, always kept a terrarium in her elementary school classroom, and gave me a subscription to Natural History magazine. A very gentle person, always willing to listen to a child, she was my favorite aunt.

Because of my parents' hobby of horseback riding, they had bought a farm just outside of town, and we moved there just as I was transferring to the public high school. The farm, with an old but remodeled house, a barn, various outbuildings, fields, woods, and a little creek, was a small paradise to a teenage boy, even though I acquired many duties, such as caring for the horses, mowing the lawn, cultivating the garden, and helping with harvesting. There was also time, of course, for hunting and fishing, collecting snakes, turtles and caterpillars to raise to butterflies or moths, and for general exploration. School was now more interesting also. Not only did I take up the game of chess as a freshman, but I beat the local champions at it, to their great disgust because they were seniors, and then moved on to play a local adult expert, one of the teachers. Classes were a mixture of pleasure and boredom. One of my teachers, who taught biology and chemistry, had the foresight to excuse me and some of my classmates, who were members of the local science club, from his lectures, so that we were free to use the time to do experiments, both standard and wild, in the school lab. He also had the courage to intervene when some of the dangerous ones came to the attention of the school authorities and we could have been expelled. I met him again recently, and his son recalled that when told of my Nobel Prize, he said, "I always knew he would do something like that." After graduating from high school, I went on to Case Institute of Technology, an engineering school now part of Case Western Reserve University, in Cleveland, Ohio, about 200 miles north-east of Sidney. My father had recommended it, because, as he observed, he didn't know what scientists did for a living, but engineers could always get a job. But, given a choice of majors, I chose chemistry.

I had had so-called "chemistry sets" of simple chemicals and apparatus since my earliest years (I particularly liked the pungent smell of burning sulfur), and my own home laboratories even before high school. The curriculum at Case was quite general, including all forms of science (except biology) and engineering, including civil, electrical, mechanical, and chemical, and all of the related technologies such as surveying, mechanical drawing, as well as seemingly endless labs of all kinds, for which I have always been grateful. In addition to the excitement and drudgery of academics, there were also the pleasures and stresses of fraternity life, girls, and culture, as well as new friends and foods. Continuing my habit of doing things a little differently than expected, I wrote a Senior Thesis on my attempt to make an organosilicon free radical, but the advisor for it was an organic chemist who specialized in natural products.

When I graduated (with a B.S. in chemistry, because I did not qualify for an engineering degree as I had replaced a Unit Operations laboratory course with a graduate course in Quantum Chemistry), I was tired of lectures and professors, and determined to get back to lab work. I knew little about graduate study and the structure of a scientific career, so I accepted an offer to work for the Dow Corning Corporation in their Mellon Institute laboratories, where the emphasis was more scientific than technical. I was also told that I could take graduate courses at the University of Pittsburgh free as an Institute employee. There was much interesting work, I found, in our group at the Institute. Organosilicon synthesis, theories of rubber elasticity, techniques of vacuum distillation, elastomer testing, all were new to me and endlessly stimulating. I was particularly fascinated by the puzzle of how small particles strengthened rubber. I even managed to overcome my distaste for academics and take a few courses.

It had long been known that "carbon black" dramatically improved the properties of natural or synthetic organic rubbers, and it had been found that the same was true for silicone elastomers if small particles of silica were used instead of carbon, but not whether surface chemistry was involved or simply physical properties. I addressed one aspect of the problem by substituting phthalocyanine dyes for silica, and they worked perfectly, with their effectiveness decreasing as predicted when the particle size was increased by recrystalization from liquid hydrogen fluoride. Unfortunately, I never achieved a theoretical understanding of the effect, despite intense study of elastomer theory, but I had bright blue rubber and skin.

During that period, I also began to learn about nuclear magnetic resonance (NMR) from various visitors and speakers, and to read a little about that new form of spectroscopy as well. It seemed ideally suited, even at that early date, for investigating the structures and electron distributions in molecules, and various physical properties of materials. Therefore, as part of my graduate education at the University of Pittsburgh, in addition to a "literature seminar" on interstellar molecules, I gave one on a paper describing NMR properties of rubber. Before I could begin a planned collaboration on the hydrogen NMR spectroscopy of silicon compounds, however, my deferments came to an end and I was drafted into the Army and my eventual assignment was proposed to be in the SPP (Scientific and Professional Personnel) program, which my B.S. and two years of work experience qualified me for.

First, however, I was assigned by mistake to a tank battalion at Fort Knox, Kentucky. After hastily correcting that error, I was given eight weeks of minimal basic training and assigned to the SPP program, as planned, at the Army Chemical Center in Edgewood, Maryland. My specific assignment there was in the Medical Laboratories, where I learned to operate an electron microscope to measure the properties of small aerosol particles meant to carry chemical warfare agents deep into the lungs, and I also proposed, and began to set up, a light scattering apparatus to quantitate vapor absorption on aerosol particles. Another aspect of my duties was to capture and weigh experimental animals meant for chemical weapons testing, so that I became skilled, for example, at catching goats in an open field, for which my farm experience was useful. In time, I learned, from a fellow draftee in my barracks, a Columbia Ph.D., that his unit had purchased an NMR machine, but didn't know how to use it. I said, "Hey, I know all about that!", and managed a transfer to help set it up, and arranged for one of my science club buddies, Marlon Shepard, from high school, who had also just been drafted, to join me in the lab, where, among others, we had a drafted Harvard Ph.D. in physical chemistry, Norbert Muller, later a professor at Purdue for many years. We got to work enthusiastically, and I eventually published four papers from our work there, which had turned into a rather unusual opportunity for a young soldier. Perhaps, even more important for my future, I received at least second-hand scraps of a Harvard education, especially the attitudes, from Nobby Muller.

When I was mustered out of the Army, I had to decide where to go next. I even considered regular full-time graduate school, but the appeal of Mellon Institute as a familiar supportive working environment won out, especially after my group agreed to buy me my own NMR machine. When I returned to the Institute I arranged that requisition, tested the machine on a standard organosilicon compound (polydimethylsiloxane) at the manufacturer's laboratory and factory, and impatiently did the initial installation itself when it was delivered. The first critical experiments I did, however, were on 13-C NMR by retuning the instrument, as I had calculated that, if 29-Si resonances could be seen, so could those of 13-C, and a much larger variety of stable carbon compounds existed than of silicon compounds.

My first work in that area, a broad survey of carbon compounds, led to many other publications on various classes of organic chemicals, work that absorbed much of my attention for several years and eventually provided the basis for my Ph.D. dissertation. Finally completing those requirements was stimulated in part by my learning of an academic job offer to me that was planned but never made, because the department learned that I did not yet have the degree, and I had begun to be dissatisfied with Mellon Institute because of some restrictions they had placed on my activities. After I obtained that degree, I looked at several opportunities and selected one in academia, because, as I remarked, "I wanted to be free to try any silly thing I decided to do." One unexpected feature of the job offer, at the State University of New York at Stony Brook, was that it was for the rank of associate professor, so that I went directly to that level, and almost automatic tenure soon after, without even a post-doctorate appointment. I set up another new NMR lab there, and also began to learn the duties and problems of university life while helping to build the department and the institution, and especially, learning to work with students, by that time having gotten over my own distaste for professors by becoming one myself.

During the academic year 1969-1970, I took my first sabbatical leave, spending it in Palo Alto, California, in the group of John Baldeschwieler in the Chemistry Department at Stanford. In addition to the scientific opportunities and satisfactions, there were personal activities as well. I had married Rose Mary Caputo in 1962, and although she was not in good health, we sometimes visited San Francisco and we had two children, Dan and Sharon (who later renamed herself Sharyn) who enjoyed the nearly perpetual summer there. I had an undergraduate student doing work back in Stony Brook who began a new project in my lab there, calculating hypothetical 13-C spectra of denatured proteins from data for amino acid spectra. Two graduate students, José Ramirez and Skip Hutton, also remained to continue their research, mostly of isotope effects on NMR spectra, and I flew back to Stony Brook almost once a month to stay in touch with these activities.

Back in Stanford, I was trying some new NMR-related things. I went up to the Syntex research labs nearby and began research on 3-H NMR of tritiumlabeled pharmaceuticals. Only one paper on tritium NMR of organic compounds, by George Tiers, had appeared, so our discovery that one of the "standards" provided to us by Syntex was apparently not labeled in the position they thought it was interfered with our publishing those observations in the limited time we had available, but led to my later setting up a lab, with a chemistry colleague at Stony Brook, to do more such work. I also began collaborative studies at Varian Associates, in that manufacturer's service labs, of natural abundance 13C NMR spectroscopy of the protein lysozyme in their experimental new superconducting spectrometer, and published the first paper on that subject. I was also working in a lab in the Stanford Medical Center to learn to label a protein, ribonuclease A, with 13C at each of its four methionine residues for eventual NMR study. And, I suppose just to keep busy, I was working with my host, John Baldeschiwieler, and a previous visitor, Barry Shapiro, to his group and friend of mine from Mellon Institute days, to commercialize 13C isotope-enrichment technology developed at Los Alamos National Laboratories. We even started a company, "Kivatec," to use Los Alamos underground distillation methods for that purpose.

It is clear that I was actively beginning to consider biomedical NMR as a new area for application of my skills and knowledge of NMR, partly stimulated by the activities of Oleg Jardetzky, a new member of the Stanford faculty. My intense and detailed involvement in biomedical applications of NMR came, however, from an entirely unexpected direction.

After I returned to Stony Brook, by a long, leisurely automobile drive from California with my family, and settled in again to my department (where I found the same arguments continuing that had been going on when I left) another unexpected event occurred. It had its beginning several years earlier, when a field service engineer for Varian, the leading NMR company, saw an opportunity and asked for my opinion on his idea of starting his own company to make or distribute specialized NMR equipment and supplies. His business plan seemed reasonable, and I encouraged him to go ahead. For a time the company thrived, and I was a member of the Board of Directors.

In May of 1971, however, some other members of the board compared notes with the company's banker and found that the company had engaged in some very dubious business practices and was, in fact, bankrupt. At a hastily-called Board meeting, appropriate actions were weighed, and the banker, there as a guest, threatened to close the company that day unless someone he trusted could be persuaded to take over as President, Chairman of the Board, and Chief Executive Officer. I was the only academic on the Board, the semester had just ended, and the others believed that I was free for the summer, so that I was asked to take the job. I agreed, flew to the company headquarters in New Kensington, PA, near Pittsburgh, at the beginning of each week and back to Stony Brook and my family and students for the weekend.

The developments at the company could supply the plot for a novel, but the incident that is important for my purpose here is that a post-doc arrived with tumor-bearing rats to check the proton NMR relaxation times of their tumors and normal tissues and organs. I was there to observe the experiments, and noted that large and consistent differences were observed for specimens from all parts of the sacrificed animals and that the experiments seemed well-done. Some individuals were speculating that similar measurements might supplement or replace the observations of cell structure in tissues by pathologists, but the invasive nature of the animal procedure was distasteful to me, the data too complex, and the sources of differences too obscure, to be relied upon for medical decisions. As I pondered the problem that evening, I realized that there might be a way to locate the precise origins of NMR signals in complex objects, and hence to form an image of their distributions in two or even three dimensions. That story, and its consequences, is told more fully elsewhere.

Shortly afterwards, I returned to my university for the fall semester, and a colleague took over my company responsibilities. The beginning of the new academic year was a very busy time, and I found some quiet moments to test my ideas about a mathematical approach to such imaging during attendance at seminars and then to consider other practical aspects of the idea as the semester proceeded. In the meantime, I began dropping in on the new medical library of the university, which I passed each morning on the way to work, to spend a few minutes reading, in journals and books, about new developments, problems, and questions in medicine that a new imaging method might address. As I became more confident that these techniques could be both practical and useful, I gradually reoriented most of my research in that direction, then spent almost 30 years on developing its techniques and applications, while chemistry as such became mostly a subject to be taught to students.

An exception, later to be significant, was my general interest in evolution and the origin of life, a topic that I addressed in guest lectures in my university and in selected teaching experiments for undergraduate laboratories. During this period, of course, my children were growing up, as they do, but my marriage was disintegrating. I began to be recognized for my imaging work, and my earlier scientific accomplishments began to be overshadowed by this new direction. At the same time, my efforts to expand the imaging studies, now named MRI by medical doctors, began to be seriously inhibited by administrative and political problems at Stony Brook. My marriage ended in divorce, and I formed a new personal attachment with Joan Dawson, an American physiologist, working at University College, London, whose field was muscle biophysics and physiology, as studied mostly by NMR. If we were to be together, either she needed a new position at Stony Brook or we both needed new jobs elsewhere. After looking at several possibilities, and getting married in 1984, we accepted offers at the University of Illinois.

We moved to Urbana in 1985, with a new baby and high hopes for our professional lives, which were immediately dashed. A plan to share our time between the Urbana and Chicago campuses was foreclosed by technical and political problems in Chicago, and my intended equipment in Urbana, a new whole-body MRI machine associated with a hospital there, was unavailable because of a legal dispute. That problem was never resolved. The hospital eventually sold the machine, but I obtained a small animal-scale machine from the university and began new experiments. My laboratory was organized as the Biomedical Magnetic Resonance Laboratory, initially located in a rented building near campus. When the landlord, a hospital, decided to demolish the building to further its own plans, a small new building was built for my laboratory.

In the late nineteen nineties, that building, including my office, my laboratories, my staff, and all of my equipment, including that provided from university funds in 1985 and those items purchased from external grants over the years, were transferred to another university operation. My wife and I considered looking for new positions, but, in addition to having spent a great deal of time and money building a house, our daughter was in a very good high school, so we stayed. I had a joint appointment in the Department of Chemistry, and moved there, because I had already begun to think about a new approach to the origin of biology from chemistry and wanted to pursue that line of research. Thus, by the time the long-awaited Nobel Prize for MRI was awarded, I had left that field for another (and my daughter had entered college). I am now not only actively pursuing my new research interests, but leaning the new skills in time management required of a Nobel Laureate.

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Paul C. Lauterbur died on 27 March, 2007.