Martin Perl and the University of Michigan

Martin Perl (b 1927 Brooklyn; 1995 Nobel Prize for discovery of the tau lepton)

Below we have intermingled excerpts from Perl's Nobel autobiographical memoir and his Nobel Prize speech that give aspects of his early training, his graduate studies at Columbia , and his eight highly-successful years at Michigan working with Jones, Meyer, Longo, Ting and Glaser.

http://www.nobelprize.org/nobel_prizes/physics/laureates/1995/perl-bio.html

http://www.nobelprize.org/nobel_prizes/physics/laureates/1995/perl-lecture.pdf

College

I was sixteen when I graduated from James Madison High School in Brooklyn in 1942. ... I enrolled in the Polytechnic Institute of Brooklyn, now Polytechnic University, and began studying chemical engineering.

...

One of the first courses I took in college was general physics, using the textbook by Hausman and Slack. The course was all about pulleys and thermometers; physics seemed a dead field compared to chemistry. So, just as I was blind to the fascination of physics in high school, I was once again blind to its fascination in college. I ignored physics, and continued studying chemistry and chemical engineering. ... Chemistry was a very exciting field in the late 1930's and early 1940's ... There would always be a good job in chemical engineering.

Studies interrupted by war

I wanted to join the United States Army, but I was not yet eighteen and my parents would not give me permission. However, they agreed to me joining the United States Merchant Marine, I was allowed to leave college and become an engineering cadet in the program at the Kings Point Merchant Marine Academy. ... In 1945 when the war ended with the atom bomb, I left the merchant marine and went to work for my father while waiting to return to college. I knew so little about physics that I didn't know even vaguely why the bomb was so powerful.

I didn't get right back to college. The draft was still in force in the United States. I was drafted, and spent a pleasant year at an army installation in Washington, DC, doing very little. Finally, I returned to the Polytechnic Institute and received a summa cum laude bachelor degree in Chemical Engineering in 1948. The skills and knowledge I acquired at the Polytechnic Institute have been crucial in all my experimental work: the use of strength of materials principles in equipment design, machine shop practice, engineering drawing, practical fluid mechanics, inorganic and organic chemistry, chemical laboratory techniques, manufacturing processes, metallurgy, basic concepts in mechanical engineering, basic concepts in electrical engineering, dimensional analysis, speed and power in mental arithmetic, numerical estimation (crucial when depending on a slide rule for calculations), and much more.

I was trained as an engineer at and I always begin the design of an experiment with engineering drawings, with engineering calculations on how the apparatus is to be built and how it should work. My strong interest in engineering and in a mechanical view of nature carried over into my career in physics.

Industrial Interlude

Upon graduation, I joined the General Electric Company. After a year in an advanced engineering training program, I settled in Schenectady, New York, working as a Chemical Engineer in the Electron Tube Division. I worked in an engineering office in the electron tube production factory. Our job was to troubleshoot production problems, to improve production processes, and occasionally to do a little development work. We were not a fancy R&D office. I worked on speeding-up the production of television picture tubes, and then on problems of grid emission in industrial power tubes. These tasks led to a turning point in my life.

I had to learn a little about how electron vacuum tubes worked, so I took a few courses in Union College in Schenectady specifically, atomic physics and advanced calculus. I got to know a wonderful physics professor, Vladimir Rojansky. One day he said to me "Martin, what you are interested in is called physics not chemistry!" At the age of 23, I finally decided to begin the study of physics.

Graduate Study in Physics, I.I. Rabi, and Learning the Physicist's Trade

I entered the physics doctoral program in Columbia University in the autumn of 1950. Looking back, it seems amazing that I was admitted. True, I had a summa cum laude bachelor degree, but I had taken only two courses in physics: one year of elementary physics and a half-year of atomic physics. There were several reasons I could do this 1950; it could not have been done today. First, graduate study in physics was primitive in 1950, compared to today's standards. We did not study quantum mechanics until the second year, the first year was devoted completely to classical physics. The most advanced quantum mechanics we ever studied was a little bit in Heitler, and we were not expected to be able to do calculations in quantum electrodynamics.

Second, there was no thought of advising or course guidance by the Columbia Physics Department faculty - students were on their own. I was arrogant about my ability to learn anything fast. By the time I realized I was in trouble, but the time I realized that many of my fellow students were smarter than me and better trained then me, it was too late to quit. I had explained the return to school to my astonished parents by telling them that physics was what Einstein did. They thought if Einstein, why not Martin; I could not quit. I survived the Columbia Physics Department, never the best student, but an ambitious and hard-working student. I was married and had one child. I had to get my Ph.D and once more earn a living.

Ref: http://www.nobelprize.org/nobel_prizes/physics/laureates/1995/

Just as the Polytechnic Institute was crucial in my learning how to do engineering; just as Union College and Vladimir Rojansky were crucial in my choosing physics; so Columbia University and my thesis advisor, I.I. Rabi, were crucial in my learning how to do experimental physics.

.As is well known, Rabi was not a "hands-on" experimenter. He never used tools or manipulated the apparatus. I learned experimental techniques from older graduate students and by occasionally going to ask for help or advice from Rabi's colleague, Polykarp Kusch. I hated to go to Kusch, because it was always an unpleasant experience. He had a loud voice that he deliberately made louder so that the entire floor of students could hear about the stupid question asked by a graduate student.

Thus as in the course work, I was on my own in learning the experimenter's trade. I learned quickly, as I tell my graduate students now, there are no answers in the back of the book when the equipment doesn't work or the measurements look strange.

I learned things more precious than experimental techniques from Rabi. I learned the deep importance of choosing one's own research problems. Rabi once told me that he would worry when talking to Leo Szilard that Szilard would propose some idea to Rabi. This was because Rabi wouldn't carry out an idea suggested by someone else, even though he had already been thinking about that same idea.

It was Rabi who always emphasized the importance of working on a fundamental problem, and it was Rabi who sent me into elementary particle physics. It would have been natural for me to continue in atomic physics, but he preached particle physics to me - particularly when his colleagues in atomic physics were in the room. I think that most of that public preaching may have been Rabi's way of deliberately irritating his colleagues.

My doctoral thesis research (Pert, Rabi, and Senitzky 1955) was carried out at Columbia University in the early 1950’s under Professor Rabi. [I used an extension of the atomic beam resonance method invented by Rabi (for which he received a Nobel Prize in 1944) to measure the quadrupole moment of the sodium nucleus.] This measurement had to be made using an excited atomic state, and Rabi had found a way to do this

My experimental apparatus was boldly mechanical with a brass vacuum chamber, a physical beam of sodium atoms, submarine storage batteries to power the magnets - and in the beginning of the experiment, a wall galvanometer to measure the beam current. I developed much of my style in experimental science in the course of this thesis experiment. When designing the experiment and when thinking about the physics, the mechanical view is always dominant in my mind.

Michigan, Bubble Chambers, and On my Own with Larry Jones

When I received my Ph.D. in 1955, I had job offers from the Physics Departments at Yale, the University of Illinois, and the University of Michigan. At that time, the first two Physics Departments had better reputations in elementary particle physics, and so I deliberately went to Michigan.

Ref: http://www.nobelprize.org/nobel_prizes/physics/laureates/1995/

I followed a two-part theorem that I always pass on to my graduate students and post doctoral research associates:
Part 1: don't choose the most powerful experimental group or department - choose the group or department where you will have the most freedom.
Part 2: there is an advantage in working in a small or new group - then you will get the credit for what you accomplish.

At Michigan I first worked in bubble chamber physics with Donald Glaser. But I wanted to be on my own. When the Russians flew SPUTNIK in 1957, I saw the opportunity, and jointly with my colleague, Lawrence W. Jones, we wrote to Washington for research money. We began our own research program, using first the now-forgotten luminescent chamber and then spark chambers.

In eight wonderful and productive years at the University of Michigan, I learned the experimental techniques of research in elementary particle physics (scintillation counters, bubble chamber, trigger electronics, and data analysis) working with my research companions, Lawrence Jones, Donald Meyer, and later Michael Longo. We learned these techniques together, often adding our own new developments. One of the most pleasurable experiences was the development of the luminescent chamber by Jones and me with the help of our student Kwan Lai (Lai, Jones, and Per1 1961). We photographed and recorded the tracks of charged particles in a sodium iodide crystal using primitive electron tubes which intensified the light coming from the track.

Jones and I, using spark chambers, carried out at the Bevatron a neat set of measurements on the elastic scattering of pions on protons (Damouth, Jones, and Perl 1963; Perl, Jones, and Ting 1963). Later, after I left the University of Michigan for Stanford University, Longo and I, working with my student Michael Kreisler, initiated a novel way to measure the elastic scattering of neutrons on protons (Kreisler et al. 1966).

These elastic scattering experiments pleased me in many ways. The equipment was bold and mechanical, with large flashing spark chambers and a camera with a special mechanism for quick movement of the film. Data acquisition was fast, and the final data was easily summarized in a few graphs. But I gradually became dissatisfied with the theory needed to explain our measurements. I am a competent mathematician but I dislike complex mathematical explanations and theories, and in the 1950's and 1960's the theory of strong interactions was a complex mess, going nowhere.

I began to think about the electron and the muon, elementary particles which do not partake in the strong interaction. ------(Perl's lecture continues to describe the work on leptons that won him the Nobel Prize)

Comment

It is interesting that three of Perl's fellow Columbia graduate students were also on the Michigan physics faculty in those days: Peter Franken (Kusch, Nobel 1955), Gabriel Weinreich (Rabi, Nobel 1944), and , arriving just after Perl left for Stanford, T. Michael Sanders (Townes, Nobel 1964)

Ref: http://www.nobelprize.org/nobel_prizes/physics/laureates/1995/