MARTIN DEUTSCH: A SCIENTIFIC AUTOBIOGRAPHY

I was born in Vienna in 1917. Both my parents were physicians actively engaged in research and teaching. It seems to me that everybody that came to our house had a doctorate of some sort. It was always taken for granted that I should become a scientist, although as late as my twelfth year I seriously considered electrical engineering as an alternative. The Real Gymnasium (high school) where I obtained seven years of my secondary education, was very academically oriented. Victor Weisskopf graduated from this school the year that I entered it. Almost twenty years later, when I first met him in Los Alamos, he produced a photograph of the teacher who had told each of us that we would end in the gutter. In February 1934, I moved rather abruptly to Zurich, Switzerland, having participated in the resistance movement to the Fascist seizure of power in Austria. Thus I acquired a "federal" matriculation as the Swiss secondary school diploma is called. I also completed a spring semester at the Federal Institute of Technology.

In October of 1935, I accompanied my mother to the United States, fully intending to return within a few weeks to continue my education in Europe. The Ethiopian war broke out while we were on the high seas and I decided to stay in America waiting for things to quiet down in Europe. I knew nothing about American universities and it was not easy to get advice. One well-meaning friend of my parents, when told that I wanted to study physics, responded, "Oh, yes, there is a lot of money in pharmaceuticals." Somehow, I ended up at M.I.T. with permission to try for a bachelor's degree in two years. This I did, and then continued on to my Ph.D. in 1941. My fellow students were a hard-working lot, but a few of them were a good deal more than that. Richard Feynman and Charles Kittel stand out in my memory. The strength of the M.I.T. physics department at that time was in theory led by John Slater. The strongest experimental group was in optics, led by George Harrison and his spectroscopy laboratory. It was Harrison's junior course in atomic physics that had the greatest influence on me. The course was totally disorganized, and seemed to consist of a series of scientific anecdotes or vignettes. Somehow this style kindled my enthusiasm, and I still charge many of the insights into physics and the creative process which I acquired there to this influence.

I have always been a hard worker, but a lazy learner. Therefore, I naturally gravitated to the relatively new field of nuclear physics where there was much to do but not yet too much to learn. Robley D. Evans was building a group in this field at M.I.T. His interest lay largely in biomedical and geological applications, but his well-organized style and catholic tastes in physics taught me a great deal. He supervised my Ph.D. dissertation but I think that even then I found it hard to accept guidance and went my own way. Those were hard days for physicists, and Arthur Roberts worked at M.I.T. on a biomedical research grant to use radio iodine in thyroid tracer studies.

While I was a graduate student, he and I and Sanborn C. Brown, another graduate student, decided that we would develop a systematic program for the study of radioactive decay schemes or nuclear spectroscopy. This we did. Sandy was an expert on Geiger counters which were then the only practical radiation detectors available. He later built this early interest into a career in the study of electrical discharges in gases and plasma physics. Our paths crossed again in the positronium experiments.

Arthur was an electronic circuit expert. He knew all about the new techniques of electrical coincidence measurements invented by Bruno Rossi. My contribution was the development of a magnetic spectrometer for β-ray spectroscopy, suitable for the new "artificial" radioactive sources. M. Stanley Livingston came to M.I.T., and soon his new cyclotron replaced our radium-beryllium neutron source as a producer of radioactive samples.

In 1941, when I received my Ph.D., we were unraveling radioactive decay schemes on an assembly line basis, not very well by present standards, but well enough to be of scientific value. By then, war and threat of war had drawn most of our group to defense work. I was still an Austrian citizen (in fact, since 1938, due to the German occupation of Austria, a German subject) until 1941, and thus could not join them until later. By the time I finally cleared all hurdles of security investigation (not only had I been an "enemy alien" but also an active anti-fascist) the urgent radar work at M.I.T. was adequately staffed and I was limited to minor work on sonar techniques and to the teaching of physics to Navy technical personnel. This left me some time to continue my own research until finally, in 1943, I succeeded in joining the Manhattan project in Los Alamos. Much has been written about the unique atmosphere of "the hill" in those years. It was a unique opportunity for a young man, two years after his Ph.D., to work with almost all the great men in the field—Bohr, Fermi, Bethe, Segré, Oppenheimer and many others. I had the good fortune of working closely with Emilio Segré and his group on problems of fission physics rather than weapons technology.

In 1946, I returned to M.I.T., which had become a very different place than it was two years earlier. Jerrold Zacharias was building the laboratory for nuclear science in engineering. Bruno Rossi and Victor Weisskopf had joined the department. The next five or six years were harvest years for me. Ideas that had germinated during the war years led to others, and somehow, it seems to me now, everything I touched turned to gold.

While reviewing my journals and other records, in preparing this article, I find myself incredulous at the pace at which new results and new ideas came to fruition. The positronium story was the high point and, in some sense, the end of this period.

In 1953, I spent a sabbatical year in Paris, searching for new departures without much success. I believe that my experience is not uncommon. The next few years brought some interesting results: the determination of positron helicity in β-decay, measurements of nuclear magnetic moments in hyperfine interactions and others. But somehow, the thrill was gone. I was becoming an "expert."

In 1960, I decided to call a halt. Together with a graduate student, Rae Stiening, I spent an academic year at the Frascati Electron Synchrotron Laboratory near Rome, to learn the field of high energy physics. I was fortunate to find that my old expertise in Geiger counters gave me a good start with the new techniques of spark chamber detectors. We returned to M.I.T. with a good research plan to study the elastic scattering of high energy γ-rays. In the course of these experiments, we developed the first completely automatic computer scanning and measuring system for spark chamber photographs. Rae Stiening and I collaborated again after he moved to Berkeley on an experiment studying the β-decay of the K^+ meson.

Techniques have changed over the years and so has the language used to describe the problems. But I had a strong feeling of continuity between our first clumsy attempts to study the β-ray spectra in the decay of radioactive nuclei and our latest elaborate experiments to study the corresponding spectra in the decay of elementary particles. The main difference is the fact that now I am much prouder of the achievements of my students and collaborators than of my own. □