GERSON GOLDHABER: A SCIENTIFIC AUTOBIOGRAPHY

I was born in Chemnitz (now Karl-Marx-Stadt), Germany, in 1924. In 1933 my father made the wise decision to leave Germany even though it meant breaking up the family. My sister and my brother, Maurice, moved to England while my parents, an older brother and I moved to Cairo, Egypt, where I attended an English high school. In 1942, I began to study physics at the Hebrew University in Jerusalem.

My passion for science experiments, and particularly physics, had grown throughout my high-school years. Although my physicist brother Maurice and I did not see each other between 1933 and 1948 while he was at the Cavendish Laboratory in Cambridge, England and later at the University of Illinois, we corresponded, so that I was probably influenced by his example to pursue nuclear physics or, at any rate, physics at the highest energy available. At the Hebrew University in those days that meant X-rays, and I completed my Master's thesis under Ernst Alexander on a crystallographic study with X-rays.

During my years in Jerusalem I met my first wife, Sulamith Löw, who was then a chemistry student. We were married in 1947. The same year, we both received our M.S. degrees and were admitted to the University of Wisconsin Graduate School, I in physics and she in nuclear chemistry. At Wisconsin, I shifted to the next higher energy range: research with an electrostatic generator developed by Ray Herb. There I studied nuclear resonances in elements excited by bombardment of protons with energies up to 5 MeV. For my thesis work, under Hugh Richards, I devised a new—and in retrospect cumbersome—method for γ-ray spectroscopy. It consisted of loading photographic emulsions with D_2O and then observing the recoil protons from the photodisintegration of deuterium. This work also introduced me to the beauties and power of photographic emulsion techniques. However, as I was doing all my own scanning, it also soured me on the method for a while.

After receiving my Ph.D. in 1950, I accepted an Instructorship at Columbia University and at last moved to high-energy physics—I had graduated to work at a 340-MeV cyclotron! Life at Columbia was very stimulating; I was surrounded by excellence. The Department was chaired by Polycarp Kusch and Charles Townes, while I. I. Rabi was always a strong presence. Among my students in the elementary physics courses were Gary Feinberg, Jack Leitner, Mel Schwartz, and Nick Samios, to mention a few, while Leon Cooper was one of my Teaching Assistants. At the Nevis Cyclotron, just recently designed by Jim Rainwater, Leon Lederman and Val Fitch were among the graduate students, while the competition for running time was with Jack Steinberger.

Following my work with Dave Bodansky on the development of scintillation counters for pulse-height measurements, I decided to return to the nuclear emulsion technique. I was inspired by the beautiful work of Gilberto Bernardini on π meson interactions, and realized that my loading techniques would be a good method for studying $\pi^{\pm}$ p and $\pi^{\pm}$ d interactions.

When Sula completed her Ph.D. thesis, she and our son Amos Nathaniel joined me. Sula began work as a nuclear chemist at Columbia University's Nevis Cyclotron. We started perfecting techniques for emulsion-soaking in various liquids and Sula's transformation into a physicist began. It was also the beginning of our joint experimentation which lasted until her sudden death in 1965.

The most ambitious application of my soaked-emulsion technique was an experiment with Leon Lederman to find out whether the sharp rise in the π^+p cross section that Fermi had recently discovered was indeed a resonance, a feature which had then been brought into question by the Fermi-Yang ambiguity[1] in the phase shifts. To get pions of sufficiently high energy we had to expose the wet emulsions inside the vacuum tank of the Columbia University Cyclotron! Our idea was to measure the differential cross section at the peak of the resonance. Alas, the energy of the maximum was not yet known and we missed the peak by having one exposure just below it and one just above.

In 1953, I joined the faculty of the University of California and Emilio Segrè's research group at the Radiation Laboratory in Berkeley. Sula had joined the Lofgren group at the Laboratory as a physicist. As soon as the Bevatron started its first test run, Ed Lofgren agreed to place some re-entrant cavities as close as one could get to the Bevatron target for the study of short-lived particles, and both Sula and I soon started exposing emulsion stacks at the Bevatron. The early days of the Bevatron were filled with fun and excitement. We studied K mesons; initially we were intrigued by the fact that they existed at the Bevatron and decayed into τ's and θ's. Later, in cooperation with Aihud Pevsner and Dave Ritson, then of MIT, and my first student, Joe Lannutti, we discovered that the K^+ had a much longer path length than the K^-, that the Σ^- was heavier than the Σ^+, and many other facets of strange-particle properties. We showed from an example of scattering off hydrogen in emulsion, that a given θ event had the same mass as the τ meson. Luis Alvarez, together with Sula, used our close-in emulsion exposure in conjunction with exposures at a greater distance to measure the τ lifetime. These, in addition to the elaborate emulsion experiments of Bob Birge, Roy Kerth, Don Stork, Don Perkins, Chaim Richman, and others on the τ and θ masses, and the subsequent precise lifetime measurements by counter techniques, and Dalitz's elegant τ analysis, clearly established the τ-θ puzzle[2].

Then came the era of the antiprotons. In the Segrè group we mounted a two-pronged attack on antiprotons: while Owen Chamberlain, Emilio Segrè, Clyde Wiegand and Tom Ypsilantis set up the Nobel Prize-winning counter experiment to measure the mass of negative particles, Warren Chupp and I in collaboration with Eduardo Amaldi and coworkers of the University of Rome started an emulsion experiment to detect antiprotons by their annihilation. As it turned out, we were too clever for our own good! We assumed that antiprotons would be produced at fairly high momenta, and decided to slow them down in an absorber and have them stop in our emulsions. This was a mistake; we did not reckon on antiprotons having large cross sections—which was later shown to be so—and that slowing them down would make almost all of them stop in the absorber! Shortly after the antiproton was discovered, Gösta Ekspong and I proved, with lower momentum particles in our emulsions, that these negative particles of near-protonic mass—which had already been observed in the counter experiment—were indeed antiprotons! We observed that more energy was given off in the annihilation than the total energy of the incoming antiproton. I spent the next few years studying the antiproton annihilation process in greater and greater detail.

1. For a resonance in a given angular momentum state the phase shift at the peak passes through 90°. C.N. Yang had pointed out that the data observed by Fermi and coworkers on $\pi + p$ scattering could also be fitted by a combination of small phase shifts in several ways. What was needed to resolve this Fermi-Yang ambiguity was a measurement of the differential cross section in and around the peak region.

2. For further discussion see: *Adventures in Experimental Physics*, γ(3), 94(1973).

My first experiment using bubble chambers was carried out in 1959 in the propane chamber built by Wilson Powell, Bill Fowler, and coworkers. I decided to continue the study of antiprotons. As we then knew antiprotons to be a good source of pions, we decided to look for the just-predicted π-π resonance. While we did all the right things, our statistics did not allow us to find the ρ meson. With my students, Ted Kalogeropoulos and Wonyong Lee, we did, however, find a new effect for pions, a difference in the behavior of like and unlike pion pairs, sometimes known as the GGLP effect. And with Bram Pais, we even made an excursion into the theoretical interpretation of the effect, in terms of Bose statistics.

After my return from a sabbatical at CERN in 1961, it was clear that the hydrogen bubble chamber was the wave of the future. We designed a K^+ beam, and in conjunction with Harold Ticho, performed our first experiment in Luis Alvarez's 15-inch hydrogen bubble chamber studying K^+ interactions in both hydrogen and deuterium. We were able to establish that K^+ mesons are very different indeed from K^- mesons—no formation of resonances in the K^+-nucleon system (this absence was one of the stepping stones that led Gell-Mann and Ne'eman to predict the Ω^-).

Next we went on to higher energy K^+ mesons and finally hit on resonances. With Willy Chinowsky and my students Wonyong Lee and Tom O'Holloran, we measured the spin of the then newly discovered $K\pi$ resonance, the K^*.

In 1962, I teamed with George Trilling to form a research group which included Sula, John Kadyk and John Brown. Then came more detailed studies of resonances: the discovery of the A meson with my student, Ben Shen; and the study of the Q enhancement.

In the autumn of 1965, Sula and I took off for a half-year sabbatical. As it happened, we managed to get only halfway. The day after we arrived in Madras, India, where we were scheduled for a month's lectures at the Math Science Institute, she suddenly took ill, went into a coma and died within 48 hours. In the difficult period that followed, what helped me most to return to physics research was an obligation Sula and I had undertaken: a review article on Meson Resonances[3].

3. Cool, R. and Marshak, R., *Advances in Particle Physics,* Vol. II (Interscience, 1968).

Meanwhile, the group George Trilling and I headed continued to study π^+p and K^+p and K^+d interactions. Then followed the observation of the ω-ρ interference with Don Coyne; a study of the Q meson and, later, the discovery of the anti-omega-minus with Alex Firestone and my student, David Lissauer.

In 1969, I married Judith Margoshes Golwyn, a science writer at the Lawrence Berkeley Laboratory. We now have two daughters, Michaela and Shaya.

By 1971, the construction of the SPEAR ring was well underway. Next to be tackled was the problem of the construction and software implementation of the detector. We agreed, as a group, to join in this exploratory experiment. The following year, on a sabbatical at CERN again, I worked with Carlo Rubbia's group at the ISR, and made my latest major switch from bubble chambers to electronic detectors. The outcome of that switch is related in my Discovery Story.☐