BOGDAN MAGLICH: A SCIENTIFIC AUTOBIOGRAPHY

I was born on August 5 1928 in Sombor, Yugoslavia. My father was a lawyer; my mother, although she graduated with a degree in philosophy, was a housewife. My parents had little interest in science, but my father had a keen interest in engineering and wanted me to be an engineer to build wide span bridges.

When Germany, Italy, Hungary and Bulgaria invaded Yugoslavia in April 1941, I was in my second year of *Real Gymnasium* (8-year high school). To escape from the Hungarian occupation, my mother, brother, sister and I fled to Croatia; but there we were imprisoned by the quislings in a concentration camp for Serbs. My mother succeeded in getting us out of the camp in September 1941, by proving that we were residents of the Hungarian-occupied zone of Yugoslavia. But when Fascist pogroms on Serbs began there that winter, we fled Sombor to Belgrade, a German-occupied zone, where we joined our father.

The high school work in occupied Belgrade, consisted only of weekly assignments and final exams. Throughout the war, I was privately tutored daily by my namesake uncle, a retired Yugoslav artillery general of Austro-Hungarian schooling and gallantry. In 1945, my family moved to Vukovar. There, the process of my conversion to Physics began. The inspiring lectures on the atomic structure by my chemistry teacher Alexander Schweitzer (now Alexander Sharon, in Israel) were like sparks, illuminating horizons. Fellow physics enthusiast Zarko Stojanac and I read all the supplementary, semi-popular scientific books that we could get and had long discussions every night. By my seventh year of high school (age 17), I understood some special relativity and some atomic and nuclear physics.

I entered the University of Belgrade in 1946, attracted by Professor Pavle Savitch who in 1938-9 had done experiments with Eve Joliot-Curie in Paris on the bombardment of uranium with neutrons—experiments which led to the discovery of fission.

I did my B.S. thesis in Physics with Tihomir Novakov and Steven Koicki at the newly built Institute for Nuclear Sciences in Vinca (10 miles from Belgrade), where we had moved to live. The experimental program there was run by Robert Walen who came from the Institute Curie in Paris. The group I joined had successfully and rather quickly built a 100 KeV neutron generator. After my graduation in 1950, I began working with the new 1.4 MeV accelerator, measuring the polarization of protons from dd reactions. Using emulsions, I observed that the polarization had a negative sign; but when I later showed the data to some experts in England, they convinced me that I must have confused the directions and signs, as often happens. So in 1958 I published only the value of the polarization without including a sign, which implied a positive sign to the reader. A few years later someone told me that at the energy of my experiment, the polarization was found to be negative! I then made a firm decision that I would henceforth publish exactly what I observed irrespective of established theory or other expectations and opinions.

In 1954, I met Professor Bernard Feld of MIT who was the first official American visitor to view Yugoslav work on nuclear research. In the fall of that year, I went to Liverpool, England to begin my Ph.D work. Working with

Brian Hird, and under the academic supervision of J.R. (George) Holt, we carried out an involved proton-gamma correlation experiment with scintillation counters, at the 36-inch cyclotron. I completed and submitted my master's thesis in 10 months, and left for the United States to become a graduate student at MIT. My thesis work was done under Bernie Feld, who also participated in the experiments. In addition, I received periodic help from Dave Frisch and Louis Osborne. The work was fun and involved three accelerators: the Harvard 150-MeV cyclotron, MIT's 350-MeV electron synchrocyclotron; and the Brookhaven 3-GeV Cosmotron. The experiments were polarization and angular distribution measurements. In all of them I used nuclear emulsions immersed into a liquid hydrogen target, a technique which I had developed at MIT.

When I arrived at LBL in July 1959, the fact that I had to start running night shifts the very first day had, at first, a depressing effect on me. What kept me going then was my fascination with antimatter. As I looked at the thousands of annihilation pictures, a sudden realization swept over me that I was observing events never before seen by Man! I vividly remember my feverish state of mind as I was besieged by an endless stream of naive questions: "What happens in the intimate contact between antimatter and matter? How, on a microscopic scale, does an antiproton-proton pair become 'transformed' into mesons? Is there antimatter in the Universe?" And on it went. Like any privilege, the access to antimatter and the phenomena emerging from it almost diminished in my mind the importance of the world of 'ordinary' matter around me!

My exposure to Luis Alvarez's thinking and professional predictions resulted in my adopting certain 'uncompromising attitudes' of his. "An experiment must stand on its own two feet," and must not "lean" on theory or other experiments to establish its own credibility. Theories can be quite beautiful, yet the experimentalist must be the supreme judge of all. This philosophy greatly influenced my thinking which was anyhow so inclined, but I had not heard it formulated so strongly or implemented so vigorously.

Nothing interests me more than the challenge of a new field. In September 1961, after the discovery of the ω, I asked Ed McMillan, then LBL's director, what he considered the newest and most promising inventions in particle detection. "Spark chambers," he said.

An hour later I was watching beautiful sparks in the spark chambers of Bill Wentzel at LBL. What impressed me was that, in addition to seeing sparks, I could *hear* their cracking sound. This observation led me to ponder the possibilities of locating sparks without photography. I immediately left the Alvarez group and bubble chambers and, with Fred Kirsten, invented and patented the acoustic spark chamber at Berkeley (1962).

In May 1962 I moved to CERN where, during the next two years, I colaborated with Eric Taylor and the (then) "British group." We developed an acoustic spark chamber system to the point of delivering reliable information. Shortly thereafter, Ian MacLeod and I organized the first meeting on 'filmless spark chambers' at which I demonstrated spark chamber operation with an online computer. This helped open the new field of automatic spark chambers[1], which have been replacing bubble chambers in certain types of experiments.

1. Macleod, G. R. and Maglich, B. editors, Proc. Informal Meeting Filmless Spark Chambers, *CERN Report 64-30*, (1964).

Meanwhile, the number of mesonic resonances grew, all being found in bubble chambers. I was looking for a new method that might enable physicists to scan the entire mass spectrum for meson resonances and measure them all, like optical spectral lines, thus determining empirical rules and regularities. What I was contemplating, could be described as "fishing with a net, not a hook." Theorist Nino Costa of Padua, who had studied at MIT with me, joined me, and the 'Jacobian-peak' missing-mass method was conceived[2]. Dick Harting before leaving CERN, helped me form the experimental group. The construction of the first missing-mass spectrometer with an on-line computer was underway by 1964. We observed four new heavy mesons (R, S, T and U), three of them heavier than 2 nucleon masses, and established an empirical regularity in the boson spectrum[3]. The first new meson we observed, the R (now called the g meson), is considered established today; it has spin 3, the exact assignment obtained from our straight-line regularity. When we improved our operational accuracy and resolution, we observed structures in some broad mesons similar to the one I had observed in the ρ several years before. Yet, the controversy these structures stirred make the debate over ρ-ω interference seem mild. I believe that whenever resolution is improved more structures are seen in any spectroscope. I am certain that if someone reproduces the accuracy and reliability of our missing-mass spectrometer, he will find the same results.

In 1967, I returned to the United States to work at the University of Pennsylvania and the Princeton-Penn Accelerator. It was there that I conceived the idea of teaching physics through personal discovery stories, a concept which eventually led to the publication of the *Adventures in Experimental Physics* series. My experimental work with the Penn group at PPA was also rewarding in that we developed a new technique capable of detecting resonances[4] produced with miniscule cross sections of 10^{-35} cm^2/sterad.

In 1969, I joined Rutgers University and formed a high-energy group by hiring well-qualified young physicists. This group has been successfully performing imaginative new types of experimentation at Brookhaven and Fermi Lab since its formation.

Ever since colliding beams became operational in the mid-1960's, I had been thinking of how one could produce true $\pi\pi$ scattering. Thus motivated, Robert Macek and I conceived the idea of self-colliding orbits or "precetron"[5], which turned out to be impractical. But later it was realized that the application of the precetron principle to controlled fusion might lead to a totally clean, radioactivity-free nuclear power source—the *migma* fusion power cell[6]. Within six months of its conception, we had built an experimental model at Rutgers, but shortage of funds prevented us from obtaining conclusive experimental results. In our new independent *Migma Institute of High-Energy Fusion* in Princeton, New Jersey, with a carefully selected group of brilliant and highly motivated young physicists and engineers, we are now attacking the 'problem of the century'—controlled fusion—in an entirely new and unconventional way which, strangely enough, originated in particle physics.□

2. Maglich, B. and Costa, G. *Phys. Rev. Lett.* 18, 185(1965).
3. Focacci, N. N. *et al.*, *Phys. Rev. Lett.* 17, 890(1966).
4. Brody, H. *et. al.*, *Phys. Rev. Lett.* 24, 948(1970).
5. Macek, R. and Maglich, B. *Part. Accel.*, 1, 121 (1970).
6. Maglich, B. *Nuclear Instr. and Methods*, 111, 213(1973).