

Preface: The Training of an Elementary Particle Phenomenologist

This book is about the *phenomenology* of the elementary particle. Phenomenology is not experiment, and it is not really theory; it is the intermediate region that ties these two areas of physics together. Academically, physicists who are interested in elementary particles carry out theses in *experimental* particle physics or in *theoretical* particle physics, but not in phenomenological particle physics. There is no formal academic program that prepares a person to become a phenomenologist.

When a physicist decides to learn a new area of physics, he or she often does a phenomenological survey to get up to speed — to learn the “big picture.” One asks: “What are the experimental facts that have been established in this field, and how well has theory succeeded in tying these facts together? What are the unanswered questions? Where is the frontier? Is there some place here where I can make a contribution?” My personal venture into the field of elementary particle phenomenology started in 1969. This preface is a brief summary of the route I followed to get there.

At the time when I started into physics at the University of Michigan, right after WWII, there was no field of specialization labeled as “elementary particle physics.” There were not enough particles known to justify it. Of course, that situation changed very quickly in the 1950’s. In those days we graduated in physics with the label of “experimentalist” or “theorist,” and that was pretty much it. I was the former. The *Physical Review* itself was a rather slender journal that encompassed all of physics. Its division into several thick sections came much later.

My training in science began with my induction into the U.S. Navy as a 17-year-old volunteer in the spring of 1944, right after completing a shortened high-school curriculum. I enrolled in training school to



Fig. 1. The Institute of Theoretical Physics, Copenhagen, Denmark, Fall 1960. The occasion was the gathering for the official photograph of the year's attendees at the Institute. Professor Niels Bohr is addressing us, and he is delivering a short lesson on optics. He points out that "if you can't see the camera, the camera can't see you." Professor Bohr's index finger is aimed in the direction of the present author.

become a radio technician, which included both radio communications and also the newly-implemented radar systems. We were taught the fundamentals of electromagnetic circuit theory, and we spent several months finding and repairing problems in radios and radars that our instructors purposely created in the equipment. The entire program was 11 months long, and included (in my case) pre-radio school in Chicago, Illinois, primary school in Monterey, California, and secondary school (as a member of the Navy Air Corps) in Corpus Christi, Texas. I ended up with the classification of Aviation Electronics Technician's Mate First Class. After completing my training, I served as an instructor and repairman with a Naval air squadron stationed in the US, and later as an instructor in radar navigation. After the war ended in August 1945, my specialty was one of the last ones to be discharged, since they needed AETM's to keep the planes flying



Fig. 2. The author Malcolm Mac Gregor and his bride-to-be Eleanor Derda in the Fall of 1948, visiting the campus at Purdue University. The occasion was a Purdue–Michigan football game. Eleanor was attending nearby Valparaiso University.

properly. I was finally released in June 1946, and I enrolled at the University of Michigan in July 1946. My undergraduate work was completed in June 1949, and I emerged with a Bachelor of Arts degree in mathematics.

In 1948 I obtained a summer job as a member of a University of Michigan research team doing physical optics measurements in central Michigan. We mounted large fluorescent light sources in fire towers spaced some 30 miles apart, and then photographed them at various times of day to measure the object “shimmer” produced by variations in the density of the atmosphere. My job was to obtain the loan of several Navy jeeps and install and maintain radio transmitters in them. This was our communications system. In addition to carrying out our research duties, five out of eight of us young men in the project ended up marrying girls that we met in the Michigan heartland during the summer. I met a young lady from South Bend, Indiana named Eleanor Derda, who was vacationing at a nearby lake. We

were married in September 1949, just as I was entering graduate school at Michigan. To make a long story short, 57 years later she is still my constant companion and closest friend.

In graduate school I completed a masters degree in physics in 1950 and a doctorate in 1953 (which was formally awarded in 1954, several months after I had completed my thesis and taken my oral finals, and we had moved to California). I did an experimental thesis on beta decay under the direction of Prof. Mark Wiedenbeck. George Uhlenbeck, a leading expert in beta decay theory, was a member of my thesis committee. Since the physics department at that time had very little electronics equipment (of the type developed during WWII), Prof. Wiedenbeck's way of proceeding was to provide the money for equipment, and to have the students purchase or make up the equipment that was required, which was then left with the professor after the student completed the thesis. Thus we put together our own scalars and cathode ray tubes (built from kits), wound our own magnets, designed and built the power supplies for them, and constructed our own spectrometers and vacuum systems. My thesis was on the beta decay of rubidium 87, which has a relatively low peak energy (275 keV) and very long third-forbidden lifetime ($\sim 6 \times 10^{10}$ years). In order to obtain very thin sources for the half life and energy spectrum measurements, I learned, among other things, how to evaporate aluminum and rubidium onto very thin zapon films floating on water. In addition to the equipment we built ourselves, we also had assistance from the physics department machinists and the glass-blowing shop. An experimental thesis was a very hands-on project.

After graduating from the University of Michigan, I accepted a position at the Lawrence Livermore National Laboratory, which was at that time named the Lawrence Radiation Laboratory in Livermore. I first had a brief stint in a nuclear weapons group, and then transferred a few months later into P-Division, the experimental physics division, whose main task was to provide experimental neutron cross-section data. A Cockcroft-Walton accelerator was operational that generated 14 MeV neutrons by impinging a deuteron beam onto a tritium-loaded target, and a variable energy cyclotron was being completed that could provide monoenergetic neutrons from 7 to 28 MeV. I decided to take on the task of measuring total nonelastic scattering cross-sections, which was accomplished by placing a sphere of the material to be measured around the detector, and then removing it. The counting rate with the sphere in place gave the nonelastic scattering cross-section, because the elastic scattering, to a good approximation,

canceled out. The counting rate with the sphere removed gave the incident neutron flux rate. The detectors were plastic scintillators, and I developed a technique for running them with multiple biases so as to evaluate the effect of the cutoff point applied to the scintillation pulses. This technique was later adopted by other experimenters in P-Division. The detector counts were recorded individually in scalars, but there was at that time no electronic recorder to store the total sums, so we connected each scalar output to a mechanical “traffic counter” that was originally designed to count the number of automobiles passing down a street. (The only problem we encountered with this was that the traffic counters were not designed to process millions of counts, and we had to keep replacing their mechanical parts as they wore out.) In order to assure that the neutron beam rate was holding steady in the nonelastic cross-section measurements, we had to do a whole series of sphere-on, sphere-off measurements, which soon led to the development of a mechanical arm for placing and removing the sphere automatically from a location outside of the radiation area. These techniques were first implemented for the Cockcroft–Walton neutron source. When the variable-energy cyclotron came on line, my group was the first one ready to run on it, and for a time we had the sphere experiments operating 16 hours a day. Since the group consisted of only three people, we each put in a lot of hours. We finally succeeded in measuring total nonelastic cross-sections for a series of elements all the way from lithium to plutonium at neutron energies ranging from 7 to 28 MeV. These experiments seem somewhat primitive when compared to the accelerator experiments carried out today — which involve two-mile long accelerators and detectors as large as three-story buildings. But this work was at the forefront of contemporary accelerator physics, and nearby Berkeley was for several years the leading center for much of the development. Ernest Lawrence spearheaded the accelerator work at both Berkeley and Livermore. (His powder-blue Cadillac convertible was frequently in evidence at the Livermore parking lot.)

In 1956, after completing the neutron nonelastic scattering measurements, and after nearly a decade in experimental physics, I decided to transfer over to T-Division, the theoretical physics division at Livermore, to see how life was on the “other side of the fence.” I joined a small program that was aimed at carrying out elastic proton–proton phase shift analyses on the Laboratory’s computers. In 1958 I submitted a paper on this work to the Paris International Conference on Nuclear Physics, and I was invited to deliver it as a Rapporteur at the Conference. After my talk there, Aage Bohr, who heard the talk, invited me to spend a year at the Institute for

Theoretical Physics (popularly called the “Niels Bohr Institute”) in Copenhagen. It took two years to make all of the arrangements, including the obtaining of support from a NATO Fellowship. In the summer of 1960 my wife, two young sons, and I set sail on the S.S. United States for France and on to Denmark. During the year at the Institute, I completed the first full-fledged phase shift analysis of elastic proton–neutron scattering. The computer calculations were carried out at a Danish computer center in Copenhagen. My family and I returned to the U.S. the following fall, after having traveled over much of Eastern and Western Europe in a series of automobile trips, during which time I delivered talks on nucleon–nucleon scattering at a number of universities and institutes, and had the privilege of meeting a few pioneering physicists such as Leopold Infeld in Warsaw and Paul Dirac at Cambridge.

During the 1960’s our group at Livermore extended the proton and neutron elastic scattering phase shift analyses to include all types of scattering experiments at energies up to the inelastic scattering threshold at around 400 MeV, and to treat all of these data together in terms of matrix searches and error matrix representations. In addition to this work, I taught several courses in physics at U.C. Berkeley, including a graduate course in Scattering Theory that I proposed, set up, and taught for the first semester. I also served for several years as a thesis adviser in theoretical physics with the Berkeley physics department. As a part of this program, Richard Arndt completed a thesis on the phase shift analysis work, and our Livermore group published a series of ten serially numbered papers in *Physical Review* that carried the nucleon–nucleon elastic scattering analyses pretty much to completion with respect to the available experimental data worldwide. Richard Wilson at Harvard, Alex Green at the University of Florida, and I organized an International Conference on Nucleon–Nucleon Scattering, which was held at Gainesville, Florida in 1967. The Proceedings of the conference filled a whole issue of *Review of Modern Physics*.

Professionally, this work in nucleon–nucleon scattering was very rewarding. I was offered a full professorship with a special Chair at an Eastern university, and the title of University Professor. For personal family reasons I did not accept this offer, but I still regard it as the nicest honor that has ever happened to me in physics. In 1967 I was invited to Zurich, Switzerland as a keynote speaker for their new cyclotron program, and in 1968 I went to Dubna, USSR as a conference session chairman for their nucleon–nucleon conference. In 1969 I was elected to Fellowship in the American Physical Society. However, the work on elastic nucleon–nucleon

phase shift analyses had now reached a natural stopping point for me. The next step, which involved going up into the inelastic scattering region, was a whole new venture, and it was not clear just how reliable theory would be in handling inelastic scattering. Potential models were in a transitional stage between schemes based on the analytic continuation of scattering amplitudes and quite different schemes based on the use of discrete quarks. Thus it seemed to be the time for a little reflection about the best thing to do next. The nucleon–nucleon scattering studies had been carried out in a professional manner, and, as one user commented in print, they served as the world standard for a decade. But the work, in my opinion, was missing one desirable ingredient — it contained no really new ideas or concepts. Every physicist likes to think that he or she can come up with something original — some new idea which can be added to other great ideas — ideas that contribute to the complex body of information we denote as the “field of physics.”

This story of my personal adventures along the physics path I followed before getting into particle phenomenology leads up to one decisive “event” (a personal “aha”) that occurred during the third week of June, 1969. For some time I had been promising myself that I would set aside one week of full-time concentrated effort during which I would study the elementary particle database, as contained in *Review of Particle Physics* (RPP) 1969, to see what if anything I could deduce about the field. Finally the time arrived. Armed with the RPP and some scratch pads and pencils, I sat in isolation at my office desk for five full days, eight hours a day. (I actually lost eight pounds in weight during this week of “doing nothing except thinking.”) At the end of this time I had reached two conclusions: (a) the muon mass serves as a fundamental elementary particle mass quantum; (b) quark internal binding energies are modest (a few percent). These two conclusions, which were both outside of the conventional thinking of the time (and still are), have formed the basis of the work I have done in the area of elementary particle physics since that date. This work has for the most part stayed at the level of phenomenology, but if the phenomenology is not correct, there is little point in building an elaborate theoretical superstructure: the foundation for theoretical work must be rock solid. The present book, *The Power of α* , displays the results of my efforts in their most recent form. The research path I have followed between June 1969 and April 2006 (the date as I write these words) is documented in the Postscript to this book, and in the annotated bibliography displayed in Appendix D. The path I have taken has not been an easy one, but it is one I would repeat again if it

were necessary and possible to do so. The end of the path is still obscure, but it is the journey and not the destination that remains in my mind, and in my heart. Thus far there have been only a few companions on this path, but these few have made all the difference.

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