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FROM PIONS TO PROTON DECAY: Tales of the Unexpected

D.H. Perkins
Sub-Department of Particle Physics, University of Oxford, Oxford OX1 3RH, England; email: d.perkins1@physics.ox.ac.uk

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■ **Abstract** This account recalls early observations of elementary particles from cosmic ray experiments, using the nuclear emulsion technique. Discoveries in this field in the 1940s and 50s led to the development of high energy particle accelerators and associated detectors, resulting eventually in the observation of the quark and lepton constituents of matter and of the fundamental interactions between them, as described in the Standard Model. The concept of unification of the fundamental interactions led to the prediction of proton decay, and although this has not been observed, the unwanted background due to atmospheric neutrino interactions led to the discovery of neutrino oscillations and neutrino mass, and the first indications of new physics beyond that of the Standard Model. In all this research, unexpected developments have often played an important role.

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BEGINNING IN HIGH ENERGY PHYSICS

This account recalls some of my early experiences in research on cosmic rays, and later in experiments on neutrino physics using accelerators at CERN. Research in experimental particle physics really commenced in earnest after World War II, receiving a big boost from the discoveries of pions and muons and strange particles in cosmic rays in the late 1940s and early 1950s. That led to 50 years of accelerator development, providing the intense, controlled, and ever higher energy beams, plus the associated detectors, which were necessary to put the subject of particle physics on a sound quantitative basis. I was fortunate to have entered the field as all this was beginning, when progress was easy. In fact, after over half a century in the field of particle physics, what has impressed me, almost as much as the logical development of the subject, is the extent to which chance (both long shots that came off and near misses that did not) and serendipity have often played a role. If past experience is anything to go by, we can all continue to look forward to the unexpected.

My own entry into the field was indeed smoothed by chance events. Medicine, rather than physics, was my first choice as a career subject. I had no physics teacher at high school (he had gone off to war), so I was taught by an old chemistry teacher whose physics notes went back to the time of the Michelson-Morley experiment. I learned about relativity and quantum theory only from books. To launch into a medical career, I would first have had to qualify in mathematics and physical sciences. I was offered a scholarship to Trinity College, Cambridge, where I hoped to switch to medicine after taking a science degree. However, Trinity insisted that I should first pass a Latin examination. My foreign language qualifications were in French and German; I had thought Latin a waste of time. Imperial College in London also offered me a scholarship to read physics. I found that I would not need Latin, and the required examination in scientific German was likely to be straightforward. So that was how I ended up reading physics as a direct result of shortcomings in my classical education. Ironically, a few decades later, Cambridge dropped the Latin requirement, and Imperial College started degree courses in medicine! I know of several well-known physicists who originally never intended a career in the subject. For example, the late Fred Reines (awarded the 1995 Nobel Prize for his first detection of the neutrino) told me that he had always wanted to be an engineer.

To graduate in physics at Imperial College, it was necessary to qualify in a preliminary mathematics examination, one year before the final physics examination. Again I was fortunate. This was taking place during World War II, and the examination was in July 1944, a few weeks after D-day. There were four mathematics papers which had to be passed, and I knew that I might fail the last one, which was on analysis. But on just that morning of the examination, the Germans started sending over V1 flying bombs, not in small numbers, as previously, but in hundreds. The V1—an early version of the cruise missile—flew at heights up to 5000 ft, and its journey was terminated by a time switch, which cut out the ramjet engine and sent it into a dive to Earth. At the typical heights at which the V1 operated, there followed up to 14 seconds of complete silence before it hit the ground. Thus one heard the noise of the ramjet (audible for several miles), then a sudden silence, and after a delay, a loud bang. This was happening every minute or so, and one can imagine that under these conditions—and especially for those V1s passing almost overhead—the atmosphere was hardly conducive to solving cubic equations. After more than half an hour of this racket, the invigilators suddenly announced that they had decided to abandon the examination, and that we would be assigned the average mark from the other three papers. What an escape!

Chance also played a role in my choice in 1945 of cosmic ray studies as a research topic for a PhD degree at Imperial College. I had just happened to read a statement by Karl K. Darrow, onetime president of the American Physical Society, which described the study of cosmic rays as remarkable “for the minuteness of the phenomena, the delicacy of the apparatus, the adventurous excursions of the experimenters and the grandeur of the inferences.” I thought that sounded quite promising. My research supervisor was G.P. Thomson (son of J.J.), a somewhat forbidding person, known to everyone as “G.P.” After graduating, I had expected to be directed into war work (although the war was just over) and had already been provisionally allocated to research in the steel industry in Sheffield. A week before I was due to go, I heard from G.P. that I had obtained a first class degree and qualified for a postgraduate research scholarship. I immediately went down from Yorkshire to London. As I was the first student he saw, he was able to offer me a choice of research topics, ranging from infrared spectroscopy, plasma physics (aiming toward fusion), crystal growth, nuclear structure, to the study of nuclear disintegrations by cosmic rays using the photographic method. I told him that my interests were in cosmic rays, so he sent me off to read the 1937 papers in Zeitschrift für Physik by two Austrian physicists, Marietta Blau and Hertha Wambacher. They had recorded approximately 30 nuclear disintegrations in Ilford plates exposed to cosmic rays on mountains in the Bavarian Alps. Although there was no indication of any momentous discoveries, it appeared to be a fairly straightforward technique, and I am sure the prospect of some Alpine skiing also played a part in my choice!

Several physicists worldwide had been experimenting with the nuclear emulsion technique during the 1920s and 30s (the method went back to the recording of alpha-particles from radioactive sources by Kinoshita in 1910, and even before that the effect of radiation on photographic plates had led Becquerel to the discovery of radioactivity in 1896). In addition to Blau and Wambacher in Austria, other scientists were studying nuclear emulsion, including Schopper and Schopper in Germany, Heitler and Powell in England, Zhdanov in Russia, and Wilkins, Rumbaugh, and Locher in the USA. All had succeeded in recording the tracks of low energy (few MeV) protons, using standard emulsions (Ilford Half-Tone and Agfa K) specially sensitized with dyes such as pinakryptol yellow. I first experimented with Ilford Half-Tone emulsions, without much success, but fortunately I was invited to join a panel set up by Patrick Blackett (1948 Nobel Physics Prize) under the auspices of the then Ministry of Supply and under the chairmanship of Joseph Rotblat (1995 Nobel Peace Prize). Its purpose was to promote development of special photographic emulsions to record nuclear particles. The members of the panel included Cecil Powell from Bristol, who was to receive the 1950 Nobel Prize for discovery of the pion and his work on the nuclear emulsion technique, Otto Frisch from Cambridge, George Rochester from Manchester, myself, and a few others, including, most importantly, two industrial chemists, Mr. Waller and Dr. Berriman from the photographic firms of Ilford Ltd. and Kodak Ltd., respectively.

By mid-1946, Waller at Ilford had succeeded in producing thick (50 μm) layers of these so-called nuclear emulsions on glass backing, with four times the normal halide to gelatin ratio and with sensitivities to charged particles with ionization above about six times the minimum value. They were labeled A, B, C... in order of increasing size of the grains (silver halide microcrystals) and 1, 2, 3... in order of increasing sensitivity. These were the first emulsions able to record the tracks of mesons. Although it was obvious that these new emulsions would be greatly superior to what had been available before, no one had the slightest idea of what would be found when they were exposed to the cosmic radiation. Considerable uncertainty and guesswork was involved in the technique; no one knew the level of background that could be tolerated, the amount of fading of the latent image with time, and so on. So it was just a question of trying things out to see what would happen.

Exposing emulsions to cosmic rays at high altitudes was something of a hit-and-miss affair. Mountains, balloons, and aircraft were the obvious choices. I quickly abandoned hydrogen-filled rubber balloons, as used by the Meteorological Office. They often went up to 60,000 ft or so, blew up, and came straight down. I had dreams of a trip to the Andes, but G.P. decided that was far too expensive; he told me bluntly not to waste his time on trivialities over which mountain to choose: simply buy an atlas of Europe and find an alp! In due course I would end up at the HFS (Hochalpine Forschungsstation) at the Jungfraujoch in Switzerland. I chose this because it was the only European mountain site that I could find above 10,000 ft with a railway all the way up. After all, why walk if you can ride? Other people were more energetic. Guiseppe ("Beppo") Occhialini at Bristol had been a mountain guide in Brazil during World War II, and he toiled with boxes of C2 emulsion up to the Pic du Midi in the Pyrenees.

However, to arrange my use of the Jungfraujoch laboratory with the secretariat in Bern and to prepare a series of experiments there (for example with various types of absorber) would take some time. Meanwhile, I was fortunate to have a supervisor who was not only a knight of the realm and a Nobel Prize winner (with Davisson and Germer, for the discovery of electron diffraction), but also, far more importantly, had been chairman of the famous and crucial Maud Committee. Formed in 1940, the Maud Committee had reported in mid-1941 on why and how construction of an atomic bomb, based on uranium 235—and possibly plutonium—would be feasible. Their report was sent to the United States, and as a result American scientists, originally unconvinced, began to take the matter seriously and persuaded their government to set up what was to become the Manhattan Project. So G.P. had considerable influence in the corridors of power, and he contacted the Air Ministry to ask if they could arrange exposures of my emulsions on aircraft. Eventually I was put into contact with the RAF Photographic Reconnaissance Unit (PRU) stationed at Benson, near Oxford. During the war they had flown unarmed aircraft at high altitude deep over enemy territory (suffering appalling losses in the process) and had, for example, produced the first crucial photographs in 1943 of the V2 rocket development site at Peenemünde in the Baltic. In the course of their peacetime duties, they very obligingly carried my emulsions to 35,000–40,000 ft on their photographic sorties over the British Isles that had been requested, so they told me, by the Ordnance Survey people. I believe this actually led to finding some of the first aerial evidence for previously unknown sites of Iron Age settlements. However, for cosmic ray work, the exposures were much too short, just an hour or so per day. That was always the problem with military aircraft, and I was to encounter it again, many years later, with U2 flights in the United States.

FIRST RESULTS WITH EMULSIONS

In November 1946 I got back some 50 μm thick B1 emulsions from the PRU. After developing and scanning them with an ancient monocular microscope, I found several cosmic ray "stars"—disintegrations of nuclei in the emulsion, with emission of protons and α-particles. In one of these, the disintegration was clearly produced by an incoming negative meson (later to be identified as the pion), undergoing nuclear capture (1), and leading to disintegration of a light nucleus (as deduced from the ranges of the secondary protons produced). My observation was confirmed just two weeks later by Occhialini and Powell in Bristol, who found six such events in the much improved C2 emulsions exposed on the Pic du Midi (2). I had heard, third hand, but not seen, the results from the famous 1946 experiment by Conversi, Pancini, and Piccioni in Rome at sea level (3). In those days, it took months for copies of the Physical Review to cross the Atlantic—they were sent by sea—and the prospect of contacting Rome University direct by telephone from the UK would have been quite hopeless. Marcello Conversi and his colleagues, using a Rossi-type array of Geiger counters and magnetic lenses built from bars of magnetized iron, were able to select positive or negative mesons stopping in an absorber under the magnet. Their counting rate was abysmally low, less than one event per hour. Those were the days of heroic experiments! With negative mesons stopping in an iron absorber, they observed no secondaries from decays or other interactions. They asked the theoretical physicist Bruno Ferretti what negative mesons were expected to produce (apart from low energy protons and α-particles, which would stay in the absorber) and were told that probably there should be gamma rays from nuclear excitation. Because gamma rays would, however, also be absorbed in the iron, they added a carbon absorber of graphite blocks, and were amazed to find that essentially all the negative mesons stopping in the carbon then decayed to electrons, with the same lifetime (2.2 μsec) as the positives. At that time, I thought they had been influenced by a 1941 paper by Auger, Maze, and Chaminade, who reported erroneously that negative mesons stopping in aluminum all underwent decay. However, I learned later from Oreste Piccioni that it was the quest for nuclear gamma rays that had caused the change to carbon (4).

Clearly, there was a big difference between my negative meson stopping in a light element at 35,000 ft, with the mass energy of the meson disrupting the nucleus, and the negative mesons of the Italian group all decaying to electrons when stopping in a light element at sea level. Obviously I was aware of Yukawa's

Figure 1 Cecil Powell (on the left) and Guiseppe ("Beppo") Occhialini in discussion at Bristol in 1947. The two women are operating a projection microscope used at that time for photomicrography and to study events at high magnification (courtesy of R.R. Hillier, University of Bristol).

1935 paper on the strong nuclear quantum and in my paper denoted the negative meson by a "Y" in his honor. But at that time I had no clear idea of what it all meant, or how the two experiments could be reconciled. I did not even know then of the existence of the Sakata and Inoue paper of November 1946 (11), proposing the two-meson hypothesis, that a parent, strongly interacting meson would decay into a daughter, weakly interacting meson (what was later termed the pi-mu decay). This paper did not reach England until months later. What I did learn from all this was that, compared with the expertise and organization of the large Bristol group working with emulsions, I was just a one-man band.

During this period I met two well-known physicists from abroad. The first was the late Louis Leprince-Ringuet, visiting from École Polytechnique, who came to see G.P. Thomson and took the opportunity to scrutinize my negative meson event. Later in that year, he exposed some B2 emulsions for me at the Vallot refuge near Chamonix high in the French Alps, and in those I observed my first pi-mu decay, in July 1947. My second visitor was Viki Weisskopf. He had seen my first results on the energy spectra of protons and α-particles from nuclear disintegrations induced by high-energy cosmic rays and how well they fitted a Maxwellian distribution, as predicted by his model likening the excited nucleus to an evaporating liquid drop. At this time I also exchanged correspondence with Yoshio Yamaguchi (5), who noted that, in the disintegrations I had observed of heavy nuclei (silver and bromine, as indicated from the total charge of the emitted protons and α-particles), a small proportion of the secondary protons had energies well below the Coulomb barrier height. This he ascribed to radioactive decay via proton emission, in analogy with α-decay. Some 30 or 40 years later, I would meet both Leprince-Ringuet and Yamaguchi on a regular basis as fellow members of the CERN Scientific Policy Committee. Yamaguchi spent some time in the CERN Theory Division and wrote one of the first (1959) papers predicting proton decay (28), and he also became the first president of ICFA (International Committee for Future Accelerators). Later on, I met Weisskopf again when he became CERN Director-General, at the time of our first neutrino experiment.

For some reason, my thesis advisor G.P. Thomson did not display very much interest in mesons, and he thought I ought to focus on studying the high energy nuclear disintegration "stars" produced by cosmic ray protons. The traditions of nuclear physics, which had started with Rutherford 30 years before, indeed died hard. So nuclear disintegrations were the main subject of my thesis. Later, I continued their study for a time as a joint effort with two other graduate students, Sam Lattimore and Brian Harding (6). The energy spectra and angular distributions of protons and α-particles emitted from the heavy (silver and bromine) nuclei in the emulsion, typically involving excitation energies of 200–700 MeV, were studied in considerable detail. They were found to be in excellent agreement with nuclear evaporation models, taking account of cooling as the nucleus de-excited. However, these were rather pedestrian results that did not lead to anything very fundamental. In retrospect, it certainly turned out to be a wrong track.

The big event in May 1947 had been the publication by the Bristol physicists of two events in C2 emulsions (7) in which a positive particle, the pion, came to rest in the emulsion and decayed into a muon (and neutrino). Although in the second event, the muon just passed out of the emulsion surface, it clearly had only a small residual range and it was obvious that, within the expected small (4%) variations due to straggling, the full range of about 600 μm was the same in the two cases, indicating a simple two-body decay. As I have said, I found the third such event shortly afterward, but never published it (except in my thesis). At the time, I did not even know that my event was only the third pion decay ever to be observed, but I do distinctly remember being totally convinced by those two Bristol events, so that I thought that published confirmation—even from a different laboratory, with different emulsions and at a different altitude—was quite unnecessary. Today, when formal confirmation for new phenomena from independent sources is usually required, that attitude would never do!

The nature of the two-body decay of the pion was put beyond doubt in October 1947, when the Bristol group (8) published a total of ten events, all with the same range of the muon secondary. Again, this involved a chance train of circumstances. A box of C2 emulsions was taken out to Mount Chacaltaya in Bolivia by Giulio Lattes, who had been brought over to Bristol from Brazil by Occhialini. Arthur Tyndall, the director of the physics department at Bristol, wanted Lattes to fly out on a British plane, but Lattes preferred to travel by Varig on one of the new

Super-Constellations. The British plane that he would have taken crashed in bad weather at Dakar, killing all aboard. If Lattes had taken Tyndall's advice, not only would he have died, but those 10 crucial pi-mu decay events would not have been found.

In between the publications of the first two pi-mu decays in May and the next ten in October, the Shelter Island conference took place in June 1947. There Marshak and Bethe (9) as well as Weisskopf (10) proposed the two-meson hypothesis, unaware not only that it had already been proposed by Sakata and Inoue in 1946, but also that it had been discovered experimentally by the Bristol physicists. Fifty years ago, communications were not very satisfactory! Obviously, the experimental verification of the two-meson hypothesis—that strongly interacting pions, produced in the high atmosphere, decayed to weakly interacting muons penetrating to sea-level and producing the Rome events—was an important step. It clarified all the confusion and mystery of the observations of mesons in the 1930s, when mesons, assumed to be identified with the strong Yukawa quanta, were never observed to interact as they traversed metal plates in cloud chambers.

Perhaps I should comment here on the meson nomenclature and how it arose. Val Fitch and Jon Rosner, in their excellent review in "Twentieth Century Physics," state that Powell named the two particles he found as pi (π) and mu (μ) simply because these were the only two Greek symbols on his typewriter. A nice story, but unfortunately quite untrue. In fact, in the beginning there were several Greek symbols in use, to describe the different but not obviously related phenomena observed in the early emulsions. Mesons which came to rest in the emulsion and apparently did nothing were termed rho (ρ): they were mostly positive or negative muons (with the odd case of nuclear capture of a negative pion with emission of neutrons only). Mesons giving nuclear disintegrations at the end of the range were termed sigma (σ) mesons, identified later as negative pions. The nomenclature π (standing for pi-meson, now called pion) and μ (standing for mu-meson, now called muon) was reserved exclusively for the particles in the pi-mu decay chain. Only later, with electron-sensitive emulsions and after measurements of the sign of the particle charges, could all the phenomena be confidently ascribed to either pions or muons.

At the time when they were first observed, it was not immediately clear to everyone that the pi-mu events actually represented decay processes at all. Charles Frank (12) at Bristol considered the possibility that the events could represent the capture of a negative meson in a Bohr-type orbit, which then catalyzed a fusion reaction of the nuclei in the molecule, with enough energy release to eject the meson again with a few MeV of energy. He argued that this could not happen in the emulsion—and was in any case disproved when the pion and muon masses were shown to be different—but Andrei Sakharov concluded that such a process would be possible in hydrogen isotopes ^{2}H and ^{3}H. Ten years later, precisely such a process was discovered quite independently by Luis Alvarez in observations of muons in a hydrogen bubble chamber at Berkeley. When the muon comes to rest in the liquid, it displaces an electron and forms a mesic μH_2 molecule, which diffuses

Figure 2 Marcello Conversi, flanked on the left by Niels Bohr and on the right by Carlo Rubbia, at a conference in 1961 (courtesy CERN Information Services).

around until (because of the reduced mass effect) it can switch over and bind itself in a mesic μHD molecule. The ensuing reaction is $\mu^- + \mathrm{p} + \mathrm{d} \rightarrow {}^3\mathrm{He} + \mu^- +$ 5.5 MeV, called muon-catalyzed fusion. Because the muon is (usually) ejected, it can repeat the process some 200 times for suitable concentrations of deuterium. As was explained by Dave Jackson however, this process as a source of (very clean) nuclear power was unfortunately impossible because the 0.5% probability that the muon sticks to the helium 3 (and finally decays) is about a factor 10 too high. Another near miss!

The next advance in the emulsion technique was in producing thick electron-sensitive emulsions, which would record minimum-ionizing charged particles. This was achieved first in 1948 by Kodak with the NT4 emulsion, with Ilford following shortly afterward in 1949 with the G5. An early success for these emulsions was the recording of the first Kπ3 decay [in those days called the τ-decay (13)]. Interestingly, Kodak (London) sent the exact recipe for making the NT4 emulsions to their parent company, Eastman Kodak in Rochester, New York, but they were never able to reproduce them. Perhaps there was a little "black magic" in the photographic technology. Waller at Ilford once told me that he was worried about running out of gelatin, which was a prewar stock and had come from the hooves of Argentinian cattle (gelatin from English cattle didn't work so well). The gelatin not only suspended the microcrystals of silver halide as an emulsion, but it also supplied crucial trace elements, such as phosphorus and sulphur, which were occluded onto the microcrystals and formed the traps (so-called f-centers) necessary for capture by the microcrystals of the electrons liberated by the ionizing particle, thus forming the latent image. In any case, while the Ilford G5 emulsions were very reproducible, Kodak had problems in that respect and in the end, they abandoned the project as not commercially viable.

These early emulsions were made in 50 or 100 μm thicknesses, but it was clear that in order to record more details of the events, much thicker emulsions would be required. The processing of thick emulsions (up to 400 μm) needed a special technique, namely the temperature cycle method invented by Dilworth, Occhialini, and Payne (14). In this technique, the emulsion was first immersed in developer solution, which was kept cold and chemically inactive until it had penetrated throughout the emulsion; then it was heated so that development proceeded uniformly with depth. I can remember Beppo Occhialini once explaining all this to me, with dramatic gestures to emphasize the shock when the heating process started. Everything that Beppo dealt with was nearly always described in dramatic and unusual terms. He frequently spoke in parables, some of them hard to follow. Discussing the road to success in physics, he was fond of saying "Paul Revere was a very successful man, but the horse didn't like it." He was also fascinated by the English Civil War, and classified all the physicists he knew as either Cavaliers or Roundheads.

Another development was of so-called stripped emulsions. This technique had actually been used over 30 years before by Kinoshita, who didn't know how to develop thick emulsions and so settled for stacks of thin ones. The emulsion was poured and dried on a glass backing in the usual way, then stripped off the glass. In this way, large sensitive volumes could be obtained by stacking a pile of stripped emulsion sheets like a pack of cards, registration of adjacent sheets being made by using a narrow X-ray beam marking the edges.

RESEARCH AT JUNGFRAUJOCH

In the early days, people did some unusual experiments. We understood little of the underlying physics, and much of the research was just trial and error. Figure 3 shows a picture of a pipe being lowered into the Aletsch glacier near the Jungfraujoch research station. The idea was to measure the absorption of the star-producing radiation in ice, using emulsions lowered down the pipe, and compare it with that in air. Both substances had roughly equal nuclear absorption lengths (measured in gm cm^{-2}) but very different densities, so if the initiating radiation had any significant component particles with a short lifetime, there could be a difference (of course there was not, because as we now know, in the GeV energy region there are no strongly interacting unstable particles with a decay length comparable with the scale height of the atmosphere). Another long-shot experiment on the glacier, by Ugo Camerini and others at Bristol, consisted of tying cocoa tins containing

Figure 3 Norman Barford (left) and the author carry out one of the Imperial College cosmic ray experiments, lowering emulsions down a 100 ft pipe which had been sunk into the Aletschgletscher near the Jungfraujoch in the Swiss Alps.

emulsions to a long vertical pole stuck into the ice, the idea being to measure the pion lifetime by recording the relative numbers of upward-traveling pions and muons as a function of height above the ice. I recall that their answer of 6 ± 3 ns was wrong, but so also were the competing measurements by Richardson, Panofsky, and others at the Berkeley synchrocyclotron. Indeed, the first four measurements of the charged pion lifetime turned out later to be all wrong by six times the stated errors!

In the late 1940s and throughout the 1950s, we thought of the pion as a fundamental particle. This inspired physicists to push ahead with a worldwide accelerator building program to study pions and their interactions, which was to lead eventually to the discovery of the elementary quark and lepton structure of matter. That was one of the two main legacies of the emulsion technique. The other is that it set a pattern of collaboration on an international scale, which is universal in "big science" today. The technique was simple and tailor-made for international collaboration. Small research groups, with the meager resources available in a Europe emerging from the catastrophe of World War II, were provided with an instrument allowing them to easily contribute at the forefront of physics research. The impact for European collaboration on a major scale, in the formation of CERN in 1953, hardly needs to be emphasized. The 1953 balloon flights from Sardinia, mentioned below, involved 22 laboratories from 12 countries, ranging from Dublin in the west to Warsaw in the east, and from Trondheim in the north to Catania in the south. These early collaborative efforts were, to my mind, the greatest achievement of the emulsion technique.

AN UNWELCOME DIVERSION: MEASURING NEUTRON FLUXES

Every researcher has the experience of going up blind alleys, and I was no exception. My supervisor, G.P. Thomson, told me that he felt the study of cosmic rays by the photographic technique was all very well, but it was not really testing my practical or innovative abilities. While I am sure he was right in principle, he proposed that I should remedy this by performing measurements of neutron fluxes from a suitable radioactive source, for reasons that totally escaped me. For him, I believe it was a throwback to the years preceding World War II, when he and many other physicists had been carrying out experiments with slow neutrons. I had heard lurid tales of physicists going out in rowing boats on the Serpentine lake in nearby Hyde Park, trailing radioactive neutron sources and thermal neutron detectors in the water! In the set-up which I used, a phial containing a radon-beryllium neutron source was placed in a large tank of water to thermalize the neutrons. The tank was located in the yard just outside the main workshop, and I was constantly having to assure the workshop foreman that his technicians would be adequately shielded from gamma radiation. I had to integrate the neutron flux as a function of distance from the source, using an array of indium foils from which the induced beta activity could be measured, using a thin window Geiger counter to record the betas. I was expected to build the experiment, including all the electronics, from scratch—and those were the pretransistor days of vacuum tubes—as well as the thin-window counter. After spending weeks on this, I still had not made a satisfactory counter, so I asked G.P. if I could buy a counter off the shelf—total cost £17, or less than $500 in today's money. He eventually agreed, but made it a condition that I should give some help to a fellow graduate student, Alec Hester, who was actually building a small Van de Graaff accelerator in order to study (p,α) reactions in light nuclei. With only a little help from me, Hester finally obtained his PhD degree and a few years later became the CERN librarian—an absolutely vital post. So, in a way I can claim that I was contributing a little to the success of CERN even before it was formed!

Eventually I managed to get the neutron source calibrated. Despite G.P.'s evident conviction that making research students build all their apparatus was good for them, I was no electronics wizard like Piccioni in Rome, and had really learned very little and regarded the exercise as a waste of time, as I was not able to apply it to anything useful, and the final result could not be of the remotest interest to anyone. All my hand-built power supplies, scalers, etc. were just thrown away afterward. A lot of the time was also lost every week in traveling with my phials of powdered beryllium up to Amersham, north of London. There a Dr. Ross, housed in a cave in the chalk hills, filled them with radon pumped from radium needles sent from the London hospitals. Eventually I asked him if he could not send them with someone at a designated day and time, whom I could meet, for example at Euston station in northern London. He said that although that was not possible, he could put the sources on a train if I could arrange to meet it. To my concern that it was illegal to send radioactive substances by rail (and these had strengths of around 0.5 curies), Ross told me, "That's no problem. I'll put the source in a large wooden box and label it 'Medical Supplies.' "

Of course, the inevitable happened. One day there was a major hold-up on the Underground (subway) and I arrived at Euston more than an hour late. No train. No box. Then, in the distance, I saw a railway porter actually sitting on it. Only about 5 mm of lead and less than a foot of packing material separated my source from his genitals! I remember making some attempt to discover if he had any children (without raising his suspicions and without success), collected my box, got back to Imperial College and immediately telephoned Ross. Having established that the unfortunate railway employee could not have been in contact with the source for more than an hour, he told me blithely, "Quit worrying; he will very probably survive."

In those days, despite the lessons of Hiroshima and Nagasaki, people were much too relaxed about the dangers of radiation. On top of all that, after I had been regularly smashing beryllium nuts to powder for my sources, I learned of the death in Berkeley of Eugene Gardner from beryllium poisoning, following his work on the Manhattan Project. Incidentally, the service provided by Dr. Ross and his small staff was later to grow into Amersham International, now a major (and safety conscious) global supplier of radio-isotopes.

THE STUDY OF KAONS AND ANTIPROTONS

The year 1947 was memorable not only for the discovery of the pion, but also for the publication by George Rochester and Clifford Butler of two V-events found in a small cloud chamber triggered on air showers at sea-level in Manchester (they were most likely examples of $K^0 \rightarrow \pi^+ + \pi^-$ and $K^+ \rightarrow \mu^+ + \nu_\mu$). The events were published (15) more than six months after they were found, and it was only years later that Clifford Butler told me why. Patrick Blackett was the professor and head of department, and like many professors in those days, required that he should see and approve all papers before they were sent to the publisher. Blackett insisted on checking all the calculations on the V-events himself, and the paper apparently had to be sent back no less than fourteen times before he was satisfied with it! Anyone reading that paper today would be impressed by the quality of the presentation, both of the physics and the English. After those two V-events, no more were to be found anywhere for another two years. Eventually the results were confirmed and extended in cloud-chamber studies at Cal Tech, Indiana, MIT, Princeton and École Polytechnique, as well as by the Manchester group, who took their chamber and magnet to the Pic du Midi. The Jungfrau laboratory would have been easier, but the Swiss were not willing to risk taking the massive magnet sections up in the lift from the railway tunnel to the Sphinx ridge, where the cloud chamber would have been located.

By the early 1950s, the study of K-mesons was getting into full swing. I had moved to Bristol, which at that time was the most prominent emulsion group. As

Figure 4 Moments before the launch of a hydrogen-filled plastic balloon at Cardington, Bedfordshire. At sea level, the hydrogen occupied only about 1% of the total volume. Flying balloons in England eventually became too difficult because of the need to avoid the air lanes.

Louis Leprince-Ringuet said at the 1953 Rochester Conference, "En Europe, il y a pour les emulsions Bristol, le grand soleil, et puis un tout petit nombre de petite satellites, tres inferieure." There we began a program of construction of hydrogen-filled plastic balloons, in order to float large stacks of emulsion for many hours in the stratosphere (see Figure 4). Initially they were launched from an airship hangar at Cardington near Bedford. But in the increasingly crowded air lanes over southern England, getting permission to fly balloons became increasingly difficult, and eventually the decision was made to relocate to the Mediterranean area, with launching from Cagliari in Sardinia (with recovery at sea) and from Nove Ligure near Genoa. In her 2002 contribution to this series, Milla Baldo-Ceolin has already described those developments, and I will limit myself to a few remarks.

As I have mentioned above, a providential consequence of relocating and internationalizing the cosmic ray balloon program was the initiation of large collaborative experiments, with many university groups, even those with modest means, able to take a full part in the joint effort. This European collaboration on flying large plastic balloons (with the actual fabrication in Bristol and Padua) was formally launched as a result of a meeting in Rome hosted by Eduoardo Amaldi in 1952. At a reception at the university I ran into Bruno Touschek. When the reception finished, he offered me a lift back to my hotel on his motor bike. At that time he was busy designing what was in fact the world's first electron-positron

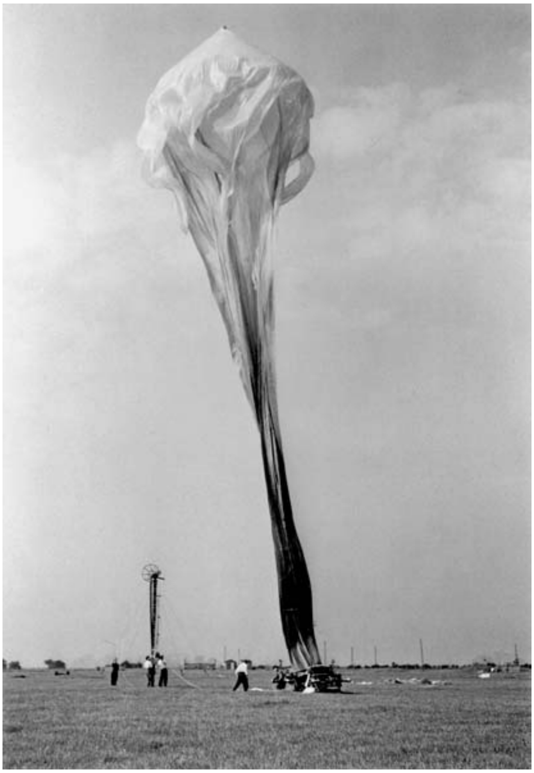

Figure 5 A few seconds after launch of one of the Bristol balloons in Italy. The emulsion stacks carried on these balloons provided some of the early information on decay modes of the strange particles (kaons and hyperons).

collider (Anello Di Accumulazione, or ADA), and on our journey through Rome he described his ideas, gesticulating with one hand. He was also very proud of the fact that his bike was Austrian, like him, and could easily beat any bike made in Italy. He proceeded to demonstrate this by passing every motor cycle or scooter he caught sight of, giving them a rude sign with his other hand as he did so. So for a lot of the journey, Bruno's bike more or less steered itself. For me, that was indeed a lost opportunity. I am afraid I felt compelled to concentrate more on the traffic than on his exposition of the principles of electron-positron colliders. Touschek was a brilliant physicist, full of optimism and charm, who unfortunately died young (16).

The study of strange particles in cosmic rays was the province of the nuclear emulsion and the cloud chamber, and this was beautifully illustrated in the 1953 conference at Bagnères de Bigorre in the Pyrenees. Everyone who was there has testified that this was the very best conference they had ever attended, before or since. It had everything going for it. First, there was a big fight. Gregory and Peyrou from École Polytechnique had made mass measurements, from magnetic curvature (momentum) and range in a multiplate cloud chamber, of mesons decaying in what is now called the Kμ2 decay mode, with a mean value of 920 $\pm$ 40 m_e, while Menon and O'Ceallaigh from Bristol had mass values from scattering measurements in emulsion for the Kπ2 and other decay modes of 1075 $\pm$ 100 m_e. The average of the two sets would have been close to the well-measured mass for the Kπ3 of 965 m_e, as Bruno Rossi suggested in a masterly summary of the meeting. This conference also saw the first presentation of the Dalitz Plot in the analysis of Kπ3 decay. Undoubtedly, however, the prize for the best experimental contribution went to Bob Thompson of Indiana (17). He had developed cloud chamber technology to a fine art, practically eliminating gas distortions, and produced the first clean separation between the decays of lambda hyperons and neutral kaons, and the first precision measurements of the decay $K^{\circ}{}_s \rightarrow \pi^+ + \pi^-$.

A unique feature of the conference venue at Bagnères was that it had a dance hall on one side and a casino on the other, so there were plenty of distractions if one got tired of the physics. And the hospitality extended to us by our hosts, the University of Toulouse, was out of this world. Milla Baldo-Ceolin has already described how the work on kaons and hyperons in cosmic rays developed at Bagnères and at subsequent meetings in Italy. The study of the charged kaon mass(es) and decay modes was to continue for some time, in my case ending in 1955 working with the Richman Group with an accelerator kaon beam from the Bevatron at LBL, using (for the first time) quadrupole focusing magnets and again using large stacks of nuclear emulsion to record the events. Just two years before parity violation in weak decays was accepted, people went to extraordinary lengths to get around the problem. Our experiment at LBL showed that the mass of the particle responsible for the Kπ2 decay mode differed by less than 1 MeV/c^2 from that for Kπ3, although the final states concerned were of even and odd parity, respectively. Undaunted, Luis Alvarez (as well as many others) suggested that two different kaon states of opposite parity were involved, and that one underwent radiative decay to the other: $K \rightarrow K' + \gamma$, with $K \rightarrow 2\pi$ and $K' \rightarrow 3\pi$. We couldn't actually disprove that because it would have been hard to detect Compton scattering or conversion of the gamma ray. As happened in many laboratories at that time, at the end of such discussions, people would suggest that perhaps parity was not conserved after all, but they never stated it with any real belief, and it was left to Lee and Yang later on to make the convincing argument.

An experiment to detect antiprotons, again with emulsions, was made by the same group in 1956. It would parasite on the experiment of Owen Chamberlain, Emilio Segre, Tom Ypsilantis, and others, who had designed the beam at the Bevatron. The Bevatron energy of over 6 GeV per proton was just above threshold for production of a proton-antiproton pair in the collision of a beam proton with a stationary proton. The laboratory momentum of the antiproton would then be about 1 GeV/c. Their experiment used Cerenkov and time of flight counters to discriminate the antiprotons from the much more abundant negative pions, and was tuned to this secondary momentum. For the emulsion experiment, using the same beam, it was necessary therefore to use a degrader to slow down the antiprotons, so that by ionization they could be distinguished from the relativistic negative pion background. Unfortunately, because of the high cross-section for annihilation in flight, the degrader employed led to the loss of nearly all of the antiprotons before they ever got to the emulsion stack and only one antiproton event was found. Later researchers realized that the calculations of thresholds and secondary beam momenta had neglected the full effects of Fermi motion in the internal copper target of the Bevatron, which would not only reduce the threshold energy but also meant that the spectrum of the antiprotons produced would peak at lower momentum. So, running at a reduced secondary beam momentum of 600 MeV/c, there was no need for any degrader at all and many annihilation events were observed (18). But that experience showed me once again how very easy it was to get things completely wrong through simple mistakes.

STARTING ON CERN NEUTRINO EXPERIMENTS

My entry into the field of accelerator neutrino physics was indirect, by way of my experience in cosmic rays. The CERN laboratory was inaugurated in 1953, and by late 1959, the construction of the proton synchrotron (PS) was completed and CERN was beginning to consider embarking on their next project, a proton-proton collider called the ISR (Intersecting Storage Rings), which had been specified as one of the goals of CERN in the 1953 Convention. What sort of new physics might the ISR be expected to uncover? For practical guidance, the only indication could come from cosmic rays, and I was invited to a meeting in CERN to explain what amazing new things—if any—could be expected in this new energy region (which corresponded to the interactions of cosmic ray protons of a few TeV incident energy). My research at Bristol had been into the study of meson production at such energies, my principal colleague in this work being the late Peter Fowler, son of R.H. Fowler and grandson of Ernest Rutherford. We used stacks of emulsions interleaved with thin sheets of heavy element (tungsten alloy). Electromagnetic showers were generated from the decay of neutral pions produced in the nuclear interactions of primary protons. The showers developed rapidly in the sheets of heavy elements and at these energies could be easily detected (without a microscope). The exposures of these quite massive (up to 0.25 ton) stacks were on balloon flights in Texas and on Comet and VC10 commercial jets on proving flights to Beirut and Sydney. The object of this research was to relate the high energy gamma ray flux in the TeV region to the sea-level muon spectrum (measured with spectrometers and from the range spectrum deep underground) via the production spectrum of the parent pions and kaons (19).

All this worked out very well, but unfortunately, my general message at the CERN meeting had to be that there was no evidence from the available data that anything dramatically new was occurring at some magic energy threshold. All the measurable quantities—cross-sections, transverse secondary momenta, kaon/pion ratios, meson multiplicity, etc.—were either constant or varying very slowly and smoothly with energy. Of course, with only a few hundred events, these experiments were quite incapable of finding evidence for quark substructure by observing the very rare wide-angle jets. In any case, it was fairly clear that a decision to embark on the ISR had almost been made, largely, I believe, because the accelerator physicists thought it would be a great challenge to build it. But I guess that some compelling physics reasons would have been useful for the CERN Council. For me, one positive result of this meeting was that I got to know Colin Ramm and the people in his group. Colin played a major role in constructing the magnet system of the PS and was then head of the NPA (nuclear physics apparatus) division at CERN. A year or so later, this division would be the center for neutrino experiments.

Almost a quarter of a century afterward, by one of those ironies of fate, I was chairman of their Scientific Policy Committee, and the CERN Council asked our advice about closing the ISR. Although a magnificent technical achievement, the ISR had produced very little in the way of new physics, and reluctantly we had to recommend shutting it down, in order to release vital funds and manpower for the LEP collider project. Other quite fruitful activities, ranging from bubble chamber operations to those of the radiation biology group, also had to be terminated for the same reason. So the scope of operations at CERN became ever narrower. Today it is much the same story: essentially all the effort at CERN has to go into building the LHC and associated detectors.

Shortly after the CERN meeting, Sula Goldhaber invited me to attend a summer study in 1961 in Berkeley, in connection with the proposal to build a 200 GeV proton accelerator near Sacramento, which by 1973 was to mature as a 400 GeV machine at Fermilab. Again I was there as a cosmic ray expert, and my task was to calculate the characteristics of the secondary beams to be expected. On this I worked with Guiseppe Cocconi and Lou Koester. We produced some simple analytical formulae for secondary particle fluxes, which worked reasonably well until much better calculations by Hagedorn and others came along. As an extension of this work I calculated neutrino fluxes in a narrow-band beam (as far as I know, the first time such a beam was ever proposed). This, together with the paper on the proposed neutrino experiment at Brookhaven written by Mel Schwartz, sparked my interest in accelerator neutrino physics. A year later, when Colin Ramm suggested participating in the bubble chamber part of the forthcoming CERN experiment, I was eager to do so.

This 1963 neutrino experiment was actually the second one at CERN. The first, in 1961, with an internal proton target, was characterized by zero events and zero flux, and the less said about that the better. It was abandoned, and the way left open for the Brookhaven experiment (20) to discover neutrino flavor in 1962. The second CERN experiment had an extracted proton beam from the PS, incident on an external target placed inside Van der Meer's magnetic horn, and with muon shielding borrowed from the Swiss national steel reserve. The neutrino beam went into a small heavy liquid bubble chamber holding about a ton of heavy freon (CF_3Br), and thereafter into a spark chamber detector. Technically the beam, the monitoring, and both detectors worked perfectly. Colin Ramm had his own novel method for ensuring high beam intensities. If, in a particular shift, the PS machine operator had performed very conscientiously, he would nip round to the control room, and when no one was looking, slip a bottle of champagne behind one of the electronic racks, then go back to his office, telephone the control room and suggest a search. In time, like Pavlov's dogs, the operators learned that if they did their job well, they would be rewarded.

The confirmation of the BNL result, with the identification of two distinct neutrino flavors, ν_e and ν_μ, took only a couple of days' running. Unfortunately, however, the physics interpretation of the other results was fairly disastrous. After missing out on the discovery of neutrino flavor, CERN was desperate for a discovery, and hoped to find the intermediate W-boson, possibly with a mass as low as 1 or 2 GeV. The spark chamber results on possible W-decay events were presented at the Siena Conference in July 1963. Near the end of the meeting, Luis Alvarez got up and asked: "When I get back to Berkeley, how do I reply to the first question when I step off the plane, which will be: 'Has CERN found the W-boson?' " Gilberto Bernardini, CERN research director, thereupon made a five-point declaration, to the effect that CERN had dilepton events that might or might not be due to decay of the W-boson; but, if it did exist with a mass of 1 or 2 GeV, CERN had discovered it! I've often wondered if that wasn't the low point of neutrino physics at CERN.

Measurements were made in the bubble chamber events of weak nucleon form factors from the elastic events, on weak pion production, and in early attempts to check the CVC and PCAC hypotheses. Although thought to be important at the time, these are now long forgotten. Apart from the W search, there were plenty of other things that went wrong with both the spark chamber and bubble chamber experiments. The failure to find neutral currents in the spark chamber was ensured because a secondary charged lepton was one of the trigger requirements (and a proposed run without a lepton trigger had been voted out). The bubble chamber was just too small to discriminate between neutral current events and neutron background, and the group could only give limits, but we didn't even get those right (21). Our limit (5% of the charged current cross-section) for the elastic neutral current process $\nu + p \rightarrow \nu + p$ was incorrect, due to a book-keeping error, eventually discovered by a research student from Strasbourg (22). Even seven years later, when I was visiting UCLA, J.J. Sakurai was to castigate me over this. "Perkins," he said, "you set back particle physics by 20 years!" I think he may have been joking, but with J.J. one was never quite sure.

Another major misfortune, for which I have to take the main responsibility, was the failure to interpret—or at least to ponder and think seriously about—the rapid increase of the total neutrino cross-section with energy in the bubble chamber data. In retrospect, we now know that this was the first indication of pointlike structure inside the nucleon. However, I could hardly classify that as a near miss. The results did provide clues which might have been followed up, and were not; but one has to remember that, even years after the invention of the quark model by Gell-Mann and Zweig in 1964, high energy physicists worldwide thought of quarks—assuming they were real things rather than mathematical fictions—as extremely massive and strongly bound objects. So the discovery of scaling and pointlike behavior by Jerome Friedman, Henry Kendall, and Richard Taylor in inelastic electron-nucleon scattering at SLAC in 1968 came as a revelation. The idea that in high energy collisions, quarks could behave as light, weakly bound particles required a stretch of the imagination of which, in 1963 or 1964, we were just not capable. It needed a Bjorken and a Feynman for that. Even today, it is a mystery why the pointlike scattering from quasi-free constituents, as indicated by the linear rise of the cross-sections with energy, applies at neutrino energies that are far below the asymptotic regions where perturbative QCD could be expected to apply. This precocious scaling was indeed a piece of good fortune, if only we had had the sense to see it!

THE GARGAMELLE EXPERIMENTS

One positive outcome from the Siena meeting was that André Lagarrigue (whom I knew from my cosmic ray days) was sufficiently impressed by these early bubble chamber results that he embarked on a project to build a much larger heavy liquid chamber (Gargamelle) in which many of the limitations of the small CERN chamber could be overcome. Gargamelle's great contribution was to be the discovery of neutral currents, the search for which—way down at number eight (out of ten!) in the collaboration's priority list in 1970—suddenly became a top priority following the 1971 't Hooft paper (23) proving the renormalizability of the electroweak theory. Gargamelle was a complicated device; the optical system involved transporting the images through the magnet yoke via a meter-long lens train to the eight cameras, through all of which the film had to be threaded. However, the geometry of the chamber, a cylinder 5 m long by 2 m diameter, was ideal for the study of neutral currents.

The 1971–73 neutrino experiments in Gargamelle benefited not only from the much larger chamber, but also from the higher intensity with the PS fast cycling booster and from the much improved two-component magnetic lens, invented by Fred Ašner, which replaced Simon Van der Meer's single horn lens. This meant that one was able to select events with high energy transfers, greatly simplifying the calculation of neutron background. The other advantage was that the experimental analysis became a joint effort by several groups (Aachen, Bruxelles, CERN, Ecole Polytechnique, Milano, Orsay, and University College London). The result was that several independent analyses could be run in parallel and the results compared, and this was vitally important in producing convincing evidence for neutral currents. The announcement of their discovery was made by Gerald Myatt at the Bonn international conference in July 1973, where he also included the (at that time) positive but as yet unpublished results from the Harvard, Pennsylvania, Wisconsin, Fermilab counter experiment at Fermilab. Later, this group reconfigured their detector and unfortunately succeeded in wiping out the signal, with the result that the claim of the Gargamelle collaboration (24) was not generally accepted—and certainly not by most people in CERN!—until it had been confirmed by other experiments at Argonne, Brookhaven, and Fermilab, almost a year later.

Confidence in the electroweak theory prior to the Gargamelle discovery was not very high, even among its strongest protagonists. I recall presenting the very early Gargamelle data at the Chicago/Fermilab conference in summer 1972. The emphasis was on a comparison of the neutrino and antineutrino charged current results with those from deep inelastic electron scattering at SLAC, which provided a unique test of the quark-parton model, and in particular measured the mean square valence quark charge (5/18) in the nucleon and also revealed the presence in it of the quark-antiquark "sea." At that time, the analysis of hadronic neutral current events was still at a very early stage. The safest prediction regarding the electroweak theory was on muon antineutrino-electron scattering, for which the 't Hooft paper gave the cross-section in terms of the mixing angle, and for which the background could be accurately calculated and was expected to be very small. However, at that time we had no events, so I could only give limits on the weak mixing angle, which I referred to as the Weinberg angle. At the coffee break, Abdus Salam—who was to share the Nobel prize with Glashow and Weinberg for the invention of the electroweak theory—rushed up to me in great agitation. He pushed under my nose a reprint of his 1964 paper with John Ward, in which they had also introduced a mixing angle. He asked me, "Why do you keep referring to it as the Weinberg angle?" I apologized and assured him that, in the written version of my talk, I would correct this, including all the other names. But I also told him that I did not know why he was so upset. We had at that time absolutely no definitive evidence in support of the electroweak model, and I thought it might very well be complete rubbish. "You really think so?" replied Salam. "In that case, better keep my name out of it!"

There were several amazing coincidences involved in the neutral current story. In the spring of 1973, before we were quite ready to publicly claim an effect, I was asked to give some lectures on neutrino physics at a high energy physics school run by the Paul Scherrer Institut laboratory. The venue was a private high school at Zuoz, in the Engadine valley. The lecture theater was actually in the basement, and artificially illuminated. I eventually got to talking about neutral weak currents, and I said that definitive results were expected soon. Exactly on my second mention of the words "neutral currents," all the lights went out! It turned out that during that weekend, reservists in the Swiss army were on manuevers nearby, and some idiot had managed to cut through a power cable. It took more than an hour to restore power. I believe the incident made a deep impression on the audience—but whether that was in favor of or against neutral currents, I could not say. One of them, Norbert Straumann (a onetime postdoc of Pauli's), reminded me of the occasion 30 years later, when he gave a lecture in Oxford on a more modern topic, the dark energy in the universe.

In a second incident, I recall presenting the Gargamelle results at a summer institute in Hawaii, in August 1973. Richard Feynman was at this meeting, and initially he did not like either neutral currents or the electroweak theory, although in the end he came to accept them. He certainly gave me a very hard time when I presented the experimental data. I remember one amusing (and amazing) coincidence. Because the signal was now apparently so clear, Feynman wanted to know why we had not found neutral currents in the previous neutrino experiments. I explained that it had been hard to tell genuine neutral current events from neutron background because the neutron absorption length was comparable with the diameter of the smaller bubble chamber. In 1963 we had indeed observed more of these (allegedly) neutron events than expected, and we looked first for other sources of neutron background, in particular skyshine neutrons leaking through the shielding. I set Enoch Young, a Chinese graduate student from Hong Kong, to make an estimate using a Monte Carlo simulation. His conclusion was that the calculated shield leakage was some three times less than the observed rate, but because of uncertainties in parameterizing the nuclear cascade in the shield, the difference could not be considered significant. I had got to the point of explaining all this, when who should walk into the back of the auditorium but Enoch Young himself! This left me somewhat speechless, and Feynman wondered why, but I was quite unable to tell him! Apparently Enoch was on his way from Hong Kong to a cosmic ray meeting in Denver, and had stopped in Honolulu to change planes, and just looked in at the meeting by pure chance. That was indeed a very long shot, with the Hawaii theorist San Fu Tuan calculating the odds against such a happening as about 100,000 to one. Another tale of the unexpected!

QUARKS AND QUANTUM CHROMODYNAMICS

The last neutrino experiments in bubble chambers at CERN took place when the SPS started operating in 1976. One experiment used the BEBC bubble chamber filled with a mixture of liquid neon and hydrogen, with a narrowband beam from the SPS, and was a collaboration of Aachen, Bonn, CERN, Imperial College London, Oxford, and Saclay. One of the main aims of the experiment, and that of the CDHS (CERN, Dortmund, Heidelberg, Saclay) counter experiment located directly behind BEBC, was to measure neutrino and antineutrino cross-sections on nucleons up to the highest possible energies (around 250 GeV). These results confirmed the earlier SLAC data on deep inelastic electron-nucleon scattering, identifying the parton constituents of the nucleon with the long-sought quarks. We attempted to go further and measure the deviations from exact scaling predicted by perturbative quantum chromodynamics (QCD), notably for the q^2 dependence of the moments of the nonsinglet (valence quark) distributions of quarks in the nucleon. The momentum transfers involved were in the range of $q^2 \sim 2$–$100\ \text{GeV}^2$, so hardly in the perturbative region. Nevertheless, when one included the effect of those wonderful Nachtmann mass corrections, the results (25) were in astonishingly good agreement with perturbative QCD and the anomalous dimensions of color SU(3).

PROTON DECAY AND ATMOSPHERIC NEUTRINOS

At about the same time, some experiments were conducted with wideband beams in BEBC filled with hydrogen and deuterium, the idea being to measure the neutral current couplings of the up and down quarks separately by comparing the results with those using a neon filling. The values of $\sin^2\theta_w$ measured in these experiments came out quite low—probably as a result of an unlikely fluctuation—and the result was that by 1980, the world average value (26) of $\sin^2\theta_w$ was only 0.21 (compared with the presently accepted value of 0.23). This was to have far-reaching consequences which no one could have foreseen. In my opinion, it was the most important wrong result ever obtained at the CERN laboratory.

At this time, in the late 1970s and early 1980s, and following the success of the electroweak theory, the unification of the fundamental interactions—or at least the strong and the electroweak interactions—became an important goal of particle physics. Proton instability followed as a consequence of these grand unification (GUT) schemes, such as that proposed by Pati and Salam, and the SU(5) scheme of Georgi and Glashow (27). Of course, strong limits (above 10^{26} years) on the proton lifetime already existed, from experiments going back to the early 60s, including a 1960 experiment by a CERN group using Cerenkov counters in the Lötschberg railway tunnel, inspired by the speculations of Yamaguchi mentioned above (28). Theoretical estimates of lifetime ranged widely, the record being that by Sakharov of 10^{50} years (assuming the decay to be mediated by particles of the Planck mass). However, with the anomalously low value of the weak mixing angle as found in 1980, it seemed that the SU(5) model was the most serious contender, because the three running couplings of the strong, weak, and electromagnetic interactions appeared to meet at a unification energy of around 3×10^{14} GeV. This predicted a rather definite proton lifetime of 10^{30} years. When a number of people (including myself) realized that this would provide a unique test of grand unification, and that the expected rate was of the order of one proton decay per day in a kiloton of material, several experiments were started with kiloton-size detectors situated deep underground (to reduce the cosmic ray muon background). This rush for the mines was indeed a very long shot, based on an upward extrapolation from known energies by over ten orders of magnitude, assuming that no new physics occurred in the famous "desert" between the electroweak scale and the GUT scale.

Unfortunately, the observed lower limit on the lifetime soon turned out to be more than two orders of magnitude larger than the prediction. Worse, it soon became clear that interactions of atmospheric neutrinos would pose a serious background, because their rate was of order 0.5 per kiloton per day, not much less than that for the original proton decay prediction (29). Some people thought of quite desperate measures; Salam and Pati even proposed doing the experiments on the Moon which, with no atmosphere, should have very much smaller background.

Atmospheric neutrino interactions had first been observed back in 1963 and reported at the IUPAP Cosmic Ray Conference in Jaipur at that time. The atmospheric muon flux had been measured with Conversi tube arrays in deep (6000 ft) gold mines, at the Witwatersrand mine in South Africa, and the Kolar mine in India. A few multiprong interactions from the mine walls, due presumably to atmospheric neutrino interactions, were also observed. There was interest then in comparing their rate with that expected using the cross-sections measured in our 1963 CERN PS experiment. By 1965, the observed rate of atmospheric neutrino interactions with muon secondaries appeared to be in fair agreement with expectations, although there was some indication (30) that the number was somewhat smaller than that expected—perhaps a harbinger of future results! Atmospheric neutrinos were thereafter quietly laid to rest, at least until 1982.

At the International Conference on High Energy Physics in Paris in 1982, I was given the task of reviewing the situation and concluded that, despite a claim from the Kolar group, there was no clear signal from the various experiments. However, I did mention that one ought at least to study the background carefully, to check that it was really understood. If one could not even understand that, one would not be able to lay claim to interpreting any proton decay events. In any case, the background of neutrino interactions were all that we had. At the time, I had absolutely no idea how prescient my remarks were to prove! But by 1993, five experiments from three continents (IMB and Soudan 2 in the USA, NUSEX and Frejus in Europe, and Kamiokande in Japan) were presenting ratios of numbers of events with muons (due to ν_μ) and electrons (due to ν_e), and there was clear evidence for a discrepancy, particularly in the Kamiokande, IMB, and the Soudan 2 experiment (a collaboration of ANL, Minnesota, Oxford, RAL, and Tufts). The ratio of muon to electron events in the GeV energy region was found to be approximately 0.6 of the value which was expected just by counting the numbers of muon and electron neutrinos in the pi-mu-e decay chain (31).

However, not everyone was convinced that this was a real effect or that the effect was due to neutrino oscillations. At the 1992 Dallas Conference on High Energy Physics, Hamish Robertson gave a review which concluded that the interpretation of the low value of the ratio of muon to electron events might be uncertain because the cross-sections in water (for the Cerenkov experiments) of ν_e and ν_μ might be different. At the energies involved, the relative magnitudes of ν_e and ν_μ cross-sections had already been checked at the few percent level in freon and propane in the 1960s CERN PS bubble chamber experiments, as one test of electron-muon universality. So a factor two discrepancy in water appeared to be quite impossible. The relative fluxes of atmospheric electron and muon neutrinos were reasonably well known, and they could be deduced directly from Conversi's 1950 measurements of high altitude muon fluxes as a function of altitude and geomagnetic latitude (32). Conversi had exclusive use of a B29 aircraft that carried him and his Geiger counter array back and forth between Alaska and Bolivia, measuring the latitude effect. From these muon fluxes, assuming them due to pion decay, it was possible to estimate directly the sea-level ν_e and ν_μ fluxes at different latitudes, at least up to energies of about 1 GeV. These results were later reinforced and greatly extended by calculations using the primary proton spectrum and a Monte Carlo of the nuclear cascade in the atmosphere. The absolute fluxes might be uncertain by as much as 30%—the typical variation in the absolute normalization of the measured primary proton spectrum—but the ν_e/ν_μ flux ratios were not, and in fact had been calculated as a function of energy by many people, including Osborne et al., Volkova, Tam, and Young (33), and more recently by Gaisser et al. and Honda et al. (34). All were in substantial agreement at the few percent level. Of course, because particle physicists had been looking for evidence of neutrino oscillations at accelerators and reactors for almost 25 years without success, it was difficult for some to accept that evidence for such oscillations had finally been found, using the rather feeble atmospheric fluxes and massive but quite crude detectors originally intended for a quite different purpose.

The discovery of oscillations of atmospheric neutrinos was indeed another tale of the unexpected. They would not have been found at all if people had not been searching for proton decay in massive detectors deep underground. In the 1960s (after the Jaipur conference mentioned above), and even after the original suggestion of oscillations by Pontecorvo and Maki et al. (35), nobody had requested (and probably would not have obtained) funding to put massive and expensive kiloton detectors deep underground on the off chance that they were right. Accelerator neutrino beams were much more intense, and they were the "obvious" way to check for such phenomena. Even with the later proton decay detectors, oscillations would probably not have been found, had the geomagnetic field been three or four times larger or the Earth's diameter two or three times smaller. Then both primary proton and secondary neutrino energies would have been higher and the fluxes very much lower, and the typical ratio of L/E, the ratio of neutrino path length to energy, might have been too small for the effect of oscillations to show. It is just a happy accident that the relevant neutrino mass difference is well matched to the Earth's magnetic field and diameter and that the relevant mixing angle is large (in fact, maximal). So the search for proton decay, although it failed in its original purpose, was a long shot which actually paid off in the end.

The measured values of the neutrino mass differences, from both atmospheric and solar neutrino observations, indicate tiny neutrino masses, of millivolts or less. In the Standard Model, neutrinos are left-handed and massless, and it is proposed that small but finite masses may be due to mixing with very massive right-handed Majorana neutrinos at the GUT energy scale, according to the "seesaw" mechanism. Thus the atmospheric results give indications of physics beyond that of the Standard Model.

There was even an unexpected bonus to the proton decay investigation. The IMB and Kamiokande detectors recorded the 1987A supernova, the first detection of a neutrino source outside the solar system. This observation provided confirmation of the correctness of our description of the final stages of evolution of very massive stars. So sometimes experiments in high energy physics turn out quite differently, and perhaps even better, than what had been originally expected.

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