7

The CERN Neutrino Experiment

7.1 Introduction

In 1959 the proton synchrotron at CERN, Geneva, Switzerland started running, followed a little later by the AGS (Alternating Gradient Synchrotron) at Brookhaven, Long Island, USA. Both machines accelerated protons to approximately the same energy, 28 and 30 GeV respectively. They were the biggest machines of that time, with a diameter of about 200 m. The intensity was respectable: about \(10^{11}\) protons per 3 seconds. Physicists started working with these exciting new toys. The era of big high-energy physics had started.

At that time the state of affairs concerning particle physics in Europe (with England as an exception) was simply dismal. Ravaged after World War II, Europe started to get back on its feet. Perhaps the biggest problem was the absence of leading physicists; many Jewish physicists had left for the US, and notably also E. Fermi (who was not Jewish, but his wife was). No substantial experimental effort existed anywhere in Europe before 1957, although here and there cyclotrons were built, used however almost exclusively for nuclear physics (the study of the structure of the atomic nucleus). In 1957 a 3 GeV proton synchrotron called Saturne started up in France, but it did not play any role of significance in the development of particle physics that I know of, except perhaps in educating experimenters.

In the US the influence of Fermi cannot be overestimated. To me he is an example of how one man can make a big difference.
Bruno Pontecorvo (1913–1993) and Melvin Schwartz (1932). Pontecorvo has had essentially all the ideas for neutrino experiments. He was the first to think of the so-called chlorine-argon method for detecting neutrinos (including neutrinos from the sun), and he also introduced neutrino mixing (in 1957). The chlorine-argon method was put into practice and further developed by Davis, who demonstrated that reactor antineutrinos were different from neutrinos, and who detected neutrinos from the sun (Nobel prize 2002).

The idea for neutrino experiments at the big machines is due to both Schwartz and Pontecorvo. Schwartz went on to do the experiment, together with Lederman, Steinberger, Goulianos, Gaillard, Mistry and Danby. Lederman, Schwartz and Steinberger received the 1988 Physics Nobel prize for this landmark experiment.

Pontecorvo, a devoted communist, already politically active in the thirties, moved to Russia in 1950 in a somewhat fugitive way. He was one of those scientists who were blamed for defecting to Russia taking along atomic bomb secrets. In his case there is not much substance to that; he was never actually involved in weapons research. He just believed in communism. I guess he paid the price.

Schwartz later suggested beam dump experiments at SLAC, and he had in fact a short run. He showed me a few pictures (dubbed Melons by some) at the time of the 1971 Amsterdam conference, and to me it was immediately clear that he had observed neutral current neutrino events. Conflicts with the SLAC directorate (Panofsky) led a somewhat embittered Schwartz to leave physics, and he started a successful electronics company called Digital Pathways. Personally I believe that he was a better physicist than businessman. He is too honest.
Tsung Dao Lee (1926) and Chen Ning Yang (1922). They shared the 1957 Nobel prize for their work on parity violation. This concerns the behaviour of physics laws when considered through a mirror. Thus do two sets of experiments, and observe the results directly, but also, independently, in a mirror. The question is whether the laws deduced from such experiments will be the same. They analyzed the situation assuming that this is not so, and indeed it is not. When observing the decay of a pion at rest into a muon (and an antineutrino) the muon spins in a left-handed way along the direction of movement in ordinary space, while in the mirror one observes a muon spinning in the opposite way.

Lee and Yang collaborated till 1962, when they broke apart for reasons of their own. In my opinion the sum was better than the two individually, an example of synergy. They had just started on a systematic investigation of vector bosons (the $W$ and $Z$ of weak interactions), and there is no telling how far they could have gone in developing the Standard Model. Lee was very strong on Feynman diagrams, while Yang was together with Mills the originator of gauge theories (also called Yang-Mills theories) that are an essential ingredient of the Standard Model.

The idea of Schwartz for a neutrino experiment caused Lee and Yang to analyze the situation in precise detail. Their work, published in 1960, became the guiding light for both the Columbia and CERN neutrino physicists. Together with Markstein from IBM Lee and Yang did one of the first large scale computer calculations concerning the possible detection of the vector bosons in a neutrino experiment. None were actually seen, they were too heavy.
In Europe the problem was different from country to country. In the Netherlands there were essentially no experimenters in the domain of particle physics, and in fact there was simply no money available to start anything substantial. As to theorists, there were a few of international stature, in particular H. Kramers who contributed in a fundamental way to particle theory. But after Kramer's death in 1952 only one of his pupils (N. van Kampen) worked in a prominent way in particle physics (to which he contributed substantially), switching however to statistical mechanics around 1958.

In most other European countries the situation was equally sad. L. de Broglie, in France, had a very negative influence on the development of theoretical physics there. In Germany there was an aging W. Heisenberg whose image had been tarnished, and who moreover had gone off on a tangent theory-wise. There were some excellent theorists there, but they worked mainly on rather highly abstract subjects, far away from experiment. Italy and England were in better shape, especially in the field of particle theory but also experimentally. In Switzerland there were a number of excellent theorists, notably Pauli and Stückelberg.

In Europe the big breakthrough was the creation of a new international organization for doing research. The CERN treaty was signed in 1953, and only seven years later the first big machine started up in Geneva. Since then CERN has been essential to particle physics in Europe, not only because of the big machines, but also because it functions as a centre of physics that no country could afford by itself. There you could meet all the well-known physicists, mainly the Americans, that had made and did make the field.

This is not the place to start describing the European state of affairs after World War II, although I am not clear who else is doing it. Usually they paper it over. I entered the domain as late as 1961, and cannot competently speak on these matters. When I arrived at CERN in 1961 however, there was no question about it: in particle theory we were nowhere comparable to the US.
myself did not know the difference between kaons or pions at that time, although they had been around since the war. At CERN, it was an American physicist, S. Berman (a student of Feynman), who did put me on the right track, neutrino physics. There I also started a long-time collaboration and friendship with John S. Bell, who was one of the very rare people at CERN actively interested in weak interaction theory and neutrino physics.

I started by trying to compute the production of vector bosons by neutrinos. If the vector bosons had a mass of less than 1.5 GeV their detection would have been within experimental reach at that time. The reader knows already that they are much heavier, around 80 GeV, but we did not have any idea about that. These calculations were quite tedious, had been done before by T. D. Lee, P. Markstein and C. N. Yang, and were essential to the experimenters who were starting up the CERN neutrino experiment. It is in this way that I came into contact with that group, at the end of 1962. The experiment started in June 1963, and from the point of view of physics it was to me a happening of overwhelming influence. Nothing compares to entering a new domain where no one has been before, and that was what I experienced by watching the experimental results come in. In the end the experiment was a failure, but that does not take away my feeling towards that experience.

If asked why that experiment was a failure I would say that it was simply because this was still Europe getting back on its feet. We were all learning. Hardly anybody had any experience to speak of. Others, in particular Lee and Yang, told us what to look for. Technically speaking the experiment was a great success. The CERN engineering staff, among them a sizeable number of Dutch engineers, was second to none. The weak point was physics, and that included the theoretical part. The responsibility for the theoretical part, if there was really anything like that, was mainly in the hands of J. S. Bell and myself, and to this day D. Perkins, experimenter from England, blames us for not discovering scaling (never mind what it is) in the plots that
Leon Lederman (1922) and Jack Steinberger (1921). They shared with Schwartz the 1988 Nobel prize for the discovery of the muon-neutrino at the Brookhaven neutrino experiment.

After the original idea of Schwartz two groups (BNL and CERN) were formed to do the actual experiment. CERN had the advantage. Steinberger, to the dismay of the others in the BNL group, took a sabbatical and joined the CERN group. However, the CERN experiment was aborted unexpectedly when von Dardel, a Swedish physicist, discovered errors in the event rate estimate. The CERN group restarted, using beam extraction and adding van der Meer’s focusing horn (horn of plenty), but they lost their advantage. As Schwartz put it: “Early in 1961 it looked like Jack and his associates at CERN would certainly have the first neutrino events. Then we received a piece of fabulous news. CERN had cancelled the neutrino experiment.”

Steinberger returned to Columbia, and re-joined the BNL group although not without some arguments. A few years later he went again to CERN. He did several fine experiments, but he was also the auctor intellectualis of the split field magnet, a detector with a complex magnetic field that supposedly could unravel anything. However, the thing was too complex and became a computer programmer’s nightmare, or rather cemetery. Cynics changed spl into sh.

In 1950 Steinberger discovered the neutral pion (with Panofsky and Steller). He also produced a theoretical explanation that became important later on.

In 1978 Lederman became the director of Fermilab, as successor to Wilson. Before, in 1977, he essentially discovered the bottom quark in a Fermilab experiment.
he produced on the basis of events found in this neutrino experiment. Scaling, incidentally, was discovered much later, at the end of the sixties, by the theorist J. Bjorken from Stanford, and the SLAC experimenters that did the relevant experiment received the 1990 Nobel prize for this work (not Bjorken). Well, sorry about that, Don.

Scaling was not the only thing missed in the CERN neutrino experiment. But let me discuss it systematically. However, first the experimental set-up must be described, and the physics objectives as seen at that time.

### 7.2 Experimental Set-up

The Italian physicist Pontecorvo (Joint Institute for Nuclear Research, Dubna near Moscow) together with the American physicist Schwartz (Columbia University) can be credited for suggesting experiments with high energy neutrino beams made at the big machines. The main idea of neutrino experiments in the form to be described was due to Schwartz. He started thinking about these things after stimulating conversations with T. D. Lee, who asked if it would be possible to do weak interaction experiments at high energy. The basic set-up suggested by Schwartz in October 1959 along the lines of his and Pontecorvo’s ideas is this.

First let the accelerator run at the highest possible energy, and then collide the protons with some stationary target. In the proton-nucleus collisions in the target many, many charged pions will be created. These charged pions decay mainly into a muon and a neutrino, but you have to wait a bit because the decay occurs only after some time. Thus after the target there is a decay area of some 25 m in which the pions can decay. After that there comes a massive amount of shielding, meant to block every particle except neutrinos. The latter cannot be blocked within earthly distances. After the massive shielding then there are the detectors.

The big point about the idea is the flux of neutrinos that one obtains in this way. Are there enough to give a reasonable
probability of a reaction in the detector? The idea, at that time, was outrageous. But as it happens the neutrino flux is adequate for experimental purposes, although only barely.

Much of the feasibility of the experiment depends on the size of the detector. Evidently, as the amount of matter in the detector increases, the number of neutrino induced events will go up proportionally. As luck had it, the Japanese physicists Fukui and Miyamoto had just invented a new device, the spark chamber. The great thing about spark chambers is that one can make the plates of relatively thick and heavy material. In this way a detector of tens of tons could be constructed. Thus the detector is also the target. The suitability of using spark chambers for a neutrino experiment was first suggested by the American physicist Irwin Pless.

The earliest neutrino experiment was performed at the Brookhaven laboratory, by a group from Columbia University including Schwartz. This will be discussed later. For now the set-up of the CERN neutrino experiment will be described. The figure below gives a sketch of the experimental set-up at CERN in June 1963.

To increase the neutrino flux two important steps were taken. First, new at that time, full beam extraction was achieved. Before that one just placed a target inside the PS machine. Secondly,
Helmut Faissner (1928) and Frank Krienen (1917) talking to Yang at the time of the CERN neutrino experiment. Faissner and Krienen (and one of the original inventors, Fukui, who participated in the CERN experiment) were largely responsible for the spark chamber set-up. The quality of those chambers was excellent, and once the experiment started running they reproduced in a few days the BNL results (run of eight months, 56 events). Then the search for the $W$ started in earnest, and in particular Faissner worked so hard at it that if it had been possible to create such events by sheer will power they would have been there. Alas, no such luck.

Later Faissner and his group in Aachen found the first electron neutral current event of the type neutrino + electron $\rightarrow$ neutrino + electron. That was in 1972, in a photo from the huge French bubble chamber Gargamelle exposed to a high flux neutrino beam. That was strong partial evidence in favour of what is now known as the Standard Model. It convinced many physicists of the correctness of gauge field theory applied to weak interactions.

Krienen was one of the really excellent Dutch engineers that started working at CERN right from the beginning. While there were virtually no Dutch experimental physicists at CERN, Dutch engineers such as Krienen, Kuiper, van der Meer, Middelkoop, de Raad, Zilverschoon and others had a major impact. As mentioned before, Krienen later developed digitized spark chambers. He contributed to many experiments, notably a big $g-2$ experiment at CERN (this is discussed in Chapter 9) involving a muon storage ring. In 1982, retiring from CERN, he went to the US, and started a whole new career and a whole new family including two children.
Building the shielding for the CERN neutrino experiment.

Installing the Heavy Liquid Bubble Chamber.

The spark chambers with a track produced by a cosmic muon.
S. van der Meer invented a magnetic horn. This device, placed around the target, generated a magnetic field such that charged particles were focussed in the forward directions. These two measures led to an increase in the neutrino flux to the point that a bubble chamber could be used as detector: roughly one event per 2000 cycles for a bubble chamber of 0.75 ton. Since the bubble chamber is expanded and a picture taken at every cycle (3 sec.) of the proton synchrotron this means one event per 2000 pictures, or roughly 15 events per day if running optimally.

The spark chamber setup was quite elaborate. First there were relatively light spark chambers, supposedly showing finer details, which indeed they did. This region was called the production region. Following the production region there were two magnet coils around some 5 spark chambers. The idea was to determine the sign of the charge and the magnitude of the momentum of the charged particles coming out of the production region. Finally there was a set of quite heavy spark chambers, called range chamber, meant to determine whether the charged particles seen were muons or something else. This is based on the fact that muons can go a long distance through matter without doing anything, and only they could produce long tracks in the range chambers.

The experiment started in June 1963, and continued till the end of August. Results were presented at a conference at Brookhaven Laboratory by representatives of the engineering group (C. Ramm), the bubble chamber group (R. Voss) and the spark chamber group (H. Faissner). To me fell the task of presenting the conclusions. Quite an experience as you can imagine, since the “fine fleur” of the world’s particle physics community was present. Moreover, this was in fact the entry of CERN into the big world of particle physics. The spectacle of a theorist presenting the conclusions of an experiment can perhaps best be appreciated by quoting V. L. Telegdi from the University of Chicago: “A theorist telling the experimenter what they are doing is like a newly married couple taking a gynecologist along for their wedding night.”
7.3 Neutrino Physics

The theoretical side of neutrino physics was dominated by T. D. Lee and C. N. Yang. They had received the 1957 Nobel prize for their work on weak interaction theory, more precisely the analysis of parity violation in weak interactions, which is something that we need not go into right now. At Columbia University Lee inspired M. Schwartz, who devised the basic mechanism by which neutrino experiments were done. Lee and Yang wrote a paper investigating the physical aspects of neutrino physics, and this became the guiding light to the experimental groups.

The following two questions came to the foreground:

1. The two-neutrino hypothesis;
2. The vector boson hypothesis.

The neutrino hypothesis is the following. The neutrino made its entry in 1930, through a proposal by W. Pauli. Study of beta-decay (of which neutron decay is the prime example) showed that the total energy of the visible end-products did not add up to the initial energy. Here is neutron decay as understood now:

\[ \text{neutron} \rightarrow \text{proton} + \text{electron} + \text{antineutrino} \]

Thus the energy of the proton and the electron did not add up to the energy in the initial stage, which is the mass-energy of the neutron, and we understand now that the remainder is carried off by the antineutrino.

This hypothesis was generally accepted. For a long time the neutrino remained a spooky particle, because it was only seen as an absence of energy and momentum. This changed in 1956, when F. Reines (Nobel prize 1995, shared with M. Perl for the discovery of the tau meson) and C. Cowan succeeded in observing neutrino induced events in scintillation detectors. The neutrinos came from the Savannah River nuclear reactor. A nuclear reactor produces a considerable flux of (anti)neutrinos due to beta decay of the fission products. The experiment solidified the neutrino
idea, but it must be said that quantitatively speaking much was still unclear.

After the war other reactions were observed where presumably neutrinos carried off energy. Examples are:

\[ \text{pion} \rightarrow \text{muon} + \text{neutrino} \]
\[ \text{muon} \rightarrow \text{electron} + \text{neutrino} + \text{antineutrino} \]

For theoretical reasons people started to ask themselves if all these neutrinos were the same, i.e. if there was more than one kind of neutrino. More specifically, it was suspected that neutrinos (or antineutrinos) produced together with electrons might not be the same as those produced together with muons. Thus the neutrino (never mind that it is actually the antineutrino) in neutron decay, produced together with an electron, would be a neutrino of the electron type, while the neutrino from pion decay, produced together with a muon, would be a neutrino of the muon type. And in muon decay, with two neutrinos there would be one electron-neutrino and one muon-neutrino. This idea, that there are two kinds of neutrinos, is called the two-neutrino hypothesis. The way the idea is implemented is by means of two quantum numbers: electron number and muon number. Electrons and electron-neutrinos have electron number 1 and muon number zero, while negative muons and muon-neutrinos have muon number 1 but zero electron number. Of course, the antiparticles have the corresponding negative value for their muon or electron number. All other particles have zero electron or muon number. In pion decay one starts with muon number zero (the pion) and ends with a muon number zero (a positive muon has muon number –1, the muon-neutrino has +1). In muon decay, starting with a negative muon, the initial value of the muon number is +1. In the final state there is a muon-neutrino (muon number 1), an electron (electron number 1) and an anti-electron-neutrino (electron number –1).

All this amounts to the following. In a neutrino experiment the great majority of the neutrinos come from pion decay, and since
Here a special spark chamber picture from the CERN neutrino experiment. The neutrino beam was sufficiently intense to cause sometimes the occurrence of two events simultaneously. Both events show at least one straight ongoing track, typically a muon. It is from this type of event that the BNL group concluded to the existence of two neutrinos, as there always seemed to be a muon and practically never an electron. Electrons (and positrons) are very easy recognizable in a spark chamber because they produce a shower, a multitude of relatively small tracks. The CERN experiment produced in a short time many events as shown above, thus confirming the results of the BNL group and the existence of two neutrinos.

Neutral current events are characterized by the absence of either a muon or an electron. Just imagine that the muon is not there. The lower event would have been relatively easy to identify. The trouble is that stray neutrons coming somehow around the shielding could produce something quite similar. The problem of seeing neutral currents became one of how to eliminate that possibility. But as there was really no interest in neutral currents at that time no one thought of doing that.

Sometimes I try to imagine how history would have gone if indeed neutral currents had been established by the CERN experiment. I guess they would have become part of the weak interaction phenomenology, and that would be it. If there is no theoretical framework, which indeed there was virtually none at the time, it is very difficult to see that this points to some type of theory.
The pions decay almost exclusively into a muon and its neutrino; it follows that the neutrinos in these experiments are almost exclusively muon-neutrinos. There are some electron-neutrinos in the beam, for example from kaons that decay into neutrinos and electrons (and other stuff), but they amount to very little.

The question is what these neutrinos do in the detectors. If the two neutrino hypothesis is correct then an event starting off with a muon-neutrino must wind up with either a muon or again a muon-neutrino in the final state. However, there should not be an electron in the final state, although electrons can appear in addition to the muon, see below. So here was experimental objective number one: establish if the two-neutrino hypothesis is correct. The procedure is simple: look to neutrino induced events and see if they have muons and/or electrons in the final state. The muon goes a long way unperturbed through material and thus produces long tracks, the electron gives rise to a shower, so this point can be easily checked provided you have neutrino events in the first place.

The vector boson hypothesis is another story. At this point there is no real need to delve into the theory of this object; the only thing to know is that they can be produced in neutrino events in addition to the muon. A vector boson may decay into pions or kaons, but in addition it will decay with some probability into an electron-neutrino and with the same probability into a muon-neutrino pair. This is the way a vector boson can be established: a muon-neutrino collides in the detector and produces a muon and a vector boson (and possibly some further debris, never mind that). Now the vector boson decays quite quickly and in a certain percentage of the cases it will go in a electron-neutrino or muon-neutrino pair. In those cases one observes in the final state with equal probability an electron (plus neutrino) or a muon (plus neutrino) coming from this vector boson. In short, since the neutrino is invisible one sees either two muons, or a muon and an electron. We did not make any effort to get the charges correct, or state precisely which is the particle, and
One of the very first bubble chamber pictures seemed to have just the right signature for a $W$ production event. The small figure on the right shows the event redrawn. The neutrino beam entered from the right. After the collision several particles came out, and there was a recoiling nucleus (N in the drawing). The negative muon is recognized as such because it rarely interacts and does not lose energy. The remaining tracks are electrons and positrons that lose energy fairly rapidly and they actually come to rest. The tracks curve due to a magnetic field. The positrons curve in the opposite way as compared to the electrons. What one sees is a shower. The photons, invisible, generated by a previous electron or positron, convert to electron-positron pairs (the seagulls). One such photon is indicated by a $\gamma$. There is a single electron (see arrow) perhaps kicked out of an atom.

In the first instance the shower was seen as due to a single positron. Thus it seemed as if there was just one muon and one positron. The $W$ interpretation however was discarded later on because the energies and momenta did not check out. The angle between the muon and the positron was too large to be a $W$ event. But in the first few days everyone thought we were going to get many such pictures, thus discovering the $W$. However, no such picture was ever produced again in the bubble chamber. It was a nasty little joke of Nature.
which the antiparticle, because at this point that does not matter. Just look for two muons, two long tracks, or one muon and an electron (or positron), i.e. one long track and a shower. That is the signature for a vector boson event.

In 1960 no one knew what the mass of the vector boson was, and the main issue then was whether neutrinos in the neutrino beam were sufficiently energetic to allow the production of such a boson. At CERN one started off with 25 GeV protons, but by the time one has neutrinos their energy goes down considerably. In practice vector bosons could have been observed provided they were lighter than 1.5 GeV. That is a far cry from the value established now: 80 GeV.

These were then the main objectives for the initial neutrino experiments. Two machines were starting up, the CERN PS and the Brookhaven AGS. In fact, the Brookhaven machine would reach completion about 6 months after the CERN machine started, thus CERN had a 6 month advantage to achieve these physics goals. Since no one would see vector bosons in these experiments it became a matter of who would verify the two-neutrino hypothesis. That was where the prizes were, and indeed the 1988 Nobel prize was given for that. The history is amusing, so let’s tell some about it.

**7.4 The First Neutrino Experiments**

After the initial idea of a neutrino experiment Schwartz undertook its implementation. He obtained money, collaborators, and most importantly, the cooperation of the director of the Brookhaven lab, the well-known physicist M. Goldhaber. While at CERN J. Adams, director till August 1961, was an engineer, Goldhaber was perhaps more aware of the potentialities and the importance of the experiment, and he was willing to grant the Columbia group the necessary privileges, including 8 months of running time. I guess he also risked his own life if I may believe Lederman’s story as told in his book. Columbia physicists consider the Brookhaven machine
their private property. US laboratories have usually experimental physicists as director, while at CERN one has had engineers and theorists next to some experimenters.

At CERN a neutrino group was formed under the direction of G. Bernardini, an Italian physicist. He had considerable experience in particle physics, and he was a first-class physicist. Most other members of the CERN group were newcomers to the profession. They had a head start, in principle, of six months on the Brookhaven group. There was naked competition.

So, the CERN group started out by designing a neutrino beam. For speed's sake beam extraction and the magnetic horn of van der Meer were not part of the first design. A target would simply be inserted into the proton machine itself. No kicker magnet, no beam extraction, no horn. Also no spark chambers, instead two bubble chambers and a large scintillation counter set-up with a Wilson cloud chamber. A group of three people computed the neutrino flux and the event rate on the basis of this crude set-up.

What happened then is very difficult to get straight as different people have different recollections, and I cannot claim to have the complete truth nailed down here. I arrived at CERN in September 1961, and did not know of all the commotion till long afterwards. Anyway, here is at least an important part of the story as I learned from various letters written in that period.

In May 1961 the physicist Guy von Dardel (from Sweden) discovered flaws in the neutrino flux calculations. Remarkably, he was not even a member of the CERN neutrino group, but had been asked to verify by measurement the estimates used for the initial pion flux. The synchrotron consists of a ring of magnets, separated by small straight sections. The target was to be placed in one of those straight sections. The pions coming out of the target would have to pass rather narrowly by the subsequent magnet in the ring, and the magnetic field influences the pions. The amount of influence is directly related to the magnitude of the straight section where the target would be put inside the machine. Von Dardel then investigated the original calculation and discovered
Helmut Faissner (1928), Guy von Dardel (1919) and Giampietro Puppi (1917) at the CERN terrace in June 1962. Guy von Dardel is related to Raoul Wallenberg, the Swedish diplomat who helped many Jews during World War II. He did that in Budapest, and at the end of the war, when the Russians entered Budapest they took him and he disappeared in the goelag, to die after two and a half years. For a long time the Wallenbergs, and notably also von Dardel tried to find Raoul, but until 2000 the Russians would not acknowledge his existence, or even that he ever was in Russia. On the web you can find out more about this.

Puppi, an Italian experimental physicist, got some fame for the introduction of the Puppi triangle, suggesting that among others the processes $\mu \rightarrow e + \nu + \nu$ and neutron $\rightarrow$ proton $+ e + \nu$ would go at equal strength. That triangle was too simple and disappeared when Cabibbo introduced his angle (Chapter 3), but even so it contained an important truth: lepton-quark symmetry. It is that symmetry, often cited by Gell-Mann, that led Hara to introduce the fourth quark now called charmed quark (see at the end of Chapter 8).

Puppi was for a few years director of the research division of CERN. He was candidate for the position of director of CERN (after Weisskopf), together with Gregory, and we had a hard time deciding to whom we should be friendly. Gregory won.

The CERN terrace, where you can see the Mont Blanc on the horizon, is very popular among high-energy physicists. You can meet there just about everybody in the business. Many initiatives were started there, and many ideas were born in that environment. So far you can still smoke a cigar there.
that it contained errors on this point. Also, they had not taken into account a very elementary fact: when a pion decays the resulting neutrino does not go on in exactly the same direction as the pion, but it may deviate sideways, see figure below.

\[
\pi \rightarrow \mu + \nu
\]

According to von Dardel it would take about 6 months to obtain some 3 or 4 neutrino events, with the machine totally dedicated to the neutrino experiment, suspending all other experiments. Von Dardel got very emotional about it and called it a scandal. In the summer of 1961 CERN decided to postpone the experiment.

At CERN the straight section had a length of 1.5 m. At Brookhaven it was actually 3 m, so the pions at Brookhaven were liable to be influenced a lot less than those at CERN. The Columbia experiment at Brookhaven started running in December 1961, and in 1962 the result was announced on the basis of 56 events obtained in 8 months of running time: there are two neutrinos. Mostly muons were observed, only a few electrons. The three leaders of the experimental group, Lederman, Schwartz and Steinberger shared the 1988 Nobel prize for this discovery.

Should CERN have gone ahead despite the set-back? No one knows for sure. But by all accounts the neutrino flux seemed too low, and the detector mass (no spark chambers) too small. One thing is certain: when Schwartz heard about the CERN decision to postpone he was overjoyed!

### 7.5 Vector Bosons

After the publication of the Columbia experiment results CERN found itself in the unenviable position of having been scooped. The consequence of that was clear: CERN would have to focus on the discovery of the vector boson. On the other hand, they now had the time to install beam extraction and the magnetic horn,
and the neutrino flux was certain to be many times larger than that of the Brookhaven experiment. Also they now had a large spark chamber set-up. With great anticipation the experiment was started in June 1963. Spirits were high. It was decided to put also a bubble chamber in the neutrino beam, since it seemed that the neutrino flux was sufficiently large to produce events even in this relatively low mass device.

It was actually possible to follow the experiment very closely. The spark chambers produced pictures that were developed very quickly. Moreover, one could go into the space where the spark chambers were set up, and then see them fire if there was an event. Here a short digression about the triggering of the spark chambers.

One knew by that time that neutrino events almost always have a muon in the final state. In particular vector boson events would have a muon together with the vector boson. Everybody was very nervous about background and stray particles (coming somehow around the shielding), and it was decided to trigger the spark chamber only if a charged particle was coming out of the production region. In addition, there was an anti-trigger against the case that a charged particle entered in the beginning of the production region. Neutrino events, all things considered, are very rare events and almost anything else is overwhelmingly more frequent. Much depended on the quality of the shielding, not only in front of the detectors, but also on the side, above and below. In retrospect the shielding was very good, but that was not known beforehand. The bubble chamber cannot be triggered, it has to be fired every time the proton synchrotron discharges its protons, so if the background had been that bad that instrument would have been flooded. But such was not the case, and the bubble chamber produced about 240 reasonably clean events out of 461 000 pictures.

So, one could sit near the spark chambers and see the events coming in. This was extremely exciting. You could try to guess if an event was of the vector-boson type for example. It was like entering a new world. To me this was one of the most fascinating
periods of my scientific life. It tied me forever to this profession. There is a quality to such a trip in the unknown that I can only compare to the landing of the Viking spacecraft on Mars. Those having followed that event on television may understand what I mean.

As the reader knows by now no vector boson was found in the CERN neutrino experiment. Even after a few days it became clear that if there was anything like that it was not going to be easy. This had a disastrous effect on the morale of the group. The interest in the experiment collapsed almost immediately. It looked like nothing would come of it. This was perhaps most clear in the systematic scanning of the spark chamber pictures as they came in. In the beginning everybody was hanging around the scanning tables (the pictures were projected onto some large surface where they could be looked at and measured). But after a little while almost nobody bothered to look. I believe that Bernardini and I are the only ones that have actually seen all the pictures coming from the spark chambers in the production region. We became great friends, he, the relatively old and experienced Italian experimenter, and me, the would-be theorist. I think of that time with the greatest fondness for Bernardini. And here you have the reason why I came to represent the CERN experiment at the Brookhaven conference. Bernardini, disappointed because of the negative result of the experiment, did not like to go there and instead he made me his representative.

There were many incidents in that period, and I am not going to detail any of them. In my opinion, if the spark chamber group had been more realistic, if the various participants had been more tenacious, quite interesting results would have been obtained from the data. But they had lost interest. In the end the events were not measured, and no systematic analysis was made. It is my feeling that this is an important part of scientific discovery: do not give up till the last stone has been turned. Moreover, always try to do the extra bit, go the extra distance. Ten years later we saw another example of this phenomenon, the discovery of the $J/\psi$ particle at
SLAC (Stanford) and Brookhaven. This particle has a mass of 3096 MeV, and could be discovered in an electron-positron collider of that much energy. This was just above the energy of such a collider at Frascati, called ADONE, whose design energy was 3000 MeV. When the people at Frascati heard about the SLAC discovery they needed only a few days to screw up the energy of the machine and observe that particle. As someone told me: they were on strike or something, but when the news broke everybody, from the cleaning personnel upwards, showed up at work on the very next day. Now why had nobody tried to do the extra thing before? I am sure some people there are still gnashing their teeth!

At the Brookhaven conference I reported that no vector boson had been seen. This was in the form of a limit. To quote verbatim: “Neglecting uncertainties in the branching ratio in the decay of the $W$, we conclude that $M_W > 1.3$ GeV.” In other words, if there is a $W$ in the experiment, for which there is no evidence, its mass must be larger than 1.3 GeV or else it would have been seen. The misery of the CERN experiment did not end there. Shortly after the Brookhaven conference (9–11 September 1963) another conference was organized in Siena, Italy (30 September–5 October 1963). Also there the conclusions were presented by a theorist. I was not present as I had gone straight from Brookhaven to SLAC, but J. S. Bell, functioning as neutrino theorist (together with the Norwegian theorist J. Løvseth) did present that talk. As he felt that I had contributed to the subject he added my name to the paper, without ever showing it to me. He thought he was doing me a favour, but that was not the case! Communication between Europe and the US was in those days less easy than it is today. Anyway, physicists from all over the world were in attendance at the conference and demanded a clear statement from the CERN group: was there or was there not a vector boson in their experiment? I have heard of nightly gatherings on the top of a tower in Siena where the CERN people were tormenting each other over this question. Would they miss an important discovery? In some halfhearted way they admitted to the existence of a $W$. 
John S. Bell (1928–1990, right) and I at CERN in Bell’s office 10 years after the neutrino experiment. We were the quasi-official theorists of that experiment. We did not do very well, all things considered, because of inexperience and ignorance. After the experiment, in 1963, we both went to SLAC, where I wrote my computer program Schoonschip and he developed his famous inequalities. We also discussed other things, even wrote a paper together that was never published. He considered his work on the fundamentals of quantum mechanics as a hobby, mainly to be done in the evening, at home. He told me that he intended to do away definitely with this nonsense of hidden variables, and so he did. Later he drifted more and more into this subject, and as I consider it as some sort of foolishness not good for anything having to do with the real world, I once asked him: “Why are you doing this? Does it make the slightest difference in the calculations such as I am doing?” To which he answered: “You are right, but are you not interested and curious about the interpretation?” He was right too, up to a point. While his work became very important, as it could be verified by experiment, often in this branch of physics the discussions are on the level of finding out how many angels can dance on the point of a needle. But even so: there are interesting things there.

In Ann Arbor a happening was organized on the occasion of my sixtieth birthday, in 1991. They asked Bell to talk there, but he died suddenly. When I came to CERN some time later I sat in his office and accidentally touched his computer keyboard. The screen lighted up and there was his last e-mail, to Ann Arbor: “O.K., I will sing.” It was a sad moment.
I think that Bell just presented what the experimenters told him, and there is in “our” paper the ominous statement “We would be very surprised if it (the $W$-mass) rose as far as, say, 2 GeV.” In other words, the $W$ was there and it had a mass below 2 GeV. This was the low point in the CERN neutrino experiment.

### 7.6 Missed Opportunities

Opportunities were missed due to a number of factors, but I would say that the major one was the failure to analyze the spark chamber data in a systematic way. Some 2000 neutrino events had been registered in the production region in the period June–August 1963, and they have never been digested in any serious manner.

The bubble chamber group was much more serious with its analysis. They were however dealing with a much smaller number of events, about 240 in the period mentioned. It is hard to do much with this small sample, though not impossible.

What physics was there in those data? There are essentially two issues that may be discussed here, namely neutral currents and scaling. Let us start with neutral currents.

As stated before, if there are two neutrinos there is a new quantum number, namely muon number. Assuming now that the neutrinos in the neutrino beam are all muon-neutrinos it follows that in every event induced by such a neutrino there must be in the final state either a muon or a muon-neutrino. The latter case means that one has a reaction with a neutrino coming in and one going out, and no muon. This type of event is called a neutral current event. While one cannot see the neutrinos one can see the other products of the collision, such as the nucleus breaking up in addition to new particles such as pions. The difficulty is that this is not a very clear signature: no long muon track, no electron shower. It looks in the first instance quite a lot like an event that would be induced by a stray neutron, hitting a nucleus and making it come apart. Thus identifying this type of event requires
a clear background analysis. To see how such events look like take all the events containing a muon and take away the muon. There were 2000 examples of events with a muon.

Theoretically there was a rather heavy bias against this type of events (without a muon). In quite different circumstances things like that, involving two neutrinos and no electron or muon had been found to be absent with a high degree of certainty. By ‘involving two neutrinos’ we mean events where there were two neutrinos, either one neutrino coming in and one going out, as in the neutrino experiment, or two going out, as in certain decay type reactions. As we will see, those things are theoretically very similar. Since no one had any idea about the details of neutrino interactions no one thought twice about this type of reaction. Whether they were there or not was not a burning issue. Lee and Yang had not made the point with any force. Neutral current events are in fact mentioned in their article, but not more than that. At the end of their influential article they literally state “the question of a neutral vector boson will not be examined here”. Consider the expression ‘neutral vector boson’ as a synonym for ‘neutral currents’. Here I can only repeat: there was not much interest in that question at that time, something that changed after 1971.

Experimentally it had been made sure that even if there were such events they would not be discovered. Living in fear of the background there was the muon trigger, requiring a muon leaving the production region. If there was a neutral current event the spark chamber would not fire. At some meeting of the group the issue of running without a muon trigger was raised, but it was voted down. Even then there was something that could possibly be seen in the data, as there were sometimes two events on one picture. In any case, no neutral current events were ever reported.

The bubble chamber was in a different position. Here there was no muon trigger. In fact, we know now that there must have been a substantial number of neutral current events in the bubble chamber pictures. An analysis was made, but (1) no one was
Gilberto Bernardini (1906–1995). In 1963, when I started taking an interest in the neutrino experiment, Bernardini was essentially the boss. I got in because I had redone calculations concerning $W$-production (first done by Lee, Markstein and Yang) adapted to the CERN experiment requirements. Bernardini and I became friends instantly, although one can of course not ever become as good friends with an Italian as another Italian. One lacks the refinement in language, choice of words, and knowledge of Italian literature. Bernardini was a very cultured man.

From time to time we used to walk the CERN corridors. He would occasionally put his arm around my middle, which embarrassed me greatly. It must have been a remarkable sight, he the little Italian (from my perspective) and me, the much younger rather blocky Dutchman. We shared however one thing: passion for physics. When the initial excitement was over most spark chamber people did not show up in the evening or at night. However Bernardini and I were there every night, looking at the spark chamber pictures and hoping to see the $W$, or even better, the unexpected. It was an exciting period.

The rather famous picture of Bernardini above shows one of those things. The neutral current ratio $R$ was given as to be less than 5% (the equation below his arm at the level of his middle). In actual fact the number is about 15%. This error was due to some misidentification in the bubble chamber pictures. A correcting article was published before gauge theories, demanding neutral currents, became popular (after the 1971 Amsterdam conference).
interested and (2) errors were made. Today some (including me) believe that neutral current events could have been established on the basis of those bubble chamber data, but just marginally.

After 1971 neutral current events became overwhelmingly important as they would testify to the gauge structure of weak interactions (this will be discussed later). Let me digress on this for a moment.

Neutral current events are like neutrino events with the muon missing. Background events induced by stray neutrons are different in a number of ways. Firstly, neutron induced events look different, the secondary products tend to be much more spherically distributed. Second, since neutrons are absorbed rather easily, these neutron events tend to be located in the first part of the bubble chamber. If the bubble chamber is too small, that effect may not be sufficiently manifest. After 1971 when the very large French heavy liquid bubble chamber (called Gargamelle) was placed in the neutrino beam this analysis became feasible and indeed neutral current events due to neutrinos were established.

The issue of scaling is more complicated. This has to do with the probability of a neutrino event depending on the energy of the neutrino, and the question of elastic versus quasi-elastic events.

A neutrino basically collides with a neutron in a nucleus, and whether or not the remainder of the nucleus remains intact is of little consequence. Of interest is the basic mechanism, the neutrino colliding with a neutron. The following type of event is called a quasi-elastic event:

\[
\text{neutrino} + \text{neutron} \rightarrow \text{muon} + \text{proton}
\]

Normally an elastic event has the same particles in the final state as in the initial state. In some global way that is what we have here if we see the muon and its neutrino as two members of one and the same family, and similarly for the neutron and the proton. An inelastic event is when extra particles are created, for example

\[
\text{neutrino} + \text{neutron} \rightarrow \text{muon} + \text{proton} + \text{pion(s)}
\]
For higher neutrino energy inelastic events become much more probable than quasi-elastic events. This is well understood today because we know that the neutron is made of quarks, and the reaction is basically a neutrino-quark collision. A sufficiently high-energy neutrino will simply break up the neutron, which will show up as debris in the form of pions as seen in inelastic collisions. Just as we ignored what the other nucleons in a nucleus do we should ignore what the other quarks in the neutron do. That is how we see it now. The break-up of a neutron is more complicated than that of a nucleus, but there is no basic difference. But this was not understood in the old days, and in 1963 no one had an inkling about quarks.

The figure below shows a plot of the number of neutrino events seen depending on the energy of the neutrinos. It is assumed here that the neutrino flux is the same for neutrinos of all energies (which of course is not true, but that can be taken into account). For low energy neutrinos only the quasi-elastic reaction is possible. Initially the number of such events goes up strongly as the energy of the neutrinos increases,\(^a\) but then levels off to a constant. However, for higher neutrino energy the inelastic reaction becomes possible, and as the energy goes up reactions with more and more secondary pions are seen. All in all, the inelastic events tend to compensate for the leveling off of the quasi-elastic reaction, such that the strong increase at low energy persists to higher energies if inelastic reactions are included. This whole behaviour can be understood by assuming that the neutrinos essentially collide with a quark inside the neutron or proton, and at higher energies the neutron or proton breaks up, which is manifested by extra pions being emitted. The basic reaction, however, always remains the same (neutrino-quark scattering), and one knows of that reaction that its probability increases with the neutrino energy in the manner seen.

\(^a\)Proportionally to the neutrino energy squared.
In the figure we have cheated a bit to make the point. At low energies only the above mentioned quasi-elastic event involving initially a neutron can happen. A neutrino impinging on a proton cannot produce a quasi-elastic reaction because of charge conservation. The neutrino becomes a negatively charged muon (it must because of muon number conservation) and therefore the positively charged proton would have to become a doubly charged nucleon, which does not exist. The only nucleons with a mass below 1000 MeV are the proton and the neutron. For inelastic events there is no such problem, there may simply be an extra $\pi^+$. Thus inelastic events also happen with initially a proton, and that must be taken into account when making the plot.

7.7 Epilogue

The spark chamber pictures of the production region remained at CERN in some cupboard, and no one worked on them. In 1971 I visited CERN and decided to have a look at them, because it had become clear to me that neutral currents were important in connection with the developing theory of weak interactions. I looked at the place where I had last seen them, but I could not find anything. Eventually I discovered that they had been burned. To this day I find that one of the most incredible things about that experiment.