Neutrino: the mutant particle
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A story of neutrino oscillations

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1. Introduction

1998 is commonly considered the year of the discovery of the neutrino oscillations. Indeed, the story is rather more involved and lasted for about 30 years. In those years, a hot debate had developed about the lack of electron neutrinos coming from the Sun as well as the lack of muon neutrinos originating in the cosmic ray showers. However, this debate was restricted among the researchers directly involved in the experiments. In 1998, the evidence was so clear to cancel all the doubts and the results was accepted by the whole scientific community. Due to the fact that this complex story had many protagonists, the Nobel prize has been awarded only in October of 2015, after 17 years, to Arthur McDonald for the oscillations from Sun neutrinos and to Tataaki Kajita for the oscillations to atmospheric neutrinos. This award follows that assigned in 2002 to Raymond Davis jr. and Masatoshi Koshiba, which had as official motivation the first detection of astrophysical neutrinos from the Sun and from the 1987 supernova. This short history is focused to the period up to June 1998, when the evidence of neutrino oscillations became clear.

An extended version of this article can be found at the address www.lnf.infn.it/sis/preprint.

2. Neutrinos from the Sun

The 7th of March 2003 in a conference at the *Accademia dei Lincei*, organised by Milla Baldo Ceolin, Arthur McDonald, spokesperson

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of the Sudbury Neutrino Observatory (to become a Nobel laureate in 2015), pointed out that the deficit of Sun neutrinos (neutrinos of electron type) was known since 1968, the date of the first experimental work on the Sun neutrinos flux with the chlorine experiment of the Nobel laureate R. Davis jr. [1].

The deficit was large, about 30%, and it became statistically very significant. Already in the '70 it would have been possible to think at oscillations as a cause, and an experimental program for a more precise study could have started then. Nicola Cabibbo asked why this did not happen and why 30 years were needed to accept the neutrino oscillation phenomenon, which was already predicted by Bruno Pontecorvo in 1957. I remember McDonald to reply that it was a problem of scientific sociology.

Until the early fifties of the past century, particle physics had developed via the study of the cosmic rays. Then, there was the a quick development of the accelerators, and all the energies of the researchers were addressed to the research with accelerators. In a short time, the achievements became enormous and, immediately, people though that particle physics could be studied only with accelerators. The idea that one could do particle physics without accelerators at that time was proposed by a minority of researchers and mainly discussed in cosmic rays conferences.

Furthermore, the refined radio–chemical techniques used by Davis and collaborators where often not understood by the experimental physicists of that times, and the complex theoretical estimation of the Sun neutrino flux of John Bachall was suspiciously considered. All this incomprehension lead, in the USA, to non approving a second generation radio–chemical experiment on the solar neutrinos with Gallium. Experiments with Gallium were only approved in the second half of the 80's, and in Europe, at the Gran Sasso laboratories (Gallex) and in Russia (Sage).

In addition to these experimental prejudices, there was also a theoretical prejudice. In analogy with similar phenomena for the quarks, it was assumed that neutrinos oscillations would be too small to produce the observed large reduction of flux. In 1978 the influence of propagation through matter on the neutrino oscillations (matter effect) was studied by Mikheev, Smirnov and Wolfenstein [2], [3]. This effect could produce an amplification of the oscillations. Therefore, a solution of the problem of solar neutrino oscillations, which saved the theoretical prejudice, was formulated: the original oscillation amplitude was small, but it was amplified by the matter effect.

A few theoreticians [4] indicated that, given the possibility of solar neutrinos oscillation, there was a solution in terms of small oscillation amplitudes and mass differences of about 10 eV. In this case neutrinos would have a relevant role in cosmology and they could explain the dark matter problem. On the base of these considerations, two experiments, CHORUS e NOMAD, were approved at CERN, aiming at the search for muon neutrino oscillations on the distances of the order of km.

Anyhow, I want to recall the presence of a small group of theoreticians not aligned to that dominant way of thinking. After the first confirmations of the deficit of solar neutrinos, and the beginning of the anomaly of the atmospheric neutrinos, G.L. Fogli started to work on the problem, and, in the 1994, the group of theorists from Bari, (G.L. Fogli, E. Lisi e D. Montanino) published a work [5] about a global analysis of the neutrino oscillations in solar and atmospheric neutrinos. This is the first of a set of papers which continues nowadays.

3. Atmospheric neutrinos and neutrino beam from CERN to Gran Sasso

The story of the deficit of muon neutrinos coming from showers, produced by the interaction of cosmic rays with the atmosphere, is even more complicated than that of solar neutrinos. In addition to the above outlined prejudices, a further problem arose because the various experiments were giving different results: today, these should be considered as due to statistical fluctuations and also to wrong data analyses. In the '80s the theoreticians of the great unification (GUT) predicted that the proton could be unstable with a half–life value which should produce visible effects in 1000 tons detectors. Two different techniques were proposed: detection by means of the Cherenkov effect in water (IMB in the USA and Kamiokande in Japan) and detection by a calorimeter with iron plates separated by tracing detector (Frejus in France, Nusex in Italy, under the mont Blanc and Soudan in USA). The search for proton decay was limited by atmospheric neutrinos which could produce events similar to those expected from proton decay.

Early on, in 1986, the IMB experiment [7] observed the first atmospheric neutrinos and it turned out that the number of the detected muon neutrinos was smaller than expected, while the number of electron neutrinos was compatible with the predictions. This provoked a great excitment, as it was immediately clear that a possible cause for this effect was the oscillations of the muon neutrinos. The result of IMB, and then of Kamiokande, was not confirmed by Frejus nor, with smaller statistical evidence, by Nusex. Many people though that the effect was due to the differences between neutrino interactions in iron and in water.

The situation was further complicated in 1992, when the IMB collaboration published an analysis bases on muon produced by muon neutrinos and stopped inside the detector ("stopping muons") [8]. In this paper it was stated that there was no evidence for oscillations. Based on this analysis, they excluded wide regions of the values of two important parameters of the oscillations, the amplitude and the mass squared differences. In particular, they excluded just the values of the parameters which we have now well measured.

We show in figure 2 a plot similar to that published on the prestigious journal Physics Review Letters. This result seemed to be a definitive proof that the muon neutrino deficit was an instrumental issue. Other indications, confirming this result, were coming from that category of events called "upward muons" in the IMB, BAKSAN and Kamiokande itself: they seemed to exclude a muon deficit.

Despite all this, and in a restricted circle, the community was convinced that something should be there. I remember that in 1979, A. Zichichi, then chair of INFN (National Institute for Nuclear Physics), started the project of the underground laboratory under the Gran Sasso mountain. Since the beginning of the project, the possibility of oscillation experiments on a path of 732 km from CERN to the Gran Sasso was considered [9].

Around 1992, the Nobel laureate Carlo Rubbia, CERN director from 1989 to 1992, began to be interested in the issue [10]. Rubbia reconsidered the old idea of the beam from CERN to Gran Sasso and pushed for project of neutrino beam. With that beam one could test, in a controlled manner, the atmospheric neutrino anomalies.

However, the beam was never seriously considered, on the basis of the dominant prejudices. Even after 1998, the community of the European experimental physicists was divided, and the CERN – Gran Sasso neutrino beam was approved by INFN only in december 1999, during the INFN chair of E. Iarocci. At that time the competing projects of MINOS in the USA and K2K, T2K in Japan were already in an advanced state of development. As an example of the difficulties encountered, we can mention that some of the European countries refused to participate to the project, even though it was almost completely financed by Italy. For the approval of the CERN – Gran

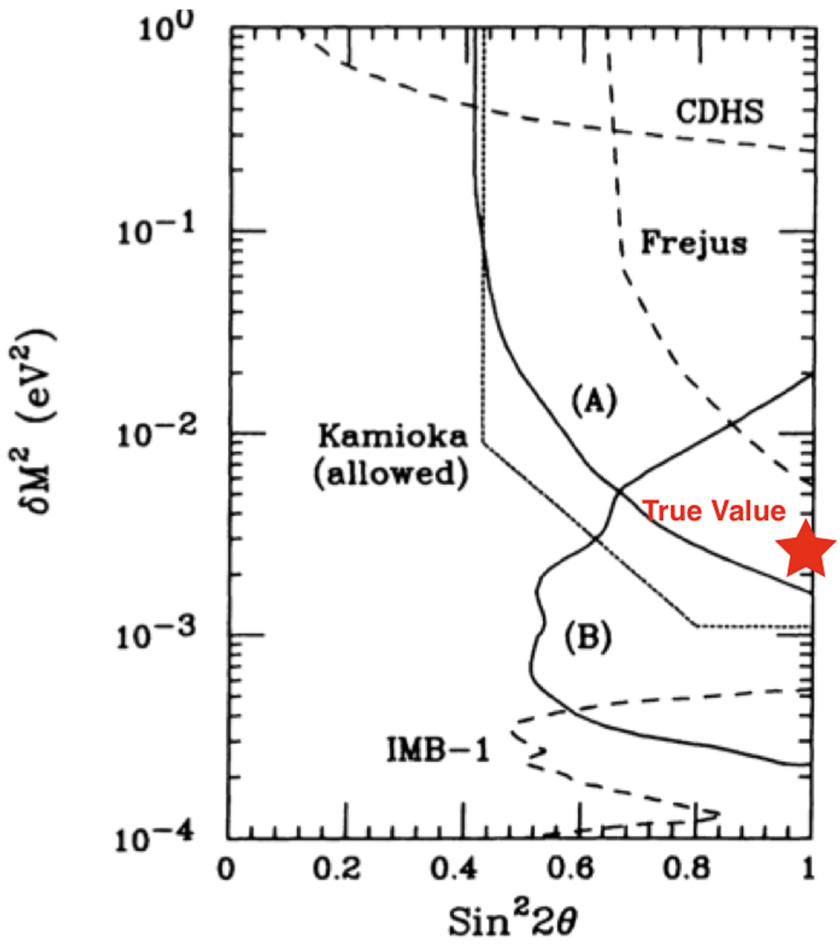


Figure 1. Figure analogous to that of the IMB paper published on Physics Review Letters in 1992 [8]. All the curves include excluded regions, except that of Kamiokande, which defines the allowed region. One should notice that the curve B of IMB completely excludes the red star that represents the presently accepted oscillation values. This wrong result generated great confusion and slowed down the claim of the discovery

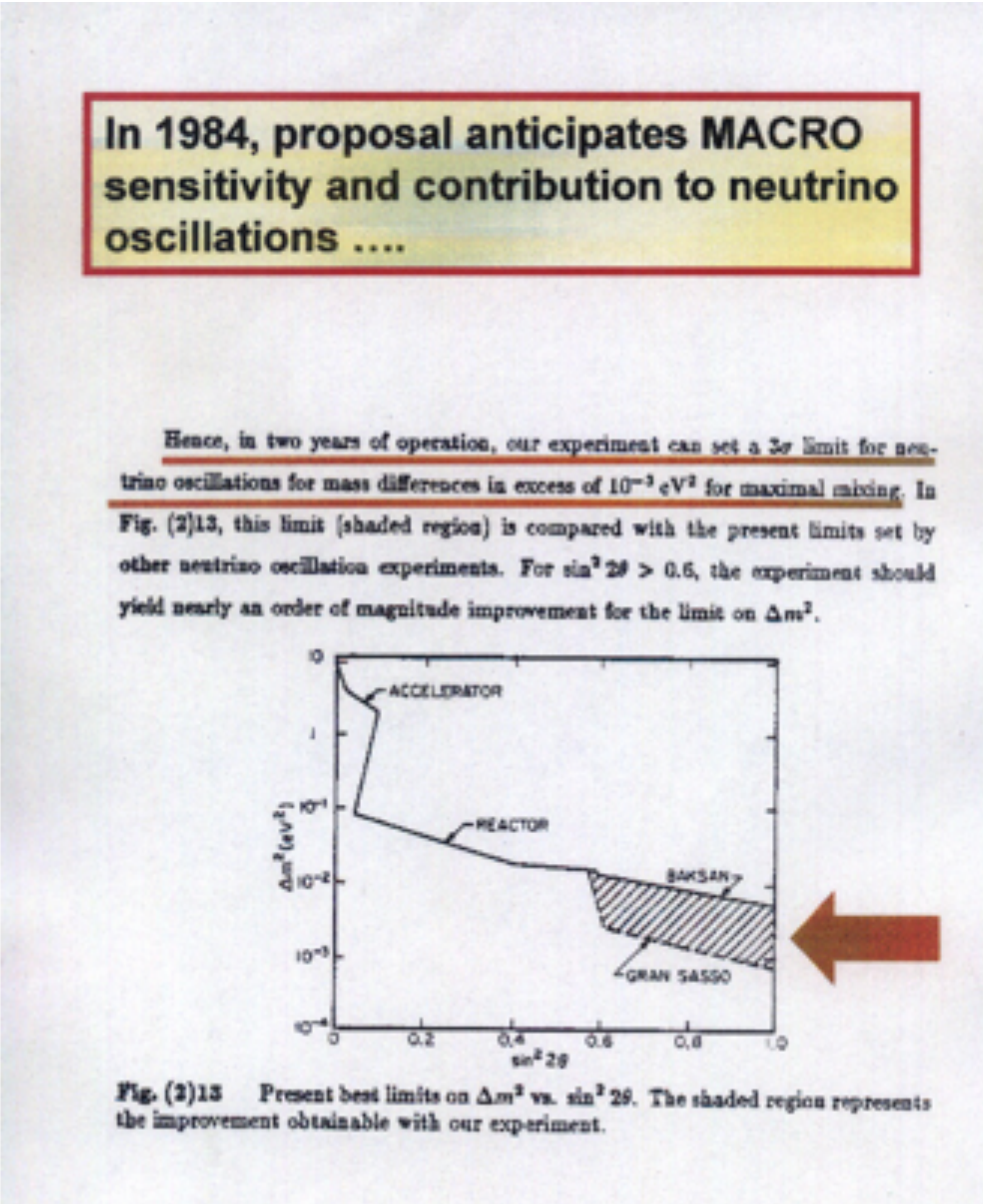
Sasso beam it was decisive the fact the Director General of CERN was Luciano Maiani.

4. MACRO and the atmospheric neutrinos

At this point I must insert some personal recollections, due to the fact that, in 1989 the MACRO experiment at the Gran Sasso lab began partially operative. The principal goal of MACRO was the search for magnetic monopoles predicted by Grand Unification theories. But the same apparatus could also reveal atmospheric muon neutrinos. The detection was based on the observation of upward muons produced in the rocks below the detector by the muon neutrinos. The muon direction was identified by measuring the times of the scintillator counters. The search for neutrino oscillations was one of the goals of MACRO since the beginning. The Figure 3, contained in the 1984 proposal [11] shows the region of the oscillation parameters accessible to MACRO. This region included the oscillation parameter values as we know them nowadays.

My involvement in the analysis of the neutrinos was partially accidental, since the Italian spokesman at that time, Enzo Iarocci, wanted to know if the third layer of scintillators, which was not yet built, was really needed in this type of analysis. Iarocci asked me to study this problem since he knew of my experience about time of flight in previous experiments.

Since MACRO was still under construction, data were taken in unstable conditions, and therefore great care was taken in formulating statements about the neutrino flux. However, already at that time, the deficit of events was clearly identified. This was particularly concentrated on the vertical direction. Many of us believed that this could be an instrumental fact due the unstable data acquisition, or that could be due to the presence of underground lakes or caverns, fact that was proved wrong. Preliminary results, based on 45 events on the neutrino astronomy were presented at the fifth conference "Neutrino Telescope" of Venice in march 1993 [12].



In 1984, proposal anticipates MACRO sensitivity and contribution to neutrino oscillations ....

Hence, in two years of operation, our experiment can set a 3σ limit for neutrino oscillations for mass differences in excess of 10^{-3} eV^2 for maximal mixing. In Fig. (2)13, this limit (shaded region) is compared with the present limits set by other neutrino oscillation experiments. For $\sin^2 2\theta > 0.6$, the experiment should yield nearly an order of magnitude improvement for the limit on Δm^2.

Fig. (2)13 Present best limits on Δm^2 vs. $\sin^2 2\theta$. The shaded region represents the improvement obtainable with our experiment.

Figure 2. Page of the proposal of MACRO of 1984 shown by B. Barish in the last MACRO meeting of January 21rst 2010 at the Gran Sasso Lab. The dashed region represented the result of the analysis of MACRO sensitivity in 1984. The oscillation signal was later found in this region.

In MACRO, a group of people was formed to perform this specific analysis. The historical group constituted by Paolo Bernardini, Doug Michael, Antonio Surdo, Teresa Montaruli and Maurizio Spurio was then complemented, in various times, by Ed Diehl, Bob Nolty, Colin Okada, Eugenio Scapparone. This group had theoretical support by Paolo Lipari and Stanislav Mikheev. It was this small group of people that, usually, presented the results of MACRO at various conferences. These presentations were often left to us since there was a skeptical attitude about these results, even from the other members of the collaborations. Many people were convinced that the observed effects were due to uncontrolled efficiencies.

A more stable data acquisition, although limited to the lowest part of the apparatus, was available only in 1993 and preliminary results with the limited statistics of 74 events were published in 1995 [13]. We observed 73% of the expected events and the deficit in the vertical direction was confirmed. However, because of the limited statistics and of the negative results of IMB shown in fig. 2 we were very, perhaps too much, cautious in our conclusions. The abstract said:

> At the 90% confidence level, the data are consistent with no neutrino oscillations or some possible oscillation hypotheses with the parameters suggested by the Kamiokande contained– event analysis.

The phrase was diplomatic since, as already mentioned, this result was in contrast with what reported not only in IMB but also BAKSAN and Kamiokande. I remember that Kamiokande gave contradictory results between events contained inside the detector and events not contained.

The project of the CERN–Gran Sasso beam was not making progress. The then director of the Gran–Sasso lab, Piero Monacelli, tried to stimulate, with scarce success, the CERN and the INFN adminitrations. Piero Monacelli also invited proposals for experiments with a possible CNGS beam.

In 1998, we published on Astroparticle Physics [14] an important experimental result which had been refused by Physics Review D in 1997. The topic of the article concerned the observation of upwards charged particles produced by muons in underground detectors. In our opinion this article was very important since we had discovered a background source in the search of upwards muon neutrinos that had not been considered by IMB or BASKAN. This background was dependent on the intensity of the cosmic rays, and this intensity in IMB and BASKAN was much larger than in MACRO because of the smaller shielding depth. This background, in our opinion, raised doubts on the IMB and BAKSAN results and in particularl on that reported in Figure 2. This very strong, but correct statement was perhaps the reason of the rejection by Physics Review D, and, consequently, of our following submission to the European journal Astroparticle Physics.

So we arrive at the year 1998. The author of this note had been designated, already in 1997, to speak for the MACRO experiment at the XVIII neutrino conference in Takayama, scheduled on the 4th –9th of June 1998. Furthermore, Paolo Bernardini was designated to present at the Vulcano workshop of the 25th – 30th May 1998, which would have taken place just few days before the Takayama conference.

During 1997 and 1998 the MACRO statistics had increased, the analysis had improved, and we had carried out three parallel analyses and verified the compatibility of the results. One of these analyses used an alternate electronics for the measurement of the times (the circuit PHRASE developed in Pisa for the time measurement). We had also answered to a set of questions asked by Barry Barish to test the apparatus efficiency, questions also raised by Giorgio Giacomelli co–spokenman for the italian group. We found the reason why IMB and BAKSAN gave results we considered wrong. We were then ready to make stronger and explicit statements in support of the neutrino oscillations. We had only one perplexity, the region preferred by the MACRO data did not correspond to that proposed by Kamiokande (later on, other analyses of Kamiokande moved the preferred region).

With this attitude we gathered at the yearly MACRO meeting in USA where, in particular, we had to discuss about the presentations at the summer conferences. The first two were the Vulcano workshop and the " neutrino 1998" in Japan. The collaboration meeting was held on April 18th–20th in Boston, in coincidence with the famous maratone. The discussion on the presentation of the results was very hot. We have to consider that in the American group there were people who had taken part in IMB and people members of the Super–Kamiokande experiment. Furthermore, negative results of

CHOOZ, a reactor experiment aimed at testing the Kamiokande results, assuming that there were disappearance oscillations of electronic anti–neutrinos, were about to be published. CHOOZ, which had among its members a group of scientists of MACRO, excluded one of the oscillation possibilities, but could not make any statement on the other possibility (muon neutrino in tau neutrino). This contributed to generate a skeptical atmosphere about oscillations.

For all these reasons, in spite of the efforts and of the opposition of the Italian part of the neutrino group, the majority decided that no statement should be made about neutrino oscillations. In particular, the figure of the allowed parameter region should not been shown.

Honestly speaking, I do not know what our presentations would have been like, in the wake of the negative decision of the MACRO group. Probably, I would have presented the same talk that I did, but with different spirit. However, the discussion had taken place in the absence of Barry Barish which was sick. By the way, this is the only time, to my knowledge, that Barry Barish was indisposed. Fortunately, the day after, Barish was again well, came to the meeting and asked what had happened. Later on, respecting the agreements taken when he had asked to test the efficiencies, he acted with his resolute behaviour and convinced the American group to change their mind.

5. The Takayama neutrino conference of June 1998

The conference started on Monday, June 4th. Immediately, people rumored that there was going to be a great announcement by Super–Kamiokande: there was therefore a great expectation. On the first day, there was a session dedicated to the solar neutrinos. In succession: the experiments in Homestake (the experiment of Davis with Chlorine), then Gallex, Sage and Super–Kamiokande.

It is impressive to observe how the deficit of the revealed neutrino flux was observed, in different manners, by all the experiments: the radio–chemical ones as well as those in water. Furthemore, Super–Kamiokande had thousands of events where it was possible to begin studying small effects such as those due to the variation of the Sun–Earth distance. In his afternoon talk about the flux predicted by calculations J. Bachall pointed out that at this point the deficit was an effect of about 20 standard deviations. Many people expected the great announcement already on Monday, but that was not the case.

It became evident that there would have been more important results from Super–Kamiokande in the atmospheric neutrino sector. The morning of June the 5th was dedicated to this topic.

The schedule had in the successive order the talks of E. Peterson (Soudan2), F. Ronga (MACRO) and of the 2015 Nobel laureate T. Kajita (Kamiokande and Super–Kamiokande). This schedule worried me since I knew that Super–Kamiokande was an experiment of much higher quality than MACRO and therefore it was possible that, in case of discrepancy, the data of Super–Kamiokande would have received more consideration. Furthermore, we knew that the Kamiokande contained events favoured oscillations with mass differences much larger that those we observed, and we believed that this could be confirmed in the presentation of Kajita. For this reasons I waited with anxiety for the conclusions of Kajita.

After the conference, somebody thought that the presentation of MACRO was "adjusted" by knowing what Kajita was going to present. This is not true, since the guidelines of the presentation had been decided in the April Boston meeting, and the presentation was similar to that of Paolo Bernardini at the Vulcano workshop in the afternoon of the May 29th 1998. This latter workshop had a participation smaller that of *Neutrino 1998* therefore the echo had not reached the large public. We could therefore state that the first announcement of the neutrino oscillation was given in May the 29th 1998 at the Vulcano workshop by MACRO and not at Takayama.

The presentation of SOUDAN2 confirmed the deficit of muon neutrinos and solved, finally, the discrepancy iron–water, but did not draw any conclusion about oscillation parameters.

I show in Figure 4 two of the most significant slides of the MACRO presentation. The first one is the plot of the confidence region which shows that, in 1998, MACRO had and effect larger than 99% confidence level in favour of oscillations from muon to tau neutrinos. The allowed region was not much different that that of Super–Kamiokande of Figure 5. The second slide of MACRO shows, in the conclusions, that the sterile neutrino was disfavoured (there was a factor 8 between the probabilities in tau neutrino and sterile neutrinos).

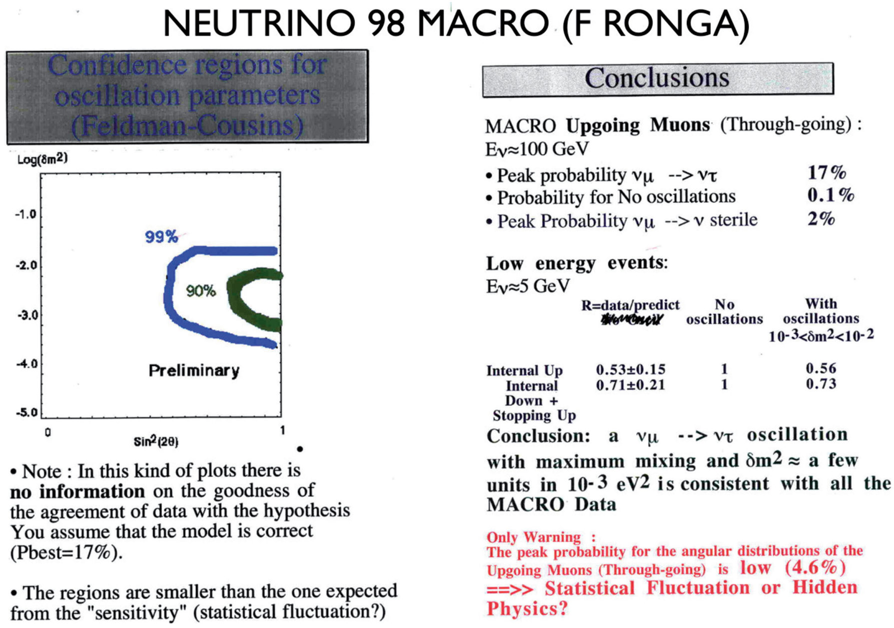


Figure 3. Slides of the MACRO presentation at "neutrino 1998". The slides are still on the conference link http://www–sk.icrr.u–tokyo.ac.jp/nu98/scan/index.html. Similar slides had already been shown by Paolo Bernardini six days earlier at the Vulcano workshop 1998

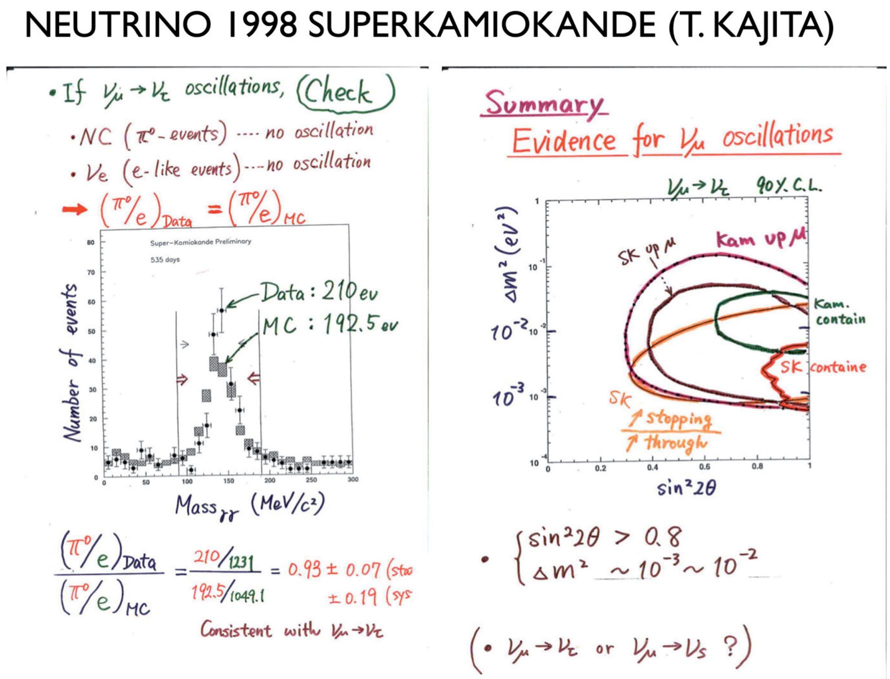


Figure 4. Slides of the Super–Kamiokande presentation at neutrino 1998.

This analysis had been possible thanks to the work of Paolo Lipari which had been working on the matter effect for some time. These results were published in the conference proceedings and, even earlier, submitted on June 29th 1998 at Phys. Lett. B [15].

I show in Figure 5 two slides, among the most significant ones, of the Super–Kamiokande presentations. The first one regards the analysis done to exclude the sterile neutrino with a study of the topology of the events. The second slide is the conclusive one, with the famous plot which is nowadays remembered all over the world. The strength of the Super–Kamiokande result laid on the fact that the analyses carried out with different kind of events agreed in the result. The orange line (stopping/through) disproved completely the IMB result of Figure 2. These results were immediately published and they are among the most quoted in particle physics [16]. We have to observe that the Super–Kamiokande results also contradicted, in part, the Kamiokande result (green curve) and were in total agreement with the MACRO result of Figure 4.

The Japanese organised a press conference to advertise these results all over the world. The news spread with great success even with the general public. The result of MACRO disappeared however in the press releases, and the INFN was surprised about that, despite the efforts of G. Giacomelli. This happened, in part, because of the doubts and the perplexities mentioned above.

To confirm that MACRO collaboration acknowledged the role of Super–Kamiokande but that it had a relevant impact in the discovery, I want to stress that the preprint arXiv number of MACRO paper [15] is 9807005, while that of Super–Kamiokande [16] is 9807003. The MACRO paper was ready before that of Super–Kamiokande, but Giorgio Giacomelli (co–spokesperson of the collaboration) waited for the green light from his colleague and friend Koshiba to submit to the arXiv the paper soon after that of Super–Kamiokande.

The conference continued in minor tone after this historical event.

I just want to recall the signal, possibly due to sterile neutrinos, of the LSND experiment, since it had a great relevance in the European and Italian debate and in the approval of the CERN – Gran Sasso beam. There was a proposal for a neutrino beam on the short distance at CERN to test the LSND results, but the proposal was not approved. The LSND effect is not yet explained despite dedicated experiments at

Fermilab in USA. A negative effect of this debate was a further delay in the approval of the CNGS beam. There were also different proposal such as that of a beam from the CERN to the mountains of the Jura with distances of about 17 km. Many physicist were reluctant to work at the Gran Sasso lab, in an environment certainly more difficult than that of a large laboratory such as CERN.

Perhaps these considerations of social type, in addition to the financing problem, hindered the construction at CERN of a near detector. This detector would have widened the possibilities of the beam by comparing near and far measures. However there was perhaps the worry that the near detector would absorb great part of the interest. Eventually, the beam was approved in the December of 1999 with the scientific program of the appearance of tau neutrinos with he ICARUS and OPERA experiments. In this research the near detector is not needed.

6. Conclusions

The year 1998 was a turning point since the community of elementary particle physicists convinced itself of the neutrino oscillations after 30 years from the first indications. Later, many experiments have been proposed, approved and built.

In 2002 the Sudbury Neutrino Observatory collaboration published the paper on the direct evidence for neutrino flavor transformation from neutral–current interactions putting down another milestone in the solar neutrino oscillations [17]. This was the motivation for awarding the Nobel prize for solar neutrinos to Arthur B. McDonald.

We are now with the third generation of experiments on the neutrino oscillations and all the terms of the the oscillation matrix have been measured, but one parameter. I have some personal regrets as an Italian and European, the unsufficient appreciation of the MACRO results, the division of the neutrino physicists community in Europe about scientific programs and the hostility of part of the community of particle physicists. As an example of these problems I recall that when financial restrictions on the construction of LHC appeared, one of the actions taken was to close the very small group of the CERN working on the OPERA experiment. This was certainly a signal psychologically very negative for the OPERA collaboration and for the European community. The behaviour of the Japanese was completely different. They approved with determination the first beam of neutrinos on a long–distance (K2K) well before 1998, believing since the beginning in this type of physics, despite all the doubts exposed in this note. For these reasons the recent 2015 Nobel was well deserved.

After the end of the data acquisition of the CNGS, today the neutrino physics with particle beams is no longer present in Europe, neither for short nor for long distance. Perhaps, this is appropriate from the point of view of the division of tasks at world level, but it leaves a bitter taste. Fortunately, in Italy, at the Gran Sasso lab, the neutrino physics without accelerators is still present with the BOREX (neutrinos from the Sun and from a source), CUORE e GERDA (neutrino mass, and Majorana neutrinos).

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