Neutrino oscillations: personal recollections

F. Ronga
INFN Laboratori Nazionali di Frascati, via E. Fermi 40
Frascati I00044, Italy

This short neutrino oscillations history is focused to the period up to June 1998, from the point of view of a researcher working in the past MACRO experiment at the Gran Sasso Laboratory, scheduled to talk at the Takayama conference just before the historical Tataaki Kajita's talk

1 Atmospheric neutrinos

In the spring of 2015 (before the neutrino Nobel prizes) Giampaolo Co, a physicist of the Lecce University in Italy, planned a special number of the online magazine Ithaca dedicated to neutrino. G. Co and Paolo Bernardini asked me to write an article on the neutrino history. Later in 2016 a book *Neutrino the mutant particle* [1] was published by Aracne editions. This paper is a summary of the chapter on the neutrino history contained in this book, focused on the MACRO experiment.

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 theorethical and sociological 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.

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") [2]. In this paper it was stated that there was no evidence for oscillations. Based on this analysis, they excluded just the values of the parameters which we have now well measured .

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 with beams from CERN to the Gran Sasso was considered [3]. The CERN - Gran Sasso beam was approved only in the December of 1999, after the discovery of the oscillations, perhaps too late.

2 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

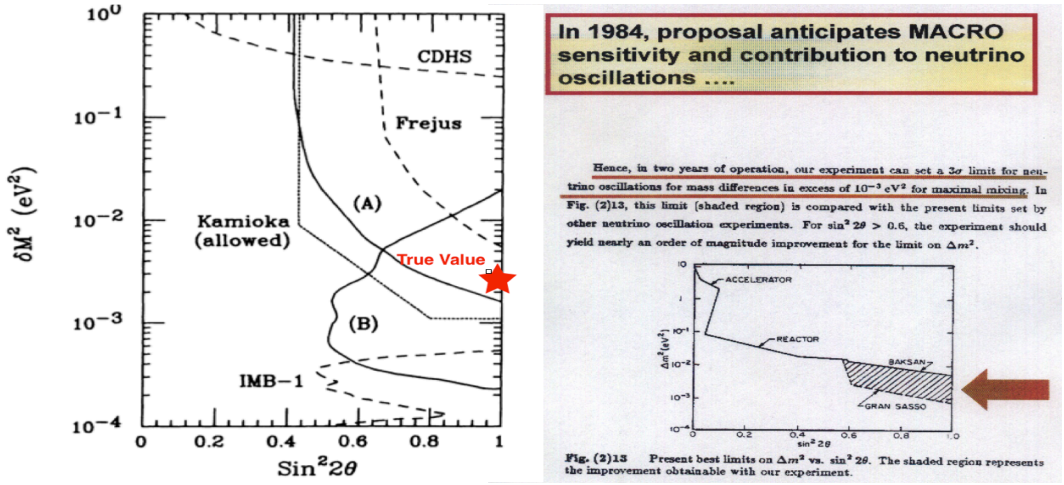

Figure 1 – Left :Figure analogous to that of the IMB paper All the curves include excluded regions, except that of Kamiokande, which defines the allowed region. One should notice that the curve B of IMB completely excludes the red star that represents the presently accepted oscillation values. This result generated great confusion and slowed down the claim of the discovery.
Right: Page of the proposal of MACRO of 1984 shown by B. Barish in the last MACRO meeting of January 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.

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 search for neutrino oscillations was one of the goals of MACRO since the beginning. The Figure 1, contained in the 1984 proposal [?] shows the region of the oscillation parameters accessible to MACRO. This region included the oscillation parameter values as we know them nowadays.

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 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. Preliminary results, based on 45 events on the neutrino astronomy were presented at the fifth conference "Neutrino Telescope" of Venice in march 1993 [4].

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.

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 [5]. 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. 1 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 (upgoing-througoing muons) .

In 1998, we published on Astroparticle Physics [6] 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 BAKSAN.

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. 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. 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.

After a long discussion the collaboration (spokes-mans B. Barish and G. Giacomelli) agreed to present the oscillation analysis and the allowed regions.

3 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. It is impressive to observe how the deficit of the revealed solar neutrino flux was observed, in different manners, by all the experiments: the radio-chemical ones as well as those in water in Super-Kamikande.

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.

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

I show in Fig. 2 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 an effect larger than 99% confidence level in favour of oscillations from muon to tau neutrinos. allowed region was not much different that that of Super-Kamiokande of Fig. **??**. 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).

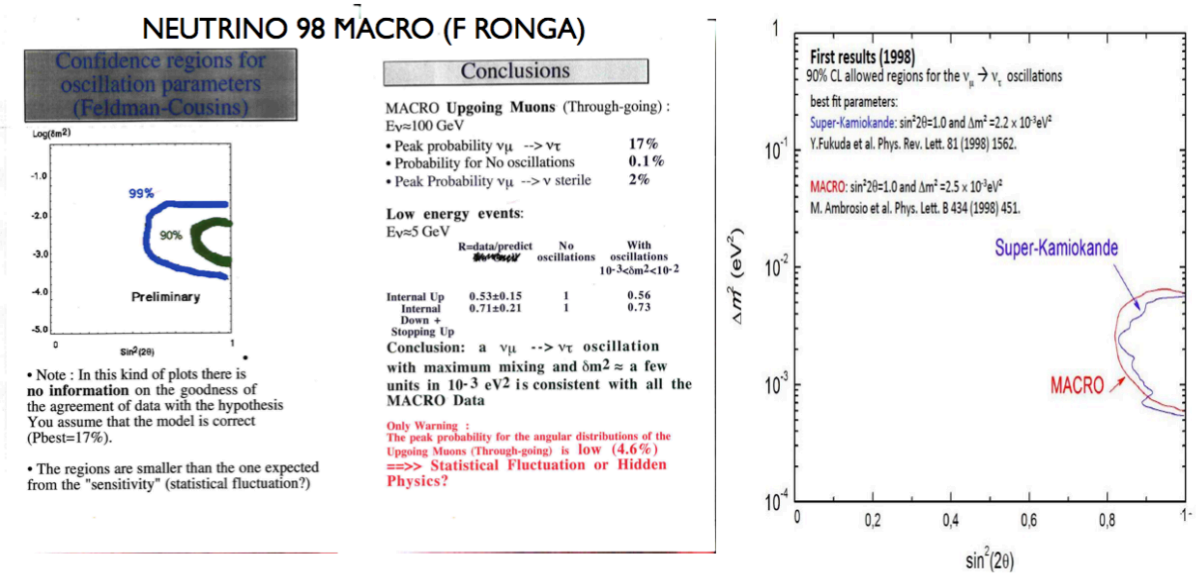

Figure 2 – Thw last two slides of the MACRO presentation at "neutrino 1998" and comparison of MACRO and Super-Kamiokande in 1998. The slides are still on the conference link http://www-sk.icrr.u-tokyo.ac.jp/nu98/scan/index.html. Similar slides had already been shown by Paolo Bernardini six days earlier at the Vulcano workshop 1998.

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 [7].

We have to observe that the Super-Kamiokande were in total agreement with the MACRO result. The comparison is done in Fig.2 right. Of course the MACRO statistical evidence was much lower than the one of Super-Kamiokande. It increased to 5σ with the full data set in 2000 [8]

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[7] is 9807005, while that of Super-Kamiokande[9] 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.

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