

Neutrino Mistakes: Wrong tracks and Hints, Hopes and Failures

Maury C. Goodman

High Energy Physics Division, HEP362, Argonne, IL. 6039 USA



In the last two decades, the field of neutrino physics has made enormous progress in measuring the strength and frequency of neutrino and antineutrino oscillations. Along the way, there have been many instances of misunderstanding which led to wrong measurements or speculation for new features of neutrino physics that are not now accepted as correct. This is part of the natural process of science, but given the well-accepted notion that we learn from our mistakes, it is worthwhile to look at some examples and see what the lessons might be. With that goal in mind, I have a list of results which might be termed “neutrino mistakes”, with the fact in mind that there is no well-accepted definition of a mistake, and no unique threshold for counting something as a mistake when you change your mind after you obtain more information. After making the list, I chose seven of them to discuss. No clear conclusions were drawn from this exercise, but some interesting issues regarding putative wrong results are discussed.

1 Introduction

In the last two decades, the field of neutrino physics has made enormous progress in measuring the strength and frequency of neutrino and antineutrino oscillations. At the Neutrino History Conference, this progress was described along with many other stories since before the idea of the neutrino was formulated by Pauli in 1930. In addition to this progress, there were some claimed discoveries which did not hold up under scrutiny. There were rumors of new effects which generated interesting discussion. And there were other things which might fall under the category of *mistakes*. The title, “Neutrino Mistakes: Wrong tracks and Hints, Hopes and Failures” was given to me by the organizers, and is what this paper tries to describe.

A list of neutrino *mistakes* considered for this paper is given in Table 1¹. I prepared the list first, and then tried to come up with an algorithm for deciding what was chosen for this particular list. Wrong theories or theoretical frameworks did not make it to the list. The idea of neutrinos as hot dark matter in the eV mass scale led to a number of experiments and could certainly have fit the label of “wrong tracks and hints, hopes and failures.” Likewise, SU(5) led to a series of underground nucleon decay experiments which certainly had a strong effect on the history of neutrino oscillations. Another class of possible mistakes that were not included related to experimental hardware issues. The catastrophic loss of phototubes at Super-Kamiokande was

not included, nor was the leaky collapsed bag which led to the demise of the IMB experiment, although interesting stories could be told about such episodes. And finally I’ve restricted this to the field of neutrinos, although a large number of seeming mistakes permeate our field, from direct dark matter claims² and Cygnus X-3 observations³ to the split A2⁴ and the Oops-Leon⁵. Besides instances of what I call a consequential semantic issue, my a-posteriori definition for what was included on the list would be an experimental search for new ν physics for which there was an apparent error of the first kind or error of the second kind. Such a mistake might be due to an unusual statistical fluctuation, a systematic error that wasn’t taken into account, a wrong interpretation of good data, or a theoretical misunderstanding. The threshold for exploring new opportunities to find new physics is necessarily low. As scientists, we are constantly asking “What if ...?” Some of the alleged mistakes in the list are unpublished and some were no more than rumors.

SIN report of $\mu \rightarrow e\gamma$
High γ anomaly
NuTeV helium bag events
Klapdor’s $0\nu\beta\beta$ signal
LSND and eV “sterile” neutrinos
IMB limit on ν oscillations
Alternating neutral currents
Reines-Sobel ν oscillations
Vannucci PS191 oscillations
BNL 776 & 816 oscillations
BEBC oscillations
HPWF “super” events
Oscillations in Bugey
Majoron emission in $0\nu\beta\beta$ PNL/USC
SPT vs. V-A
Superluminal ν s
17 keV ν
NuTeV anomaly
Tritium endpoint negative m^2
Kolar events
Early atmospheric ν lack of polarization
MINOS $\bar{\nu}\theta_{23}$
God’s mistake
ν grammar
Labels for Δm_{ab}^2
PDG $m(\nu)$ encoding
Which ν is a particle?
Karmen time anomaly
Time variation in Troitsk m_ν^2
30 eV ν from ITEP

Table 1: An unordered list of neutrino topics which might be regarded as involving mistakes¹.

Let me also list some of my personal guides in evaluating experimental results: 1) there are an infinite number of tests of the null hypothesis, 2) there is no theory of systematic error, 3) you can’t prove anything in physics, 4) the union of two confidence levels is not a confidence level, and 5) the commonly used 5σ criterion is based on misunderstandings and is wrongly used.

While the word “mistake” carries negative connotations, for the most part there should be

no impugning of the scientists that participated in the experiments mentioned in the list above. We learn from our mistakes. It is for that reason that this subject was included in the 2018 Neutrino History Conference in Paris. But the conclusion of this paper will present no unifying theme or lesson from the topics considered here. Instead, many of the cases described raised a set of unique issues that may interest some readers. But to repeat, it is not the goal here to criticize any physicist or collaboration for reporting a result which is later considered to be incorrect.

2 Report of $\mu \rightarrow e\gamma$

Since neutrinos have mass, there is a standard model diagram for the unseen decay mode $\mu \rightarrow e\gamma$. The predicted branching fraction is:

$$B = 5 \times 10^{-48} [\Delta m_{21}^2 (eV)^2]^2 \sin^2(\theta_{12}) \cos^2(\theta_{12}) \quad (1)$$

We now know these neutrino mixing parameters so that this is of the order of 10^{-60} . When I was a graduate student in the 1970's, I heard a rather specific rumor that this process had been seen by an experiment at the SIN facility in Switzerland. I never heard a talk about this result and in fact when SIN published their results a few years later, they published a limit, $B < 1.0 \times 10^{-9}$ at 90% CL⁶. In that publication, they reported: "No evidence for the existence of the process has been found." They also reported, "The measured positron-photon energy distributions are completely described by the decay $\mu^+ \rightarrow e^+ \nu_e \bar{\nu}_\mu \gamma$ and accidental coincidences." I don't know if initial interest in those events was the source of the rumor, but I mention it as a possibility. However a published indication of this rumor does exist in a theoretical paper at that time on muon number nonconservation by Bjorken and Weinberg⁷. They wrote: "*It would be disingenuous for us not to acknowledge that our interest in this question was kindled by an experiment now in progress at Schweizerisches Institut fur Nuklearforschung [cf. Physics Research in Switzerland, Catalog 1975 (Swiss Physical Society, Bern, 1975), p. 207], and by rumors of a positive signal. However, our considerations here do not depend on any assumptions about the eventual outcome of this experiment; indeed, we believe that even if this measurement were to yield a null result, it would be worthwhile to push on to the greatest accuracy.*" In fact, searches for this process are continuing since any observation at a level larger than that implied by Eq. 1 would indicate new physics in the lepton sector.

But at the time, this rumor led to a series of lectures at Fermilab by Robert Shrock, who was then a postdoc in the Fermilab theory group⁸. These lectures included a detailed look at the time at the theoretical basis and phenomenology of neutrino oscillations, and was where I learned about the subject in detail for the first time. For me personally, this rumored result, which was wrong, never described and never published, was extremely useful in my career.

3 Report of 17 keV neutrino

Tritium decay is a popular object to use for studying the beta spectrum, as the shape of the distribution near the 18.6 keV endpoint could be sensitive to non-zero neutrino mass. But in a study reported in Simpson⁹, a kink was reported at 1.5 keV in the spectrum, corresponding to a possible neutrino mass of $18.6 - 1.5 = 17.1$ keV with a mixing probability (P) of 3%. This was followed by other experiments which failed to see a signal and set a limit at that mass and $P < 0.3\%$ ¹⁰. Then Hime and Simpson repeated the search in ³⁵S and reported a kink corresponding to 16.9 keV with a mixing probability 0.7%¹¹. This was followed with another experiment in ³⁵S which reported an 8σ observation at 17 keV with $P = 0.8\%$ ¹². A timeline of some positive and negative results is shown in Fig. 1¹³. There were more nonobservations, but the definitive exclusion is considered as coming from an experiment led by Stuart Freedman¹⁴ in ³⁵S which reported the mixing probability of $-0.0004 \pm 0.0008(\text{stat}) \pm 0.0008(\text{syst})$ consistent with zero.

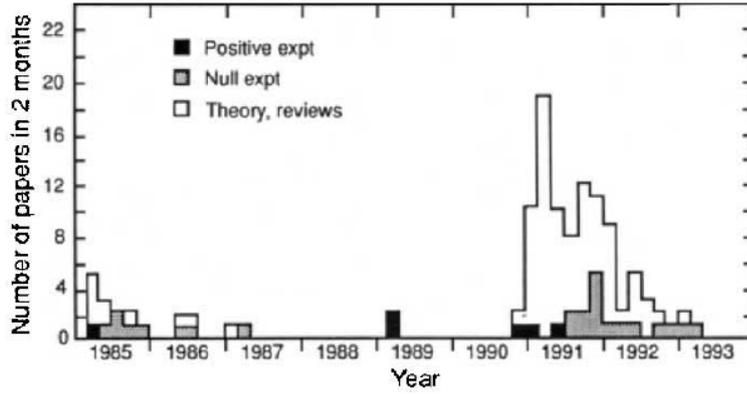


Figure 1 – Timeline of positive and negative reports of a 17 keV neutrino ¹³.

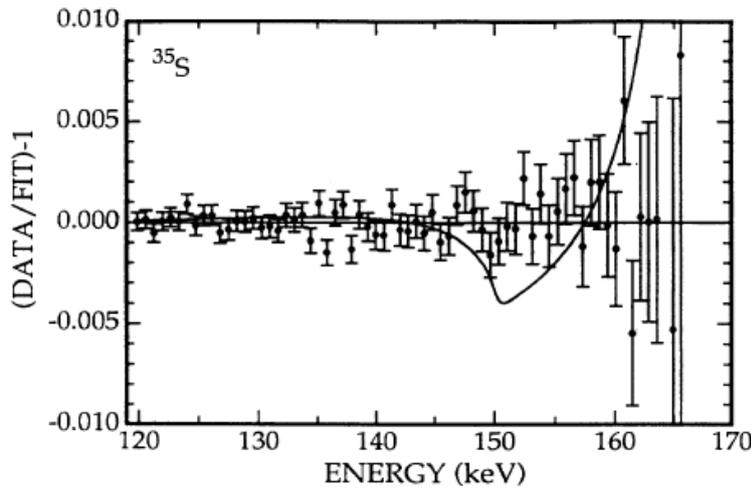


Figure 2 – Shape function as reported by Mortara ¹⁴ in ³⁵S with that expected if there was a 17 keV neutrino.

Figure 2 shows the residuals from a fit to the pileup corrected energy spectrum, along with the shape of an expected signal from a 17 keV neutrino. Figure 3 shows the 95% upper limit on mixing as a function of neutrino mass along with the results from previous positive experiments. Both figures are from Mortara ¹⁴.

Andrew Hime did further calculations which explained the wrong signal in Hime and Jelley ¹². He showed in Hime ¹⁵ that scattering effects were likely responsible. Compare the shape factors in Figure 4 on the left from Hime and Jelley ¹² with those on the right from Hime ¹⁵. The difference involved a more complete electron response function with intermediate scattering.

4 Neutrinoless Double Beta Decay

In isotopes from a few select nuclei, decays can only happen by double beta decay. In the last 30 years, several measurements have been made of two neutrino double beta decay. But if the neutrino is a Majorana particle, i.e. is its own antiparticle, then neutrinoless double beta decay should also take place at rates that are low but predictable, given other neutrino parameters, up to nuclear matrix elements. There are a variety of calculations of matrix elements which differ on a linear scale. In 2001, the Heidelberg-Moscow collaboration, using ⁷⁶Ge in an experiment at the Gran Sasso Laboratory, set a limit on the lifetime for neutrinoless double beta decay greater than 1.9×10^{25} y at 90% CL ¹⁶ in a paper signed by 14 authors. Later that year, a subset of four authors claimed evidence for a signal with a lifetime $1.5^{+16.8}_{-0.7} \times 10^{25}$ y at 95%

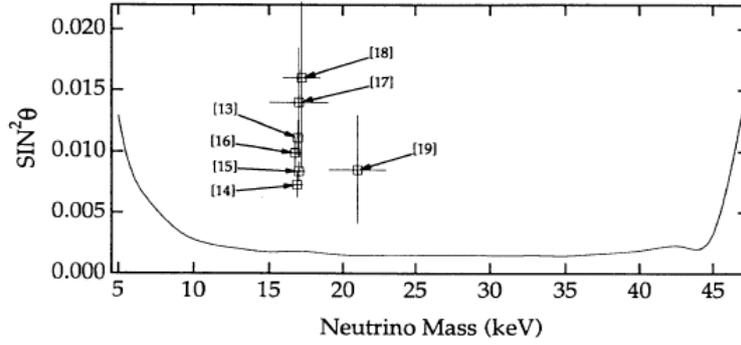


Figure 3 – Limits on neutrino mass and mixing from Mortara¹⁴ along with values from previous positive reports.

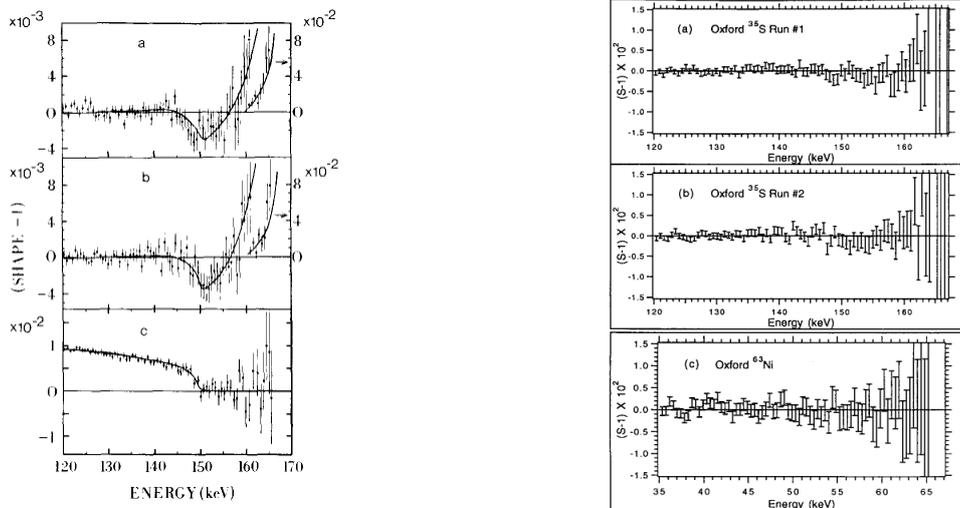


Figure 4 – Comparison of shape factors in ³⁵S as first reported in Hime and Jelley¹² and later in Hime¹⁵.

CL¹⁷. This is shown in Fig. 5 from Klapdor-Kleingrothaus¹⁸ where a fit finds an excess above background at the known two electron energy, along with other known and unknown lines. Soon thereafter, a critique of this claim appeared on the arXiv with several authors from the nuclear beta decay community and was later published¹⁹. They wrote, “We discuss several limitations in the analysis provided in that paper and conclude that there is no basis for the presented claim.”

Mention of the evidence appeared in my January 2002 newsletter²⁰ which goes to a large fraction of the neutrino physics community. I mentioned the lifetime, neutrino mass, arXiv numbers from Heidelberg-Moscow’s positive and negative reports under the headline “Evidence that neutrinos are Majorana particles”. John Beacom wrote to me²¹ pointing out his own criticisms of the result and opining that this report should not meet the standards of my newsletter. I replied to him that I didn’t have standards but I did have deadlines, a comment he has repeated back to me with a wry smile. Then in my February newsletter, I reported on the critique in the arXiv under the headline, “Neutrino Mass may not be .39 eV”²². I received an email from Klapdor-Kleingrothaus²³ who was “surprised to see that you handle the comment put on the web as hep-ex/0202018 on the same level as our published paper...” and “May I propose that you better take out this unserious Comment from your web page.” I responded that my newsletter gave equal attention to the discovery of neutrino oscillations and a novel about a Neanderthal neutrino physicist who appeared in the SNO detector. Professor Klapdor-Kleingrothaus never subscribed to the newsletter.

This episode raised in my mind that there are a number of unscientific factors which affect

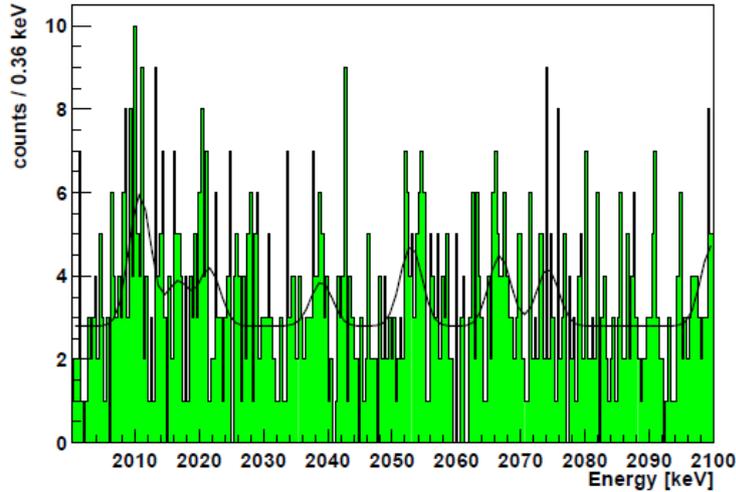


Figure 5 – Spectrum of the Heidelberg-Moscow experiment claim for $0\nu\beta\beta^{18}$.

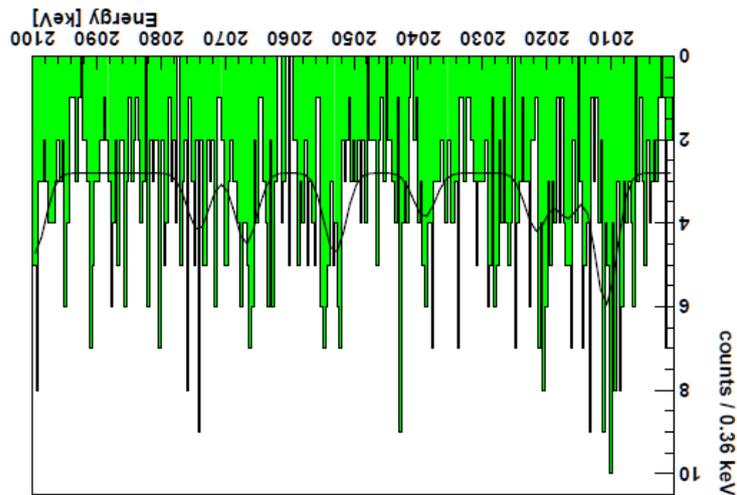


Figure 6 – Spectrum of the Heidelberg-Moscow experiment claim for $0\nu\beta\beta^{18}$.

whether or not we believe a result that we hear, particularly when we know that we don't have the background or experience to fully comprehend every scientific issue. In this case, there were many such factors. The result was published before there was a preprint. It was published in a journal on which one author was associated. A significant fraction of collaboration didn't sign the paper. One test of the validity of a *peak* in a distribution is to look at the distribution upside-down and looking for a *dip* (see Fig. 6). The only talk which I heard from Klapdor-Kleingrothaus seemed arrogant and he repeatedly touted this with the unrelated DAMA claimed discovery of Dark Matter, another result that much of the community did and does not believe. There was a public argument with one of his collaborators, and it was clear that he was not sharing the data with them. As we left that talk, Doug Michael, a colleague of mine known for his insightful language, commented "Even if it's right, it's wrong" ²⁴. I interpreted this to mean that he thought that if neutrinoless double beta decay exists in this channel, this particular analysis had enough flaws to preclude being considered a discovery. My opinion is that the result is probably the result of a-posteriori analysis.

It didn't appear to me that this result was believed within the neutrino community. It did not lead theorists to write many papers despite their apparent predilection for Majorana neutrinos. But it couldn't be ignored either, and the result became a benchmark for $0\nu\beta\beta$ experiments on their way to achieving sensitivity to the non-degenerate inverted neutrino hierarchy. In the limits published by the EXO-200 collaboration²⁵, GERDA collaboration²⁶ and KamLAND-Zen collaboration²⁷, the limits are specifically contrasted with this claimed positive result. In the first two papers it is done in the conclusion, while in the last it was done in the abstract. This is entirely appropriate, but I mention it as a point of irony.

5 Superluminal Neutrinos

The OPERA report of neutrinos traveling from CERN to the Gran Sasso with a velocity apparently faster than light led virtually every particle physicist to have a story to tell. I will start with a paper from the MINOS collaboration which had the goal of measuring the time of flight of neutrinos from Fermilab to the Soudan mine, 730 km in distance. This was potentially sensitive to a delay in arrival of low energy neutrinos due to neutrino mass, though the sensitivity was far from interesting on the mass scale relevant to atmospheric neutrino oscillations.

MINOS published a paper based on a study of the arrival time of events at Soudan compared to the batch structure of the beam for 473 events in the far detector²⁸. The velocity was measured to be $(v-c)/c = 5.1 \pm 2.9 \cdot 10^{-5} \text{ eV}^2$ at 68%CL which was used to limit the effective neutrino mass $m_\nu < 50 \text{ MeV}/c^2$ at 99% CL. It is probably typical within the field of High Energy Physics that we have not read a majority of our own papers. This is less true for neutrino experimenters, as opposed to members of collider collaborations, but this was one paper on MINOS that I hadn't read when we published it in 2007, and I certainly wasn't aware that we had an almost two sigma superluminal result. But in the course of repeating this measurement with much greater accuracy, some members of the OPERA collaboration were well aware of it.

In their first preprint, OPERA reported a measurement $(v-c)/c = (2.48 \pm 0.28 \text{ (stat.)} \pm 0.30 \text{ (sys.)}) \times 10^{-5}$ in a preprint²⁹ dated 22 September 2011. With comparable statistical and estimated systematic errors, this was reported as a 6σ measurement. On 23 September a seminar at CERN was broadcast live on the web, at which it was reported that they had obtained this result months before and tried to find an experimental explanation before they presented it to the rest of the scientific community. On the same day, CERN issued a press release³⁰ and interest in this result went way beyond the scientific community. There was some scrutiny of the result, which led to a revised preprint on 17 November 2011 which was submitted for publication, although the paper was never published³¹. A possible loose connector was identified as the probable explanation on 25 February 2012, and the final study taking this into account appeared in the arXiv on 12 July 2012^{32,30}.

It seems to me that it was the press release from CERN, and not the public seminar, which led to the world-wide attention for this reported result. As a form of *Gedanken history*, let me imagine that the same preprint was released and seminar given without the press release. I would imagine that some science reporters would have tracked down some scientists for comment, and that two or three weeks later, there might have been an article in the science section of the New York Times. But the idea that Einstein might have been wrong about the speed of light would not have reached everyone's grandparents who have a TV or read the popular press. The story in the scientific community would have been the same. The embarrassment to science outside of the community would not have happened.

This point led to lively discussion at the History of Neutrino meeting. I have the unverifiable sense that a majority of our colleagues wish that this result had not become a subject of the popular and non-scientific press. The point was brought up that it becomes too easy for the public to disbelieve all science that they want to disbelieve, when they have examples like this to use, despite the careful caveats in the press release. And this does harm in places such the

climate debate, where scientific and non-scientific arguments get mixed up. But the view was also expressed that wide mention of our field does some good. One neutrino physicist mentioned that as a result of the reports of this experiment, his family expressed interest about what he was doing for the first time.

The OPERA result led to a burst of interest and energy for MINOS to repeat its measurement with greater accuracy. New techniques and timing devices were deployed to reduce the systematic error and take advantage of the greatly increased statistics in the MINOS far detector. A new result was presented at a conference³³ but a new draft article remains unpublished³⁴. This seems due to a combination of lack of interest from the referees after the unpublished OPERA result was explained, together with reduced time to deal with details as competing demands from elsewhere took precedence.

6 LSND and eV sterile neutrinos

In 1994, the LSND experiment at Los Alamos presented an analysis at the Neutrino 1994 conference showing an excess of 8 electron neutrino candidates with a background of 0.9 consistent with $\bar{\nu}_\mu \rightarrow \bar{\nu}_e$ oscillations followed by inverse beta decay³⁵. The next year they published an excess of $16.4^{+9.7}_{-8.9} \pm 3.3$ events³⁶. The two neutrino parameter space suggested by this excess is shown in Fig. 7. Not every member of the collaboration signed the paper. In the same issue of Physical Review Letters, one author used the same data employing cuts rather than a likelihood formula to obtain an excess of 5 events with a background of 6.2, setting a limit on $\bar{\nu}_\mu \rightarrow \bar{\nu}_e$ ³⁷. Additional data was reported in Aguilar³⁸. While LSND was originally designed to search for non-zero values of what we now call θ_{12} , given the resolution of that channel as related to the solar neutrino measurements, LSND is now considered to be evidence for sterile neutrinos. The MiniBooNE experiment was designed to test this idea with different values of L and E_ν but similar L/E_ν . Originally it reported that its data was inconsistent with LSND^{39,40}. Further data with both neutrinos and antineutrinos is interpreted by some as supporting the LSND sterile neutrino idea^{41,42} but what appears to have happened is that the data looks the same, while the previous low-energy excess, outside the original blinded search area, is now considered potential signal. The case for sterile neutrinos is sometimes buttressed with the 20% Gallium anomaly⁴³ and the 3% reactor neutrino anomaly⁴⁴.

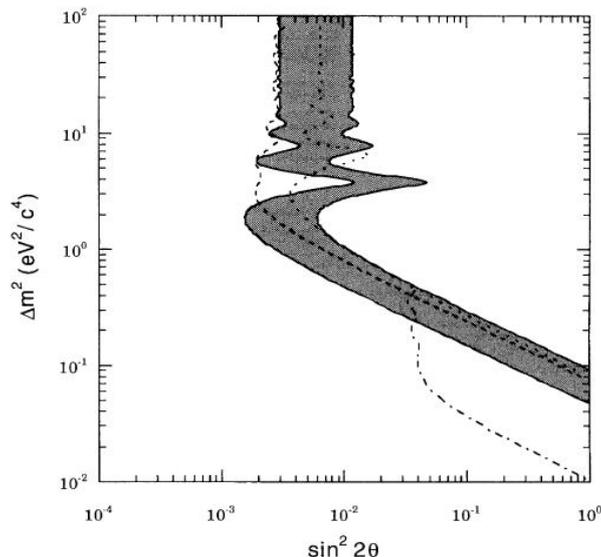


Figure 7 – Parameter space suggested by the original LSND publication using a 2ν analysis³⁶.

Unlike most other results mentioned in this paper, there is a sizable minority of neutrino physicists who consider that these results motivate an aggressive continued search for sterile

neutrinos as an explanation of these short-baseline anomalies⁴⁵. It is not the role of this paper to evaluate the strengths and weaknesses of the sterile neutrino interpretation of the anomalies. A number of issues that would be part of such an evaluation would be the apparent inconsistency of 0.3%, 3% and 20% effects, the Karmen result and Karmen’s limit being better than its sensitivity, the LSND decay in flight result, the continued use of 2ν formulae, cosmological constraints, inconsistency of ν_e appearance with ν_μ disappearance and limits from MINOS, NOvA and Ice-Cube.

There are anomalies, which cannot be explained within the 3ν paradigm. In Gariazzo⁴⁶, a global fit to all the data is performed allowing for a sterile ν in the $3+1$ scheme and a best fit is shown in Fig. 8. But the best fit is a bad fit. I have the impression that even many of the advocates of sterile neutrino searches aren’t confident that the answer lies in $3+1$ sterile neutrinos, but rather that something more complicated is going on that might involve new physics, and that sterile neutrinos might play part of the answer. But that makes a definitive experiment impossible. If you don’t know what you are looking for, you might find it, and might not find it, but you cannot logically rule it out. With this in mind, I coined an answer to the simple question why physicists disagree: *If the data doesn’t agree with the null hypothesis or the alternative hypothesis, some say you need more data, while some say you need more hypotheses.*

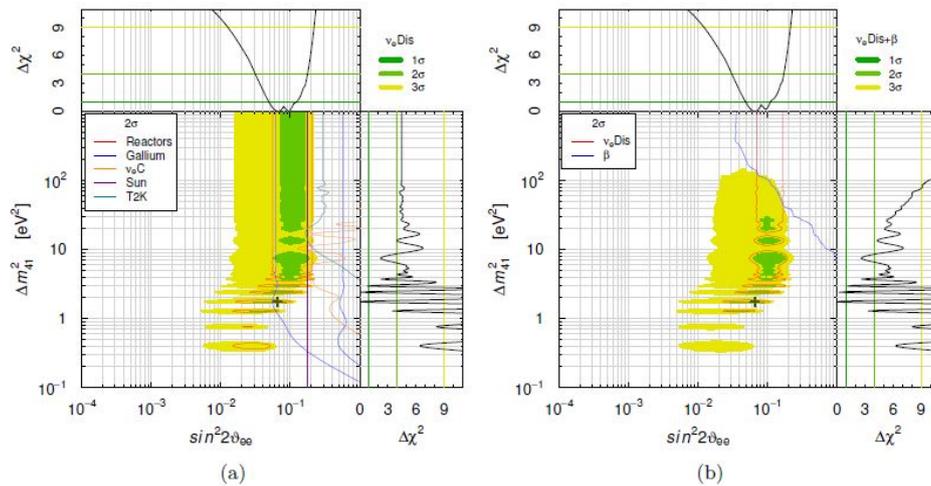


Figure 5. Allowed regions in the $\sin^2 2\theta_{ee}-\Delta m_{41}^2$ plane and marginal $\Delta\chi^2$'s for $\sin^2 2\theta_{ee}$ and Δm_{41}^2 obtained from: (a) the combined fit of ν_e and $\bar{\nu}_e$ disappearance data; (b) the combined fit of ν_e and $\bar{\nu}_e$ disappearance data and the β -decay constraints of the Mainz [83] and Troitsk [84, 85] experiments. The best-fit points corresponding to χ_{\min}^2 in table 4 are indicated by crosses.

Figure 8 – Allowed region for sterile neutrino parameters from a global fit in Gariazzo⁴⁶.

7 IMB neutrino oscillation limit

In 1992, IMB published a neutrino oscillation limit based on the ratio of upward-going stopping μ from atmospheric ν to upward going μ ⁴⁷. Based on this analysis, they ruled out a region of parameter space labeled B in Fig. 9. As shown by the blue rectangle which I have added to the figure from Becker-Szendy⁴⁷, that is just the region in which neutrino oscillations turned out to be. As it became clear this limit must be wrong, an explanation for why it was misleading was sought. Part of the IMB collaboration submitted an abstract to the 1999 International Cosmic Ray Conference⁴⁸ suggesting that a wrong neutrino cross section model was used. But the contents of and results from “a more realistic cross section model” did not appear in the proceedings. One part of this story is that long-baseline neutrino experiments were being proposed and compared in the mid 1990’s and the IMB limit was used to argue for shorter baselines than were in fact needed. The argument was unsuccessful and the longest-baseline

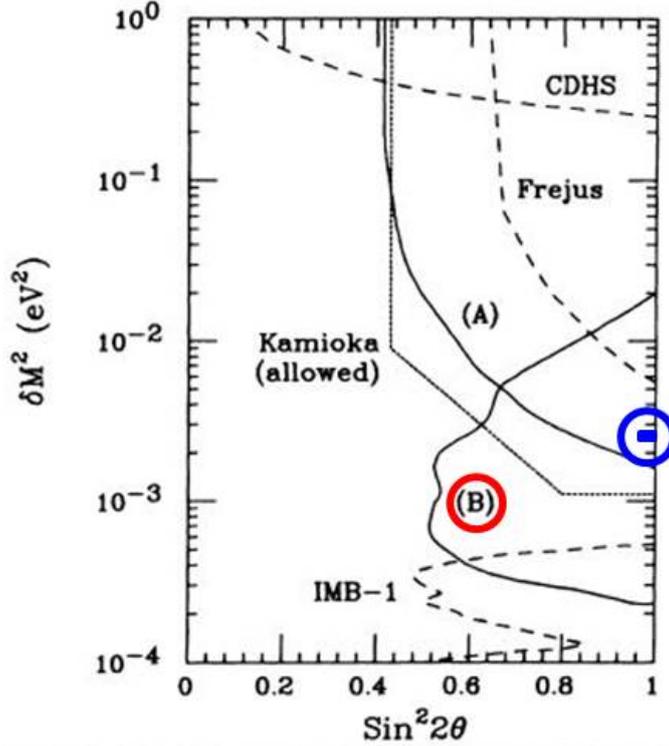


FIG. 2. 90% C.L. limits on ν_μ to ν_τ oscillations from rate (A) and stopping fraction (B). Dashed curves show limits from IMB-1 [14], Frejus [3], and CERN-Dortmund-Heidelberg-Saclay (CDHS) [15]. Dotted curve shows the allowed region from Kamiokande [16]. The Frejus limit is 95% C.L.; others are 90%.

Figure 9 – Region ruled out by analysis in Becker-Szendy⁴⁷ (B) with the current allowed region added in blue.

choice was chosen (i.e. MINOS over BNL’s P889). Due to matter effects and resolution of the mass ordering, it turns out that even longer baselines would have been desirable, and that is part of the motivation for the DUNE program.

8 God’s mistake

The search for neutrino oscillations has benefited from fortuitous experimental sensitivity to parameters, which led to a light-hearted argument circa 1995 that there was an intelligent design of neutrino parameters⁴⁹. The argument is that God made: the optimum choice for $\Delta m_{21}^2 = 8.2 \times 10^{-5} \text{ eV}^2$ which gives a resonant MSW effect transition in the middle of the solar neutrino energy spectrum; an optimum choice for $\theta_{12} \sim 30^\circ$ causing the effect to be big enough to be seen in KamLAND; an optimum choice for $\Delta m_{32}^2 = 2.3 \times 10^{-3} \text{ eV}^2$ giving the transition from no oscillation to full oscillation in the middle of the range of possible distances that atmospheric neutrinos travel to get to the detector; the optimum choice for $\theta_{23} \sim \pi/4$ leading to the dramatic large effects that were easy to see in atmospheric neutrinos; and the then unknown θ_{13} being small enough not to confuse the interpretation of the above, so that the story up to that time could be adequately explained with 2ν formulae. But the acid test was mentioned as whether θ_{13} would be large enough to see CP violation and determine the mass ordering. This was confirmed in 2011-2012 when the reactor neutrino experiments Double Chooz, Daya Bay and RENO found θ_{13} to be as large as possible consistent with the previous limits.

So in 2012 I extrapolated the intelligent design concept to the still unanswered questions

about neutrinos. This implied (1) the CP violation parameter $\delta \sim 3\pi/2$ to most quickly determine the mass ordering and to get large CP violation; (2) the inverted mass order so that we can more readily measure $0\nu\beta\beta$ to distinguish Dirac and Majorana neutrinos, and perhaps measure the beta decay endpoint, and (3) neutrinos should be Majorana which seems to be the more interesting case for theorists, and we want our theorists to be happy.

Question 3 hasn't been answered yet, but early comparisons of T2K, NOvA and reactor data suggest $\delta \sim 3\pi/2$ may be close to the answer. However there is increasing evidence that the mass order is normal, in contradiction to the apparent "Intelligent Design" answer. Did God make a mistake? The more likely answer is that the normal mass order is just what we want and we aren't intelligent enough to realize why yet.

9 Discussion of Issues raised

In this paper I have examined a number of historical contexts and issues regarding some wrongly interpreted results. However, there is no firm conclusion about how to recognize a wrong result or how to proceed after one is presented to the community. Just as serendipity often leads to a breakthrough (searches for nucleon decay leading to the discovery of neutrino oscillations is an obvious example), so too some of the examples I gave had positive consequences. But there are clear downsides, and time spent pursuing mistaken results that could have been more usefully spent elsewhere is impossible to estimate.

A number of questions have come up in this exercise. Is it fair that a wrong result can become a benchmark and get a huge number of citations? When should a possible paradigm shift become actively publicized outside our community? What should referees do with a result they don't believe is scientifically accurate but don't know what mistake was made, if any? When should a collaboration stop trying to resolve a new hard-to-believe result internally and announce it? Are rumors useful or counterproductive? How should we view papers which a full collaboration does not sign? Should there be more active skeptics of hard-to-believe results, or is the fate of being ignored satisfactory?

Some of the examples in Table 1 are published, some are presented at conferences, and some only make it to the rumor stage. Some elicit prompt critics, and some are faced with a "let's wait and see" attitude, till they are refuted or ignored. Some are followed by published retractions or explanations, and others are only followed by a loss of interest once a definitive exclusion becomes well known. Some motivate creative theoretical speculations while others fail to motivate any such ideas. Some lead to numerous follow-up experiments and others fail to do so. Some lead to useful thinking outside the box, and others do not. Could the understanding of the rightness or wrongness of these examples have been made more quickly?

There is one mistake in my view which is common in our field and that relates to the so-called 5σ criterion for discovery. We often consider a null hypothesis which is that the data can be understood without new physics, and a particular new effect as the alternative hypothesis. We design a test statistic that is sensitive to the difference and quota a chance probability that the data is described by the null hypothesis, usually turning P into $x\sigma$, assuming a Gaussian probability distribution. But this is only valid if the hypothesis and statistic are specified a-priori. Of course we do a-posteriori analysis all the time. It is part of our job to look for unusual aspects of the data. But while the number of σ is calculated in an identical way for an a-priori and a-posteriori hypothesis, the meaning is totally different. I cringe when I hear colleagues dismiss an a-priori 3σ effect and demand 5σ because "I've seen so many 3σ effects go away." Those were likely all calculated a-posteriori.

I suspect that as our interesting paradigms change, there is a strong time-dependent effect on our mistakes. Fifty years ago, a curious neutrino result might have been interpreted as SPT deviations from the V-A theory⁵⁰. Today the same result might be analyzed as a sterile neutrino.

10 Conclusion

While calling a result a mistake has a connotation of criticism, I do not criticize the vast majority of these reported results. While we want to avoid noise, sharing results we don't understand sooner rather than later might help the field get to the truth in a more efficient way. Our field of particle physics does a poor job in my opinion of presenting statistical arguments in a consistent way. In particular it is often difficult for an outsider to distinguish between a $x\sigma$ effect calculated from an a-priori test and an $x\sigma$ effect calculated from an a-posteriori test. We also do a poor job explaining to ourselves and others how we conclude anything based on whatever combination of data, theory and instinct that we use. Nevertheless, we seem to do an excellent collective job of taking seriously results which get vindicated and being skeptical of results which do not. That is probably the best test of how well our field is doing. And once again, the field of neutrino physics has been thriving.

Acknowledgments

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11 Other

I will try to briefly describe some of the other “mistakes” from Table 1 not already covered more fully. I divide them into 3 categories: (1) other reports which could be interpreted as oscillations which didn't pan out; (2) a few issues which may be regarded as semantic but I feel have some substance, and (3) everything else.

11.1 Other oscillation reports

A reactor experiment by Reines reported a strange CC/NC ratio and showed an allowed parameter space for neutrino oscillations⁵¹. Hank Sobel thinks that a changed cross section might have been responsible⁵². An early ITEP measurement of the tritium spectrum was consistent with a 30 eV mass neutrino⁵³. Later measurements seemed to give negative values for m^2 ⁵⁴. And a CERN beam dump result was interpreted as $\nu_e \rightarrow \nu_\tau$ in De Rujula⁵⁵.

11.2 Substantive semantic issues

For many years, the PDG reported neutrino masses associated with flavors, such as electron or tau. We now know that's like saying “the hole that the electron went through in the two slit experiment”. This was fixed in 2003⁵⁶. A related issue is that ν_e , ν_μ and ν_τ are flavor eigenstates, but not particles, which are the mass eigenstates. The PDG updated its chart of fundamental particles at the beginning of the 21st century⁵⁷, but CERN and Fermilab, among others, haven't fixed this in their graphics. Many people still confuse Δm_{23}^2 and Δm_{32}^2 . These differ by a sign and the difference is one of the main goals of new experiments. It is possible to define them with the opposite convention than usually done (in the usual convention Δm_{21}^2 is positive) but I don't think this is the actual source of the mistake.

When long-baseline or short-baseline is used as an adjective, which is almost always, there needs to be a hyphen.

11.3 Other neutrino mistakes

Unexpected y distributions ($y = E_{had}/E_\nu$) were reported in FNAL E1 at kinematic low x ⁵⁸. This was contradicted by CCFR and CHARM⁵⁹. The Kolar experiment reported events deep underground consistent with decays of a new particle in the air outside the detector⁶⁰. This was never confirmed. A search at NuTeV for supersymmetric particles decaying in a Helium bag found three events⁶¹ which did not match the signal hypothesis. The original atmospheric neutrino flux calculations used for the “ratio of ratios” did not take into account the fact that the muon is polarized⁶². Even after this was corrected, this was cited as a reason not to take the atmospheric neutrino anomaly seriously. An oscillation signal was reported by Bugey 2 at Neutrino 1984 with $\Delta m^2 = 0.2 \text{ eV}^2$ ⁶³. Two contradictory (different L/E) positive results from BNL involved low energy electron excesses in a ν_μ beam. BNL 776 saw 23 ν_e compared to 13.1 expected and BNL 816 saw 110 ν_e compared to 53 expected. Both were reported at Neutrino 1988⁶⁴. When Experiment 1 first saw three μ events, there were seminars about “super” events, but no claim was ever published⁶⁵. There was a timing anomaly in Karmen, which was interpreted as a massive new particle⁶⁶, but this was ruled out by Daum et al.⁶⁷ and shown by J. Reichenbacher to be caused by neutrons⁶⁸. A double beta decay experiment reported evidence for a Majoron⁶⁹, which was contradicted in⁷⁰ and the argument continued in⁷¹. It does not seem to be a topic of current interest. When MINOS measured neutrino oscillation properties separately for ν and $\bar{\nu}$, the numbers looked different for θ_{23} ⁷². MINOS invented an a-posteriori test and quoted the difference as 2% chance probability, or about 2.4 σ . MINOS then asked for and received additional $\bar{\nu}$ running and the discrepancy disappeared⁷³. Before neutral currents were firmly established, there were conflicting results known as “alternating neutral currents”^{74,75,76,77}. More recently, the NuTeV collaboration measured the NC/CC ratio to determine the Weinberg angle and got an unexpected result⁷⁸ known as the NuTeV anomaly.

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