AC

UNIVERSITY OF OXFORD

Department of Physics

PARTICLE AND NUCLEAR PHYSICS

OUTLOOK

D.H. Perkins

International Europhysics Conference on
High Energy Physics
27 July - 2 August 1995

Ref: OUNP-95-19

Address: Department of Physics
Particle & Nuclear Physics
Keble Road
Oxford OX1 3RH
U.K.

OUTLOOK

D.H. PERKINS

Nuclear Physics Laboratory, University of Oxford, Keble Road, Oxford OX1 3RH, U.K.

1 Introduction

My task today is not to try to summarize this conference but is, I believe, rather to make comments on the outlook and prospects in the field of high energy physics, both short and long-term. Let me say at once that I have the clear impression that particle physics, as a subject, is in extremely robust health, with a multitude of very challenging questions and problems which will keep us busy far into the future. The world community seems to be awash with first-rate experimental facilities — accelerators and detectors, either running now, or under construction, or being actively designed. The prospects for research, now and in the foreseeable future, look to me to be extremely good.

In this report, I will firstly discuss the current trends in particle physics; the technological aspects (accelerators, detectors etc. required to do the physics): funding needed to provide and exploit these facilities, and associated political and sociological questions; the possibility or otherwise of investigating ultra high energies using natural radiation (cosmic rays); and finally, a few remarks on what I think could be promising new developments.

Let me repeat that this is in no way intended as a conference summary: indeed, I shall try to keep mainly to subjects which have not been discussed in the other plenary sessions.

2 Physics — Present Trends

Some 90-95% of experimentalists today appear to be involved in work connected with the Standard Model (SM) which, for 20 years and more, has inspired both pride and frustration — pride that it works so well, frustration that it works too well. At this conference we have heard once more of the impressive agreement between experiment and the SM predictions, with no deviation above the 4σ level.

Apart from determining, as accurately as possible, the 17-odd free parameters (masses, couplings, mixing angles etc.) of the SM, there are still a number of missing components which have to be found in future experiments. These include

- ν_τ, still to be found as a free particle.
- Triplet gauge couplings $WWZ, WW\gamma$ etc, whose values are crucial to a renormalizable theory, and the measurement of which is — or was — the main raison d'être of LEP200. (There are also quartic gauge couplings, but these may never be measured).
- Higgs scalar sector. Detection of the Higgs — or an equivalent mechanism for spontaneous symmetry breaking in electroweak interactions — is the main goal and principal challenge of the LHC.
- Extensions of SM. The fact that radiative corrections are apparently independent of the virtual effects from much higher mass scales (up to M_{planck}) is the main justification for the SUSY models, which (provided the SUSY mass scale is below 1TeV) contain the necessary cancellations of divergent terms. Again, the LHC (or perhaps even LEP200) should be able to detect such particles if they exist.
- Measurement of direct CP violation in the CKM matrix. This is an important subject: according to cosmologists, we are only here today because of CP violation in the early universe. The study of direct CP violation in B decay will be the subject of a concerted effort at LEP, HERA, LHC and the B-factories now under construction. Incidentally, it may also be worthwhile to push still further the precision on electron and neutron electric dipole moment experiments: in SUSY models, the level of CP violation and dipole moments may be only two orders of magnitude below existing limits.

All this present programme of experiments related to the SM will be carried out at LEP200, HERA, LHC, NLC and B-factories, over at least the next 20 years. Of course, we hope that, in the process of these experiments, cracks in the edifice of the SM will appear, pointing to future physics. Already, interesting possible deviations are observed on R_c and R_b measured at LEP, so perhaps the cracks have even started.

Whether the SM is solid or not, there are many questions which it simply does not answer. Everyone of us has their own list of such questions. My list is neither unique nor comprehensive, but is probably typical of what anyone would write down, and is as follows:

3 Questions, Old and New

- Why 3 families? (Rabi first posed this one nearly 50 years ago, when there were only 2 families. Some might answer that 3 is the minimum number of quark doublets to ensure CP violation in the CKM matrix, but the true reason is surely deeper than that).
- Why is $M_{top} \sim G_F^{-\frac{1}{2}} \simeq v$ in the MSM? In other words, why is the Yukawa coupling of the top quark so close to unity?
- Is there quark substructure? Experiments at HERA are already probing to $q^2 \sim 10^4 \mathrm{GeV}^2$, some two orders of magnitude beyond the values possible in fixed target experiments on lepton-nucleon scattering.
- Is there a quark-gluon plasma? Can one ever attain, in the laboratory, the conditions required for its existence?
- Are electroweak and strong interactions unified at a GUT scale? If so, is there no new physics between the electroweak scale of $G_F^{-\frac{1}{2}} \sim 1\mathrm{TeV}$ and the GUT scale of perhaps $10^9 - 10^{13}\mathrm{TeV}$?
- Why are different levels of the physical world effectively decoupled from one another? In mundane terms, ice-skaters depend for their success on a phase transition in water. However, were they to be told that at a more fundamental level, water consists of electrons and quarks and gluons, that would not help them: the bulk properties of matter can be completely specified without any reference to inner structure. Similarly, the physics of the Fermi (electroweak) scale appears to be decoupled from the effects of whatever new and unknown physics there is at much higher energies.
- Why is there a baryon asymmetry in the universe? CP violation is necessary to discriminate unambiguously between matter and antimatter, but is CP violation in the electroweak sector sufficient to provide the observed asymmetry? If (as most people believe) it is not, what is the relevant source of CP violation?
- Cosmologists tell us that the universe started out with $N_B = 0$, while today $N_B \sim 10^{79}$. We are in a sense in a non-equilibrium situation — a condition necessary to develop any baryon asymmetry in the first place — and in due course can expect to revert to $N_B = 0$ via proton decay. So, do protons decay?
- What is the nature of dark matter? This question was discussed in two of the plenary talks and I hardly need to dwell on it here. If dark matter particles have typically weak interactions, then two orders of magnitude improvement in present sensitivity might well discover them.
- Are neutrinos massive or massless? Do right-handed neutrinos exist? Pauli made famous pronouncements on both questions. It is a fact that we do not know very much more about neutrino mass today than Pauli had surmised 65 years ago.
- What is the rôle of gravity? Is it unified with other interactions? This has been the subject of intense theoretical activity over more than 10 years, and of course the speculations go back much further (over 70 years). Unfortunately, progress so far has been painfully slow.
- Can we perhaps find new clues to particle acceleration from cosmic rays, which achieve beam energies some 10^8 times what has been possible on earth?

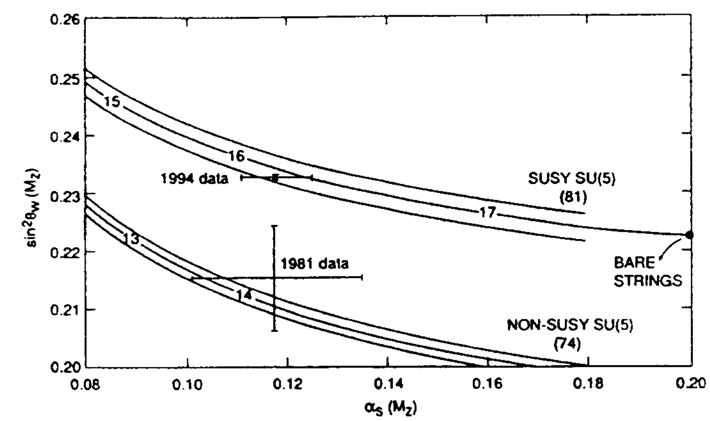

Figure 1: Curves show the variation of $\sin^2\theta_W$ with α_s according to SUSY and non-SUSY versions of GUT. Numbers give log of unification energy in GeV. The world average experimental values are shown for 1981 and 1994 (after Dimopoulos[1])

Some of the above questions are relatively new, others have challenged us for 50 years or more. It is probably no exaggeration to state that many of the above problems will keep the community occupied for the next century: and, of course, in tackling these questions, new questions are bound to arise. My main point is that there will be lots to do, and there seems absolutely no danger that particle physics will die off because of lack of interest.

Progress in high energy physics has been described as a long and sometimes painful story of crossed wires, theoretical misconceptions, wrong experiments due perhaps to inadequate technique, relieved by the occasional stroke of incredibly good luck, as we blunder from one astonishing revelation to the next. One should however be relaxed about mistakes — sometimes they can be beneficial. As an example, Fig. 1 shows a plot from a paper by Dimopoulos[1] at the Glasgow Conference in 1994. The lower curve shows the dependence of $\sin^2\theta_W$ on α_s (both evaluated at M_Z) predicted by the minimal SU(5)GUT. The 1981 data were consistent with this and a unification energy for the running U(1), SU(2) and SU(3) couplings just above 10^{14}GeV.

The present day values have much smaller errors and are, on the contrary, consistent with the SUSY version of SU(5), as first shown by Amaldi *et al* in 1991[2], with a unification energy M above 10^{16}GeV. Why was the 1981 value of $\sin^2\theta_W$ so low? It was strongly affected by one result from the BEBC chamber, which found $\sin^2\theta_W = 0.19$ in a neutrino experiment. I was a member of that group and naturally accept my share of responsibility. In fact, I am quite proud of this wrong result, as it probably advanced physics by at least 10 years! Why was that?

Since, according to the 1981 value, the expected unification mass was so low, the predicted proton lifetime (varying as M^4) should have been detectable in multi-kiloton detectors, and this sent several groups scuttling deep underground. Of course, proton decay was not found, but it was the first time that anyone had run such massive detectors in very low background conditions for long periods, and two major discoveries followed: the detection of the thermal neutrino burst from SN1987A, and the observation of the atmospheric neutrino anomaly. Note also that the old adage: "yesterday's signal is today's background" was also wrong in this case: the unwanted background of neutrino events has become today's signal!

4 Accelerators, Detectors and Sociology

Fig. 2 shows the Livingston plots for e^+e^- and pp($\mathrm{p\bar{p}}$) colliders, with the logarithm of the constituent CMS energy plotted against time. We all knew the initial exponential growth could not last, and there is clear evidence of a levelling-off. Energies are still increasing, but more slowly. The crucial question, to my mind, is not whether the next higher energy machine will be funded, but whether the timescale for achieving a particular factor of increase in energy might be getting unacceptably long. At present, a major experimental project can usually be completed, from design stage to final data analysis, within 10–15 years. This is not much longer than comparable "big science" projects in astrophysics involving satellite observations. However, if high energy experimentation becomes spread out over much longer timescales — so that, in an entire career, one might participate in only one or at most two experiments — the sociological effects might be very adverse. Young physicists would opt for other branches of the subject, those already in particle physics could move to another field. It is worth recalling that, two years ago, when dark stars (MACHOS) in our galaxy were observed by gravitational lensing of light from more distant bright stars, many of the American and French physicists involved were ex-particle physicists. The preprints announcing those results actually appeared in the same week that Congress cancelled the SSC. Are these things perhaps a sign of the times?

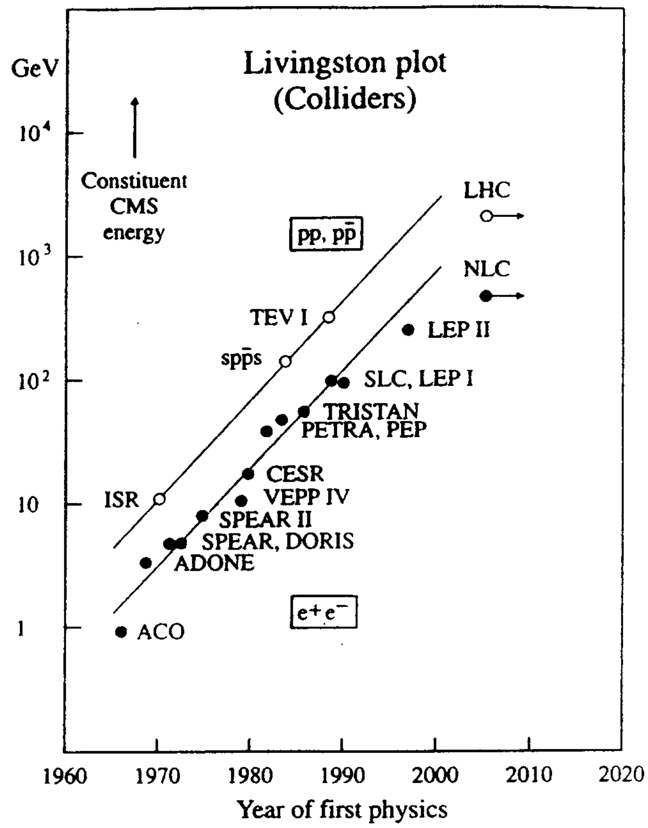

Figure 2: Livingston plots for e^+e^- and pp($\mathrm{p\bar{p}}$) colliders. The constituent CMS energy is plotted on a log scale against time.

The fundamental pointlike cross-sections, which are the main goal of new accelerator projects, vary as $1/s$ and thus the luminosity must rise with s to match, which in turn entails severe problems for on-line event selection and analysis, and of background and radiation damage (particularly to electronics). However, I cannot believe these will be limiting factors. When I was young I was greatly impressed, in the early 50s, by an experiment by Van Allen. He sent a rocket equipped with Geiger counters into the stratosphere, telemetering the cosmic ray counting rate down to earth. As the rocket ascended, the rate continually increased, and then, suddenly, there was complete silence — something had broken. The absence of signal persisted for several minutes. Then the counters suddenly started up again. Van Allen realised that the rocket must have gone through a region of intense radiation, which had saturated the Geigers so that no signal arrived. Thus, he discovered the radiation belts from the fact that the detectors recorded absolutely nothing! So, I have always had great confidence in the ingenuity and resource of experimental physicists, no matter how hostile the environment, and believe it will always be possible to dig good physics out of the detectors.

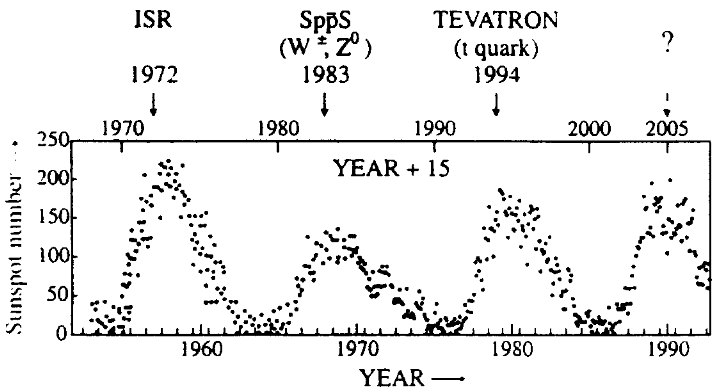

Figure 3: Sunspot number plotted against year, along the bottom scale. The upper scale shows times advanced by 15 years. The arrows refer to year of peak ISR luminosity (1971–2), to the discovery of the $W^{\pm}$ and Z^0 bosons at the CERN $p\bar{p}$ collider, and to the discovery of the top quark at the Fermilab collider.

The initiation of major projects such as LEP and LHC is based on theoretical arguments which, while compelling, can never be 100% certain. I recall that 10 years ago, on the CERN Scientific Policy Committee, one member told me that his fear was, not that the SSC would not be built, but that it would be built but would find nothing really new. Actually, very few accelerators in the past have failed to make significant discoveries, and the record of hadron colliders is quite good. Over the last 25 years, 7 hadron (pp or $p\bar{p}$) colliders have been started worldwide, and of these, 3 have actually been finished and 2 of them gave outstanding results. For amusement, I tried plotting hadron collider successes versus sunspot number, see Fig. 3. In order to make a future prediction, I arbitrarily added 15 years to the timescale, as shown at the top. At the displaced sunspot number peak in 1971 or 1972, the CERN ISR attained its full luminosity, and at that time must have been producing thousands if not millions of new particles — charmed particles, beauty particles, heavy leptons etc. Unfortunately, none of these was observed, but that was hardly the fault of the ISR — it could have been a great discovery machine.

The next maximum in 1983 marked the discovery of the $W^{\pm}$ and Z^0 at the CERN $p\bar{p}$ collider and at the next, in 1994/5 the top quark discovery at the Fermilab collider. Clearly, around 2005 promises to be an interesting time with a major discovery at a hadron collider somewhere in the world!

5 Funding

The last 45 years have seen stupendous advances in the field of high energy physics, but throughout all these innovations in theory and experiment, one thing has remained invariant: always, there are complaints about inadequate funding. Year after year, people have taken me aside to tell me about "the latest cuts", and assure me that these will mean "the end of our experimental programme". I notice however that somehow, people nearly always find a way of continuing.

High energy physics funding in the countries actively participating in research (in Europe, North America and Japan) can be described by the following rough rule of thumb:

$$\frac{High\ Energy\ Physics\ Funds}{Gross\ National\ Product} \simeq 2.10^{-4} \simeq 4\alpha^2$$

while

$$\frac{Number\ of\ High\ Energy\ Physicists}{Total\ Population} \simeq 5.10^{-6}$$

Here, $\alpha = 1/137$ is the low frequency limit of the fine structure constant (not an abelian running coupling constant increasing as one moves to higher energies!). The above numbers are not hard and fast and vary by factors of two or so from one country to another, but they do represent ball-park figures for resources over the last 10–20 years. (A more dramatic statement is that, if 25% of funds is ascribed to postgraduate student training, then over a typical 4 or 5 year period of research to the Ph.D degree, every research student becomes worth his/her weight in gold!).

The study of high energy physics is a form of basic research of mainly cultural value, and of no immediate application to improving living standards, or creating wealth. The form $4\alpha^2$ perhaps expresses the fact that, on the total scale of human endeavour, particle physics is only second order! But many people outside our field question whether this magnitude is reasonable or not. I see no way of answering this question, and it is probably futile to try to do so. The level of particle physics funding has arisen historically. I believe there were two main factors involved. First, in the '50s, nuclear research promised almost limitless new sources of energy, and it seems likely that governments simply did not appreciate the subtle differences between nuclear research and that in particle physics. The particle physicists never bothered to correct this misunderstanding.

More importantly however, particle physicists, in order to achieve the accelerators appropriate for the next step in energy, needed to start combining manpower resources and realised that it was essential to move from individual university groups to nationally shared facilities, and thence to research on an international basis. These developments took place from the early 50s onwards. The particle physicists got an early start on this, way ahead of astrophysics, condensed matter and atomic physics, chemistry and life sciences. Once funding had been determined by agreements at an international level, it was less subject to the whims of individual governments and national budget plans.

It is interesting to note that, in Europe at least, the pattern of large scale international collaboration was set in cosmic ray research just after the end of World War II. Balloon-flying expeditions over the Mediterranean were made by collaborations of 20 or more university groups covering almost the whole of Europe, from Norway in the north to Sardinia in the south, and from Ireland in the west to Poland in the east — a level of international cooperation unheard of in any other physical science at the time.

In the future, it is clear that, unless particle physics makes an unexpected discovery of economic importance, the ratio $4\alpha^2$ is not likely to increase. I would expect it to stay fairly constant or decrease slowly with time, depending on how the level of manpower in the community changes in the years ahead, and how well, as a world community, we succeed in closing down those accelerators and laboratories which have become obsolescent, and also on how well we succeed in collaborating on an inter-regional basis for future projects. Of course, this wider level of collaboration may generate its own problems, of the sort now being considered by the CERN Council in seeking non Member State contributions for the LHC project.

6 Cosmic Rays and Particle Physics

What will happen in the longer term, say more than 25 or 30 years from now? Hoping for some inspiration, I had a look at the CERN Convention, which is a remarkable document, seemingly written to cover every conceivable eventuality.

Article 2 of the Convention deals with the scientific purposes of the organization. In section 3 it discusses the accelerator programme and mentions successively the SC, the PS, the ISR and the SPS. The people drafting the document in 1953 stopped there: wisely they did not try to plan the accelerator programme more than 25 years ahead. Yet in section 2, there is evidence that they did wonder about the long-term situation, say in 60 or 70 years. Whether this is relevant to that or not, a specific aim of the organisation is stated to be work in the field of cosmic rays: in fact such research is mentioned twice in this one section. Indeed, many years ago, CERN operated a cloud chamber at the Jungfraujoch, and in 1960 carried out a search for proton decay — the very first with a liquid Cerenkov detector — in the Lötschberg railway tunnel.[3] This followed the first serious theoretical paper on proton decay by Yamaguchi in 1959.[4]

Looking back over the present century, we see that indeed the first half was marked by very important discoveries in cosmic rays. Most notable was that of the positron in 1933 by Anderson at Cal Tech, to be confirmed within a few months by Blackett and Occhialini in Manchester. The discovery of antimatter verified a unique prediction of the two greatest conceptual advances in physics in this century — the theory of relativity and the quantum mechanical description of atomic and sub-atomic phenomena. Next came the muon in 1937 (then called the mesotron). The pion and V-particles (leading to a new quantum number, strangeness) were both discovered in 1947. These were important not just in themselves, but in stimulating the accelerator "explosion", with a widespread programme of building synchrotrons of ever increasing energy to exploit the new world of "elementary particles" on a quantitative basis.

Since 1950, cosmic ray experiments have made quite a few "discoveries" which have turned out unfortunately to be wrong. Among these were the observation of "free quarks" in a cloud chamber and of "superheavy nuclei" from tracks in mica. Then there were the Centauro events (high energy interactions with anomalously low neutral pion production), disproved by the UA5 experiment at the CERN $p\bar{p}$ collider, and most recently, muon-rich air showers, allegedly due to incident γ-rays but with too many muons, contradicted by the measurement of the high energy photon cross-section at HERA.

The news for particle physics from cosmic ray experiments over the last decade or so has not been universally negative, however. In particular, there have been interesting developments in neutrino physics. As mentioned already, there was the observation of the neutrino burst from SN1987A, marking the birth of neutrino astronomy and confirming the correctness of the proposed Type II supernova mechanism, but, despite all the claims, giving little new factual information about neutrino properties. The solar neutrino deficit — discussed several times at this meeting — and the atmospheric neutrino anomaly are certainly suggestive of neutrino mixing. An actual discovery would, in my opinion, require a source that one can control (turn on and off for example), and future reactor and accelerator experiments could well establish neutrino oscillations as a fact. This is leading to a resurgence of a fixed target lepton beam programme at various accelerators.

7 Ultra High Energy Cosmic Rays

At the Workshop in Astroparticle Physics in Stockholm in September 1994, the summary talk[5] contained the statement: "Now, with the limitations on the scales of energy that can be attained in earth-bound experiments, we hope that a look to the heavens will provide new insights into particle physics and fundamental interactions". Is this hope justified? Having spent many years working in cosmic rays, perhaps I can hazard an answer.

Figure 4: Collision in emulsion of 22TeV Fe nucleus, producing about 700 secondary mesons

Let me say first of all that the acceleration of cosmic rays is a remarkable phenomenon, still little understood. To begin with, many point sources of cosmic rays have been identified. In our galaxy, the Crab is a source of γ-rays of energies extending well above 10TeV. Remarkably this pulsar, with a diameter of 20kms, has about the same dimensions as LHC, but the omni-directional intensity is enormous. At similar energies, $I_{crab} \approx 10^{23} I_{LHC}$. Even within the angular spread of the LHC beams, it is much bigger. Outside our galaxy, Markarian 421 (an AGN) has a γ-ray intensity over $10^6 I_{crab}$. So, these cosmic accelerators are quite remarkable. Like photons, neutrinos also offer the possibility to identify point sources of radiation, and there are currently several projects intended ultimately to detect neutrino point sources in the TeV region. As the neutrinos traverse the earth, they will produce upward high energy muons, which can be detected through the Cerenkov light generated as they traverse great depths of water or ice.

So far, the highest energy cosmic rays detected consist of protons or heavier nuclei. As a memento of the now defunct SSC, let me first show a couple of events from Texas — not Waxahachie, but a balloon flight from Palestine, Texas, some 35 years ago. Fig. 4 shows a rather low energy (E=22TeV) iron collision in emulsion, producing some 700 secondaries. Fig. 5 shows the electromagnetic cascades developing in a sandwich of tungsten and emulsion from the highest energy event, which we called the Texas Lone Star. The multiplicity is again about 500, the primary energy is 2000TeV, and the single most energetic γ-ray has 20TeV energy. The CMS energy $\sqrt{s} \simeq 2$TeV, about the same as at the Fermilab collider. An interesting feature of this event is that the transverse momentum of the γ-rays from π^0 decay is almost double that at low energy, as shown in Fig. 6. However, the statistics of these events in the 1000TeV region is too poor to establish this p_T increase as a general phenomenon.

Going up to what are called ultra high energies, events become so rare that they can only be detected via the extensive air showers (EAS) they produce. Several events have been observed in the 10^{20}eV region, the most energetic to date having $E = 3.2 \times 10^{20}$eV (320 million TeV). It was recorded at the Fly's Eye array (Utah) with mirrors detecting the atmospheric scintillation light from the EAS particles and displaying the shape of the cascade as it traversed the atmosphere. This event is remarkable in several ways. First, it happened on my birthday (15-10-91) which alone is a 3σ effect! Second, it is so energetic that it must have been local. Fig. 7 shows calculations[6] of the average decrease of energy of primary protons of initial energies 10^{20}, 10^{21} and 10^{22}eV as they originate at progressively greater distances, as a result of photopion production off the cosmic microwave (2.7°K) background. No protons above 10^{20}eV can survive with this energy if they originate beyond the local (Virgo) cluster. In this graph, no account was taken of the evolution of the universe. Larger distances correspond to earlier times when the universe was younger and hotter, and the CMBR temperature gets a red shift factor $(1 + z)$. For distances beyond 10^3Mpc the energy

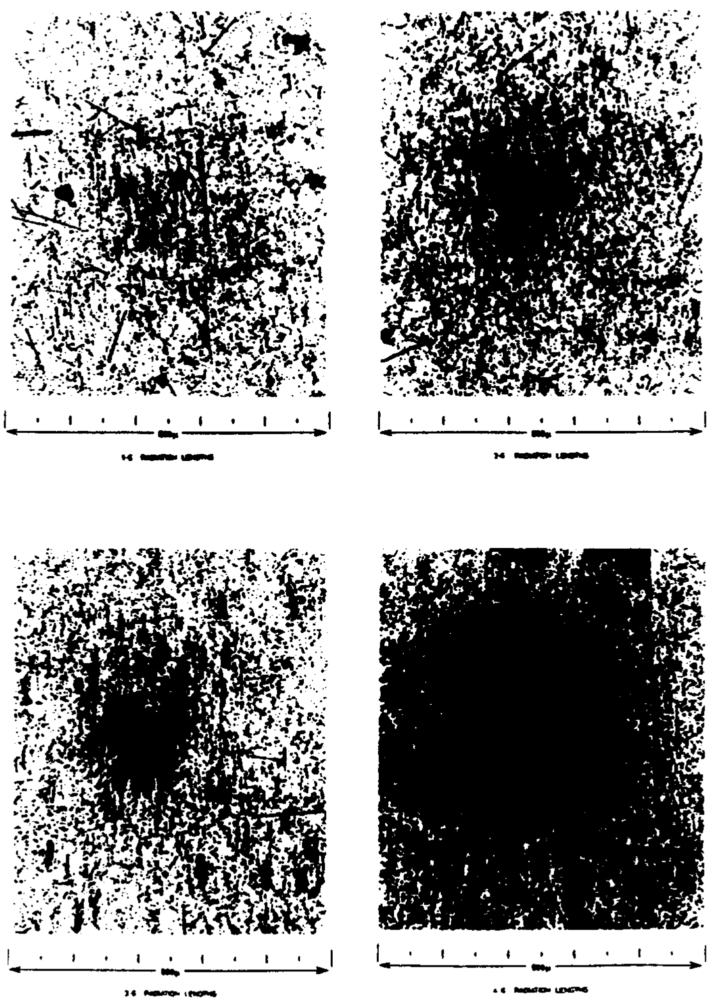

Figure 5: Cascade developments in tungsten-emulsion sandwich, of the Texas Lone Star event. Primary energy is 2000TeV, and the single highest energy γ-ray is over 20 TeV.

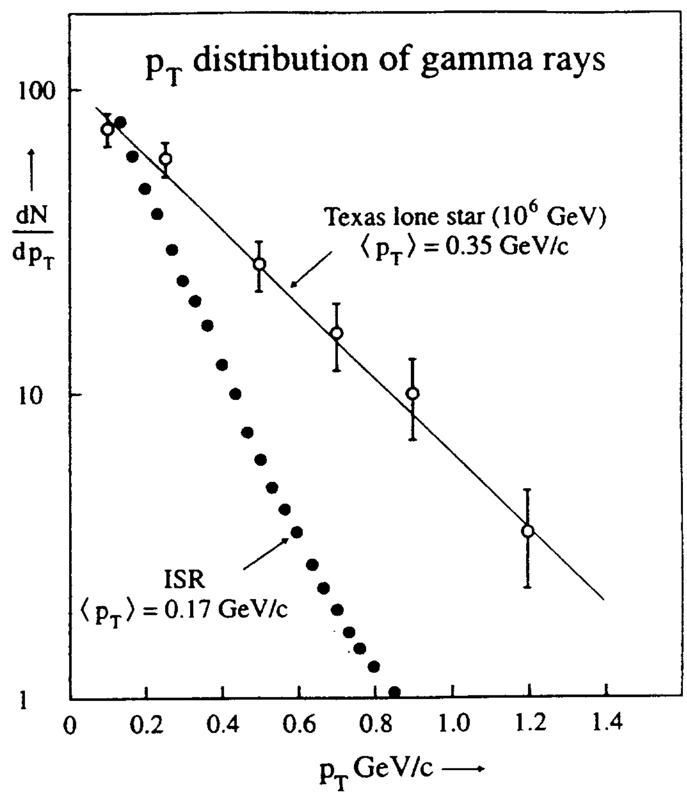

Figure 6: p_T distribution of γ-rays from Texas Lone Star event The mean value is about twice that observed at low energies.

loss is therefore underestimated.

An energy of 10^{20}eV is large enough that such protons cannot be confined by galactic magnetic fields: they must be intergalactic. Indeed, the expected magnetic bending is quite modest and remarkably, it is found that these very highest energy protons do not point to any obvious known sources.

The energy of the highest energy proton recorded corresponds, in a collision with a nucleon, to $\sqrt{s}$ = 800TeV $\approx 60\sqrt{s}$ (LHC). So the maximum CMS energy that can be attained in cosmic rays is less than two orders of magnitude larger than the biggest collider: that does not take us very far beyond the electroweak energy scale, $G_F^{-\frac{1}{2}}$. What is certainly very interesting is to try to understand the acceleration mechanisms involved, which result in beam energies 10^8 times what can be realised on Earth. To this end, there is a project[5] to build a 5000km^2 ground array to detect some 5000 air showers per year of energy above 10^{19}eV. This is to be called the Pierre Auger array, in memory of the person who discovered both the Auger effect and air showers. Perhaps even more significant is the fact that Pierre Auger and Isadore Rabi were, in my opinion, the original founding fathers of CERN. I last saw Auger at the CERN 25th anniversary. He was a tall, erect person who carried a rolled-up umbrella which he would point at objects of interest. He wandered around CERN, this object of his creation, making helpful comments in a loud voice about the general inefficiency and gross incompetence of the organization! I am very happy that this great man will be commemorated.

It seems to me that the bottom line to all this is that while the study of high energy cosmic rays is of very great interest from the point of view of astrophysical sources and the physical mechanisms responsible for their enormous energies, it does not appear that their interactions will ever be recorded either in the detail or in the numbers sufficient to allow analysis of the fundamental physics involved. In any case, the highest attainable collision energies do not push very much beyond what

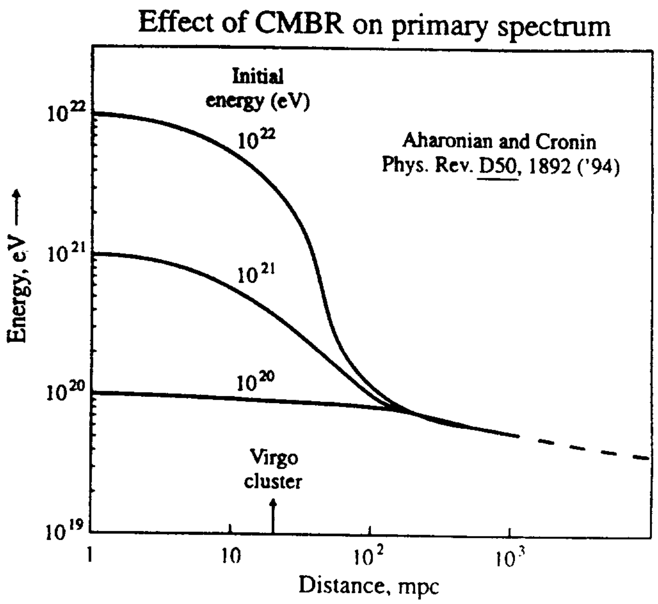

Figure 7: Energy depletion of primary protons as a function of distance of origin, due to photopion production off microwave background.

is attainable at man-made colliders in the immediate future. Naturally, I shall be very pleasantly surprised if this rather negative opinion is proved to be wrong.

8 Neutrino Oscillations

After many years of speculation and probing experiments, it seems that quite new developments are likely to take place in neutrino physics over the next few years: I thought that I ought to emphasize what I think will become a very important new avenue of research.

The usual explanation of the solar and atmospheric neutrino deficits/anomalies — assuming of course that the experiments and their interpretation are correct — is in terms of neutrino oscillations. While there is a very large number of ways of parameterising these effects, two extreme mechanisms are as follows:

8.1 MSW Mechanism

The MSW mechanism was proposed in connection with the solar neutrino deficit by Wolfenstein[7] in 1978 and Mikheyev and Smirnov[8] in 1986. As shown in Fig. 8, this provides a good description of all four experiments and their different deficits, in terms of the MSW "bath tub", with $\Delta m^2 = 0.8 \times 10^{-5}\mathrm{eV}^2$ and vacuum mixing angle $\sin\theta_V = 0.03$ (both constrained within a 10% range by the data), which gives almost total suppression of ^{7}Be and pep neutrinos, and 40% suppression of ^{8}B neutrinos. The Homestake and Kamioka experiments have essentially the same response,[9] in terms of energy, to the ^{8}B flux and they are compatible with each other.

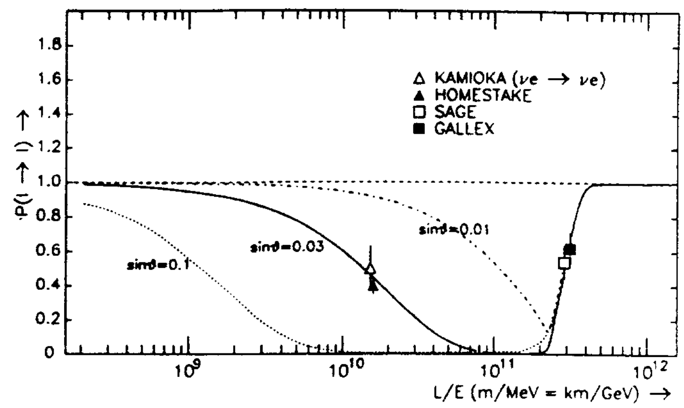

Figure 8: MSW suppression as a function of L/E (L=distance to sun, E=neutrino energy) for 2-flavour mixing and $\Delta m^2 = 0.8 \times 10^{-5}\mathrm{eV}^2$, with $\sin\theta_V = 0.03$ giving the best fit. Calculation by W. Scott (RAL).

The atmospheric results require, on the contrary, a large vacuum mixing angle ($\sim 50°$) and $\Delta m^2 \simeq 10^{-2}\mathrm{eV}^2$. Since one mixing angle is small, the three neutrino flavour problem can be treated in terms of two sets of independent two-flavour mixings and the two pairs of values of Δm^2 and θ_V indicated above. The original attraction of the MSW mechanism was that it required only a small vacuum mixing angle, just as for the off-diagonal elements of the quark (CKM) weak mixing matrix. However this feature is lost if a large mixing angle is needed anyway to account for the atmospheric results.

8.2 Maximal Mixing

The other extreme hypothesis is that of threefold maximal mixing, proposed by Nussinov[10] in 1976, and Cabibbo[11] and Wolfenstein[12] in 1978. (Note that, if either of the two hypotheses is right, Wolfenstein is bound to win!) The basic idea in maximal mixing is that the three flavours are on the same footing and thus, if there is mixing at all, it must be maximal. All flavours of neutrino have exactly the same survival probability as a function of time, CP violation is also maximal, and the MSW mechanism (which will operate wherever there is an electron density) does not affect the survival probability in any way. It might just as well not be there. In this sense, these two extreme hypotheses are mutually exclusive.

The usual objection to maximal mixing is that in the quark sector, mixing angles can be very small and CP violation is only at the 10^{-4} level. However, Harrison and Scott[13] point out that mixings will evolve with energy.

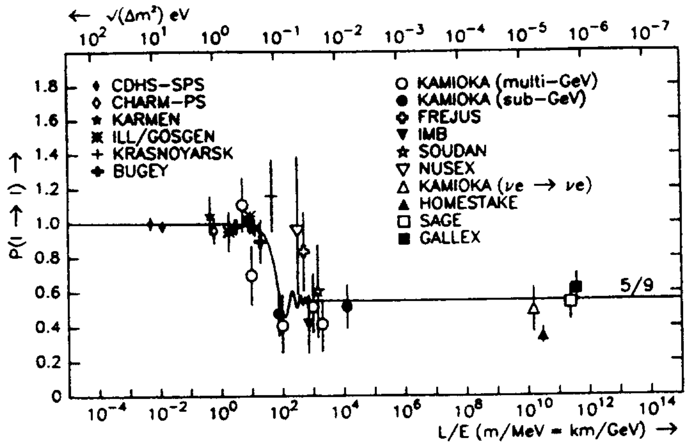

Figure 9: Neutrino survival probability as a function of L/E, according to the maximal mixing hypothesis, fitted to the available data.[14]

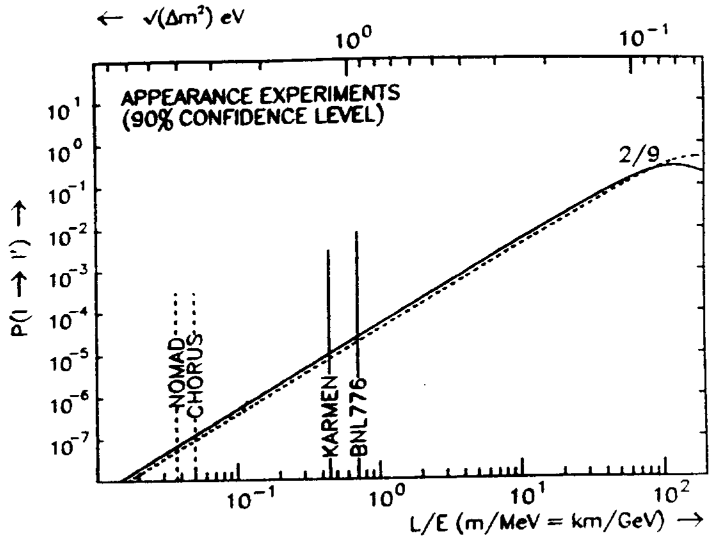

Figure 10: Appearance probability for one type of neutrino oscillating into another, as a function of L/E, for a value of Δm^2 taken from the fit of Fig. 9. The values of L/E for the NOMAD[19] and CHORUS[20] projects at CERN and the existing upper limits from BNL 776[21] and KARMEN[22] are indicated.

According to them, an experiment at the Planck scale should find maximal mixing in the quark sector also, and it is only the relatively large values of the quark masses (particularly the top) that makes the mixing small at everyday energies. For neutrinos, with masses in the eV region or below, it is plausible that the evolution to maximal mixing could be complete.

Fig. 9 shows our results for the neutrino survival probability against L/E for reactor, accelerator, solar and atmospheric experiments.[14] The curve gives the maximal mixing prediction, taking account of smearing due to the fact that experiments involve a range of energies, and calculated for the larger of the two mass differences, $\Delta m^2 = 0.72 \times 10^{-2}\mathrm{eV}^2$, and for the smaller, $\Delta m'^2 < 10^{-11}\mathrm{eV}^2$. This is consistent with all experiments (χ^2 per degree of freedom =19.2/26) with the exception of Homestake, which alone would add 16 to the value of χ^2. The solar survival probabilities are here based on the average of the Bahcall-Pinsonneault[15] and Turck-Chieze/Lopez[16] solar model predictions. If one takes instead much lower predictions[17] for the ^{8}B and ^{7}Be fluxes, the discrepancy is reduced. The corresponding χ^2 for the four solar experiments alone comes down from 18 to 7 — still not really acceptable — but of course if one includes all experiments, the overall value of $\chi^2 \simeq 26$ for 27 degrees of freedom. What is clear is that, if the experimental solar results, and the stated errors, are taken at face value, maximal mixing has a severe problem, as has been pointed out by Petcov.[18]

The crucial test of maximal mixing will however come from future accelerator and reactor experiments. Short baseline experiments such as CHORUS and NOMAD at CERN are predicted to give an appearance probability ($\nu_\mu \to \nu_\tau$ for example) of order 10^{-7} only, so that even one τ event (above background) in these experiments would kill maximal mixing (see Fig. 10).

A number of long baseline experiments have been proposed. The CHOOZ[23] and SAN ONOFRE[24] reactor experiments will have 1km and 0.75km baselines respectively. Fig. 11 shows, at top left, the event rate for the reaction $\bar{\nu}_e p \to n e^+$. Since both neutron and positron energies are measured, the antineutrino energy is known for every event.

The lower graph shows the $\bar{\nu}_e$ survival probability according to the maximal mixing fit of Fig. 9, and the energy range covers just over one oscillation, with a large suppression ($\frac{1}{9}$) expected at $E = 5 - 6\mathrm{MeV}$. First results from CHOOZ are expected within one year. Long baseline experiments at accelerators include proposals for Brookhaven (E889,.[25], for Fermilab (P875,MINOS[26]), and for KEK.[27] A CERN-GRAN SASSO beam is also under discussion. Fig. 11 includes some examples of event rate and survival/appearance probabilities, as a function of energy. Above the 5 GeV threshold for $\nu_\tau + N \to \tau + \ldots$ a substantial fraction (5–10%) of events are predicted to contain τ leptons.

Lastly, Fig. 12 shows the mass spectrum of the fundamental fermions. For the neutrino masses, I have assumed a mass hierarchy similar to that for the charged leptons and quarks, for which the maximal mixing scenario[14] places one mass eigenstate at $m_3 = 80\mathrm{meV}$ and the other two (m_1, m_2) below $3\mu\mathrm{eV}$. Of course, it

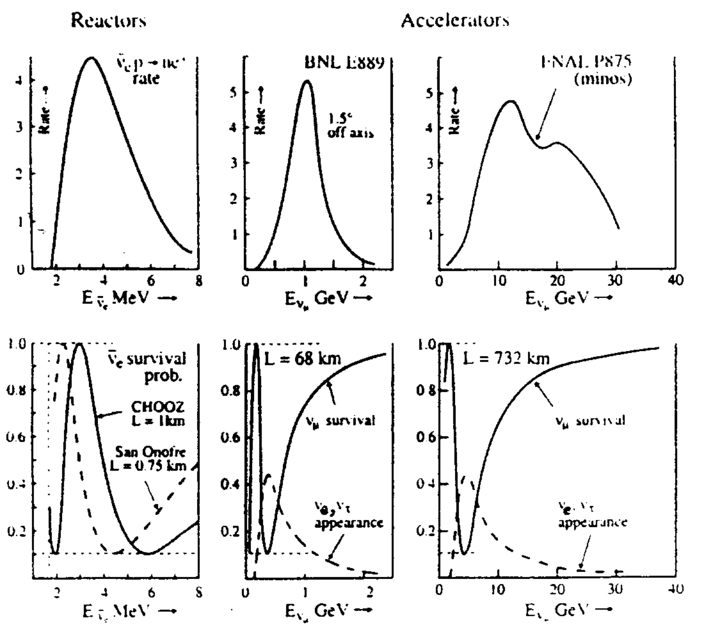

Figure 11: (Top) Neutrino event rates from three long baseline experiments. (Bottom) Survival/appearance probabilities as a function of neutrino energy, for the fit parameters of Fig. 9.

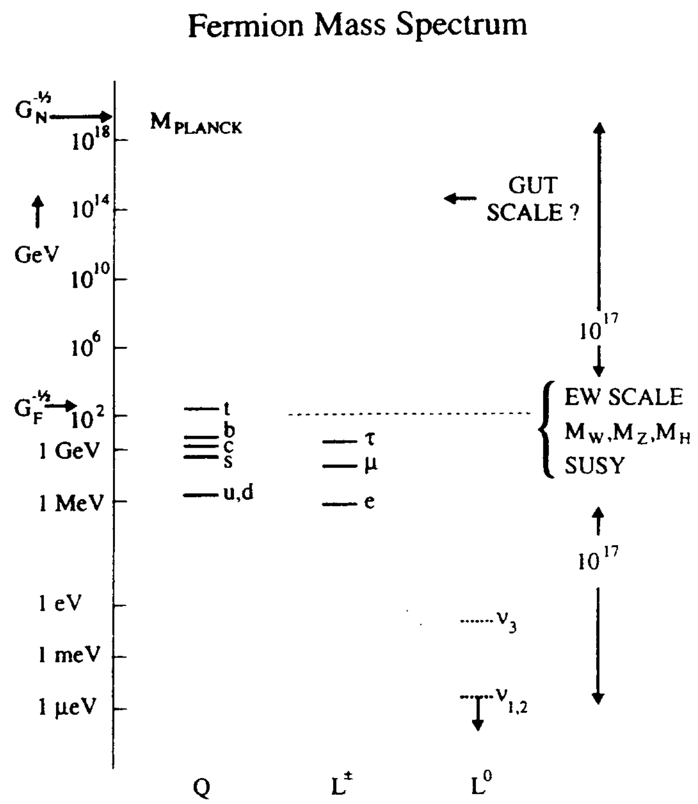

Figure 12: The fermion mass spectrum. The neutrino mass eigenstates are those assuming a mass hierarchy and the mass differences deduced from Fig.9.

is possible to have a non-hierarchical spectrum[28] with three mass eigenstates effectively degenerate with, say, 5eV each (prompted by assuming that neutrinos constitute hot dark matter). In any case, the main point of Fig. 12 is that the known fermion spectrum extends down from the electroweak scale of about 1 TeV, to eV, meV or μeV i.e. a range of up to 10^{17} in mass. From the top mass to the Planck mass is also a factor 10^{17}. It seems impossible to understand the smallness of neutrino masses in terms of the electroweak scale, and presumably they are the clearest present indication of new physics at much higher energies, for example at the GUT scale via the see-saw mechanism and massive Majorana states.

9 The Bottom Line

Finally, for some concluding remarks, I would like to quote a statement by Feynman many years ago, regarding the outlook in particle physics, but just as relevant today as it was then. In the course of lectures on the parton model at an Hawaii Topical Conference in 1973, Feynman was asked whether he thought the theory of strong interactions would reach the precision and predictive power of QED, verified by a whole range of experiments to an accuracy of 1 in 10^6 or better. His reply was never written down, but as I recall it went as follows:

"Well, QED is very nice and impressive, but when everything is neatly wrapped up in blue bows, with all experiments in perfect agreement with each other and with theory — that is when one is learning **absolutely nothing**!"

"On the other hand, when experiments are in hopeless conflict, or when observations don't make sense according to conventional ideas, or when none of the new ideas seems to work: in short, when the situation is a mess — **that** is when one is really making hidden progress and a breakthrough is just around the corner!"

Thinking today of the conflicting observations on ϵ', of the large deviations from expectations for R_b and R_c in Z^0-decay, or of the last 20 years of theoretical speculations "beyond the Standard Model" — not a single one of which has so far achieved any experimental support — I believe that we are in the second Feynman scenario and the future looks very promising.

Acknowledgements

It is a pleasure to thank Professor Peter Fowler (Bristol) for Figs.4 and 5, Dr. Bill Scott (RAL) for Figs.8 and 10, and both of these colleagues for useful discussions.

References

1. S. Dimopoulos, *Proceedings of the 27th International Conference on High Energy Physics*, Glasgow, 1994, (Institute of Physics, London), **1**, 93.
2. U. Amaldi, W. de Boer and H. Furstenau, *Phys. Lett.* **B260** (1991) 447.
3. G.K. Backenstoss et al *Nuovo Cim.* **16** (1960) 749.
4. Y. Yamaguchi, *Proc. Theor. Phys.* **22** (1959) 373.
5. J.W. Cronin, *Nucl. Phys. B (Proc. Suppl.)* **43** (1995) 343.
6. C. Aharonian and J.W. Cronin, *Phys. Rev.* **D50** (1994) 1892.
7. L. Wolfenstein, *Phys. Rev.* **D17** (1978) 2369.
8. S.D. Mikheyev and A. Yu Smirnov, *Nuovo Cim.* **9C** (1986) 17.
9. W. Kwong and S.P. Rosen, *Phys. Rev. Lett.* **73** (1994) 369.
10. S. Nussinov, *Phys. Lett.* **B63** (1976) 201.
11. N. Cabibbo, *Phys. Lett.* **B72** (1978) 223.
12. L. Wolfenstein, *Phys. Rev.* **D18** (1978) 958.
13. P.F. Harrison and W.G. Scott, *Phys. Lett.* **B333** (1994) 471.
14. P.F. Harrison, D.H. Perkins and W.G. Scott, *Phys. Lett.* **B349** (1995) 137.
15. J.N. Bahcall and M.H. Pinsonneault, *Rev. Mod. Phys.* **64** (1992) 885.
16. S. Turck-Chieze and I. Lopez, *Ap. J.* **408** (1993) 347.
17. A. Dar and G. Shaviv, *Nucl. Phys. B (Proc. Suppl)* **38** (1995) 81.
18. S.T. Petcov, *Nucl. Phys. B (Proc. Suppl)* **43** (1995) 12.
19. L. Dilella, "Neutrino 92", *Nucl. Phys. B (Proc. Suppl)* **31** (1993) 319.
20. J. Brunner, *Proceedings of the International Europhysics Conference in High Energy Physics*, Marseilles, 1993, Editions Frontières, Gif-sur-Yvette) p.555.
21. N. Ushida et al, *Phys. Rev. Lett.* **57** (1986) 2897.
22. B. Bodman et al, *Phys. Lett.* **B280** (1992) 198.
23. Y. Declais et al, "Proposal to Search for Neutrino Oscillations using a 1Km baseline reactor neutrino experiment" (1993).
24. F. Boehm et al "Proposal for the San Onofre Neutrino Oscillation Experiment" (CalTech report January 1994).
25. BNL Proposal E889 (see J. Schneps, *Nucl. Phys B (Proc Suppl)* **31** (1993) 307 and **38** (1995) 220; BNL 52459 (1995).
26. W.W.M. Allison et al, Fermilab Proposal P-875 (MINOS) (1993).
27. K. Nishikawa, INS-Rep-924, (1992).
28. D.O. Caldwell and R.N. Mohapatra, *Phys. Rev.* **D46** (1993) 3259.