PROTON DECAY, REVIEW AND FUTURE PROSPECTS

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ABSTRACT

The motivation for the search for baryon number violation is reviewed and seen to be increased over the last decades. The techniques and history of searches are summarized, and the future outlook is presented. Present 90% C.L. limits on nucleon decay are 5×10^{33} years for the most sensitive search which is for the $e^{+} - \pi^{o}$ partial mode, and within a factor of ten of this for most other modes. Prospects are good to reach 10^{34} years with Super-Kamiokande for $e^{+} - \pi^{0}$ and $\sim 10^{33}$ years for SUSY favored modes. No experimental limit exhausts theoretical bounds, though some models suggest accessable decay rates. The quest might thus succeed with megaton water Cherenkov instruments now under discussion (Hyper-Kamiokande, TITANIC, UNO, Aqua-RICH) which may reach 10^{35} year limits. Other proposed devices (Super-ICARUS, LANNDD, SCIPIO) may make better progress on SUSY favored modes with kaons in the final state with smaller but more sensitive and better background rejecting detectors. There are also prospects for significant improvement in $n - \bar{n}$ oscillation searches as well. Such instruments may also be utilized as far-detectors in concert with future neutrino factories.

1. Why Search for Baryon Number Violation?

The incentive for seeking baryon number violation has increased with time and shifted a bit, moving from undirected probing of conservation laws to cosmological necessity and the seeking clues for grand unification[1)].

Perhaps the strongest present day motivation may be stated as simply that we are here to talk about it! Since the famous 1967 paper of Sakharov[2)] it has been clear that baryon number violation was one of the necessary requirements of the Big Bang era, in order that there remain a net excess of matter over anti-matter at the required level of $O(10^{-9})$. The subject is made more complicated than originally thought as it appears that early asymmetry may get erased by the "sphaleron mechanism" of Kuzmin[3)].

The second motivation comes from attempts at finding a theory combining electroweak and QCD, a grand unified theory (GUT) of elementary particle interactions. As it happens, the models encompassing the quarks and leptons in larger groups inevitably contain the possibility of as-yet-unseen couplings, some of which can cause nucleon decay while conserving $B - L$. In fact generally such models do contain nucleon decay, unless they are tinkered to avoid such decay in ways which so far appear "unnatural". In SUSY models in fact it is problematic not to have too rapid nucleon decay from dimension five operators.

Nucleon decay can thus occur in the rare instance when the quarks are effectively close enough that they are within range for heavy quark-number-violating particles to couple them. This is the poor man's accelerator: one probes down to the Planck scale by waiting, rather than employing brute force collisions at high energies.

We can set an approximate scale for the rate of nucleon decay based simply upon dimensional analysis, with $\tau_p \propto M_X^4/\alpha^2 m_p^5$, where m_p is the nucleon mass and M_X is the mass of the boson making the decay transition (eg. turning a quark into an anti-lepton and a quark into an anti-quark). One needs a detailed model to get branching ratios, but in the simplest schemes one may expect the strongest decay to be $p \rightarrow e^+\pi^o$, as long as the transitions are in one generation. Unfortunately SUSY models favor transitions which cross generations, leading to strange particles in the final state (other quarks being too heavy), and hence the probable dominance of difficult-to-detect modes such as νK[9].

The "standard" GUT models, as above, while violating conservation of baryon number do conserve some combination of quantum numbers such as $B-L$, and have $\Delta B = 1$. There are other models (eg. by Pati and Salam[6]) which contain $\Delta B = 2$, and which can lead to such strange things as $n - \bar{n}$ transitions and $n \rightarrow \nu\nu\nu$.

One usually assumes conservation of electric charge, angular momentum, mass-energy, and CPT in considering possible nucleon decay modes, yet this still leaves about 50 possible modes! Given the indeterminacy of model predictions, the experimentalist is faced with the prospect of having to pursue all practical mode searches. Published predictions range from lifetimes which should already have been discovered, to the probably untestable (with present technology) 10^{36-37} years. We shall not survey the various models here, but refer the reader to the literature (nice summary in [10]).

2. Where to Look? A Rapid survey of Experimental Methods

Given the intention to search for nucleon decay on earth one has two extreme choices: look for a long time or in a large amount of material, or better both. One assumes that the nucleon decay, liberating as it must an energy large compared to nuclear binding energy, will always proceed independantly of the nuclear binding energy. The final state may be effected however, with pions charge-exchanging and particles scattering on the way out of the nucleus, for example. One also assumes that the nucleon decay probability is not effected by the nucleus, which to the extent that the quarks are confined to individual nucleons should be the case.

2.1. Geochemical

One may search in ancient rocks for isotopes which could not have arisen except via nucleon decay, known as geochemical searches. An example is looking for $^{130}Te \rightarrow ^{129} Xe$. Another method that integrates over time comes from searches for decay tracks in ancient mica. These methods give long exposure times ($\sim 10^9$ years) and with a few grams of material can give exposures of order 10^{33} nucleon-years. However, uncertain exposure histories for the samples and difficulties with knowing efficiency make for some lack of confidence. The best limits from this avenue are in the range of 2×10^{27} years.

2.2. Radiochemical

Another type of search uses the radiochemical technique, brought to a high state of development by solar neutrino experimenters. A virtue of such a method is that one observes, as with the geochemical method, what is left after decay, and so such experiments are broadly decay-mode insensitive. The disadvantage is that one does not observe the decay itself, and such transitions (as for example $^{39}K \rightarrow ^{37} Ar$) can come about from cosmic ray interactions. The best limits from this approach are on the order of 2×10^{26} years.

2.3. Secondary Counting

Yet a further method is to seek secondary particles to the decay, without necessarily promptly "seeing" the nucleon decay itself. For example one may seek an excess flux of neutrinos from nucleon decays throughout the earth (which have energies more than an order of magnitude greater than neutrinos from radioactive decays). This method, resembling the searches for WIMP annihilations (except that the angular spread of the PDK neutrinos would be greater), suffers from having to find an excess of neutrinos above the (inescapable) terrestrial cosmic ray neutrino background (and unfortunately which background peaks in the same energy region). Again, such limits are rather nice in that they sum over many possible decay modes, but the sensitivity achieved is not very great, of order 6×10^{26} years.

A variant on this method is the possible observation of neutrons appearing when protons disappear from deuterium (in heavy water). This search may be the most insensitive of all to decay mode, since even disappearance of charge would not effect the detection. Once again however, the limits are not strong, 3×10^{23} years. This may improve significantly in the next few years due to the SNO detector, with 1000 tons of heavy water being closely monitored and having neutron detection.

2.4. Direct Counting

The mainstream route is actively to monitor a huge volume of material for a few years. An instrument with a twenty thousand ton fiducial volume (e.g. Super-Kamiokande) has 1.2×10^{34} nucleons. Thus lifetime/branching ratios of order 10^{34} years may be accessable after a few calendar years of observation (allowing for inefficiencies and detector live time).

This has been most successfully carried out with the large water Cherenkov detectors, the IMB (8000 tons) and Kamioka (1000 tons) instruments, in the first generation in the 1980's, and now joined by the Super-Kamiokande (50,000 tons) and SNO (1,000 tons D_2O) detectors. These instruments are extremely simple, in principle, consisting of a tank of very pure water surrounded by many photodetectors catching the Cherenkov light produced by nucleon decay products (and background atmospheric neutrino interactions) as they traverse several meters in water.

The search for $e^+\pi^o$ consists, for example, of seeking events which have about 983 MeV of visible energy, no net momentum beyond that attributable to the Fermi momentum (about 200 MeV/c) of the nucleons in the oxygen nucleus, and having the energy divided between a positron (electron-like) shower and a back-to-back two ring shower due to $\pi^o \to \gamma\gamma$. The background for nucleon decay arises from cosmic ray neutrino interactions, which of course tend to exhibit the momentum of the incoming neutrino and so are largely (and easily) rejected. Consequently this search is not yet background limited, and Monte Carlo simulations made for Super-Kamiokande indicate that it can proceed to about 10^{35} years without background limitation. This requires an exposure of about a four megaton years with a Super-Kamiokande-sensitivity instrument to achieve a 90% C.L. limit for that reach.

Other instruments have been built employing counters of the tracking type more common at accelerator laboratory experiments. These tend to be stacked in a linear geometry and have poor aspect ratios. Volumetric efficiency is relatively small for cleanly containing interactions in a detector which is long and thin (a spherical detector being ideal for this task, about which we shall say more later). The most successful of these trackers is the Soudan II instrument which has layers of drift tubes interspersed with iron and concrete, with a total sensitive mass of about 1000 tons. Most importantly, and unlike similar types of underground instruments, the Soudan instrument has a fully surrounding veto counter which permits tagging of entering and exiting tracks. Such detectors generally have not had the ability to distinguish the sense of direction of tracks, and confused events have resulted in mistaken claims of nucleon decay observation.

The direct counting methods may observe whatever decays leave charged particles and photons, neutrinos escaping. Nearly 50 decay modes have had strong limits placed upon their rates, almost entirely dominated by results from the water Cherenkov instruments. Nonetheless, the tracking style of instruments have yielded important cross checks and should not be dismissed. To be honest though, in the undoubtedly biased eyes of this observer, the confusing results from tracking instruments in the early days of nucleon decay searches and in the resolution of the atmospheric neutrino anomaly have cost time and resources (with the important exception of the Soudan II results). I suggest that for the progress of a field, building the wrong detector can be worse than not building any.

2.5. Neutron Oscillations

The $\Delta B = 2$ transformation of a neutron into an anti-neutron is not forbidden by anything *except* changing the (udd) quarks into anti-quarks. As mentioned above, there are GUT models which contain such transitions (with 6 fermion operators), so the transition is well worth seeking. Two broad search methods have been implemented: examining beams of neutrons from reactors for anti-neutrons, and looking for the explosive results of $n - \bar{n}$ transitions in a nucleus. The latter would result in notable events with typically four or five pions in the final state, a total of twice the nucleon rest mass energy, and little net momentum in the laboratory.

The searches for direct $n - \bar{n}$ transitions in beams from reactors have reached 8.6×10^7 sec[11)], and are encouraged by at least one model which predicts a possible rate in the range of 10^{-8}/sec[7)]. New experiments may be executed with an HIFR neutron source at Oak Ridge National Laboratory. The potential of such avenues of study is well documented in the proceedings of a workshop at ORNL in 1996 [10)].

Interpretation of nucleon decay results in this context is however problematic, as one must estimate the suppression of the transition by the depth of the nuclear potential, and which greatly decreases the decay rate by $\sim 10^{31}$ for the present range of limits! The present limits for $n\bar{n}$ transitions are nearly equivalent from direct nucleon decay searches and neutron oscillation experiments (the PDG gives 1.2×10^8 sec from intranuclear limits[12)]). The relationship is actually $\tau_A = R\tau_{n\bar{n}}^2$, where R is a much discussed nuclear suppression factor of about $R = 2 \times 10^{23}/sec$, with an uncertainty of about a factor of two, or perhaps more if the theory is incorrect. SuperK should reach an equivalent of about 4×10^8 sec for free $n - \bar{n}$ in a few years, so progress will be slow via the intranuclear search. SNO should, however, provide some interesting limits from deuterium.

Another method for seeking $n\bar{n}$ oscillations is via a clever geochemical technique, being pursued at ORNL and also discussed in the proceedings mentioned above[10)]. The idea is to look for technicium (which is not found in geological samples!) in tin ores, and by examining the ratios of the isotopes Tc^{97},Tc^{98}, and Tc^{99} be able to discriminate $\delta B = 2$, $n\bar{n}$ decay. Apparently no progress is being made on proposals for this elegant experiment.

3. Resume' of Searches for Nucleon Decay

The history of being concerned about the possibility of baryon number violation is not as long as one might imagine. Wigner and Stuckelberg annunciated the principle of nucleon number conservation in the 1930's. The first experimental limits were published in the late 1950's and early 1960's, largely by M. Goldhaber and F. Reines and collaborators[8,1)]. It is interesting that these pioneers had little idea of what decay mode to expect (except from simple conservation of angular momentum, energy and charge), as there was no model for decay and nothing was known of nucleon substructure. Their motivation was simply that if nucleon decay was not forbidden it may well occur at some rate, and observing that rate would have to be a key to understanding elementary particles. Largely the physics community ignored such "foolish" (and as we now know, farsighted) inquiries.

Thus, lacking any theoretical guidance of what search level was worthwhile, no funding was forthcoming for direct experiments. Those searches carried out were largely ancillary results from early natural-neutrino detectors and the like. The situation changed dramatically in the late 1970's when the $SU(5)$ model was proposed[4)], which made the definite prediction of a lifetime of $10^{30\pm1.7}$ years for $e^+\pi^o$. This set off a flurry of experimental searches, and by early 1983 the IMB detector had ruled out $SU(5)$.

The searches marched steadily ahead throughout the 1980's and into the 1990's, dominated by IMB and Kamioka, but also by the Kolar Gold Fields, NUSEX, Baksan, Frejus instruments, pushing the limits up to around 10^{33} years for the best modes. A new era began with the operation of the more than order of magnitude larger and more sensitive Super-Kamioka instrument in 1996.

The limits on nucleon decay thus progressed speedily in the early years, marching to somewhat less than 10^{30} years by the mid-70's. The first dedicated large nucleon decay searches of the early '80's drove the limits to near 10^{33} years by the end of the decade. One sees the tyranny of numbers of nucleons in this game. One must have tens of kilotons now as the price of entry: building a more sensitive smaller detector will not suffice. For all modes the search sensitivity goes up linearly with mass for a while, and then turns over to square root of mass times exposure time dependence as background limitation sets in. Much progress has been made as we have gone through three generations of neutrino/pdk detectors. One can make a "Livingston plot" type of extrapolation (dangerous as such are) and guess that 10^{35} years will be reached a decade or so into the next century.

In Figure 1 we present a summary of the present limits on nucleon decay, by mode. A thorough presentation is now available in the PDG summary[12)]. New results are available from the Super-Kamiokande group on $e^+\pi^o$ at 5.0×10^{33} years[13)] and νK^+ at 1.6×10^{33} years[14)], as indicated on the figure. Other modes follow, and it appears that SK is well on track to meet limits estimated in the project proposal, which will take it to 10^{34} years for the best modes and $\sim 10^{33}$ years for the most difficult.

4. The link with Neutrino Oscillations and Neutrino Detectors

We shall not here belabor the point that the probable (implicit) observation of neutrino mass by Super-Kamiokande, with supporting information from Soudan II, MACRO and now K2K, has deep ties to nucleon decay. First there is the link with baryogenesis at Big Bang time[2)]. Because of the possible erasure of baryon asymmetry[3)], it may be possible that neutrino asymmetry carried the "message" to nucleons, as outlined in several models, which require mass and oscillations and perhaps sterile neutrinos[5)].

The mass scale of neutrinos is another tie, since the neutrino masses are so much smaller than the quarks and charged leptons. The "see-saw" suggests that the neutrino mass scale is as far below the quark-lepton scale as the unification mass is above, and this in turn links to nucleon decay. The neutrino mixing matrix (MNS matrix), which we are only beginning to resolve could well be the much needed clue to moving beyond the Standard Model, and hence to nucleon decay predictions. Note that SU(5) required zero mass for the neutrinos, so finding neutrino mass as well as no nucleon decays, doubly kills that simple model.

Not only are the physics of neutrinos and nucleon decay searches deeply intertwined, but so also are the observations in practice, at least on earth. Due to the peaking of the energy spectrum of the inescapable atmospheric neutrinos exactly in the range of nucleon decay products, a nucleon decay detector must *a forteriori* be a good neutrino detector. There is thus a beautiful synergy in these studies, one exploited for many years and probably profitable for many more.

Finally, there is the natural synergy that a giant new nucleon decay search instrument may also have a complimentary function with a neutrino beam from a distant accelerator. Given the power limitations (4-5 megawatts) of traditional ('super-beams') or a neutrino factory, the size of detectors needed for the distances favored for CP violation studies (several thousand kilometers) turns out to be in the ten's of kilotons range (and of course bigger is better). Given the $1/r^2$ spreading of the neutrino beam, a device at an earth diameter distance (13,000 km) would need to be in the megaton class. Unfortunately the machine designers are not keen on a beam pointing so steeply into the earth. However this works out, it would be a pity if the distant detector for a next generation neutrino beam were not also employed for other purposes, most prominently nucleon decay search.

5. Future Baryon Number Violation Searches

The future of nucleon decay searches is not bright without contemplating a significantly new generation of huge instruments. One route is via megaton-scale water

Cherenkov instruments (Hyper-K, TITANIC, UNO and Aqua-RICH). Other routes are being explored with more sensitive instruments of roughly Super-Kamiokande mass (Super-ICARUS, LANNDD, SCIPIO). We review these briefly below. Note that these are really all just ideas at this point, so what is said here will probably soon be out of date. It does give the appropriate flavor that there is a good deal of activity and the plans are evolving swiftly. There is lots of room for innovation.

The large water Cherenkov detectors have proved their capability to be scaled to larger dimensions cost effectively, so it is natural to think in this line. However, SuperK is already not so far from background for even $e^+\pi^o$, estimated to be at the rate of 1 event per 20 years livetime. Other modes are harder. For the kaon containing modes one is at double jeopardy, for both the signal detection is inefficient (the kaon being borne below Cherenkov threshold) and the search windows admit of necessity relatively many neutrino generated interactions. For this situation there is hope to make progress on these SUSY favored modes by building instruments with better sensitivity to kaons (for detection) and recoil nucleons (to eliminate background).

5.1. Hyper-Kamiokande

A plan for a scaled up version of Super-Kamiokande is being formulated (see NOON2000 talk by Kenzo Nakamura[15)]) by some of the Japanese collaborators on the 50 kiloton Super-Kamiokande instrument. The goal is to reach about twenty times the mass of SK, with a fiducial volume of greater than 600 kilotons. Requiring a sensitivity reaching down to 6 MeV in order to capture the tagging nuclear gamma followed by kaon decay, means that not much can be saved over the 40% photomultiplier coverage of SK. There are two versions. The main design consists a linear series of 8 compartments in a 400 m long, 70 m high by 50 m wide tunnel. An alternative (old idea called DONUT by Koshiba), would use a toroidal geometry.

The cavity size determined by rock pressure due to overburden (1000 m), and the effective attenuation length for Cherenkov photons in water (> 70 m), set the maximum dimensions for individual tanks. While multiple detectors are expensive compared to one huge tank, there is the advantage that the instrument can be assembled incrementally and serviced a segment at a time.

This instrument would reach to around 10^{35} years for $e^+\pi^o$ and 10^{34} years for $K^+\nu$. It would also have uses for extending all the SK physics measurements and searches (atmospheric neutrinos, supernova watch, solar neutrino temporal variations, searches for astrophysical neutrinos from point sources, associations with GRBs, various exotic particles, WIMP annihilation neutrinos, etc.) at least another order of magnitude. This instrument may also work well as a long-baseline detector for a super-beam from the 50 GeV JHF (10^{21} POT per year), 295 km away in Japan. They claim that using $\sim 1\ GeV$ muon neutrinos, δm^2 can be measured to around 10% using a wide band beam for one year, and that with a narrow-band beam over five years or so θ_{23} and

θ_{13} can be measured with improved systematics over competing experiments. They also argue (following A. Konaka of TRIUMF) that with a low energy neutrino beam one may get to sensitivities of order 15% asymmetry in detection of CP violation (via $\nu_\mu \rightarrow \nu_e$, if $sin^2 2\theta_{13} \simeq 0.01$, and running with 4 MW for 4 yrs).

This is an impressive and ambitious plan, but also very expensive. The cost is perhaps a good match to the JHF expenditures, and there is strong commitment to neutrino physics in Japan, so this author guesses it has a fair chance to proceed.

5.2. TITANIC

Another approach being promoted by some Japanese SuperK collaborators (see talk by Y. Suzuki at NOON2000[17)]) revives and old DUMAND idea, but with a new twist: make a huge detector in a tank in the ocean, but submerge it to only 100 n overburden. The motivation to not pay the price for instrumentation for great depth, and utilize present SK style PMTs (which are tested to ten atmospheres of pressure, or about 100 m depth). At greater depths either costly and difficult to handle pressure housings will be required, or one will need new PMTs which are their own housings (the original DUMAND intention, but development still needed 25 years later). The TITANIC proponents argue that one may cost effectively achieve a megaton mass with little or no development time, and that such a detctor can be expanded as needed. The shallow depth has disadvantages of high cosmic ray rates (significant dead time, no possibility to study solar neutrinos) and may not be effective for the $K^+\nu$ mode of nucleon decay.

Still the plan has the attraction that the mass is within the scale of supertankers already built and which can be handled by existing ocean technology experience (design of oil platforms, shipyards, tugs, anchoring). Another advantage of the ocean is that filling with fresh-water makes such a device buoyant, so it can be easily brought to the surface for retrieval and repair. There is discussion of placing such an instrument in shallow and sheltered near-coastal waters off Japan.

5.3. UNO

There is a group in the U.S. which has been considering options for a new generation water Cherenkov detector to be placed in deep underground in North America, and presumably to operate in concert with a neutrino factory at Fermilab or Brookhaven. The three major venues under consideration are the old Homestake Mine in South Dakota (home of the long running Davis solar neutrino experiment), for which there is some initiative to create a national underground laboratory since the gold mining activities have ceased there; the WIPP low-level radioactive waste storage site operated by the US DOE in Southeastern New Mexico; and a new site to be bored under the San Jacinto Mountains in Southern California. The Homestake mine is deep (6000-7000 mwe), has a long history of research, many small tunnels, and is hard rock (which makes large cavities expensive). The WIPP site is not so deep (1500 mwe, not much less than Kamioka), and already has major infrastructure in place. There is a location in Chino Canyon near Palm Springs where a horizontal (drivable, as opposed to elevators at the other locations) tunnel can achieve substantial overburden (5000 mwe) with a relatively small amount of excavation. This site is also much closer to a major metropolitan area and support facilities than the other two options being studied. Once can envisage an UNO detector in any of these three locations, but certainly the least expensive excavation would be at WIPP because digging in salt is very much easier than mining hard rock.

In order to keep costs within bounds the UNO detector is conceived to consist of three tanks of $(60\ m)^3$ dimensions, with phototube coverage of 10% on the outer tanks (for cost minimization) and 40% (as in SK) on the central tank, for total of 56,000 PMTs of the SK type (five times as many as SK). The ensemble would have 440 kilotons fiducial mass for nucleon decay studies. The cost, without contingencies, is estimated at US$500 M, comparable with costs of major new collider detectors. For nucleon decay this instrument would reach the goal of 10^{35} years for $e^+\pi^o$, and about an order of magnitude less for kaon modes. For purposes of cost the detector plan is somewhat smaller and with less resolution than the Hyper-K design, but otherwise is similar in physics potential.

Much of the UNO planning revolves around tuning the detector to function with a neutrino factory, as this seems the only likely road to funding success in the contemporary US political climate. For this purpose it is possible that a muon tracker would be added, and either internal or external magnets provided.

5.4. Megaton Aqua-RICH

Tom Ypsilantis and collaborators developed the RICH (ring imaging Cherenkov) technique for use at accelerator experiments, and have proposed utilizing a water target version near Grand Sasso for detecting a neutrino beam from CERN for the purpose of studying neutrino oscillations[19)]. We have taken the idea a bit further, in contemplating a full megaton (125 m diameter) spherical realization. We estimate that such a device would employ about twice the number of phototubes as SuperK, but of the new hybrid variety, with most tubes on a concentric inner sphere, where rings are imaged from a mirrored outer spherical surface. This device would not only take nucleon decay searches about one order of magnitude further, but would also have great utility for neutrino oscillations studies, solar neutrino temporal variation monitoring, supernova monitoring out to 2 Mpc, and all the other experiments carried out with IMB/Kamioka/SuperK carried about an order of magnitude further. The detector being of new geometry for this scale would need some development, and much computer study, though there seems to be no technical hurdles aside from improved

PMTs, of the 'hybrid' variety, which are already inder active testing in smaller sizes for LHC.

The location of such an instrument is not obvious. It is too large to fit in the Gran Sasso laboratory. Near surface deployment (as in a quarry) is environmentally problematic in Italy, and there are the problems of high cosmic ray muon rate if at the surface. A better option might be to build such a detector in the ocean, as with the TITANIC notion. Facing the pressure problem of deep submersion would permit co-location with one of the large high energy neutrino telescopes (NESTOR, ANTARES, or NEMO) under study/construction in the Mediterranean.

With the death of Tom Ypsilantis in late 2000, it is not clear who will carry on this interesting alternative, which in my view is surely worthy of more effort.

5.5. Super-ICARUS

ICARUS is a liquid argon time projection chamber. For years we have been seeing wonderfully detailed pictures of events made with ICARUS protoypes. At last the 600 ton ICARUS prototyype is now working in the laboratory, soon to be transported to the Gran Sasso National Lab in Italy. Thus we are close to having a demonstrated prototype for a much larger, few kiloton instrument, hopefully to be approved for use with the CERN long baseline neutrino beam (along with OPERA) within the next few years.

Such a detector is even more flexible in its physics scope than are the water Cherenkov detectors, since one has the track resolution capability of a heavy liquid bubble chamber. The spatial and energy resolution are largely limited by the number of wires affordable, practically a few MeV. It is by nature nearly isotropic in response as well. Such a device would be a magnificent nucleon decay detector, if of sufficient mass to be competitive.

Andre Rubbia and collaborators have made a case for a Super-ICARUS of 30 kilotons mass. As with Aqua-RICH, there is a problem of where to put such a monster, and they have considered near surface placement, with attendant physics penalties. Note that such a device scales nicely with costs rising probably much more slowly than linear in the volume.

5.6. LANNDD

In the U.S. we have considered a hypothetical 90 kiloton liquid argon detector, named LANNDD for now, to be placed in the WIPP facility. The idea is for a single large right-cylindrical dewar, similar in size to SuperK, but segmented by vertical wire planes. It would be surrounded by a magnet, with vertical solenoidal field. This device would have all the virtues of the aforementioned detectors (but less mass of course than the 6-10 times large water Cherenkov instruments). While not being notably better than SuperK for $e^+\pi^o$, this instrument will be superior to all others for kaon modes. In fact, ICARUS studies have indicated that the kaon modes may be taken to 10^{35} years with this technique before encountering atmospheric neutrino background limitations.

Another major attraction of this scheme is that when employed with a neutrino factory and having a magnetic field, such an instrument can detect the sign of both muons and electrons (the latter over more limited energy range), unlike any other scheme presented so far. This allows for much more significant tests of neutrino oscillations in a long baseline experiment. Costs for this facility are probably comparable to or less than UNO. Such an instrument may be funded and built along with a neutrino factory if it has the capability to resolve the final states needed. That situation will continue to evolve for the next few years as we get more data from Super-K, K2K, SNO, Mini-BOONE, KamLAND, Borexino, MINOS, OPERA and ICARUS.

5.7. SCIPIO

An entirely different style of massive nucleon decay detector has been put forward by Bob Svoboda, though the plan is not much developed as yet. The idea is to scale up KamLAND, a 1 kiloton liquid scintillation detector, to SuperK dimensions. Indeed it would be possible to rennovate SuperK itself for such a makeover into a 50 kiloton liquid scintillation device. Here the goal would not be primarily solar or reactor neutrinos, but sharp resolution of kaon modes of nucleon decay and compatibly strong rejection of atmospheric neutrino backgrounds. One largely gives up the geometry of track reconstruction, but gains in energy resolution. Some geometrical infromation may be gotten by using a less heavily doped scintillator, in which Cherenkov rings are detectable (as in LSND). Much study is needed, but the price could be substantially less than that of the other proposals discussed here.

6. Summary and conclusions

The search for nucleon decay moves steadily forward, with no hints of detection having appeared as yet. The motivation for such searches has increased with time, but testable predictions from models have not converged. SuperK will take most modes to the 10^{33} year range, but a new huge instrument will be needed to make significant progress beyond, up to the 10^{35} year range where one begins to push models. I argue that we need a detector with not only more mass but improved resolution for final states and particle identification. Much discussion and new results soon forthcoming on neutrino oscillations, will hopefully lead in the next decade to construction of one or more next generation nucleon decay search and neutrino study intruments. The strong cross-fertilization in joint use of such a facility for long-baseline experiments, as well as the many other potentially high payoff studies and searched, will surely lead to several decades of exciting physics.

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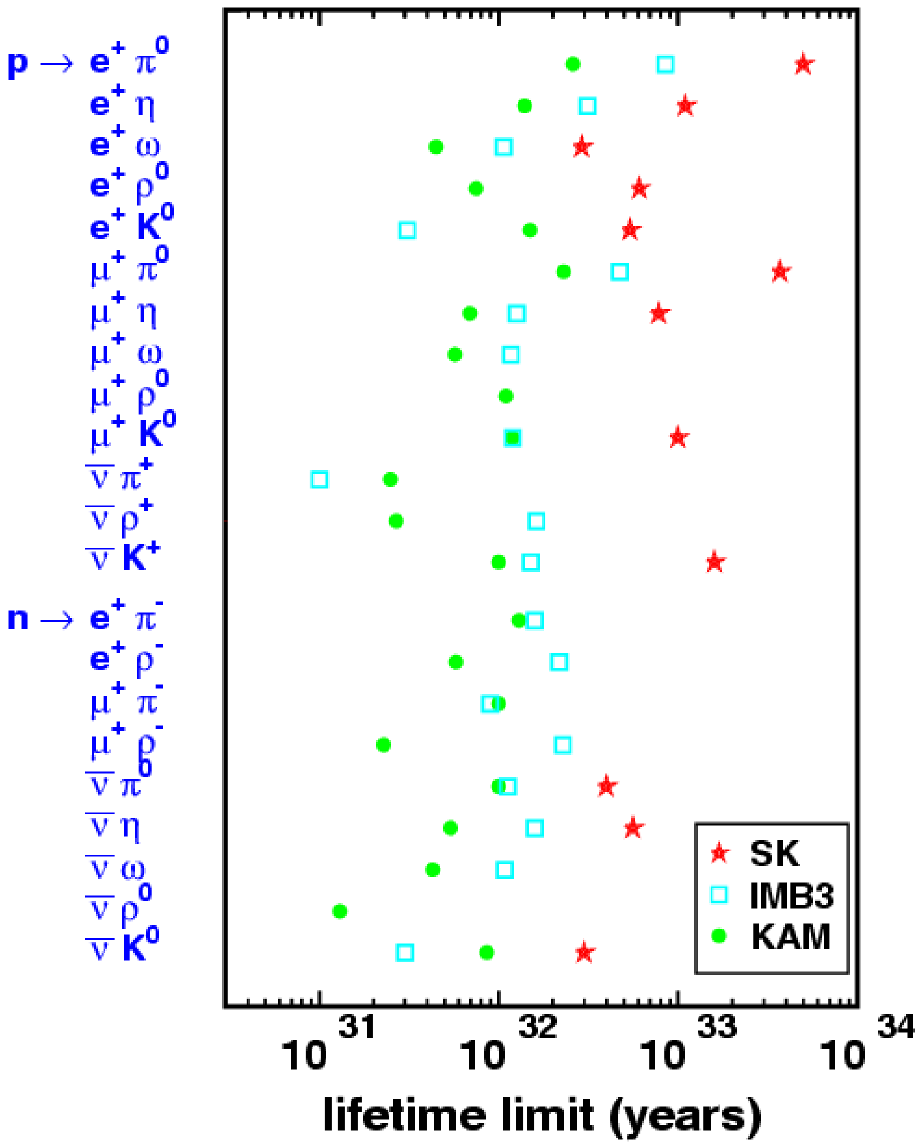

Figure 1: Graph of nucleon decay limits for 22 modes as measured with IMB, Kamiokande and Super-Kamiokande. Updated limits from 1289 days of SK data are preliminary as of June 2001.