NEUTRINO PROGRAM IN UNITED STATES

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ABSTRACT

In my remarks I would like to describe briefly the current status of the neutrino program in US and some of the options being discussed for the future. I will also comment about some of the peripheral activities of relevance to neutrino physics. More specifically, my remarks will address four general topics: the recent Department of Energy Facilities Plan report, the ongoing workshop on neutrino physics sponsored by four Divisions of the American Physical Society (APS), the status of the current neutrino program in US, and future possibilities being discussed.

Starting with the first topic, the Office of Science in the Department of Energy has recently produced a 20 year plan enumerating facilities with a projected cost greater than $50M each, which are essential to addressing the most pressing issues in the broad spectrum of sciences supported by the Office of Science. Five facilities of relevance to particle physics were listed: SNAP (next generation supernova study), Linear Collider, BTeV, neutrino superbeam and a double beta decay, the last one being viewed as a joint particle physics / nuclear physics project. The time scale and priority were determined by both the importance of physics a given facility would address but also by its current state of readiness for construction. The superbeam project was listed as tied for 21st (among all 28 facilities recommended) and as coming in the second half of the period covered. This report used for input reports prepared for this purpose by a number of advisory bodies representing the communities served by the Office of Science.

I would like to give my own interpretation of the implication of this report on the future of neutrino physics in US. This is obviously a very personal view and should not be taken as an official position of any formal policy making body. First of all, it is very positive that neutrino physics was enumerated as one of the frontier fields in fundamental science (and one of four in particle physics) and it was recognized that new ambitious facilities will be necessary to purse this physics adequately. Secondly, I do not attach too much significance to its relative ranking nor to the projected time scale. Long term plans are notoriously unreliable in their details, especially in basic science where new discoveries can drastically alter our perceptions. Thus, for example, the developments on the Linear Collider will have a major impact on the execution of the other, much smaller facilities.

Also, external developments, like the evolution of the war on terrorism and the overall economic situation will have a major impact on what gets built and on what time scale. Finally, it is important to emphasize that this plan addresses only the DOE

program; the other major player in US particle physics, the National Science Foundation, has recently increased its participation in construction and support of major new experimental facilities (e.g., LIGO, IceCube, KOPIO and MECO experiments at Brookhaven AGS). Thus there is a strong possibility that the NSF may play an important role in either providing some support for these facilities or initiating different ones.

The superbeam project is listed in rather generic terms, without discussing the details of its possible execution or its location. It takes for granted, however, that a new intense proton source will be required to produce a superbeam with properties required to address eventually the key questions in the field.

I want to turn next to the second topic, i.e., APS sponsored study. There is a growing realization in the US physics community that neutrino physics addresses some of the most fundamental questions in particle physics and has a good chance of providing answers that might influence strongly our view of the universe. Hence it is felt that the US should be a major player in this area and one should define the optimum course of action in this field.

It was in response to this situation that the High Energy Physics Advisory Panel (HEPAP), a body formed by the DOE and NSF to provide them advice about particle physics priorities, suggested formation a neutrino study group to identify most urgent issues in that field and map out a possible road map how a program might be structured.

After some discussion in HEPAP and community at large it was decided that it would be best if such a study were community-driven rather than organized even indirectly by funding agencies. Both the Director of the Office of Science and Head of the Physics Division in NSF expressed strong interest in having such a study performed on a fast time scale and are giving it strong support.

The organizational details have now been worked out and the study has been formally launched. The study is sponsored by the four Divisions of the APS which have potential interest in neutrino physics and relevant facilities: Division of Nuclear Physics, Division of Particles and Fields, Division of Astrophysics and Division of Physics of Beams. Stuart Freedman (DNP) and Boris Kayser (DPF) are co-chairs of this workshop. The co-chairs are assisted by an Organizing Committee composed of individuals representing all the different activities being pursued. The study is organized around 6 working groups, defined by different experimental approaches to various issues. The topics of the working groups are:

Solar and atmospheric neutrino experiments
Reactor neutrino experiments
Superbeam experiments and development
Neutrino factory and beta beam experiments and development
Neutrinoless double beta decay and direct searches for neutrino mass
What cosmology/astrophysics and neutrino physics can teach each other

Each group is headed by an experimentalist and a theorist and periodic meetings of different groups are being organized. In addition, joint meetings of the organizing

committee and the 12 conveners will be held regularly. The goal is to finish the study in August 2004, in time to influence the agency budgetary decisions for FY2006, starting on October 1, 2005. The report will describe potential activities, with a technical and scientific evaluation of each but without ordering them by priority. The report will also outline a possible road map for the field. The organizers also plan to produce in parallel a booklet for a general audience, aimed partly at the policy makers and officials in the relevant government bodies, which will convey the excitement of neutrino physics and outline the key questions in this area.

It is important to stress that even though this is a US study, input from our colleagues abroad is very much welcome and it is hoped that a number of them will participate in some of the workshop meetings. The road map will obviously take into account the current and proposed activities abroad and thus we are anxious to stay up to date on European and Japanese plans.

I want to turn next to a description of the current US activities in the neutrino area and their status. The MiniBooNE experiment at Fermilab aims to provide a definite resolution of the LSND puzzle, expecting sensitivity over the full LSND region still allowed by all the other experiments at the 5 sigma level. The experiment is currently in the data taking phase and apparatus is working as expected. The number of protons per unit time delivered by the Fermilab accelerator complex is however a factor of 2-3 lower than desired but a program has been initiated to rectify that. Up to now about 20% of the desired neutrino data have been accumulated.

The other neutrino experiment at Fermilab is MINOS which will utilize the new NuMI neutrino beam line, currently in the final stages of construction. The 5.4 kt far detector has been completed in the summer of 2003 and is currently taking atmospheric neutrino and cosmic ray muon data. The assembly and installation of the beam technical components and the near detector are due to be completed late this year and one is expecting first protons on target in December of 2004. The experiment aims for precise determination of the oscillation parameters in the atmospheric region, demonstration of the oscillation phenomena through observation of a dip in the energy spectrum of the ν_μ CC events and improvement on the limit for $\nu_\mu \rightarrow \nu_e$ and $\nu_\mu \rightarrow \nu_{sterile}$ transitions (or their detection). The current plans call for a run of 4-5 years with neutrinos. The sensitivity of the experiment is statistically limited and thus a great deal of effort is being put in to increase the currently available proton intensity from the Main Injector.

Another effort is planning and R&D activities aimed at future short baseline experiments designed to study the details of neutrino interactions in the low energy regime. These experiments would utilize either the NuMI beam line (MINERνA) or the booster neutrino beam line currently used for MiniBooNE (FINeSSE).

The other current activities being pursued are in the realm of next generation neutrinoless double beta decay experiments. These efforts are still in the R&D phase. One of the experiments, EXO, is driven mainly by the high energy physics community and is focused on a xenon TPC detector with simultaneous identification of the barium

nucleus resulting from the xenon decay. The other effort in this area is the Majorana experiment, using enriched germanium as the source and the detector, with participants mainly from the nuclear physics community.

One other activity of potential future relevance to the neutrino community is the planning for a possible future underground laboratory in United States. There is an active effort to convert part of the Homestake mine in South Dakota, recently closed as an active mine, into a multidisciplinary underground laboratory. Homestake mine, which was the home of the pioneering solar neutrino experiment by Ray Davis and his collaborators, extends to a depth of about 7000 mwe and thus could provide the most cosmic ray free environment for future experiments. The state of South Dakota is actively supporting development of a proposal for such a facility to be submitted to the National Science Foundation.

I want finally to say few words about possible next generation neutrino experiments in United States limiting myself to the area of long baseline experiments looking at the mass scale relevant to atmospheric oscillation domain. My comments will be flavored somewhat by my own personal views so they should be read with that in mind. There are basically two quite different approaches being advocated. The first one might be called gradualist approach and is based on several, more modest experiments which can probe different aspects of the neutrino mixing matrix but together over time can give a coherent picture of the overall situation. The second approach involves one ambitious experiment, which in principle at least, has the ability to unravel the whole neutrino situation in the domain of the MNS matrix.

The first of these approaches might involve a relatively modest start with an offaxis neutrino experiment in the NuMI beamline and a reactor experiment. These experiments, together with the JPARC first phase, would push the sensitivity to $\nu_\mu \rightarrow \nu_e$ transitions by more than an order of magnitude and could well give us some preliminary information on mass hierarchy and CP violation. In the second phase, which would require higher proton intensity, the options would be antineutrino run in NuMI offaxis, another detector at a transverse distance corresponding to the second oscillation maximum in NuMI beam line and/or JPARC PhaseII. Potential evolution and sensitivity of such an approach have been discussed at this Conference by Gary Feldman.

The second approach is the one advocated by the Brookhaven group even though the overall method can be discussed in generic terms, without specifying either the source of the protons or the location of the detector. The basic idea is to send a wide band neutrino beam, with energy covering the 0.5 – 5 GeV range to a detector some 1500 – 4000 km away whose energy resolution and background suppression are good enough so that one can map out the first three oscillation peaks for the ν_μ disappearance and ν_e appearance. This approach would require a proton beam in the 1Mw or higher range and a detector of at least 500 kt mass. If nature has chosen normal mass hierarchy, five years of neutrino running should be sufficient to measure all MNS parameters and determine mass hierarchy provided that $\sin^2 2\theta_{13}$ is 0.01 or greater. The neutrino run would not be able to

distinguish between lower values of $\sin^2 2\theta_{13}$ or inverted mass hierarchy. Thus additional antineutrino running would be required in that eventuality.

To date, significantly more work has been done on the first approach, with detailed simulations of signal efficiency and background levels, based on a well defined and realistic detector (low Z, fine granularity calorimeter) and NuMI beam. An R&D proposal has been submitted to NSF and a proposal based on the work done to date will be submitted to Fermilab in March, 2004. The only new major expenditure is the construction of the offaxis detector, estimated to cost in the $150-200M range. A new proton source would only be required for the second phase, on a time scale of about a decade.

In a related area, a study group has been formed to look at possible reactor experiments and significant work is being done within that framework. A White Paper on reactor experiments has been produced recently. Specific proposals are expected on a time scale of a year or so.

The wide band beam approach is still at a much earlier stage. There are specific plans how one could upgrade the Brookhaven AGS to the required intensity but many detector issues remain to be solved. The original option was a water Cherenkov counter but at this time it is still not understood how sufficient background rejection could be obtained with this technique. The latest calculations suggest that an additional factor of 3-5 is needed over what one can do today with current hardware, like Super-K, and current software algorithms. The other alternative would be a liquid argon detector with a mass of about 200 kt. This technique would probably provide adequate resolution and background rejection but large extrapolation in scale required from what has been achieved to date raises significant issues regarding technical feasibility, time scale, and total cost. One of the issues that needs to be explored in detail is what kind of a detector might optimize both long baseline neutrino studies and searches for the proton decay, since a detector of this magnitude would probably need to have wider application than a single experiment.

The Freedman-Kayser study should provide a more detailed comparative evaluation of these two approaches. There are clearly many issues involved that extend beyond technical feasibility, physics potential and cost, like relationship to potential programs abroad, likely time scale, and, maybe most importantly, the probability of each actually happening in the current political and budgetary climate.