Report of the joint long- and short-baseline oscillation working groups at the Project X Physics Study 2012

July 6, 2012

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1 Short-baseline physics

Short-baseline physics in the context of this report is to be understood as all flavor conversion/disappearance phenomena which take place at L/E values which are considerably smaller than those typically associated with the mass-squared splittings of atmospheric and solar neutrino oscillations. In the last 18 months, this area has seen a greatly increased scientific interest and as a result a number of workshops and documents have been produced, notably the sterile neutrino white paper [1] and the report of the short baseline focus group at Fermilab [2]. Therefore, it is sufficient to just summarize the hints for oscillations at $L/E \sim 1\,\mathrm{m}\,\mathrm{MeV}^{-1}$ and refer the reader to the aforementioned reports for more details.

There are the LSND and now MiniBooNE results which indicate a flavor conversion of $\bar{\nu}_\mu$ to $\bar{\nu}_e$ at the level of about 0.003. At the same time MiniBooNE has seen a low energy excess of events which may or may not be related to their primary signal and LSND.

The results from calibrations of low energy radio-chemical solar neutrino experiments using the reaction $\mathrm{Ga}+\nu_e \rightarrow \mathrm{Ge}+e^-$ based on artificial, mono-energetic neutrino sources (^{51}Cr and ^{37}Ar) seem to show a deficit in count rate of about 25% with an error bar of about 10%.

The so-called reactor anomaly [3] indicates a 6% deficit of $\bar{\nu}_e$ emitted from nuclear reactors at baselines less than 100 m. Interestingly, this is entirely based on the re-analysis of existing data; the deficit is caused by three independent effects which all tend to increase the expected neutrino event rate. There have been two re-evaluations of reactor anti-neutrino fluxes [4,5]; both see an increase of flux by about 3%. The neutron lifetime decreased from 887–899 s to 885.7 s [6] and thus the inverse β-decay cross section increased by a corresponding amount. The contribution from long-lived isotopes to the neutrino spectrum was previously neglected and enhances the neutrino flux at low energies.

All these hints have a statistical significance around $3\,\sigma$ and may be caused by one or more sterile neutrinos with a mass of roughly 1 eV.

There is also a somewhat more ambiguous indication from cosmology that there are more relativistic degrees of freedom in the early Universe than the Standard Model allows. At the same time, large scale structure data disfavor the existence of a forth neutrino with a mass in the eV range.

It is felt that to resolve these anomalies a new series of experiments is necessary and warranted. More specifically, the short baseline focus group recommends that Fermilab pursue accelerator-based experiments which can definitively address these anomalies on a short timescale. In conjunction with the global efforts on sterile neutrinos, many of which do not rely on a large accelerator infrastructure, it seems plausible and highly likely that, by the time Project X starts its physics program, there will have been either a discovery of sterile neutrinos, or more generally new physics at short baselines, or stringent new limits which significantly contradict the current indications. In the latter case, there will be no short-baseline program at FNAL in the Project X era. In the case of an unambiguous discovery, the task of Project X would be to deliver high intensities at energies around 8 GeV to allow detailed studies of the newly discovered sterile neutrino(s), or whatever new physics effect is behind the short-baseline anomalies.

One proposal to resolve the LSND puzzle is OscSNS [7], which aims to repeat the LSND measurement while avoiding the shortcomings of LSND. The idea is to build a liquid scintillator detector at a powerful spallation neutron source to exploit pion decay-at-rest. A high beam power of around one MW and a short duty cycle of less than 10^{-5} are key to improve the performance with respect to LSND. OscSNS is the most direct test of LSND conceivable and thus is entirely model independent and could be central to resolving the short baseline anomalies. In principle, if OscSNS can not take place at the SNS at Oak Ridge, Project X could be a suitable driver for a similar source at Fermilab. However, this seems to be hardly cost effective and probably would happen too late to impact our understanding of the short baseline anomalies. Also, OscSNS is very difficult to upgrade to a point where it could deliver precision measurements of the parameters of the sterile neutrinos.

Several proposals exist, both a Fermilab and CERN, to use pion decay-in-flight beams, as MiniBooNE did; the crucial difference to MiniBooNE would be the use of a near detector and potentially the use of LAr TPCs instead of scintillator detectors. While these new proposals would constitute a significant step beyond what MiniBooNE has done, especially in terms of systematics control, it remains to be proven that a beam which has a 1% level contamination of ν_e can be used to perform a high precision study of a sub-percent ν_e appearance effect. In particular, it should be pointed out that many of these proposals involve near and far detectors of very different sizes and/or geometrical acceptance and thus, cancellations of systematics will be far from perfect. Therefore, it is not obvious that these experiments can take full advantage of the beam intensities Project X will deliver.

The other proposed technology is to use a stored muon beam, called νSTORM [8]. Here, the neutrinos are produced by the purely leptonic, and therefore well understood, decay of muons, and thus, the neutrino flux can be known with very high, sub-percent, precision. The signals are wrong-sign muons which can be identified quite easily in a magnetized iron detector. The precise knowledge of the neutrino flux and the expected very low backgrounds for the wrong-sign muon search allow one to reduce systematic effects to a negligible level, hence permitting a precise measurements of the new physics that may be behind the short-baseline anomalies. The absence of large systematic effects would allow one to take full advantage of the high intensity offered by Project X, particularly around 8 GeV, which would enhance the luminosity by a large factor. The νSTORM concept is still in its early stages. One still needs to demonstrate that the technical design for the target/capture region can provide the beam necessary to achieve the physics goals, and possible backgrounds from meson decays are yet to be fully understood, especially at low energy.

2 Long-baseline physics

With the discovery of a large value for θ_{13}, the physics case for the next generation of long-baseline oscillation experiments has grown considerably stronger and one of the major uncertainties on the expected performance has been removed. The remaining questions are: the value of the leptonic CP phase and the quest for CP violation; the mass hierarchy; whether θ_{23} is maximal and if not, whether it is larger or smaller than $\pi/4$; and of course, the search for new physics beyond the the three active neutrinos paradigm. Based on our current, incomplete understanding of the origin of neutrino mass and the observed flavor structure in general it is very hard to rank these question in their relative importance, but with the large value of θ_{13} it is feasible to design and build a long-baseline facility which can address all three questions with high precision and significance. Therefore, the question of relative importance can be avoided.

The error on θ_{13} will keep decreasing as the reactor measurements are refined and Daya Bay is expected to yield a precision which only would be surpassed by a neutrino factory. It is an important test of the three flavor oscillation model to see whether the value extracted from disappearance at reactors matches that from appearance in beams.

A combination of the existing experiments, T2K, NOνA and reactor data, allows to obtain a first glimpse on the mass hierarchy and with extended running and for favorable CP phases a $5\,\sigma$ determination is possible. Also, new atmospheric neutrino experiments like PINGU, ICAL at INO and Hyper-K have, in principle, some sensitivity to the mass hierarchy and the actual level of significance strongly depends on the obtainable angular and energy resolution for the incoming neutrino. There are also plans for a dedicated experiment, called Daya Bay 2, which would not rely on matter effects but aims at measuring the interference of the two mass squared differences at a distance of about 60 km from a nuclear reactor. It seems likely that global fits will be able to provide a 3-5 σ determination of the mass hierarchy before the end of the next decade. I should be noted, that nonetheless a direct and precise method to test matter effects and to determine the mass hierarchy from a single measurement would be valuable even in this case.

One of the most commonly used frameworks to discuss physics beyond oscillations are so-called non-standard interactions (NSI). They can arise in many different models and their phenomenology is easy to capture in a model-independent way. For the measurement of NSI, the fact that θ_{13} is large means that interference of standard oscillation amplitudes proportional to $\sin 2\theta_{13}$ with NSI effects can enhance sensitivity substantially. If NSI are present, the extraction of the mass hierarchy form global fits is not likely to yield the correct result. Note, NSI are a straightforward mechanism to induce a difference between the reactor and beam measurements of θ_{13}. Longer baselines generally have more sensitivity to NSI and also allow a better separation of standard oscillation and NSI.

Given the likely status of the mass hierarchy measurement by the time Project X becomes active, the other very central physics goal is a measurement of the leptonic CP phase and potentially the discovery of CP violation in the lepton sector. It is important to distinguish these two goals – with large θ_{13} a measurement of the CP phase at a predetermined level of precision can be virtually guaranteed, whereas CP violation may or may not be present in the lepton sector. Therefore, we will focus on the measurement of the CP phase and regard the sensitivity towards CP violation as secondary[1]. A determination of the CP phase requires to measure any two out of the following four transitions: $\nu_e \to \nu_\mu$, $\bar{\nu}_e \to \bar{\nu}_\mu$, $\nu_\mu \to \nu_e$, $\bar{\nu}_\mu \to \bar{\nu}_e$. However, due to the long baselines, there always will be also matter effects which yield a contribution to the CP asymmetries as well; it is necessary to separate this contribution from the genuine CP violation in the mixing matrix. This separation

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[1] This is an operational statement, which does not imply that CP violation is less interesting, just, that in practice one will have to measure the phase and then one knows whether CP is violated or not.

is greatly facilitated by exploiting L/E information, ideally spanning a wide enough L/E interval so that more than one node of the oscillation can be resolved. This requirement, in combination with limitations of neutrino sources and detectors translates into the need for baselines longer than 1,000 km [9–11]. This is also clearly borne out in the discussion of the LBNE reconfiguration – shorter baselines like those available in the existing NuMI beamline, require generally a larger exposure to reach the same parametric CP sensitivity, in absence of external information.

For superbeam experiments, the control of systematic errors will be a major issue, since neither the detection cross sections nor beam fluxes are known within the required precision and thus near detectors and together with hadron production data will play an important role. However, this alone will not be sufficient to obtain per cent level systematics, since the beam at the near detector is composed mostly of ν_μ and hence a measurement of the ν_e cross section is not possible, but in the far detector the signal are ν_e, see *e.g.* [12]. Unfortunately, there are no strong theory constraints on the ratio of muon to electron neutrino cross sections either [13]. Here, a facility like νSTORM maybe helpful.

In figure 1, a comparison of the CP precision for various facilities, as explained in the legend, is shown. Clearly, the neutrino factory (blue line, labeled LENF) is the only facility which approaches the CKM precision and has the potential to even go beyond that. For the superbeams, 2020, LBNE, LBNO and T2HK we note that they span a very wide range of precision, which demonstrates the crucial importance of achieving sufficient statistics. Statistics, or the number of events is determined by the product of beam power, detector mass and running time and each of these ingredients can vary easily within an order of magnitude. LBNO has recently submitted an expression of interest [15] to CERN which outlines a much smaller detector and lower beam power which would put its CP precision somewhere close to any of the reconfigured LBNE options. Obtaining a sufficient number of events is crucial and clearly, here Project X can help with increasing the beam power at 60 GeV. The sensitivity of these results to the assumptions made about systematics is not shown in this plot – but a clear difference does exist, and T2HK exhibits a very strong sensitivity to the assumed level of systematics [14] and thus is significantly more at risk of running into a systematics limitation. Both LBNE and LBNO due to their long baselines and resultant wide L/E coverage are quite safe from systematics [14]. It would be prudent to study this question also for the NuMI based options for the LBNE reconfiguration, since they may suffer from their narrow L/E coverage. Note, at the current stage all these experiments have to rely on assumptions about their systematics. In any comparison as presented in figure 1 the relative performance can vary greatly depending on these assumptions. In the end, *both* sufficient statistics combined with small systematics will be required to perform a precise measurement of the CP phase.

Since exposure is so crucial to the performance of all facilities, it should be noted that a neutrino factory with 1/40th of its design exposure has a very similar physics performance as LBNE with 10 kt and 700 kW. This low luminosity LENF could naturally evolve from νSTORM and a detector located anywhere between 1,300 and 2,000 km from Fermilab. At those low luminosities, no muon cooling is needed and a proton beam of 700 kW at 120 GeV can be used as a driver and a 10-15 kt detector running for 2×10^8 s would be sufficient. In this scenario, the increased beam power at 8 GeV provided by Project X would allow

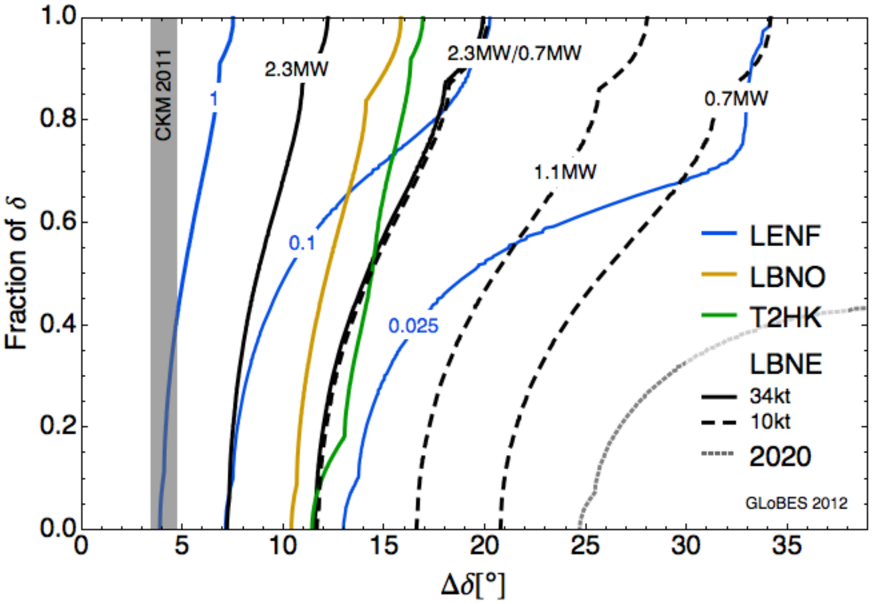

Figure 1: Fraction of values of the CP phase, δ, for which a given $1\,\sigma$ precision $\Delta\delta$ can be achieved. The various lines are for different setups as indicated in the legend. The vertical gray shaded area, labeled "CKM 2011", indicates the current errors on the CP phase in the CKM matrix. This calculation includes near detectors and assumes consistent flux and cross section uncertainties across different setups. The setups are: LENF – a 10 GeV neutrino factory with 1.4×10^{22} useful muon decays, which corresponds to 4 MW proton beam power for 10^8 s, 2,000 km baseline and a 100 kt magnetized iron detector; LBNO – uses a 100 kt LAr detector at a baseline of 2,300 km and 10^{22} pot at 50 GeV, which translates into about 800 kW of beam power for 10^8 s; T2HK – a 560 kt water Cerenkov detector at 295 km using a 1.66 MW beam for 5×10^7 s, which is equivalent to 1.2×10^8 s at 700 kW; LBNE – using LAr detectors of either 10 kt or 34 kt at a distance of 1,300 km with different beam powers as indicated in the legend for 2×10^8 s; 2020 – results obtain from a combined fit to nominal runs of T2K, NOνA and Daya Bay. All detector masses are fiducial. Plot courtesy P. Coloma [14].

to boost the luminosity of the neutrino factory by a large factor and thus to eventually outperform any superbeam.

3 Summary

For the short-baseline program, Project X most likely will play a role after a discovery has been made and in that case, the goal would be a precise measurement of the parameters of the newly discovered physics. If there is no discovery in the short-baseline program prior to Project X, it is doubtful that this program would be pursued in the Project X era. The only technology which seems to have a clear upgrade path to high precision short-baseline physics without running into systematics issues is νSTORM. νSTORM would profit considerably from increased beam power at 8 GeV.

Until the LBNE reconfiguration, a long-baseline oscillation experiment was *the* motivation for Project X in order to fully capitalize on the considerable investment made on a large underground detector and new beamline. With the reconfiguration process of LBNE still ongoing as of this writing, it is difficult to formulate a conclusion. The LBNE options using the existing NuMI beamline eventually will run into power limitations at around 1 MW and these power levels may be obtainable even without a full Project X. Therefore, if any of these options are chosen, the impact of Project X on the long-baseline program is strongly reduced. In the preferred option, a new beamline will be built. Despite significant budgetary pressure on this option it is important to ensure that new beamline is fit to accept the beam power Project X can deliver or that an upgrade to do so is easily possible. However, in the preferred option the detector is very small, 10 kt only, and it is on the surface, which may require a further reduction of fiducial mass due the need to cope with cosmogenic backgrounds. In combination, the small size, the risk of it becoming even smaller and the surface operation make the overall physics case appear marginal and only in a second phase a compelling experimental program can be realized. For the second phase, three improvements or a combination thereof can be envisaged: larger detector at the surface, deep underground detector and increase of beam power. Only the third improvement requires Project X, however it is likely to be more cost effective and yield more physics to build a deep underground detector in phase 2.

Finally, a staged muon based program starting with νSTORM, which can evolve in various, adjustable steps to a full neutrino factory, which eventually would pave the way towards a muon collider, seems to be a very attractive option. It will produce outstanding physics at each stage. At the same time, this path crucially requires Project X and thus, could be the most compelling motivation for Project X. Obviously, going beyond νSTORM requires a vigorous R&D effort, which in the form of the IDS-NF and MAP is already ongoing, but would benefit from increased levels of support.

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