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11 January 2012

Pier Oddone
Fermilab Director

Dear Pier,

This is my formal notification to you that I have decided that we should proceed with a liquid argon TPC detector at the 4850L depth for LBNE. In the attached note I summarize the process that we have followed in coming to the LBNE Far Detector decision, present my main reasons for making this choice, and also summarize the most important risks of this path. I seek your concurrence with my decision, as well as that of the Laboratory Oversight Group and of the DOE Office of High Energy Physics, as required by the Procedures for LBNE Far Detector Configuration Decision, LBNE-doc-4099, as approved by the LBNE Executive Committee, 28 July 2011.

Sincerely,

James Strait,
LBNE Project Manager

c: Young-Kee Kim, Fermilab Deputy Director, for the Laboratory Oversight Group
Milind Diwan, BNL, LBNE Co-Spokesperson
Robert Svobofda, UC Davis, LBNE Co-Spokesperson

LBNE Far Detector Decision
James Strait
LBNE Project Manager

11 January 2012

This note documents my decision that LBNE should proceed a liquid Argon TPC detector at the 4850L depth at the Sanford Underground Laboratory at Homestake. It summarizes the process that we have followed in coming to the LBNE Far Detector decision, present my main reasons for making this choice, and also summarizes the most important risks of this path.

The Far Detector Decision Process

Both a water Cherenkov detector (WCD) and a liquid argon TPC (LAr) have been proposed for the far detector for LBNE, and the LBNE Collaboration and the LBNE Project Organization combine proponents of both approaches. A year ago, the LBNE Executive Committee (EC) clearly stated that the preferred choice would be an experiment utilizing both detector types: "There was a near consensus by the Board that the preferred option for us to pursue for a 200 WCE configuration, assuming that a funding cap is not considered - or the option can be made to fit within the cap, is one with a mixed water Cherenkov and liquid argon technology." (See Milind Diwan's presentation to the LBNE Collaboration meeting, January 2011, LBNE-doc-3342, slides 19-20.) However, it became clear that a mixed configuration would be too expensive and therefore a down-selection to a single detector type would be required.

The process leading to the technology decision was summarized by Bob Svoboda in his presentation to the LBNE Collaboration meeting, December 2011 (LBNE-doc-5293), and I further summarize it here. It began with the formation of the Physics Working Group (PWG) in January 2010, which was charged to study many configurations involving WCD, LAr and combinations of the two, and included consideration of different depths for the LAr detector. In November of 2010, a document (LBNE-doc-3056) prioritizing the physics goals of LBNE was developed and approved jointly by the Collaboration and the Project management, and was further approved by the Fermilab Director and the LBNE Federal Project Director. In January 2011, the writing of three Case Study documents was launched: one for WCD, one for LAr and one for a mixed solution. These were developed mainly by the Collaboration. In parallel, the Project organization was developing conceptual designs and cost and schedule estimates.

As the higher cost of the mixed option became better known, it became clear that we needed a formal procedure to decide between WCD and LAr. The policies and procedures were developed jointly by the LBNE Project and Collaboration, and documented in "Far Detector Technology Decision General Principles" and "Procedures for LBNE Far Detector Configuration Decision." These were approved by the EC in July 2011, and are combined in LBNE-doc-4099.

The purpose of formally establishing this process was presented in in the introduction to "Far Detector Technology Decision General Principles."

> We need to select a specific far detector configuration in a manner that is open, objective and timely. The decision should be based on facts concerning the scientific capabilities, risks, and cost and schedule to implement each of the two candidate detector types. An objective process is needed to establish those facts, so that, to the greatest extent possible, all of the stakeholders agree that the information used for the decision is valid. Although the final decision is formally made by the Project Manager (as the contracted agent for the DOE through FNAL), the goal is to reach a decision by consensus among all of the stakeholders.

The general process stated in the same document is the following:

> Following a rigorous set of reviews of the science, technology, cost and schedule for each far detector, the EC, potentially augmented by additional advisors (e.g. relevant scientific or technical experts or representatives of other stakeholders) will meet to draft the far detector recommendation. The procedures for this will be defined in a later document, but will be consistent with the principles of fairness, consensus, and decisiveness established in previous EC retreats.

The last step in "Procedures for LBNE Far Detector Configuration Decision" is the following:

> The Project Manager will make the formal decision, based on the advice received from the LBNE Collaboration EC, and subject to the concurrence of the Fermilab Director, the LBNE Laboratory Oversight Group, and the DOE Office of High Energy Physics.

It was agreed that the process should yield a decision in time to allow only one technology to be carried to CD-1:

> Target date for the completion of the decision process is by the end of CY11. This target date will be revisited as circumstances dictate.

Key Reviews

The key reviews and meetings leading to the far detector decision, as outlined in the approved procedure, were: a Scientific Capabilities Review, conducted in October-November 2011 with a complete draft report delivered on 9 December; a Conceptual Design, Cost and Schedule Review, conducted 6-9 December, with a draft closeout report issued at the end of the review meeting; and a retreat of the Executive Committee 12-14 December, at which recommendations regarding the far detector choice were developed.

The Science Capabilities Review was conducted by an external committee of six highly experienced experimental physicists, who were invited by and charged by the LBNE Co-Spokespeople. It was charged "to evaluate and compare each of the two approaches to building

LBNE with respect to its capabilities to achieve the science goals of the experiment." The full charge and the final report is in LBNE-doc-5333. In its "Conclusions and Recommendations" the committee states:

- *In light of the presented materials the committee unanimously agrees that both technologies represent significant scientific opportunities, that either detector could be built at an acceptable level of risk, and that current knowledge supports the view that either is likely to deliver its expected performance, and that either detector would make world-leading measurements relevant to all of the major science goals.* (Italics in the original.)

Then in comparing the WCD and LAr, it clearly favors the LAr detector regarding CP sensitivity:

- The major unanswered question is the effect of background uncertainties on CP sensitivity. The lower background level should make LAr less sensitive to systematics in the backgrounds. In the view of the committee the greater ability of the LAr to reconstruct complicated final states may yield a further reduction in this risk.

And it favors it with regard to proton decay and supernova neutrino physics, particularly in terms of complementarity to Super-K:

- Given existing limits from Super Kamiokande, the best opportunity for a significant discovery in proton decay is in the p -> Kν channel, and in this channel the LAr detector has the clear advantage. The committee notes that the impact of continued SK data taking, and the desire for complementarity in p decay final states reinforces this conclusion.
- The greater size of the WC detector gives it a clear advantage for some of the other physics, in particular, for the SN burst measurement, although the LAr could see a striking signature of the hierarchy and give important information on collective phenomena in the neutrino sphere. If Super Kamiokande continues to run, the complementary information provided by a LAr detector in the event of a galactic SN would be valuable.

The committee report closes with the following summary of its scientific evaluation:

- The committee unanimously agrees that, that on the question of scientific capabilities, that the prospect for the LAr detector to refine our understanding of neutrino oscillations, and to be sensitive to unexpected new physics, exceeds that from the WC detector.

The LBNE Far Site Conceptual Design, Cost, Schedule and Risk Review was conducted by an external committee of 22 experienced experimental physicists, engineers, and project controls professionals, who were invited and charged by the LBNE Project Manager and Project Engineer. The charge called for the committee to "assess the status and adequacy of the conceptual design for LBNE Water Cherenkov Detector (WCD), Liquid Argon Detector (LAr), and the associated Conventional Facilities (CF) at the Far Site (Sanford Lab)." The full charge and the final report is in LBNE-doc-5242.

In its Executive Summary, the committee stated that the designs of both detectors are adequate at the conceptual design level, and further stated that the information available is sufficient to make the technology choice:

> The primary conclusions of the review committee is that the present state of understanding of the design, R&D, performance and cost & schedule for the LBNE far-site detectors and conventional facilities are capable of executing the primary science mission, that LBNE will be ready for a DOE CD-1 review in a few months, and that there is no reason not to expect success. We further believe that the technology decision can be made now and that having a single detector model will facilitate moving forward to CD-1 and beyond.

The Summary on the liquid argon detector states:

> The committee was very impressed by the status of the Liquid Argon Option for LBNE. The group has made significant technical and organizational progress since last year maintaining and reinforcing the original concept. They have now taken charge of all the areas of the project and speak coherently and with confidence. This is partly due to the maturing status of LAr in the overall neutrino community, resulting in competing proposals in Europe and Japan with different detailed technologies. The Committee believes that based on such successes as ICARUS, ArgoNeut and the recent LAPD results, there is no doubt that a large liquid argon TPC can be deployed and have a rich physics program. They have chosen a direction which minimizes the R&D and requires now mainly development related to the scaling up to the multi-kiloton size. This choice is important since they have to work underground which brings additional challenges. The group is on the right path and close to a successful CD-1 review.

The Summary on the water Cherenkov detector states:

> The LBNE Project and collaboration include long standing experts in massive Cerenkov based neutrino detectors. These are highly experienced people who have formed an effective team engaged in the design of a next-generation massive underground water Cerenkov (WC) detector as a candidate for the far detector. The design of the LBNE WC far detector is based on straightforward extrapolations of the successful Super Kamiokande II (SK-II) experience.
>
> The WC design presented can achieve the primary physics goals of the LBNE project. The team has adopted a detector design that in large measure takes into account experience gained from SK-II. This is done in order to reduce risks in design, construction, and performance and thereby cost and schedule risk to the Project. The WC cost and schedule for the design presented is credible. With continued effort we are confident they can get there from here. We cannot say that the project is ready for a CD-1 review today. However, given the enthusiasm and expertise of the team the project can be ready for a CD-1 review on the timescale of a few months.

Thus the CDR review committee endorsed the viability of both detector designs and the readiness of the teams to move to the next project step.

Executive Committee Recommendation

The LBNE Executive Committee met for two and one half days in mid-December to consider these reports, as well as many other factors. The entire committee participated, and for the first day and one half, four "advisors," two from within and two from outside the collaboration also joined the discussion. Three options were explicitly considered: WCD at the 4850L depth, LAr at the 4850L depth, and LAr at the 800L depth. There was extended discussion, in which significant numbers of committee members advocated for water or liquid argon and during which a number of straw polls were taken with somewhat variable results. Although there wasn't consensus on the detector technology, a clear consensus emerged that the experiment should, in any case, be sited at the 4850 level. At the end of the second day, a final binding vote was held in which the WCD was favored by a small majority. I, as the recipient of the EC's advice, abstained from the final vote.

As called for in the procedure, a subcommittee then drafted a written report, which was discussed and edited by the full committee. The final report was approved unanimously by the EC, and presented by Bob Svoboda at the LBNE Collaboration meeting, which began the day after the EC retreat (LBNE-doc-5293). The main conclusions were:

- There was very strong support for both technologies. The committee feels that both technologies are viable and complementary in many aspects.
- There was a very strong preference for siting the experiment at the 4850L depth.
- Given the current state of knowledge and considering the factors listed above, the committee favored the Water Cerenkov option.

As part of this final report, the EC unanimously passed a statement strongly expressing their view that getting the science done should outweigh consideration of far detector technology in the final decision making-process:

> As per the Procedures for LBNE Far Detector Configuration Decision (LBNE-doc-4099)
>
> The Project Manager will make the formal decision, based on the advice received from the LBNE collaboration EC, and subject to the concurrence of the Fermilab Director, the LBNE Laboratory Oversight Group, and the DOE Office of High Energy Physics.
>
> The Executive Committee reaffirms its commitment to the Scientific Goals of LBNE and will endorse the ultimate technology decision.

The Far Detector Decision

Since the EC recommendation regarding detector technology did not represent the consensus that was the goal of the process, I felt it necessary to weigh the other committee conclusions seriously. In particular, I felt I had to balance the recommendation of the EC to proceed with the water detector against the strong (unanimous) opinion of the Scientific Capabilities Review Committee that the LAr detector has superior capabilities for the primary physics of LBNE, plus the evaluation of the CDR Review that both detector designs are technically sound. Because we unable to reach consensus – neither among ourselves nor between the EC and the Scientific Capabilities Review Committee – and because we need to move forward and do not have time to work any longer to achieve consensus, I have taken the decision.

In considering my decision, I evaluated the information in the reports cited above, as well as from other sources. In addition, in the period after the EC retreat, I had extensive discussions, particularly about the ability to do the flagship CP-violation search, with proponents and experts in each detector technology. I considered these in the context of the approved scientific priorities of LBNE and of the agreed-upon criteria and procedures for making the far detector decision. Relevant reference documents include:

1) Redacted Mission Need Statement for a Long Baseline Neutrino Experiment (LBNE), September 1, 2009, private communication.
2) Key Assumptions: Physics Research Goals of the LBNE Project, LBNE-doc-3056, 18 Nov 2010.
3) Far Detector Technology Decision General Principles, LBNE-doc-4099, 28 July 2011; Procedures for LBNE Far Detector Configuration Decision, LBNE-doc-4099 28 July 2011.
4) Department of Energy Office of Science Review of Options for Underground Science, June 15, 2011, http://science.energy.gov/~/media/hep/hepap/pdf/june-2011/Review_of_Underground_Science_Report_Final.pdf.
5) The 2010 Interim Report of the Long-Baseline Neutrino Experiment Collaboration Physics Working Groups, October 31, 2011, arXiv:1110.6249v1 [hep-ex] 27 Oct 2011.
6) Risk workshops conducted by the LBNE Project Management Board, October 13-14 and November 16-17, 2011, LBNE-doc-2445.
7) The Report of the LBNE Scientific Capability Review Committee, December 14, 2012, LBNE-doc-5333. The agenda for the oral presentations with links to the talks and other relevant documents is at https://indico.fnal.gov/conferenceDisplay.py?confId=4900.
8) The Committee Report, LBNE Far Site Review, December 6-9, 2012, LBNE-doc-5242. The agenda with links to the talks and other relevant documents is at http://lbne.fnal.gov/reviews/farsite-design-review-dec2011.shtml.
9) The recommendations developed by the LBNE Collaboration Executive Committee during its retreat December 12-14, 2012. See Bob Svoboda's presentation to the LBNE Collaboration meeting, December 15, 2012, LBNE-doc-5293. The official recommendation text is found on slides 20-23.

My conclusion represents a judgment based on a large number of factors; however, the ones that I find most compelling are well aligned with the thinking of the Scientific Capabilities Review Committee. They are:

1) The excellent ability of the LAr TPC to identify ν_e charged current events (the signal) and to distinguish them from backgrounds (principally neutral current events with a π° misidentified as a single electron) down to a level comparable to and potentially smaller than the intrinsic ν_e component of the beam. (See slides 6-7 in LBNE-doc-4731, presented to the Scientific Capabilities Review.) This will ultimately result in oscillation measurements, particularly in the flagship search for CP violation, with small systematic errors. With a clean ν_e appearance signal, a claim that we have seen CP violation in the neutrino sector – a bold assertion! – will more easily pass the "believability" test, and not depend as critically on demonstrating that the background shape and magnitude are well understood as it would were the remaining background substantially larger. Additionally, the ability to make a near detector with essentially identical characteristics to the far detector may result in further reduction in systematic errors.

2) The ability of the LAr TPC to cleanly and efficiently search for proton decay modes that are difficult for other existing detectors, principally Super-K, to detect. The benchmark mode in this category is $p \rightarrow \bar{\nu} K^+$. A LAr detector is thus complementary to existing experiments, and a 33 kt LAr detector offers a greater reach beyond current and expected limits from Super-K than does a 200 kt water Cherenkov detector. (See Figs. 35-37 in reference 5.)

3) The complementarity of a large LAr detector relative to other large detectors in measuring neutrinos from a core-collapse supernova in the Galaxy. Essentially all other relevant large detectors in operation or planned have either water or hydrocarbon targets, and hence they are mainly sensitive to the $\bar{\nu}$ flux, while a LAr detector is mainly sensitive to the ν flux. (See Tables XIV and XV in reference 5.) Thus, in a worldwide context, we will learn more from such a once-in-a-lifetime event if the existing $\bar{\nu}$ detectors are joined by a LAr detector. The Physics Working Group report states, "A combination of different detector types offers the best physics sensitivity, because of ability to distinguish different flavor components of the supernova burst flux." (reference 5, page 44) In addition, the division of the LAr detector into two independent modules reduces the risk of missing such a rare event if one detector is out of service for planned (or unplanned) maintenance.

In addition to the advantages cited above, which focus directly on the scientific goals of LBNE, there are other reasons for choosing a LAr TPC for LBNE.

1) Building an LBNE LAr TPC will bring to full maturity a detector technology that has powerful capabilities for neutrino physics, and which could, therefore, allow substantial advances in our understanding of nature. As with other "new" detector or accelerator technologies, it has the potential to open new scientific vistas, but the development often needs an initial push from a large-scale application. Consider, for example, the development of superconducting magnets for the Tevatron in the 1970's and early 1980's, which were considered revolutionary and risky at the time but now are central to

the advance of our field; or the development of silicon vertex detectors for collider experiments in the 1980's and early 1990's, which now form the basis for entire high-precision tracking systems.

2) There is considerable world-wide interest in the development of LAr TPCs for neutrino physics, proton decay searches and other underground physics. It seems likely that, by moving forward with a large-scale LAr detector coupled to a powerful neutrino, we will attract substantial international collaboration. Substantial international collaboration could lower our costs and allow us, together with the new collaborators, to increase the size and capability of our detector. In addition, new, experienced collaborators would bring additional expertise and ideas to address the challenges of this technology. Strong world-wide support for this enterprise could be quite important.

Although I believe that a LAr TPC is the right detector for LBNE, this path is not without risk. Chief among these are:

1) The Collaboration Executive Committee is on record favoring a WCD, and going against this recommendation risks alienating some members of the collaboration. This is at least partly mitigated, however, by the final statement of the official recommendation that, "The Executive Committee reaffirms its commitment to the Scientific Goals of LBNE and will endorse the ultimate technology decision." This risk can be further mitigated by working actively with affected groups and individuals to integrate them into the LAr effort (or near detector or beam) – as we must to ensure the success of LBNE – and to help ensure continuity of their funding.
2) The committee for the Office of Science Review of Options for Underground Science ("Marx Committee"), which met in April 2011, was of the opinion that, "Overall, [the liquid argon] option cannot be considered viable until the R&D program is complete." However, since that committee issued its conclusions, ICARUS has logged 6 more months of stable operation with electron lifetime well in excess of our requirements, ArgoNeut has submitted its first physics results for publication, LAPD has achieved high purity without evacuation, and the CDR review committee (which looked at the LBNE LAr program in much more technical detail than did the Marx Committee) declared that building such a device is feasible.
3) More serious is the risk that, despite our confidence that the basic understanding necessary to successfully build a large LAr TPC is in place, we may encounter enough engineering difficulties in developing and building the detector that it would be completed much later than planned and perhaps with a reduction in mass necessitated by the need to remain within a fixed budget. Demonstrating the specific design we have chosen in a prototype of substantial scale would largely mitigate this risk, but we will need to build it quickly so that any problems can be identified early. We will need sufficient funding early in the program to advance the prototyping program vigorously and rapidly, and we will need to fully marshal the best engineering and scientific talent of the Collaboration. The next 2-3 years will be crucial ones.

I believe that, despite the risks and challenges, a LAr TPC offers the best opportunities for LBNE to do world-leading science. It will give us the cleanest oscillation signals and ultimately provide the best chance of observing CP violation; it will produce the best limits on proton decay, relative to other operating detectors in the world; and will produce unique information about a galactic supernova, should one occur during the lifetime of the experiment, and, together with other existing or planned experiments, provide the broadest view of such a once-in-a-lifetime event. This path will bring this important detector technology to full maturity, with potential applications to future experiments. The potential for substantial international collaboration will make a stronger LBNE. Finally, the feasibility of building a large LAr TPC is supported by our recent CDR review, and the scientific advantages of this path are supported by the recent Scientific Capabilities Review.