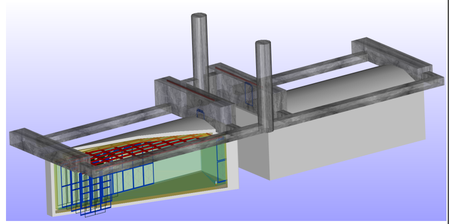
# Status of LBNE project and collaboration

Milind Diwan
CETUP workshop, Lead,
July 29, 2012



# Outline

- Description of the collaboration.
- Long-term goals and plans of the LBNE program
- Reality and Vision collide:
  The Reconfiguration of LBNE
- A phased approach to LBNE (and Project X)
- LBNE Project status and next steps
- Conclusions

# Long-Baseline Neutrino Experiment Collaboration

**Alabama:** S.Habib, I.Stancu
**Argonne:** M.D'Agostino, G.Drake.Z.Djurcic, M.Goodman, V.Guarino, S.Magill, J.Paley, H.Sahoo, R.Talaga, M.Wetstein
**Boston:** E.Hazen, E.Kearns, S.Linden
**Brookhaven:** M.Bishai, R.Brown, H.Chen, M.Diwan, J.Dolph, G.Geronimo, R.Gill, R.Hackenburg, R.Hahn, S.Hans, Z.Isvan, D.Jaffe, S.Junnarkar, S.H.Kettell, F.Lanni, Y.Li, L.Littenberg, J.Ling, D.Makowiecki, W.Marciano, W.Morse, Z.Parsa, V.Radeka, S.Rescia, N.Samios,R.Sharma, N.Simos, J.Sondericker, J.Stewart, H.Tanaka, H.Themann, C.Thorn, B.Viren, S.White, E.Worcester, M.Yeh, B.Yu, C.Zhang
**Caltech:** R.McKeown, X.Qian
**Cambridge**: A.Blake, M.Thomson
**Catania/INFN:** V.Bellini, F.La Zia, F.Mammoliti, R.Potenza,
**Chicago:** E.Blucher, M.Strait
**Colorado:** S.Coleman, R.Johnson, S.Johnson, A.Marino, E.Zimmerman
**Colorado State:** M.Bass, B.E.Berger, J.Brack, N.Buchanan, D.Cherdack, J.Harton, W.Johnston, W.Toki, T.Wachala, D.Warner, R.J.Wilson
**Columbia:** R.Carr, L.Camillieri, C.Y.Chi, G.Karagiorgi, C.Mariani, M.Shaevitz, W.Sippach, W.Willis
**Crookston:** D.Demuth
**Dakota State:** B.Szcerbinska
**Davis:** M.Bergevin, R.Breedon, D.Danielson, J.Felde, C.Maesano, M.Tripanthi, R.Svoboda, M.Szydagis
**Drexel:** C.Lane, S.Perasso
**Duke:** T.Akiri, J.Fowler, A.Himmel, Z.Li, K.Scholberg, C.Walter, R.Wendell
**Duluth:** R.Gran, A.Habig
**Fermilab:** D.Allspach, M.Andrews, B.Baller, E.Berman, R.Bernstein, V.Bocean, M.Campbell, A.Chen, S.Childress, A.Drozhdin, T.Dykhuis, C.Escobar, H.Greenlee, A.Hahn, S.Hays, A.Heavey, J.Howell, P.Huhr, J.Hylen, C.James, M.Johnson, J.Johnstone, H.Jostlein, T.Junk, B.Kayser, M.Kirby, G.Koizumi, T.Lackowski, P.Lucas, B.Lundberg, T.Lundin, P.Mantsch, A.Marchionni, E .McCluskey, S.Moed Sher, N.Mokhov, C.Moore, J.Morfin, B.Norris, V.Papadimitriou, R.Plunkett, C.Polly, S.Pordes, O.Prokofiev, J.L.Raaf, G.Rameika, B.Rebel, D.Reitzner, K.Riesselmann, R.Rucinski, R.Schmidt, D.Schmitz, P.Shanahan, M.Stancari, A.Stefanik, J.Strait, S.Striganov, K.Vaziri, G.Velev, T.Wyman, G.Zeller, R.Zwaska
**Hawai'i:** S.Dye, J.Kumar, J.Learned, J.Maricic, S.Matsuno, R.Milincic, S.Pakvasa, M.Rosen, G.Varner
**Houston:** L.Whitehead
**Indian Universities:** V.Singh (BHU); B.Choudhary, S.Mandal (DU); B.Bhuyan [IIT(G)]; V.Bhatnagar, A.Kumar, S.Sahijpal(PU)
**Indiana:** W.Fox, C.Johnson, M.Messier, S.Mufson, J.Musser, R.Tayloe, J.Urheim
**Iowa State**: I.Anghel, G.S.Davies, M.Sanchez, T.Xin
**IPMU/Tokyo:** M.Vagins
**Irvine:** G.Carminati, W.Kropp, M.Smy, H.Sobel
**Kansas State:** T.Bolton, G.Horton-Smith
**LBL:** B.Fujikawa, V.M.Gehman, R.Kadel, D.Taylor
**Livermore:** A.Bernstein, R.Bionta, S.Dazeley, S.Ouedraogo
**London**: A.Holin, J.Thomas
**Los Alamos:** M.Akashi-Ronquest, S.Elliott, A.Friedland, G.Garvey, E.Guardincerri, T.Haines, D.Lee, W.Louis, C.Mauger, G.Mills, Z.Pavlovic, J.Ramsey, G.Sinnis, W.Sondheim, R.Van de Water, H.White, K.Yarritu
**Louisiana:** J.Insler, T.Kutter, W.Metcalf, M.Tzanov
**Maryland:** E.Blaufuss, S.Eno, R.Hellauer, T.Straszheim, G.Sullivan
**Michigan State:** E.Arrieta-Diaz, C.Bromberg, D.Edmunds, J.Huston, B.Page
**Minnesota:** M.Marshak, W.Miller
**MIT:** W.Barletta, J.Conrad, B.Jones, T.Katori, R.Lanza, A.Prakash, L.Winslow
**NGA:** S.Malys, S.Usman
**New Mexico**: J.Mathews
**Notre Dame:** J.Losecco
**Oxford**: G.Barr, J.de Jong, A.Weber
**Pennsylvania:** S.Grullon, J.Klein, K.Lande, T.Latorre, A.Mann, M.Newcomer, S.Seibert, R.vanBerg
**Pittsburgh:** D.Naples, V.Paolone
**Princeton:** Q.He, K.McDonald
**Rensselaer:** D.Kaminski, J.Napolitano, S.Salon, P.Stoler
**Rochester:** L.Loiacono, K.McFarland, G.Perdue
**Sheffield:** V.Kudryavtsev, M.Richardson, M.Robinson, N.Spooner, L.Thompson
**SDMST:** X.Bai, C.Christofferson, R.Corey, D.Tiedt
**SMU.:** T.Coan, T.Liu, J.Ye
**South Carolina**: H.Duyang, B.Mercurio, S.Mishra, R.Petti, C.Rosenfeld, X Tian
**South Dakota**: D.Barker, J.Goon, D.Mei, W.Wei, C.Zhang
**South Dakota State**: B.Bleakley, K.McTaggert
**Syracuse:** M.Artuso, S.Blusk, T.Skwarnicki, M.Soderberg, S.Stone
**Tennessee:** W.Bugg, T.Handler, A.Hatzikoutelis, Y.Kamyshkov
**Texas:** S.Kopp, K.Lang, R.Mehdiyev
**Tufts:** H.Gallagher, T.Kafka, W.Mann, J.Schnepps
**UCLA:** K.Arisaka, D.Cline, K.Lee, Y.Meng, A.Teymourian, H.Wang
**Virginia Tech.:** E.Guarnaccia, J.Link, D.Mohapatra
**Washington:** H.Berns, S.Enomoto, J.Kaspar, N.Tolich, H.K.Tseung
**Wisconsin:** B.Balantekin, F.Feyzi, K.Heeger, A.Karle, R.Maruyama, B.Paulos, D.Webber, C.Wendt
**Yale:** E.Church, B.Fleming, R.Guenette, K.Partyka, A.Szelc

**347 Members**
**62 Institutions**
**25 US States**
**5 Countries**

19 July 2012 (347)

# Institutions in LBNE (62)

Argonne
Alabama
Boston University
Brookhaven
Caltech
Cambridge
Catania
Columbia
Chicago
Colorado
Colorado State
Columbia
Crookston
Davis
Drexel
Duke
Duluth
Fermilab
Hawaii
Indian Universities[BHU, Delhi U., IIT(G), Panjab U.]
Indiana
Iowa State
IPMU-Tokyo
Irvine
Kansas State
Lawrence Berkeley National Lab
Livermore

London UCL
Los Alamos
Louisiana State
Maryland
Michigan State
Minnesota
MIT
NGA
New Mexico
Notre Dame
Oxford
Pennsylvania
Pittsburgh
Princeton
Rensselaer
Rochester
South Carolina
South Dakota State
SDSMT
Southern Methodist
Syracuse
Texas
Tufts
UCLA
Virginia Tech
Washington
Wisconsin
Yale

++++ need to update.

62 institutions, ~350 collaborators

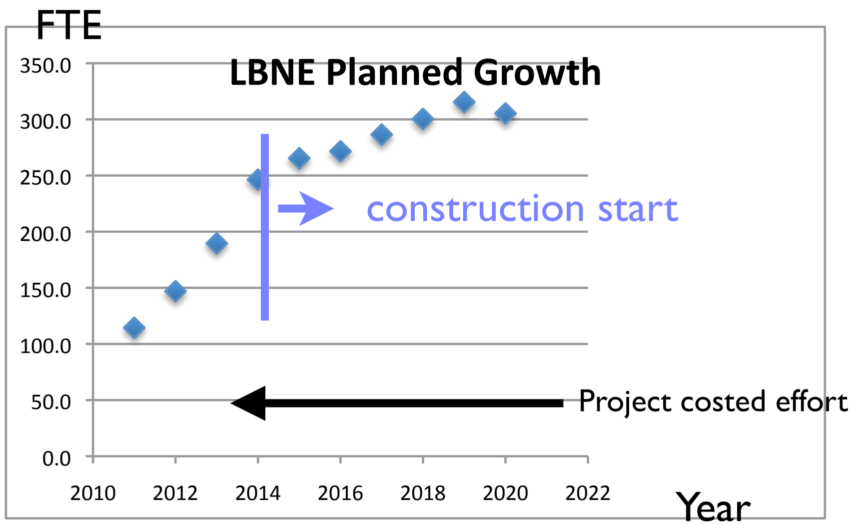
University: ~220
Laboratory: 115

Tenure Track or recently tenured: ~23

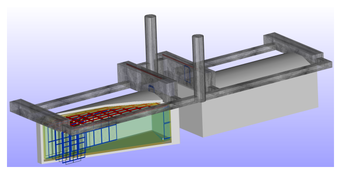
Postdocs + students: ~20

# Collaboration Growth

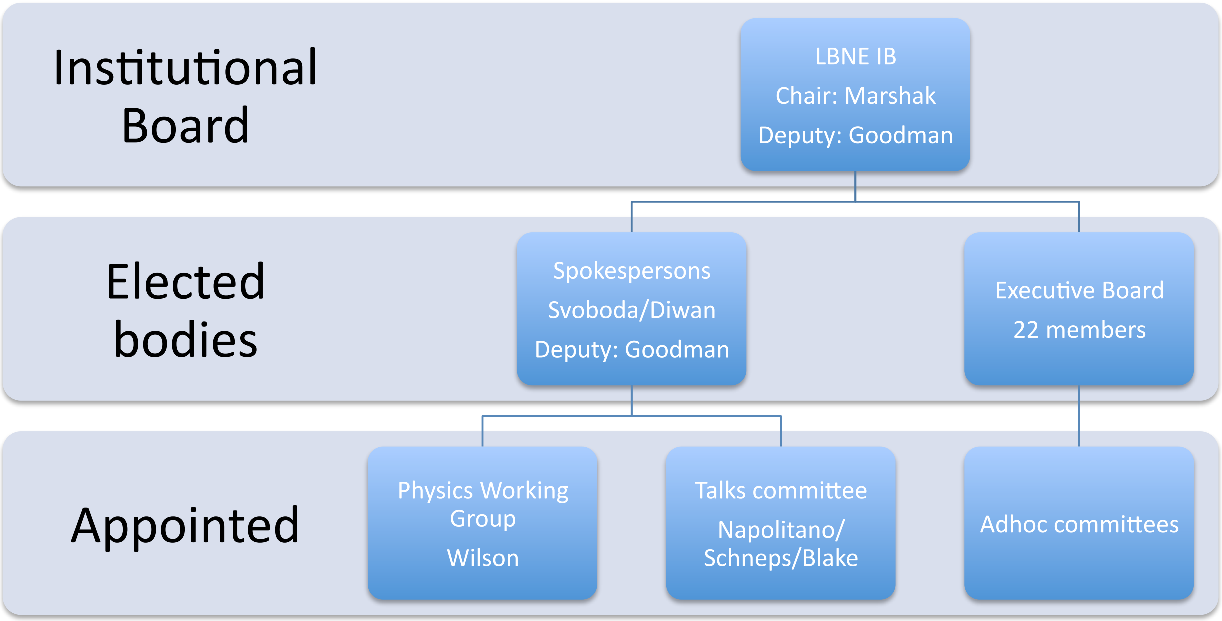
- Numbers still have large errors. With a lot of guesswork.
- Used current number of physics/ technical working groups as a guide. (there are ~15 WG)
- Includes costed project personnel ~ 30-50 FTE
- If one takes average FTE/head count ~ 0.5, collaboration needs to be ~500-600 strong.
- A large collaboration needs a diverse scientific agenda.




Future growth needs be international.

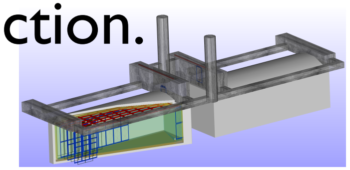


# Collaboration structures




## Physics Working Group:  Bob Wilson

Large efforts with large and diverse funding need a corporate structure.  IB is ruled by a governance document that sets the charge for each office and terms of appointment or election.



# Executive Committee

- The Executive Committee (EC) is responsible for making major scientific and technical decisions. These decisions include the total scientific scope of the experimental project, and the technical choices for the experiment.
- 6 appointed by spokespeople, 6 elected by IB, Ex-officio from collab. and project.
- EC meets on a regular basis on the phone and has in person meetings with formal agenda.
- EC is the main body where Collaboration/Project interactions takes place.

# Events over the last year

- NSB turned off NSF consideration for DUSEL.
- Review from the Lankford committee reaffirmed the science for LBNE and DUSEL
- Marx committee produced a report on the costs.  Rough costs for LBNE have been known since summer of 2011.  It was clear that we could not afford both a water and LAr detectors.
- In December 2011, the LBNE Exec Board/Fermilab/DOE have had extensive negotiations over the far detector technology. The collaboration board preferred the water detector because of its cost and schedule certainties.
- The final decision was made for a 34 kTon LAr detector based on the fact that the performance was better for higher energies (due to L/E and 1300 km) and the uniqueness of the technology.
- We were deemed ready for CD1 review in March when the Daya Bay result was announced.
- DOE/Brinkman decided that they could not afford LBNE in its full glory.

# Physics Research Goals of LBNE

The primary science objectives of the LBNE Project are:

1. A search for, and precision measurements of, the parameters that govern $\nu_\mu \rightarrow \nu_e$ oscillations. This includes measurement of the third mixing angle $\theta_{13}$, for whose value only an upper bound is currently known, and if $\theta_{13}$ is large enough, measurement of the CP-violating phase $\delta$ and determining of the mass ordering (sign of $\Delta m^2_{32}$).
2. Precision measurements of $\theta_{23}$ and $|\Delta m^2_{32}|$ in the $\nu_\mu$ disappearance channel.
3. Search for proton decay, yielding a significant improvement in current limits on the partial lifetime of the proton ($\tau$/BR) in one or more important candidate decay modes, e.g. $p \rightarrow e + \pi^0$ or $p \rightarrow K^+ \nu$.
4. Detection and measurement of the neutrino flux from a core collapse supernova within our galaxy, should one occur during the lifetime of LBNE.

Though outside of the primary objectives, the far detector placed at the proposed depth could enable studies of atmospheric $\nu$ physics, and with additional upgrades, studies of day/night $^8$B solar $\nu$ physics and relic supernova neutrinos.

These goals are in priority order. They have been accepted by funding agencies

# LBNE – Neutrino Oscillation Goals

LBNE plans a comprehensive program to measure neutrino oscillations, to:

- Measure full oscillation patterns in multiple channels, precisely constraining mixing angles and mass differences.
- Search for CP violation both by measuring the parameter $\delta_{CP}$ and by observing differences in $\nu$ and $\bar{\nu}$ oscillations.
- Cleanly separate matter effects from CP-violating effects.

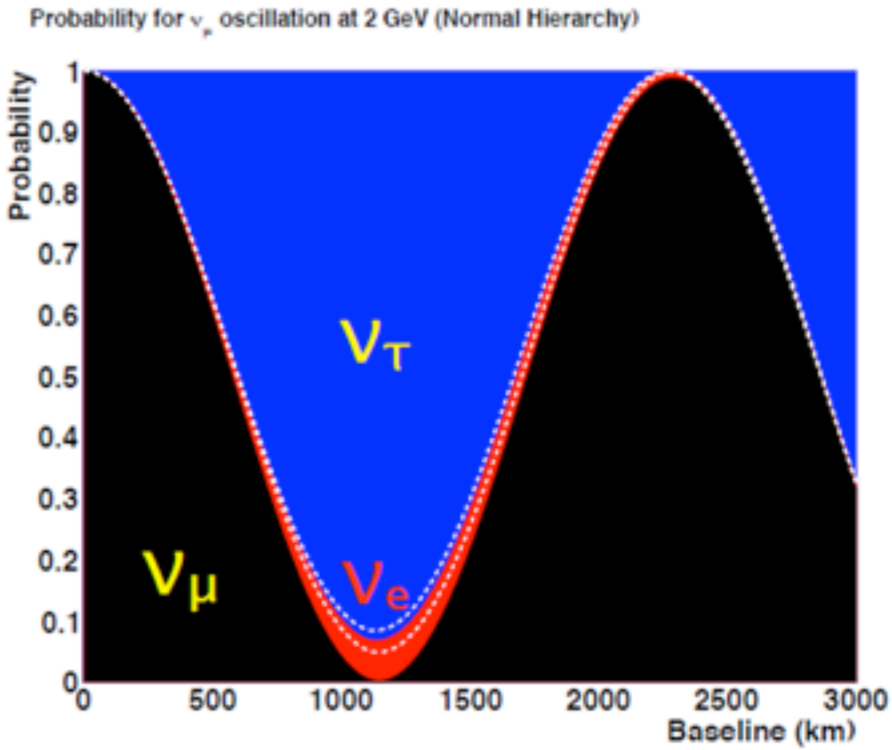
# Complete picture assembled

$\nu_\mu \rightarrow \nu_\mu$ => $\theta_{23}$, $|\Delta m^2_{32}|$

$\nu_\mu \rightarrow \nu_e$ => $\theta_{13}$, sign($\Delta m^2_{32}$), $\delta_{CP}$

$\bar{\nu}_\mu \rightarrow \bar{\nu}_e$ => explicitly observe CP violation

$\nu_\mu \rightarrow \nu_\tau$ => does it all add up?




The white lines indicate CP asymmetry for $\delta = \pm\pi/2$

- This elaborate picture of interference from the current data set needs to be tested in an oscillation experiment that is optimized properly.

# The Baseline

To do this we need the right baseline

- Long enough to cleanly separate the $\nu$ / $\bar{\nu}$ oscillation asymmetry due to the matter effect from CP-violating effect.
- Long enough to put the first and if possible second oscillation maxima at “practical” energies.
- Short enough that the matter effect does not dominate over the CP-violating effect.
- Short enough that the beam is not too difficult to build (pitch angle).

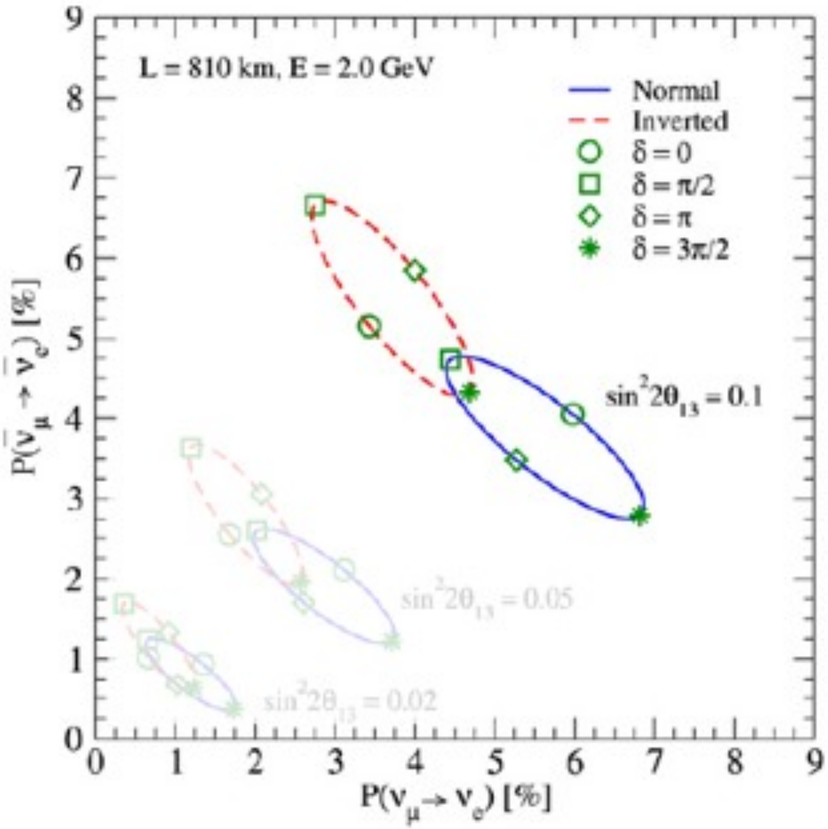
=> 1300 km (Fermilab to Homestake) is “just right.”

# The Baseline

To do this we need the right baseline

- Long enough to cleanly separate the $\nu$ / $\bar{\nu}$ oscillation asymmetry due to the matter effect from CP-violating effect.
- Long enough to put the
oscillation maxima at "pr
- Short enough that the m
over the CP-violating eff
- Short enough that the be
(pitch angle).

=> 1300 km (Fermilab to H

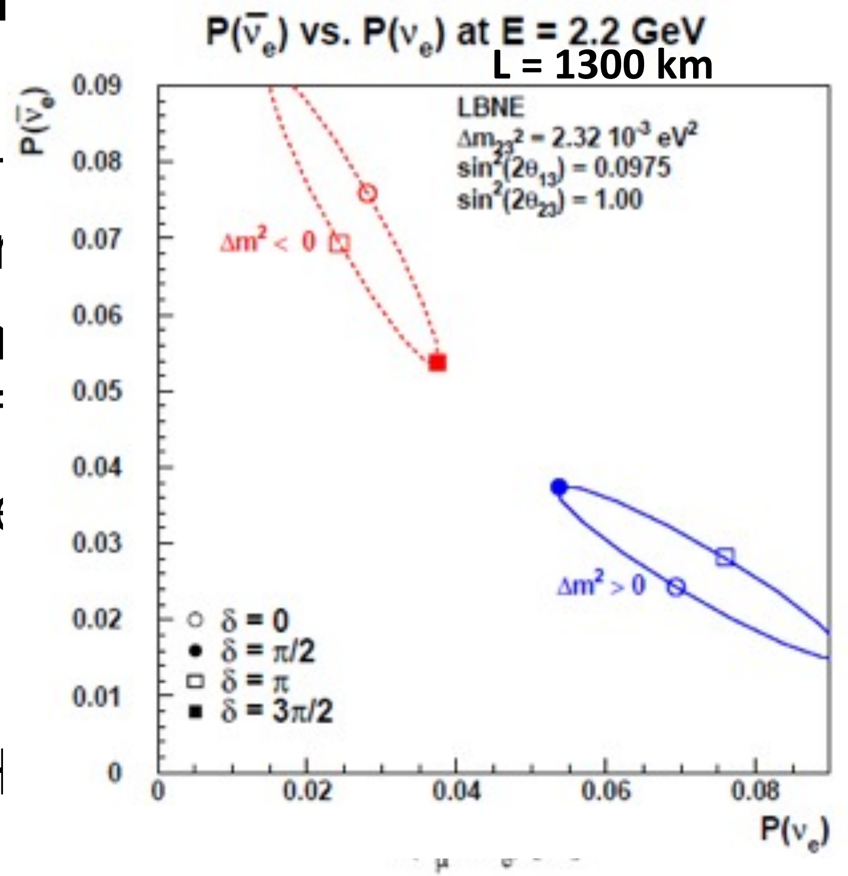


# The Baseline

To do this we need the right baseline

- Long enough to cleanly separate the $\nu$ / $\bar{\nu}$ oscillation asymmetry due to the m
effect.
- Long enough to put the
oscillation maxima at "pr
- Short enough that the m
over the CP-violating eff
- Short enough that the be
(pitch angle).

=> 1300 km (Fermilab to H





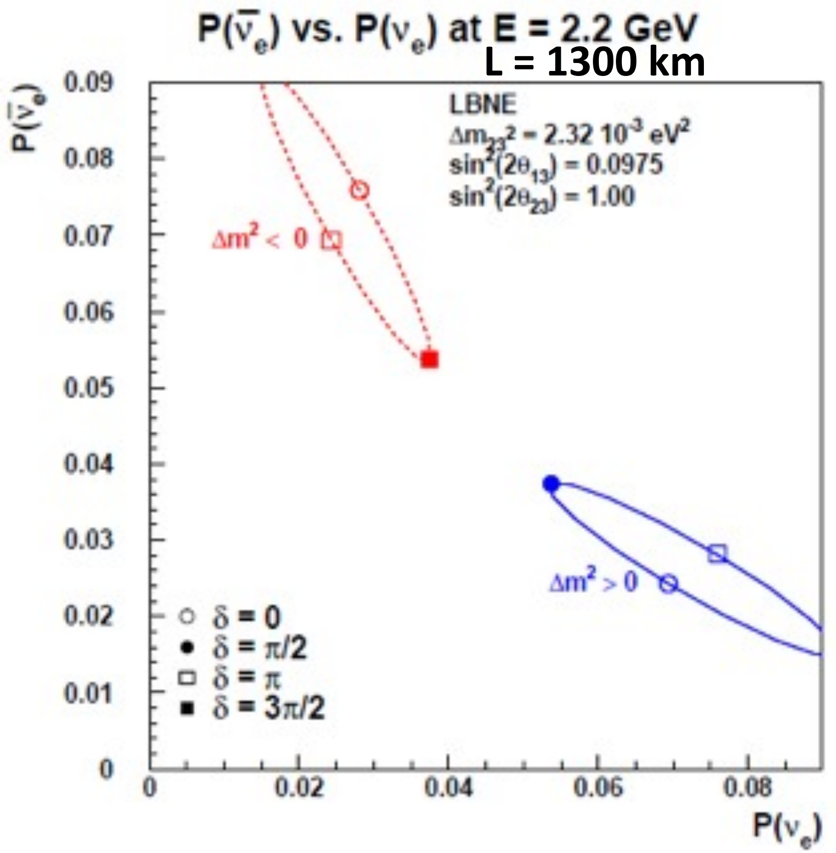

Wednesday, August 22, 12

# The Baseline

To do this we need the right baseline

- Long enough to cleanly separate the $\nu$ / $\bar{\nu}$ oscillation asymmetry due to the m
effect.
- Long enough to put the
oscillation maxima at "pr
- Short enough that the m
over the CP-violating eff
- Short enough that the be
(pitch angle).

=> 1300 km (Fermilab to H



# The Baseline

To do this we need the right baseline

- Long enough to cleanly separate the $\nu$ / $\bar{\nu}$ oscillation asymmetry due to the matter effect from CP-violating effect.
- Long enough to put the first and if possible second oscillation maxima at "practical" energies.
- Short enough that the matter effect does not dominate over the CP-violating effect.
- Short enough that the beam is not too difficult to build (pitch angle).

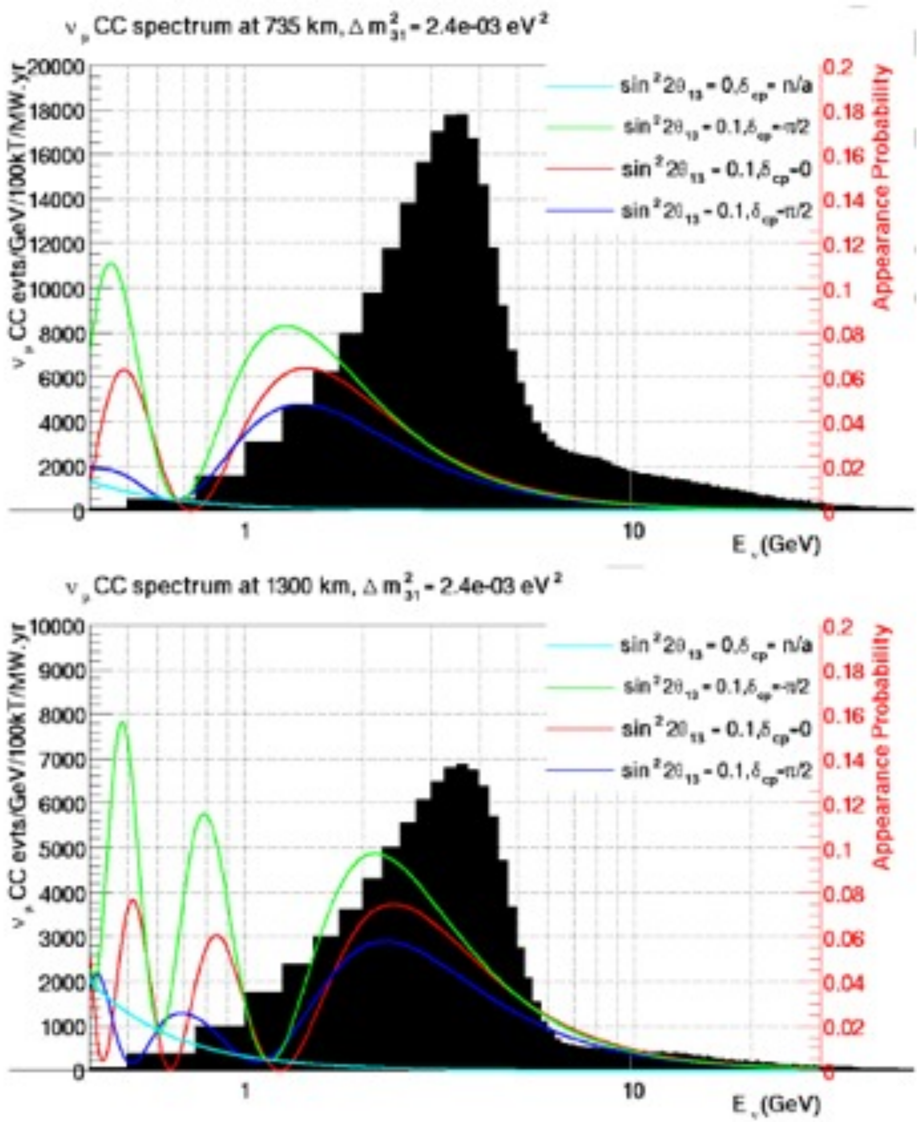
=> 1300 km (Fermilab to Homestake) is "just right."

# The Baseline

To do this we need the right l

- Long enough to cleanly se asymmetry due to the mat effect.
- Long enough to put the firs oscillation maxima at "prac
- Short enough that the mat over the CP-violating effec
- Short enough that the bea (pitch angle).

=> 1300 km (Fermilab to Hor



# The Baseline

To do this we need the right baseline

- Long enough to cleanly separate the $\nu$ / $\bar{\nu}$ oscillation asymmetry due to the matter effect from CP-violating effect.
- Long enough to put the first and if possible second oscillation maxima at “practical” energies.
- Short enough that the matter effect does not dominate over the CP-violating effect.
- Short enough that the beam is not too difficult to build (pitch angle).

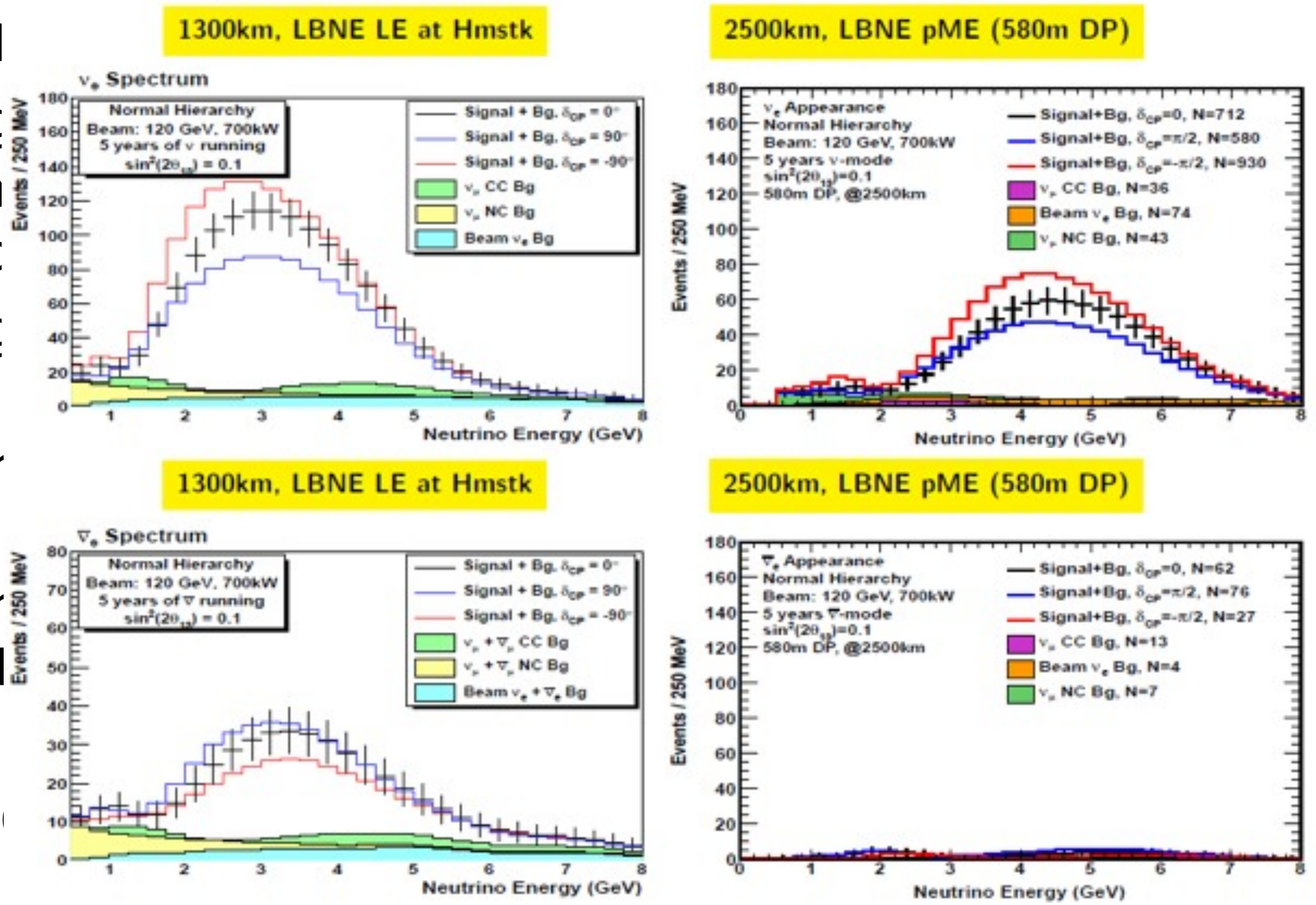
=> 1300 km (Fermilab to Homestake) is “just right.”

# The Baseline

To do tl

- Long asyn effec
- Long oscil
- Shor over
- Shor (pitcl

=> 130



# The Baseline

To do this we need the right baseline

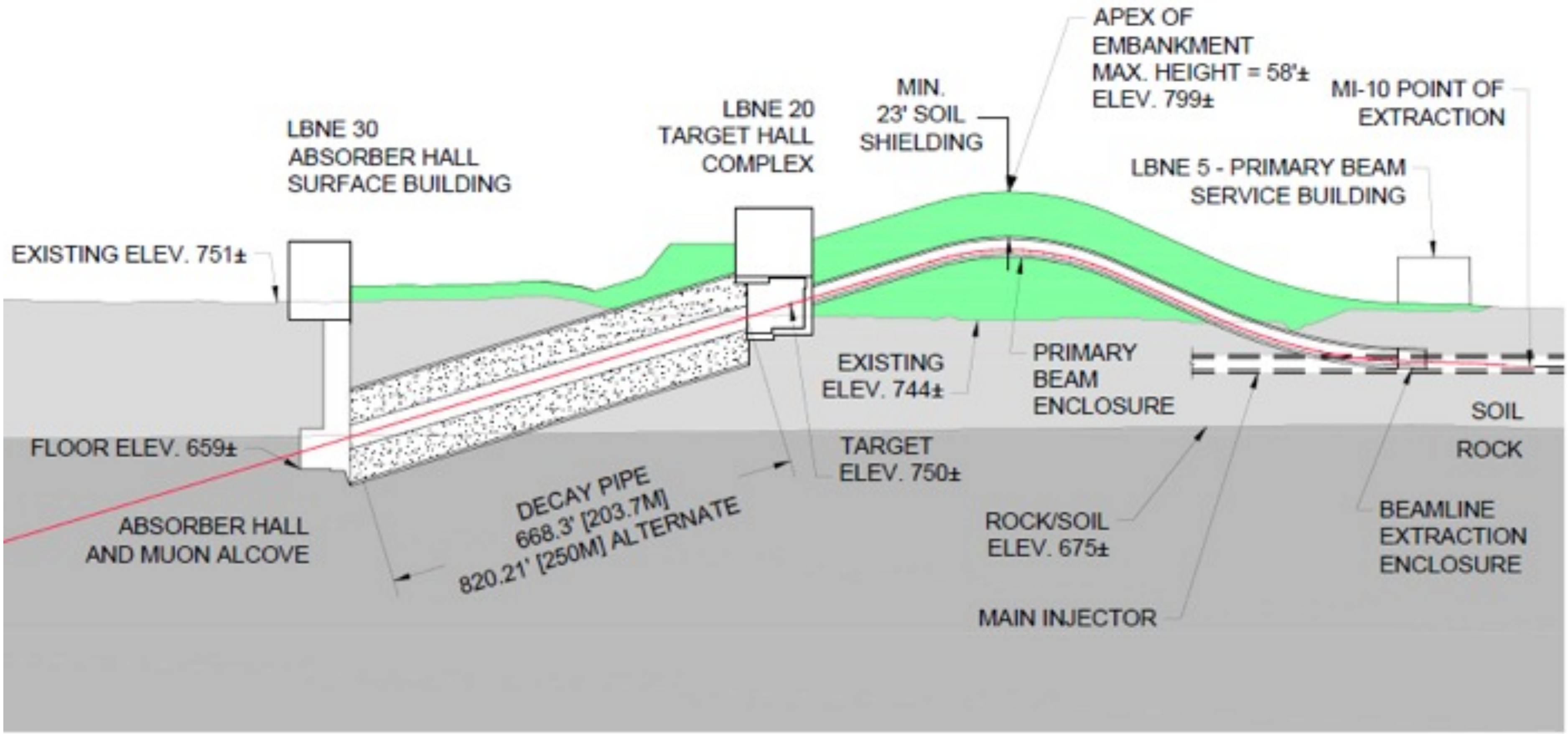
- Long enough to cleanly separate the $\nu$ / $\bar{\nu}$ oscillation asymmetry due to the matter effect from CP-violating effect.
- Long enough to put the first and if possible second oscillation maxima at “practical” energies.
- Short enough that the matter effect does not dominate over the CP-violating effect.
- Short enough that the beam is not too difficult to build (pitch angle).

=> 1300 km (Fermilab to Homestake) is “just right.”

# The Baseline

To do this we need the right baseline

- Long enough to cleanly separate the $\nu$ / $\bar{\nu}$ oscillation



# The Baseline

To do this we need the right baseline

- Long enough to cleanly separate the $\nu$ / $\bar{\nu}$ oscillation asymmetry due to the matter effect from CP-violating effect.
- Long enough to put the first and if possible second oscillation maxima at “practical” energies.
- Short enough that the matter effect does not dominate over the CP-violating effect.
- Short enough that the beam is not too difficult to build (pitch angle).

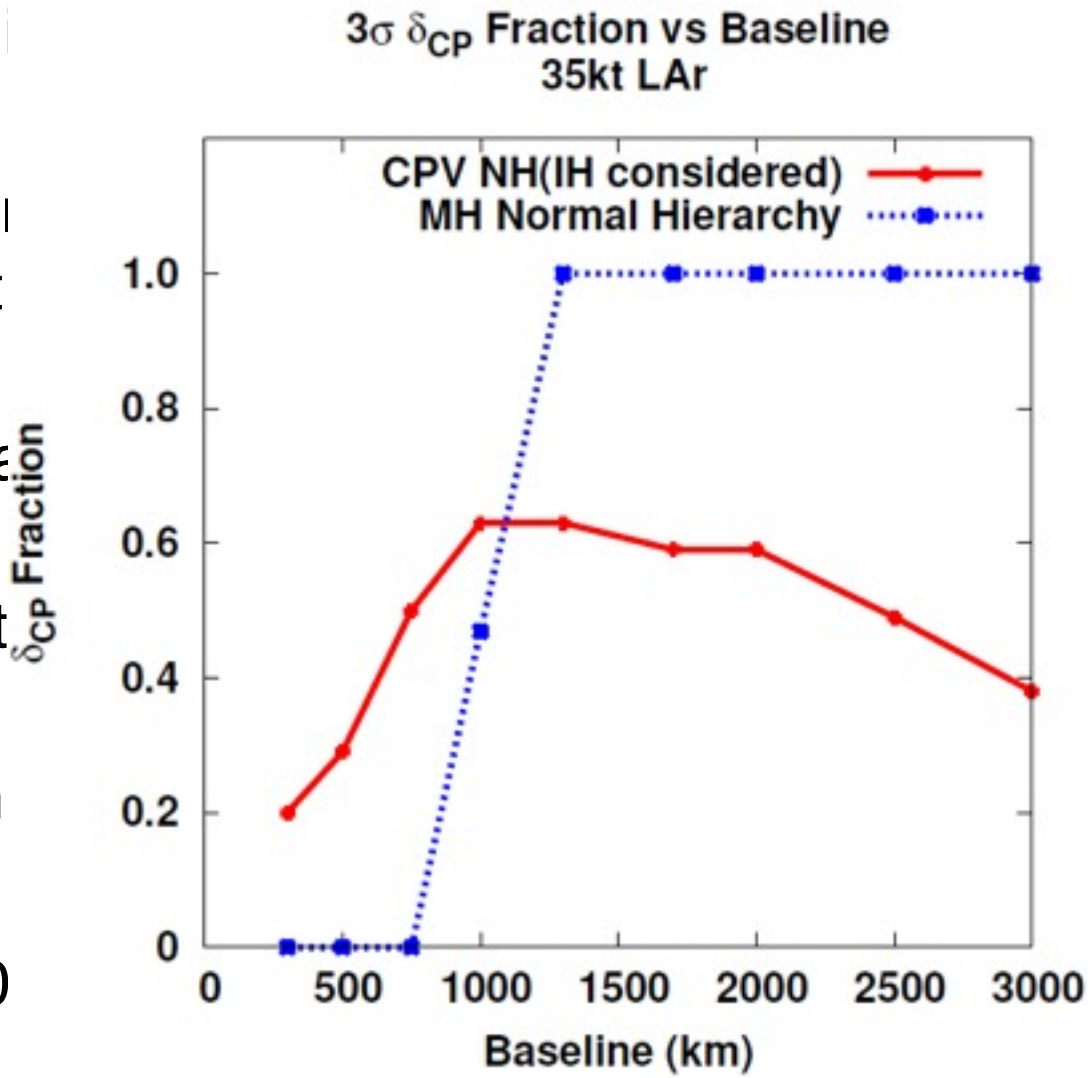
=> 1300 km (Fermilab to Homestake) is “just right.”

# The Baseline

To do thi

- Long … scillation asym… violating effect
- Long … econd oscill…
- Short … dominate over t…
- Short … lt to build (pitch

=> 1300 … ght."



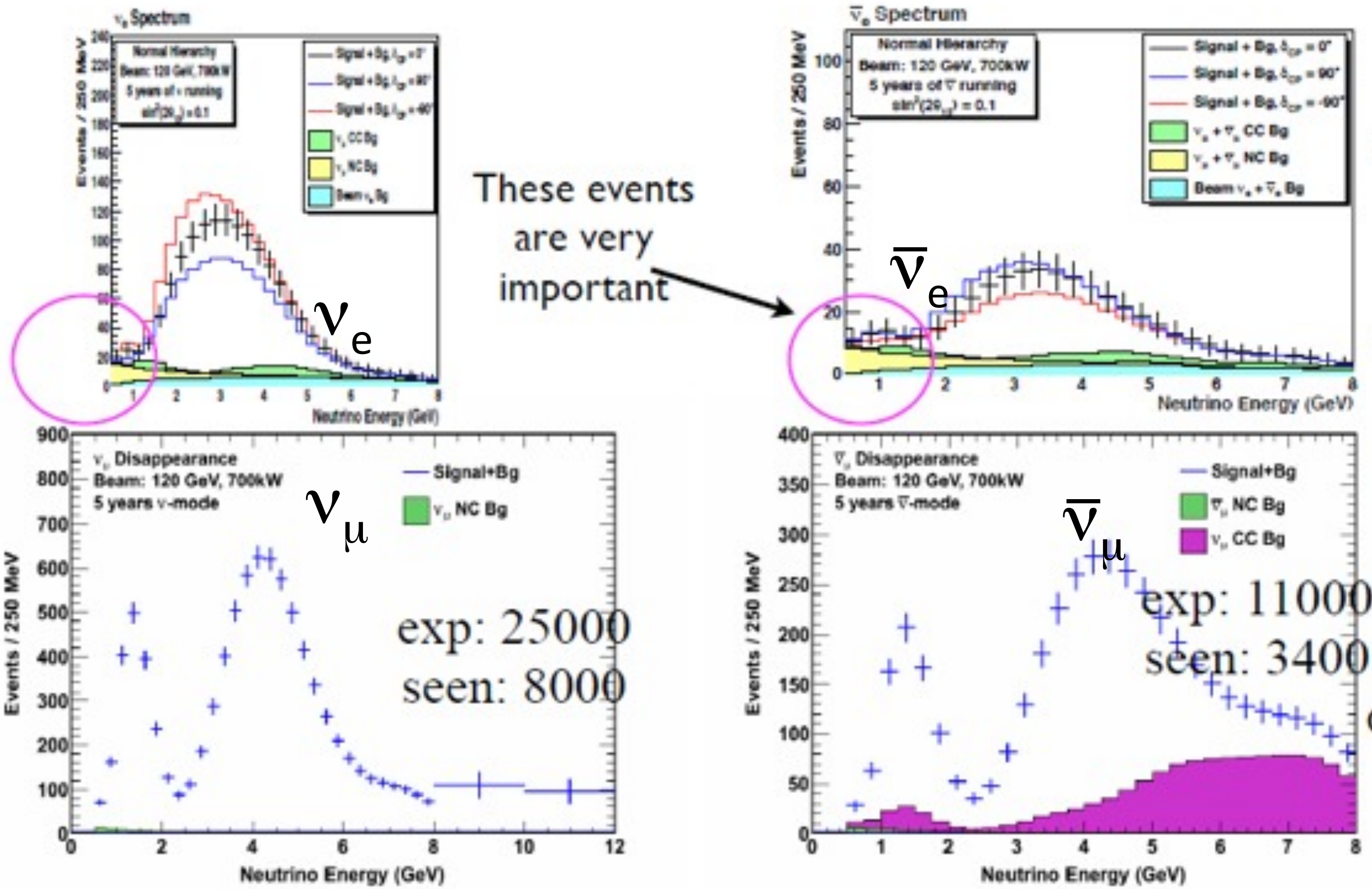
# The Baseline

To do this we need the right baseline

- Long enough to cleanly separate the $\nu$ / $\bar{\nu}$ oscillation asymmetry due to the matter effect from CP-violating effect.
- Long enough to put the first and if possible second oscillation maxima at “practical” energies.
- Short enough that the matter effect does not dominate over the CP-violating effect.
- Short enough that the beam is not too difficult to build (pitch angle).

=> 1300 km (Fermilab to Homestake) is “just right.”

# 1300 km expectation




For each bin, conversion fraction of electrons can be calculated. Matter effect can be substracted to obtain explicit CP signal.

Potential surprises:

Matter effect is not what is expected !

CPV does not have the proper energy 1/E dependence.

- With 1300 km the full structure of oscillations is visible in the energy spectrum. This spectral structure provides the unambiguous parameter sensitivity in a single experiment.

# The Far Detector

We need a large, highly capable detector to provide:

- High statistics for rare events ($\nu_e$ appearance and $\nu_\mu$ survival at oscillation max)
- Efficient detection of signal and rejection of backgrounds.
- Reconstruction of complex final states
- Placed at sufficient depth to suppress cosmic ray backgrounds to a negligible level.

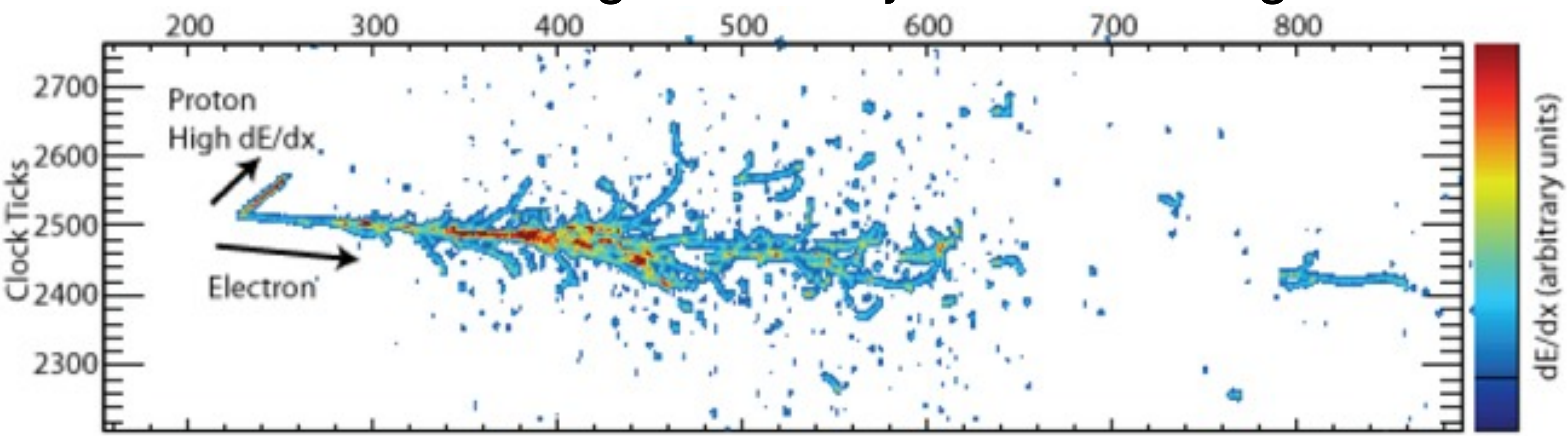
=> 34 kton LAr TPC underground at Homestake.

- Such a detector would be a powerful tool for other physics, including proton decay and supernova neutrinos.

# The Far Detector

We need a large, highly capable detector to provide:

- High statistics for rare events ($\nu_e$ appearance and $\nu_\mu$ survival at oscillation max)
- Efficient detection of signal and rejection of backgrounds.




- Such a detector would be a powerful tool for other physics, including proton decay and supernova neutrinos.

# The Far Detector

We need a large, highly capable detector to provide:

- High statistics for rare events ($\nu_e$ appearance and $\nu_\mu$ survival at oscillation max)
- Efficient detection of signal and rejection of backgrounds.
- Reconstruction of complex final states
- Placed at sufficient depth to suppress cosmic ray backgrounds to a negligible level.

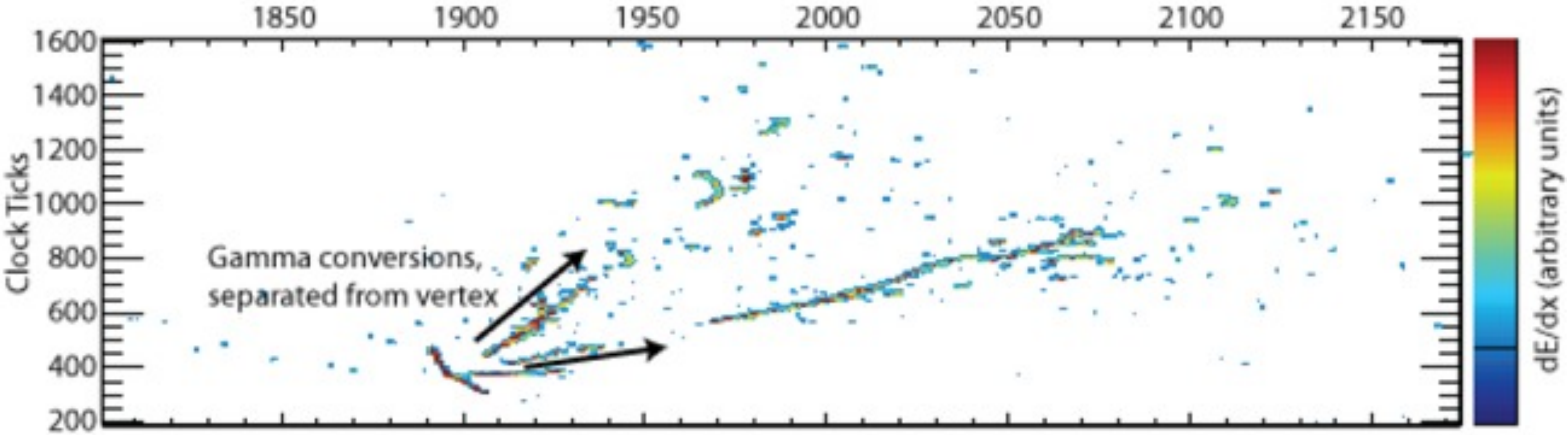
=> 34 kton LAr TPC underground at Homestake.

- Such a detector would be a powerful tool for other physics, including proton decay and supernova neutrinos.

# The Far Detector

We need a large, highly capable detector to provide:

- High statistics for rare events ($\nu_e$ appearance and $\nu_\mu$ survival at oscillation max)
- Efficient detection of signal and rejection of backgrounds.




- Such a detector would be a powerful tool for other physics, including proton decay and supernova neutrinos.

# The Far Detector

We need a large, highly capable detector to provide:

- High statistics for rare events ($\nu_e$ appearance and $\nu_\mu$ survival at oscillation max)
- Efficient detection of signal and rejection of backgrounds.
- Reconstruction of complex final states
- Placed at sufficient depth to suppress cosmic ray backgrounds to a negligible level.

=> 34 kton LAr TPC underground at Homestake.

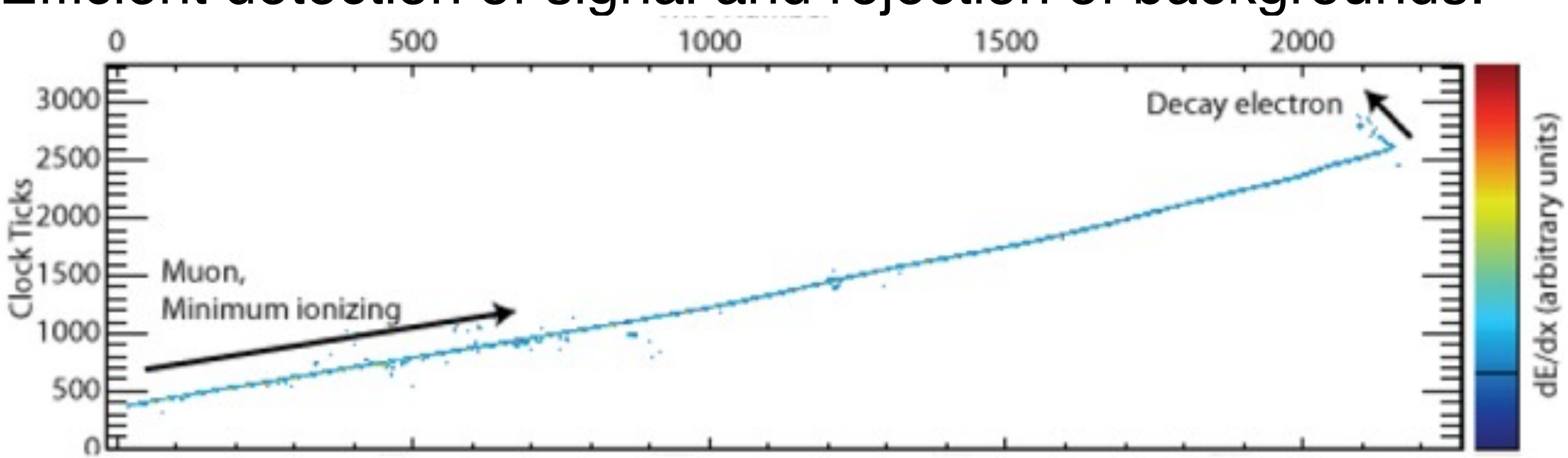
- Such a detector would be a powerful tool for other physics, including proton decay and supernova neutrinos.

# The Far Detector

We need a large, highly capable detector to provide:

- High statistics for rare events ($\nu_e$ appearance and $\nu_\mu$ survival at oscillation max)
- Efficient detection of signal and rejection of backgrounds.
- 
- 

=>




- Such a detector would be a powerful tool for other physics, including proton decay and supernova neutrinos.

# The Far Detector

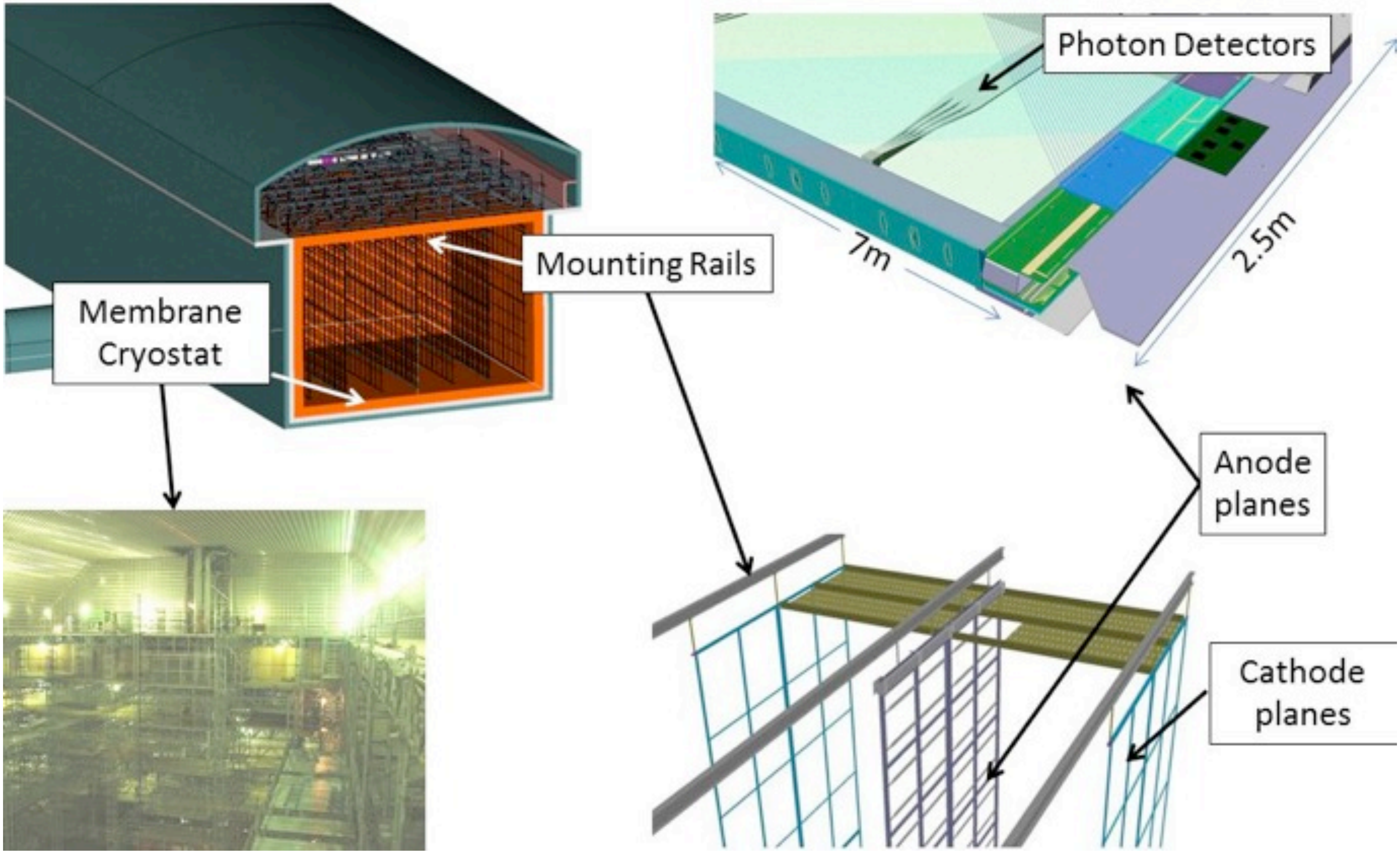
We need a large, highly capable detector to provide:

- High statistics for rare events ($\nu_e$ appearance and $\nu_\mu$ survival at oscillation max)
- Efficient detection of signal and rejection of backgrounds.
- Reconstruction of complex final states
- Placed at sufficient depth to suppress cosmic ray backgrounds to a negligible level.

=> 34 kton LAr TPC underground at Homestake.

- Such a detector would be a powerful tool for other physics, including proton decay and supernova neutrinos.

# The Far Detector



# The Far Detector

We need a large, highly capable detector to provide:

- High statistics for rare events ($\nu_e$ appearance and $\nu_\mu$ survival at oscillation max)
- Efficient detection of signal and rejection of backgrounds.
- Reconstruction of complex final states
- Placed at sufficient depth to suppress cosmic ray backgrounds to a negligible level.

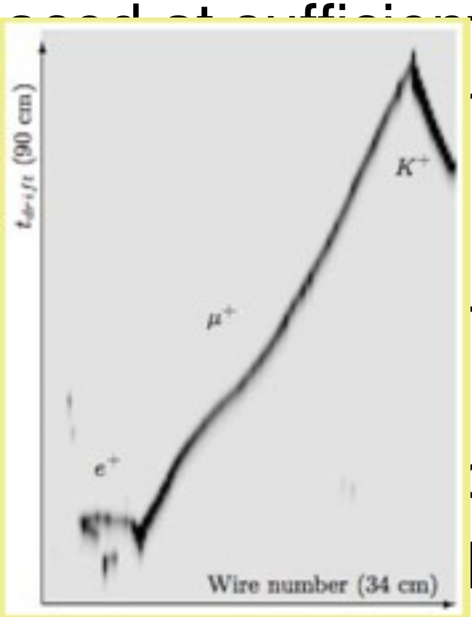
=> 34 kton LAr TPC underground at Homestake.

- Such a detector would be a powerful tool for other physics, including proton decay and supernova neutrinos.

# The Far Detector

We need a large, highly capable detector to provide:

- High statistics for rare events ($\nu_e$ appearance and $\nu_\mu$ survival at oscillation max)
- Efficient detection of signal and rejection of backgrounds.
- Reconstruction of complex final states
- Pl t depth to suppress cosmic ray ba negligible level.




=> 3 underground at Homestake.

- Su ould be a powerful tool for other ph proton decay and supernova neutrinos.

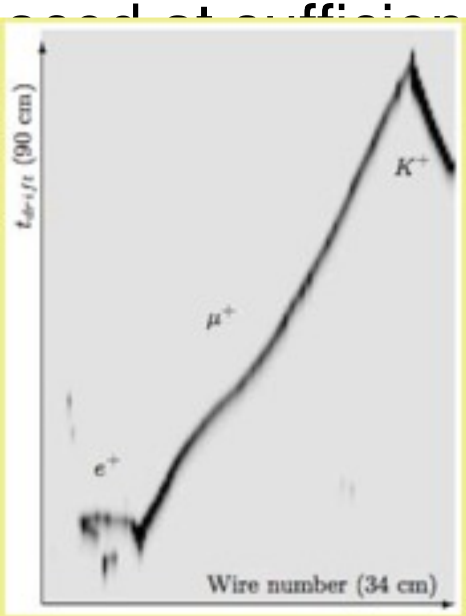
# The Far Detector

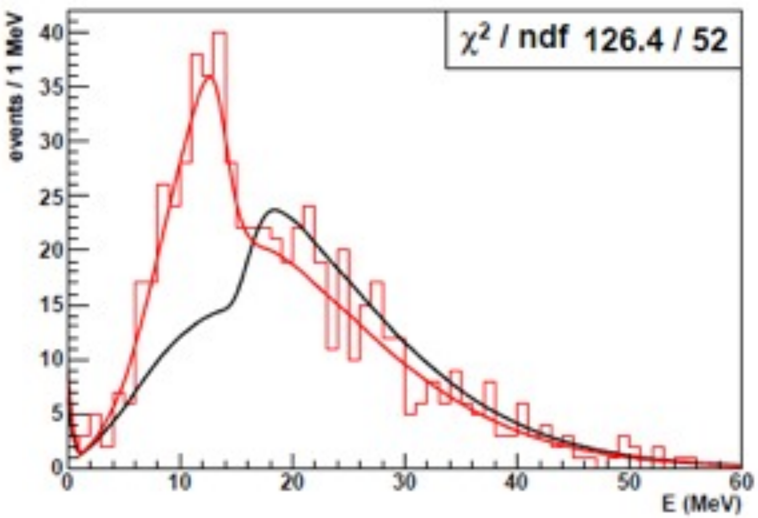
We need a large, highly capable detector to provide:

- High statistics for rare events ($\nu_e$ appearance and $\nu_\mu$ survival at oscillation max)
- Efficient detection of signal and rejection of backgrounds.
- Reconstruction of complex final states
- Pl t depth to suppress cosmic ray ba negligible

=> 3 undergro

- Su ould be a ph proton de os.



# The Far Detector

We need a large, highly capable detector to provide:

- High statistics for rare events ($\nu_e$ appearance and $\nu_\mu$ survival at oscillation max)
- Efficient detection of signal and rejection of backgrounds.
- Reconstruction of complex final states
- Placed at sufficient depth to suppress cosmic ray backgrounds to a negligible level.

=> 34 kton LAr TPC underground at Homestake.

- Such a detector would be a powerful tool for other physics, including proton decay and supernova neutrinos.

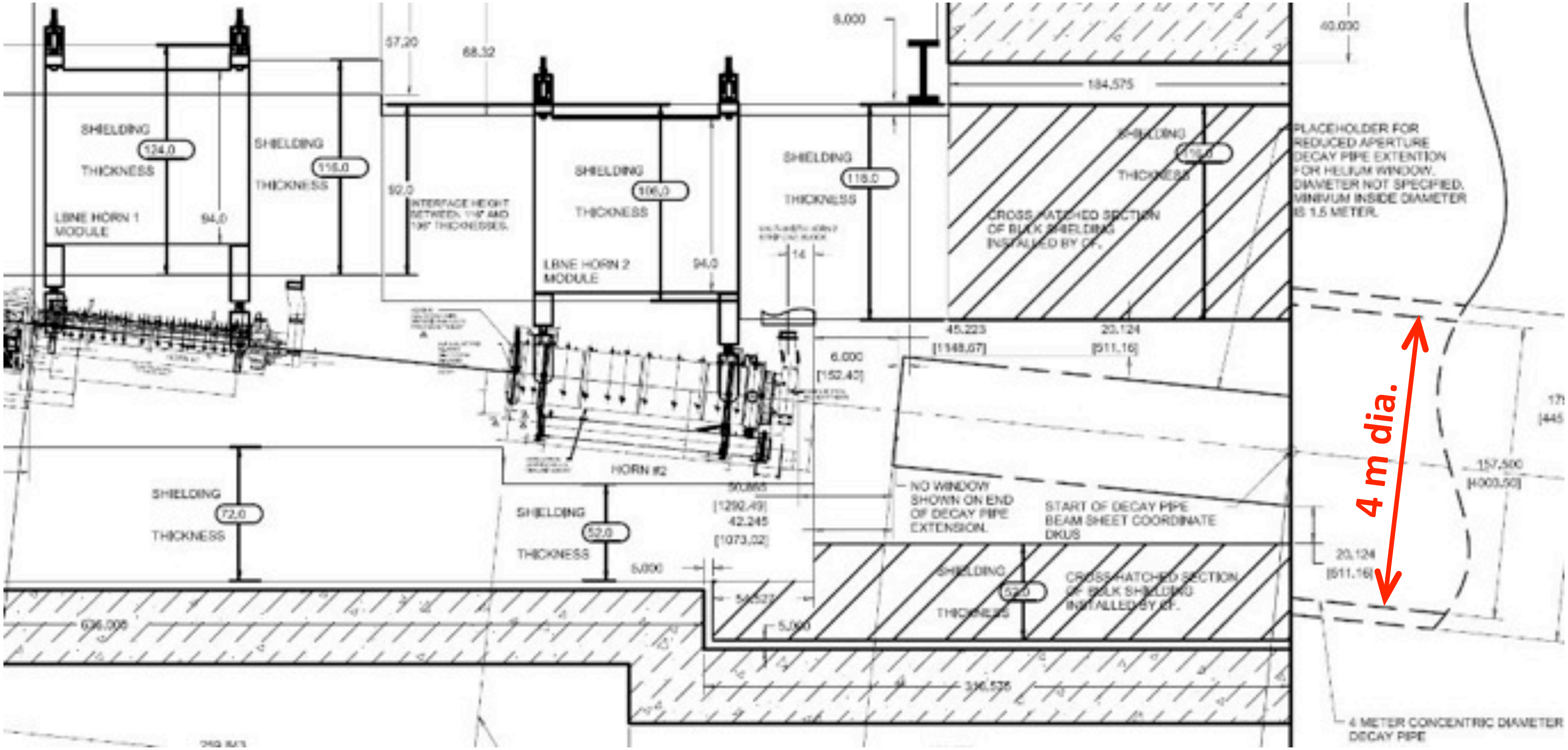
# The Neutrino Beam

We need a high-power, broad-band, high-purity neutrino beam, sign-selected beam.

- Broad-band, sign-selected => Horn Focused
- Cover first and if possible second oscillation max => large diameter decay pipe to collect low energy pions
- High purity => shorter decay pipe to reduce high-energy tail and minimize $\mu^{\pm} \rightarrow e^{\pm} \overset{(-)}{\nu}_e \overset{(-)}{\nu}_\mu$ decay in flight.
- Tunable over wide range of primary proton energy tunable spot size to optimize flux and allow study systematics.
- Capable of handling ≥ 2.3 MW from Project X.

# The Neutrino Beam

We need a high-power, broad-band, high-purity neutrino beam, sign-selected beam.



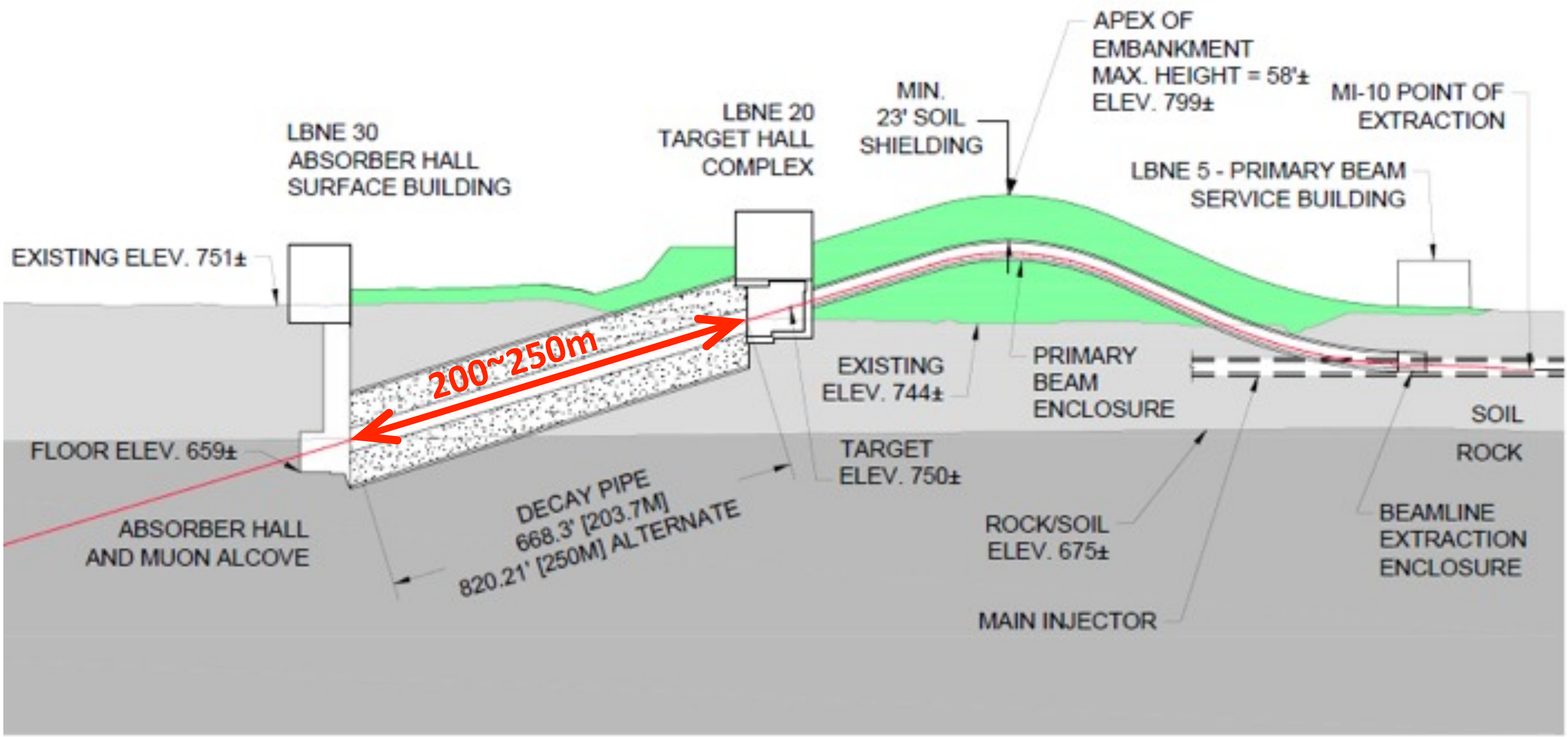
# The Neutrino Beam

We need a high-power, broad-band, high-purity neutrino beam, sign-selected beam.

- Broad-band, sign-selected => Horn Focused
- Cover first and if possible second oscillation max
  => large diameter decay pipe to collect low energy pions
- High purity => shorter decay pipe to reduce high-energy tail and minimize $\mu^{\pm} \rightarrow e^{\pm} \overset{(-)}{\nu}_e \overset{(-)}{\nu}_\mu$ decay in flight.
- Tunable over wide range of primary proton energy
  tunable spot size to optimize flux and allow study systematics.
- Capable of handling ≥ 2.3 MW from Project X.

# The Neutrino Beam

We need a high-power, broad-band, high-purity neutrino beam, sign-selected beam.



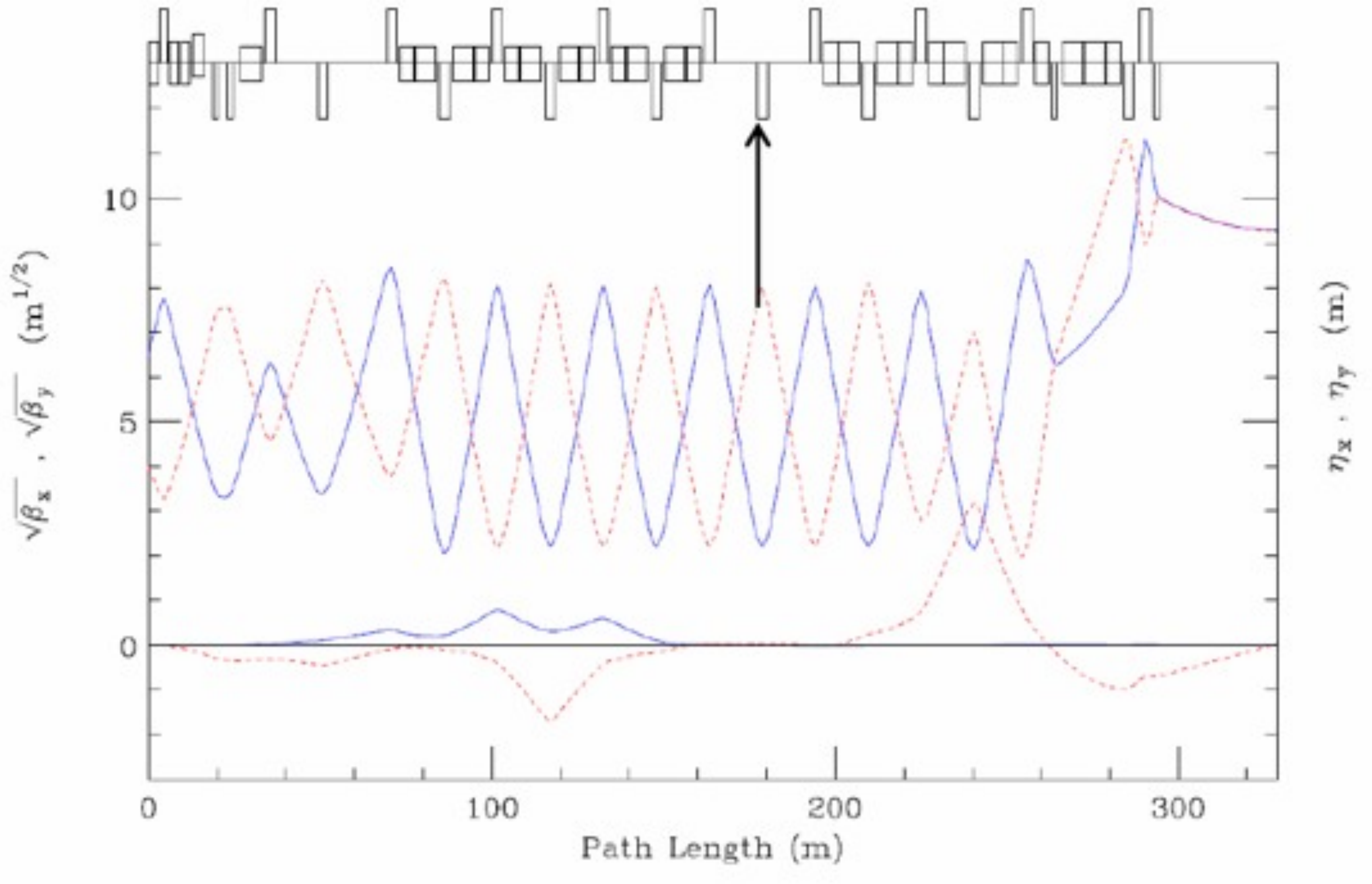
# The Neutrino Beam

We need a high-power, broad-band, high-purity neutrino beam, sign-selected beam.

- Broad-band, sign-selected => Horn Focused
- Cover first and if possible second oscillation max
  => large diameter decay pipe to collect low energy pions
- High purity => shorter decay pipe to reduce high-energy tail and minimize $\mu^{\pm} \rightarrow e^{\pm} \overset{(-)}{\nu}_e \overset{(-)}{\nu}_\mu$ decay in flight.
- Tunable over wide range of primary proton energy
  tunable spot size to optimize flux and allow study systematics.
- Capable of handling $\geq$ 2.3 MW from Project X.

# The Neutrino Beam

We need a high-power, broad-band, high-purity neutrino beam, sign-selected beam




ax

rgy pions

h-energy

rgy

dy

# The Neutrino Beam

We need a high-power, broad-band, high-purity neutrino beam, sign-selected beam.

- Broad-band, sign-selected => Horn Focused
- Cover first and if possible second oscillation max
  => large diameter decay pipe to collect low energy pions
- High purity => shorter decay pipe to reduce high-energy tail and minimize $\mu^{\pm} \rightarrow e^{\pm} \overset{(-)}{\nu}_e \overset{(-)}{\nu}_\mu$ decay in flight.
- Tunable over wide range of primary proton energy and tunable spot size to optimize flux and allow study systematics.
- Capable of handling ≥ 2.3 MW from Project X.

# The Near Detector

We need a highly capable near detector to:

- Measure the spectra of all species: $\nu_\mu$, $\nu_e$, $\bar{\nu}_\mu$, $\bar{\nu}_e$
  => magnetized detector with good $e^\pm$ capability.
- Measure events from the same target nucleus (Ar) and the same technique as the far detector.
- Measure cross-sections necessary for oscillation measurements.
- Two candidate detectors:
  - LAr TPC or
  - Straw Tube Tracker with embedded Ar Targets

# The Near Detector

We need a highly capable near detector to:

- Measure the spectra of all species: $\nu_\mu$, $\nu_e$, $\bar{\nu}_\mu$, $\bar{\nu}_e$

  => m

- Meas and

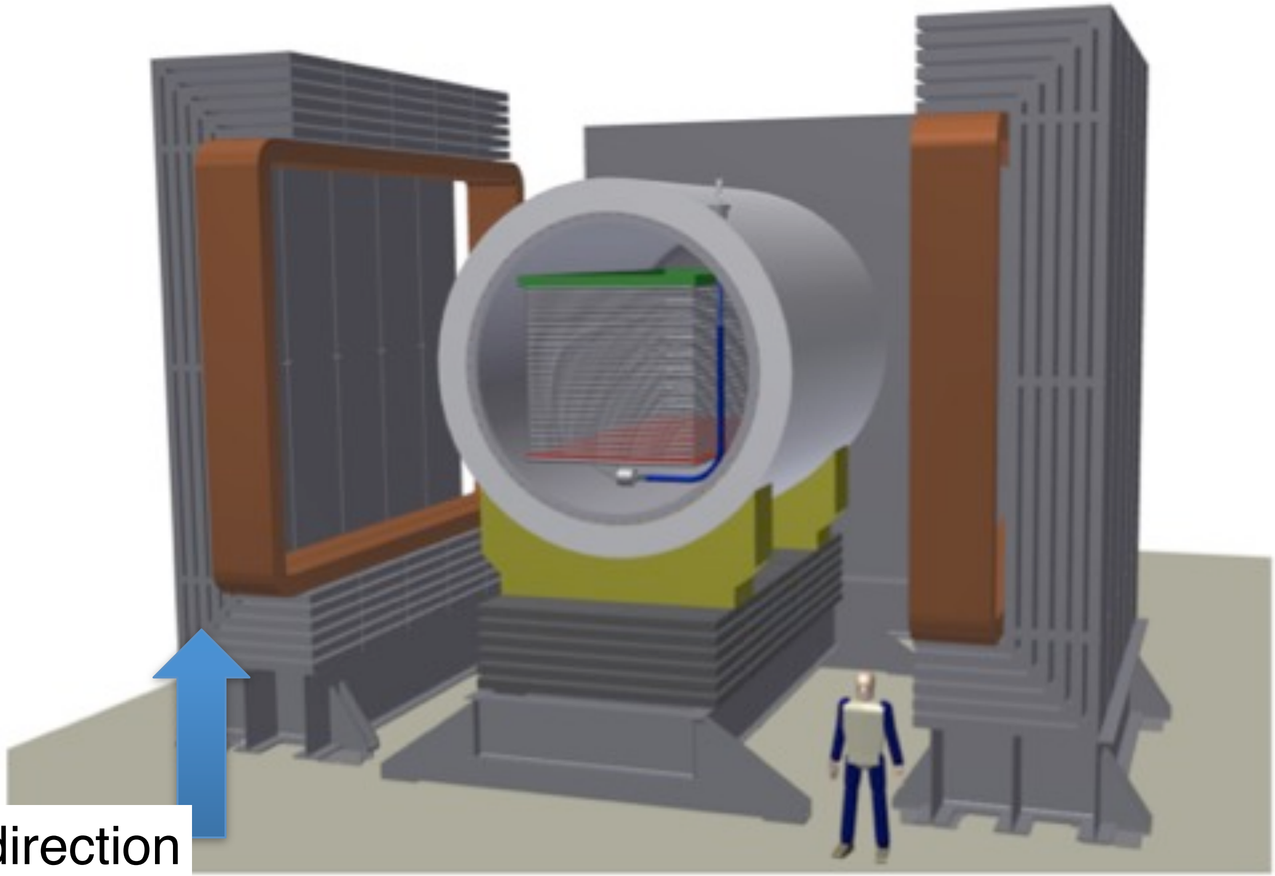
  the sa

- Meas

  meas

- Two

  - LAr
  - Stra



# The Near Detector

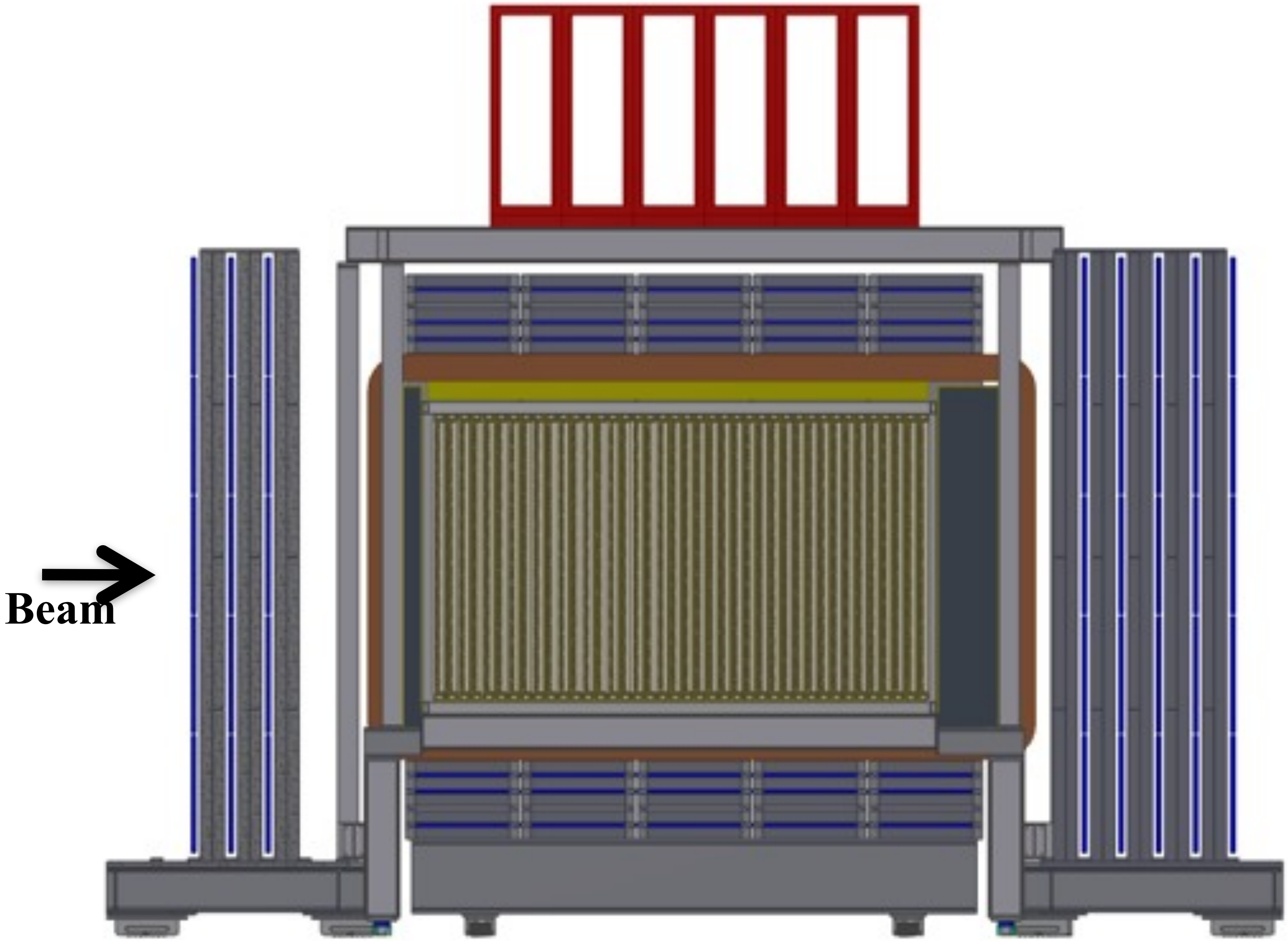
We need a highly capable near detector to:

- Measure the spectra of all species: $\nu_\mu$, $\nu_e$, $\bar{\nu}_\mu$, $\bar{\nu}_e$
  => magnetized detector with good $e^\pm$ capability.
- Measure events from the same target nucleus (Ar) and the same technique as the far detector.
- Measure cross-sections necessary for oscillation measurements.
- Two candidate detectors:
  - LAr TPC or
  - Straw Tube Tracker with embedded Ar Targets

# The Near Detector

We need a

- Measure
  => magı
- Measure
  the sam
- Measure
  measure
- Two car
  - LAr TF
  - Straw



# The Near Detector

We need a highly capable near detector to:

- Measure the spectra of all species: $\nu_\mu$, $\nu_e$, $\bar{\nu}_\mu$, $\bar{\nu}_e$
  => magnetized detector with good $e^\pm$ capability.
- Measure events from the same target nucleus (Ar) and the same technique as the far detector.
- Measure cross-sections necessary for oscillation measurements.
- Two candidate detectors:
  - LAr TPC or
  - Straw Tube Tracker with embedded Ar Targets

# Vision Encounters Reality

# Vision Encounters Reality

**Department of Energy**
Office of Science
Washington, DC 20585

**Office of the Director**

March 19, 2012

## Received on March 26

Dr. Pier Oddone
Director
Fermilab
Wilson and Kirks Road
Batavia, IL 60510-5011

Dear Pier,

Thank you for your recent presentation on the status and plans for the Long Baseline Neutrino Experiment (LBNE). The project team and the scientific collaboration have done an excellent job responding to our requests to assess the technology choices and refine the cost estimates for LBNE. We believe that the conceptual design is well advanced and the remaining technical issues are understood.

The scientific community and the National Academy of Sciences repeatedly have examined and endorsed the case for underground science. We concur with this conclusion, and this has been the motivator for us to determine a path forward as quickly as possible following the decision of the National Science Board to terminate development of the Homestake Mine as a site for underground science.

We have considered both the science opportunities and the cost and schedule estimates for LBNE that you have presented to us. We have done so in the context of planning for the overall Office of Science program as well as current budget projections.

Based on our considerations, we cannot support the LBNE project as it is currently configured. This decision is not a negative judgment about the importance of the science, but rather it is a recognition that the peak cost of the project cannot be accommodated in the current budget climate or that projected for the next decade.

In order to advance this activity on a sustainable path, I would like Fermilab to lead the development of an affordable and phased approach that will enable important science results at each phase. Alternative configurations to LBNE should also be considered. Options that allow us to independently develop the Homestake Mine as a future facility for dark matter experiments should be included in your considerations.

A report outlining options and alternatives is needed as soon as practical to provide input to our strategic plan for the Intensity Frontier program. OHEP will provide additional details on realistic cost and schedule profiles and on the due date for the report.

Thank you,

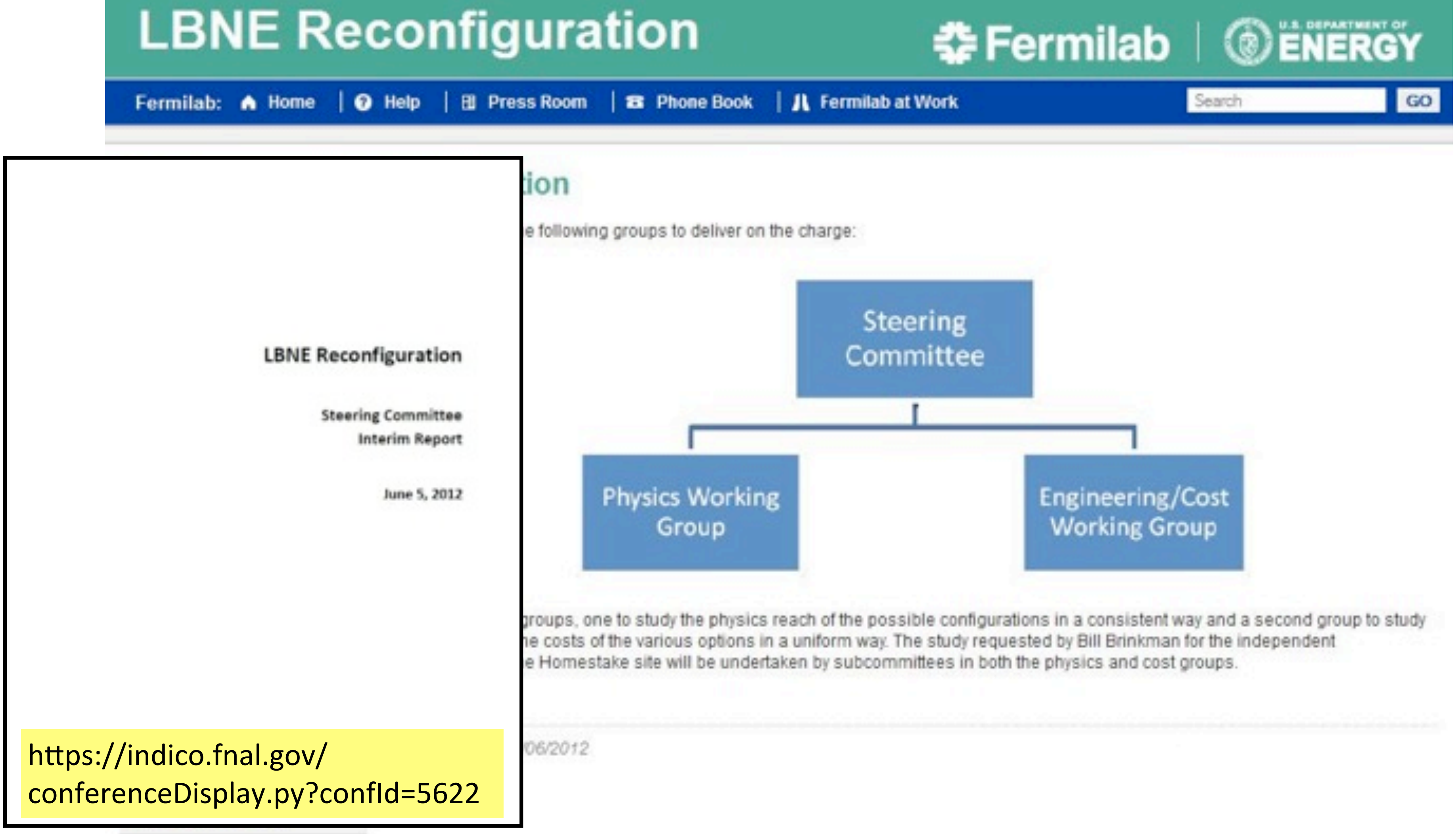
W. F. Brinkman
Director, Office of Science

# Reconfiguring LBNE

http://www.fnal.gov/directorate/lbne_reconfiguration/index.shtml

# Reconfiguring LBNE

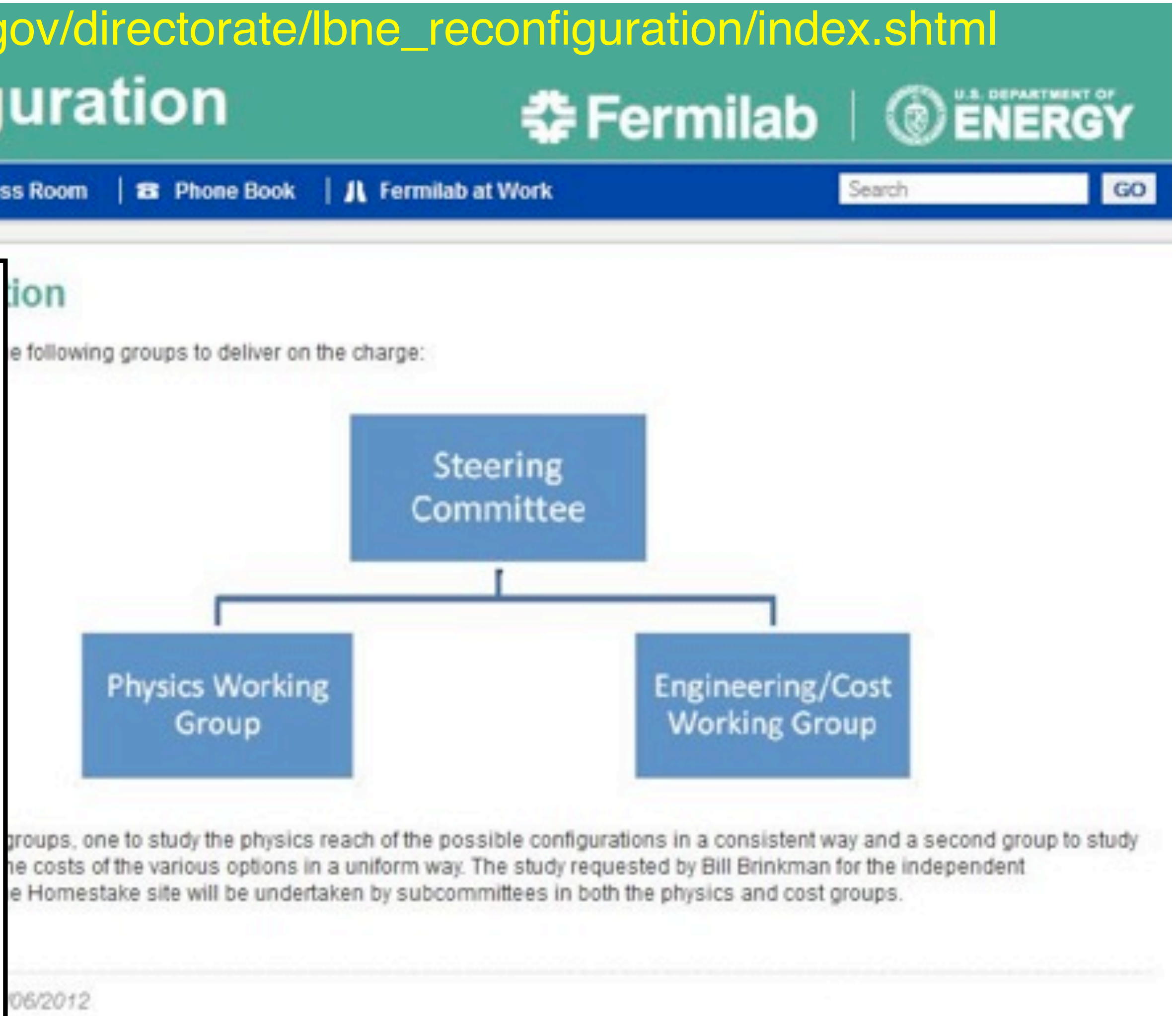
http://www.fnal.gov/directorate/lbne_reconfiguration/index.shtml

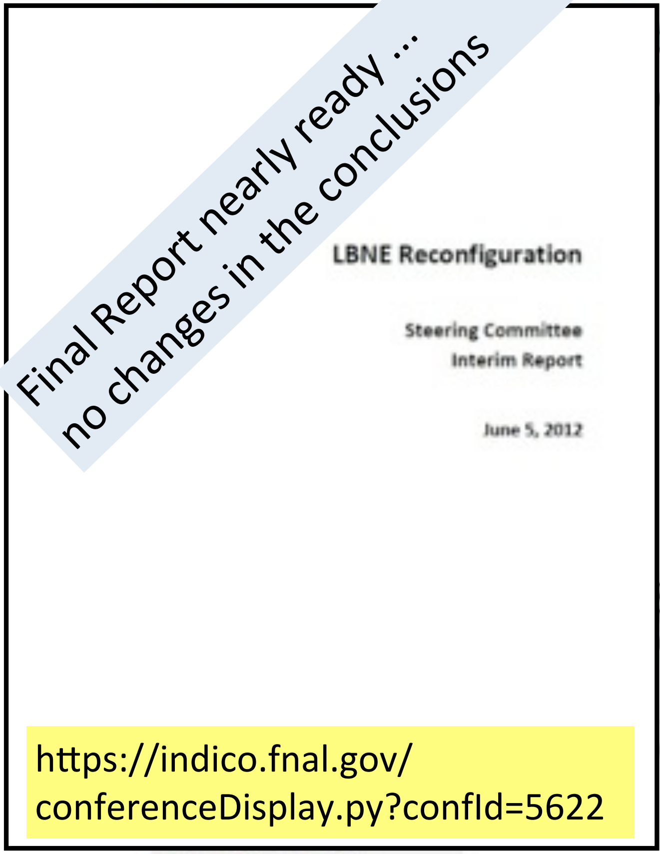



https://indico.fnal.gov/conferenceDisplay.py?confId=5622

# Reconfiguring LBNE

http://www.fnal.gov/directorate/lbne_reconfiguration/index.shtml




Final Report nearly ready ...
no changes in the conclusions

https://indico.fnal.gov/conferenceDisplay.py?confId=5622

# Reconfiguration Interim Report



## Interim Conclusions

To achieve all of the fundamental science goals listed above, a reconfigured LBNE would need a very long baseline (>1,000 km from accelerator to detector) and a large detector deep underground. However, it is not possible to meet both of these requirements in a first phase of the experiment within the budget guideline of approximately $700M – $800M, including contingency and escalation. The committee assessed various options that meet some of the requirements, and identified three viable options for the first phase of a long-baseline experiment that have the potential to accomplish important science at realizable cost. These options are (not priority ordered):

- Using the existing NuMI beamline in the low energy configuration with a **30 kton** liquid argon time projection chamber (LAr-TPC) **surface detector** 14 mrad off-axis at Ash River in Minnesota, **810 km** from Fermilab.

- Using the existing NuMI beamline in the low energy configuration with a **15 kton** LAr-TPC **underground (at the 2,340 ft level) detector** on-axis at the Soudan Lab in Minnesota, **735 km** from Fermilab.

- Constructing a new low energy LBNE beamline with a **10 kton** LAr-TPC **surface detector** on-axis at Homestake in South Dakota, **1,300 km** from Fermilab.

The committee looked at possibilities of projects with significantly lower costs and concluded that the science reach for such projects becomes marginal.

# Reconfiguration Interim Report

## Interim Conclusions

To achieve all of the fundamental science goals listed above, a reconfigured LBNE would need a very long baseline (>1,000 km from accelerator to detector) and a large detector deep underground. However, it is not possible to meet both of these requirements in a first phase of the experiment within the budget guideline of approximately $700M – $800M, including contingency and escalation. The committee assessed various options that meet some of the requirements, and identified three viable options for the first phase of a long-baseline experiment that have the potential to accomplish important science at realizable cost. These options are (not priority ordered):

- Using the existing NuMI beamline in the low energy configuration with a **30 kton** liquid argon time projection chamber (LAr-TPC) **surface detector** 14 mrad off-axis at Ash River in Minnesota, **810 km** from Fermilab.
- Using the existing NuMI beamline in the low energy configuration with a **15 kton** LAr-TPC **underground (at the 2,340 ft level) detector** on-axis at the Soudan Lab in Minnesota, **735 km** from Fermilab.
- Constructing a new low energy LBNE beamline with a **10 kton** LAr-TPC **surface detector** on-axis at Homestake in South Dakota, **1,300 km** from Fermilab.

The committee looked at possibilities of projects with significantly lower costs and concluded that the science reach for such projects becomes marginal.

# Reconfiguration Interim Report

## Interim Conclusions

To achieve all of the fundamental science goals listed above, a reconfigured LBNE would need a very long baseline (>1,000 km from accelerator to detector) and a large detector deep underground. However, it is not possible to meet both of these requirements in a first phase of the experiment within the budget guideline of approximately $700M – $800M, including contingency and escalation. The committee assessed various options that meet some of the requirements, and identified three viable options for the first phase of a long-baseline experiment that have the potential to accomplish important science at realizable cost. These options are (not priority ordered):

- Using the existing NuMI beamline in the low energy configuration with a **30 kton** liquid argon time projection chamber (LAr-TPC) **surface detector** 14 mrad off-axis at Ash River in Minnesota, **810 km** from Fermilab.

- Using the existing NuMI beamline in the low energy configuration with a **15 kton** LAr-TPC **underground (at the 2,340 ft level) detector** on-axis at the Soudan Lab in Minnesota, **735 km** from Fermilab.

- Constructing a new low energy LBNE beamline with a **10 kton** LAr-TPC **surface detector** on-axis at Homestake in South Dakota, **1,300 km** from Fermilab.

The committee looked at possibilities of projects with significantly lower costs and concluded that the science reach for such projects becomes marginal.

# Reconfiguration Interim Report

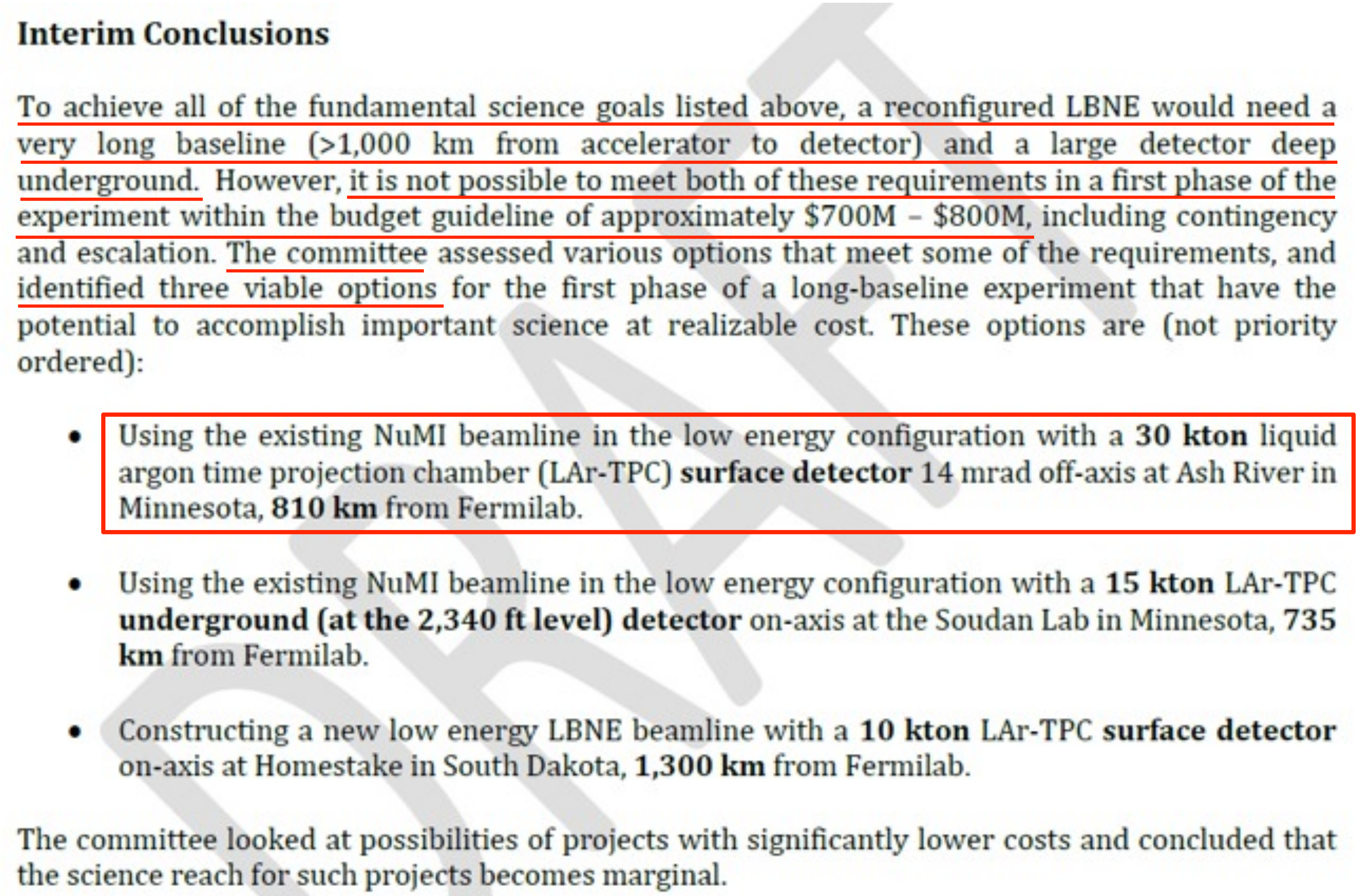
## Interim Conclusions

To achieve all of the fundamental science goals listed above, a reconfigured LBNE would need a very long baseline (>1,000 km from accelerator to detector) and a large detector deep underground. However, it is not possible to meet both of these requirements in a first phase of the experiment within the budget guideline of approximately $700M – $800M, including contingency and escalation. The committee assessed various options that meet some of the requirements, and identified three viable options for the first phase of a long-baseline experiment that have the potential to accomplish important science at realizable cost. These options are (not priority ordered):

- Using the existing NuMI beamline in the low energy configuration with a **30 kton** liquid argon time projection chamber (LAr-TPC) **surface detector** 14 mrad off-axis at Ash River in Minnesota, **810 km** from Fermilab.
- Using the existing NuMI beamline in the low energy configuration with a **15 kton** LAr-TPC **underground (at the 2,340 ft level) detector** on-axis at the Soudan Lab in Minnesota, **735 km** from Fermilab.
- Constructing a new low energy LBNE beamline with a **10 kton** LAr-TPC **surface detector** on-axis at Homestake in South Dakota, **1,300 km** from Fermilab.

The committee looked at possibilities of projects with significantly lower costs and concluded that the science reach for such projects becomes marginal.

# Reconfiguration Interim Report



## Interim Conclusions

To achieve all of the fundamental science goals listed above, a reconfigured LBNE would need a very long baseline (>1,000 km from accelerator to detector) and a large detector deep underground. However, it is not possible to meet both of these requirements in a first phase of the experiment within the budget guideline of approximately $700M – $800M, including contingency and escalation. The committee assessed various options that meet some of the requirements, and identified three viable options for the first phase of a long-baseline experiment that have the potential to accomplish important science at realizable cost. These options are (not priority ordered):

- Using the existing NuMI beamline in the low energy configuration with a **30 kton** liquid argon time projection chamber (LAr-TPC) **surface detector** 14 mrad off-axis at Ash River in Minnesota, **810 km** from Fermilab.
- Using the existing NuMI beamline in the low energy configuration with a **15 kton** LAr-TPC **underground (at the 2,340 ft level) detector** on-axis at the Soudan Lab in Minnesota, **735 km** from Fermilab.
- Constructing a new low energy LBNE beamline with a **10 kton** LAr-TPC **surface detector** on-axis at Homestake in South Dakota, **1,300 km** from Fermilab.

The committee looked at possibilities of projects with significantly lower costs and concluded that the science reach for such projects becomes marginal.

# Reconfiguration Interim Report

## Interim Conclusions

To achieve all of the fundamental science goals listed above, a reconfigured LBNE would need a very long baseline (>1,000 km from accelerator to detector) and a large detector deep underground. However, it is not possible to meet both of these requirements in a first phase of the experiment within the budget guideline of approximately $700M – $800M, including contingency and escalation. The committee assessed various options that meet some of the requirements, and identified three viable options for the first phase of a long-baseline experiment that have the potential to accomplish important science at realizable cost. These options are (not priority ordered):

- Using the existing NuMI beamline in the low energy configuration with a **30 kton** liquid argon time projection chamber (LAr-TPC) **surface detector** 14 mrad off-axis at Ash River in Minnesota, **810 km** from Fermilab.
- Using the existing NuMI beamline in the low energy configuration with a **15 kton** LAr-TPC **underground (at the 2,340 ft level) detector** on-axis at the Soudan Lab in Minnesota, **735 km** from Fermilab.
- Constructing a new low energy LBNE beamline with a **10 kton** LAr-TPC **surface detector** on-axis at Homestake in South Dakota, **1,300 km** from Fermilab.

The committee looked at possibilities of projects with significantly lower costs and concluded that the science reach for such projects becomes marginal.

# Reconfiguration Interim Report

**Interim Conclusions**

To achieve all of the fundamental science goals listed above, a reconfigured LBNE would need a very long baseline (>1,000 km from accelerator to detector) and a large detector deep underground. However, it is not possible to meet both of these requirements in a first phase of the experiment within the budget guideline of approximately $700M – $800M, including contingency and escalation. The committee assessed various options that meet some of the requirements, and identified three viable options for the first phase of a long-baseline experiment that have the potential to accomplish important science at realizable cost. These options are (not priority ordered):

- Using the existing NuMI beamline in the low energy configuration with a **30 kton** liquid argon time projection chamber (LAr-TPC) **surface detector** 14 mrad off-axis at Ash River in Minnesota, **810 km** from Fermilab.
- Using the existing NuMI beamline in the low energy configuration with a **15 kton** LAr-TPC **underground (at the 2,340 ft level) detector** on-axis at the Soudan Lab in Minnesota, **735 km** from Fermilab.
- Constructing a new low energy LBNE beamline with a **10 kton** LAr-TPC **surface detector** on-axis at Homestake in South Dakota, **1,300 km** from Fermilab.

The committee looked at possibilities of projects with significantly lower costs and concluded that the science reach for such projects becomes marginal.

# Pros and Cons

30 kton surface detector at Ash River in Minnesota (NuMI low energy beam, 810 km baseline)

| | |
|---|---|
| Pros | • Best Phase 1 CP-violation sensitivity in combination with NOvA and T2K results for the current value of $\theta_{13}$. The sensitivity would be enhanced if the mass ordering were known from other experiments.<br>• Excellent (3σ) mass ordering reach in nearly half of the $\delta_{CP}$ range. |
| Cons | • Narrow-band beam does not allow measurement of oscillatory signature.<br>• Shorter baseline risks fundamental ambiguities in interpreting results.<br>• Sensitivity decreases if $\theta_{13}$ is smaller than the current experimental value.<br>• Cosmic ray backgrounds: impact and mitigation need to be determined.<br>• Only accelerator-based physics.<br>• Limited Phase 2 path:<br>○ Beam limited to 1.1 MW (Project X Stage 1).<br>○ Phase 2 could be a 15-20 kton underground (2,340 ft) detector at Soudan. |

15 kton underground (2,340 ft) detector at the Soudan Lab in Minnesota (NuMI low energy beam, 735 km baseline)

| | |
|---|---|
| Pros | • Broadest Phase 1 physics program:<br>○ Accelerator-based physics including good (2σ) mass ordering and good CP-violation reach in half of the $\delta_{CP}$ range. CP-violation reach would be enhanced if the mass ordering were known from other experiments.<br>○ Non-accelerator physics including proton decay, atmospheric neutrinos, and supernovae neutrinos.<br>• Cosmic ray background risks mitigated by underground location. |
| Cons | • Mismatch between beam spectrum and shorter baseline does not allow full measurement of oscillatory signature.<br>• Shorter baseline risks fundamental ambiguities in interpreting results. This risk is greater than for the Ash River option.<br>• Sensitivity decreases if $\theta_{13}$ is smaller than the current experimental value.<br>• Limited Phase 2 path:<br>○ Beam limited to 1.1 MW (Project X Stage 1).<br>○ Phase 2 could be a 30 kton surface detector at Ash River or an additional 25-30 kton underground (2,340 ft) detector at Soudan. |

10 kton surface detector at Homestake (new beamline, 1,300 km baseline)

| | |
|---|---|
| Pros | • Excellent (3σ) mass ordering reach in the full $\delta_{CP}$ range.<br>• Good CP violation reach: not dependent on *a priori* knowledge of the mass ordering.<br>• Longer baseline and broad-band beam allow explicit reconstruction of oscillations in the energy spectrum: self-consistent standard neutrino measurements; best sensitivity to Standard Model tests and non-standard neutrino physics.<br>• Clear Phase 2 path: a 20 – 25 kton underground (4850 ft) detector at the Homestake mine. This covers the full capability of the original LBNE physics program.<br>• Takes full advantage of Project X beam power increases. |
| Cons | • Cosmic ray backgrounds: impact and mitigation need to be determined.<br>• Only accelerator-based physics. Proton decay, supernova neutrino and atmospheric neutrino research are delayed to Phase 2.<br>• ~10% more expensive than the other two options: cost evaluations and value engineering exercises in progress. |

Fundamental Trade-offs

- Larger detector on the surface *vs.* smaller underground
- Use existing beamline => more $ for detectors in first phase *vs.* new beamline with desired baseline and upgrade path => less $ for detectors in first phase.

# Steering Committee Conclusions

While each of these first-phase options is more sensitive than the others in some particular physics domain, the Steering Committee in its discussions strongly favored the option to build a new beamline to Homestake with an initial 10 kton LAr-TPC detector on the surface. The physics reach of this first phase is very strong; more over this option is seen by the Steering Committee as a start of a long-term world-leading program that would achieve the full goals of LBNE in time and allow probing the Standard Model most incisively beyond its current state. Ultimately this option would exploit the full power provided by Project X. At the present level of cost estimation, it appears that this preferred option may be ~10% more expensive than the other two options, but cost evaluations and value engineering exercises are continuing.

# Steering Committee Conclusions

While each of these first-phase options is more sensitive than the others in some particular physics domain, the Steering Committee in its discussions strongly favored the option to build a new beamline to Homestake with an initial 10 kton LAr-TPC detector on the surface. The physics reach of this first phase is very strong; more over this option is seen by the Steering Committee as a start of a long-term world-leading program that would achieve the full goals of LBNE in time and allow probing the Standard Model most incisively beyond its current state. Ultimately this option would exploit the full power provided by Project X. At the present level of cost estimation, it appears that this preferred option may be ~10% more expensive than the other two options, but cost evaluations and value engineering exercises are continuing.

# Steering Committee Conclusions

While each of these first-phase options is more sensitive than the others in some particular physics domain, the Steering Committee in its discussions strongly favored the option to build a new beamline to Homestake with an initial 10 kton LAr-TPC detector on the surface. The physics reach of this first phase is very strong; more over this option is seen by the Steering Committee as a start of a long-term world-leading program that would achieve the full goals of LBNE in time and allow probing the Standard Model most incisively beyond its current state. Ultimately this option would exploit the full power provided by Project X. At the present level of cost estimation, it appears that this preferred option may be ~10% more expensive than the other two options, but cost evaluations and value engineering exercises are continuing.

# Steering Committee Conclusions

While each of these first-phase options is more sensitive than the others in some particular physics domain, the Steering Committee in its discussions strongly favored the option to build a new beamline to Homestake with an initial 10 kton LAr-TPC detector on the surface. The physics reach of this first phase is very strong; more over this option is seen by the Steering Committee as a start of a long-term world-leading program that would achieve the full goals of LBNE in time and allow probing the Standard Model most incisively beyond its current state. Ultimately this option would exploit the full power provided by Project X. At the present level of cost estimation, it appears that this preferred option may be ~10% more expensive than the other two options, but cost evaluations and value engineering exercises are continuing.

*But there are risks:*

# Steering Committee Conclusions

While each of these first-phase options is more sensitive than the others in some particular physics domain, the Steering Committee in its discussions strongly favored the option to build a new beamline to Homestake with an initial 10 kton LAr-TPC detector on the surface. The physics reach of this first phase is very strong; more over this option is seen by the Steering Committee as a start of a long-term world-leading program that would achieve the full goals of LBNE in time and allow probing the Standard Model most incisively beyond its current state. Ultimately this option would exploit the full power provided by Project X. At the present level of cost estimation, it appears that this preferred option may be ~10% more expensive than the other two options, but cost evaluations and value engineering exercises are continuing.

*But there are risks:*

In the next few months the LBNE collaboration and external experts will be studying the operation of LAr-TPCs on the surface to verify that the cosmic ray backgrounds are manageable. The operation on the surface may require shorter drift times than required for underground operations and the localization of the event in the TPC coincident with the ten microsecond-long beam from Fermilab. The Phase 1 experiment will use the existing detectors (MINOS near detector, MINERvA, and NOvA near detector) as near detectors for the two NuMI options, and use muon detectors to monitor the beam for the Homestake option. The Physics working group is currently studying the impact of near detectors on the physics reach.

# Steering Committee Conclusions

While each of these first-phase options is more sensitive than the others in some particular physics domain, the Steering Committee in its discussions strongly favored the option to build a new beamline to Homestake with an initial 10 kton LAr-TPC detector on the surface. The physics reach of this first phase is very strong; more over this option is seen by the Steering Committee as a start of a long-term world-leading program that would achieve the full goals of LBNE in time and allow probing the Standard Model most incisively beyond its current state. Ultimately this option would exploit the full power provided by Project X. At the present level of cost estimation, it appears that this preferred option may be ~10% more expensive than the other two options, but cost evaluations and value engineering exercises are continuing.

*But there are risks:*

In the next few months the LBNE collaboration and external experts will be studying the operation of LAr-TPCs on the surface to verify that the cosmic ray backgrounds are manageable. The operation on the surface may require shorter drift times than required for underground operations and the localization of the event in the TPC coincident with the ten microsecond-long beam from Fermilab. The Phase 1 experiment will use the existing detectors (MINOS near detector, MINERvA, and NOvA near detector) as near detectors for the two NuMI options, and use muon detectors to monitor the beam for the Homestake option. The Physics working group is currently studying the impact of near detectors on the physics reach.

# Steering Committee Conclusions

While each of these first-phase options is more sensitive than the others in some particular physics domain, the Steering Committee in its discussions strongly favored the option to build a new beamline to Homestake with an initial 10 kton LAr-TPC detector on the surface. The physics reach of this first phase is very strong; more over this option is seen by the Steering Committee as a start of a long-term world-leading program that would achieve the full goals of LBNE in time and allow probing the Standard Model most incisively beyond its current state. Ultimately this option would exploit the full power provided by Project X. At the present level of cost estimation, it appears that this preferred option may be ~10% more expensive than the other two options, but cost evaluations and value engineering exercises are continuing.

*But there are risks:*

In the next few months the LBNE collaboration and external experts will be studying the operation of LAr-TPCs on the surface to verify that the cosmic ray backgrounds are manageable. The operation on the surface may require shorter drift times than required for underground operations and the localization of the event in the TPC coincident with the ten microsecond-long beam from Fermilab. The Phase 1 experiment will use the existing detectors (MINOS near detector, MINERvA, and NOvA near detector) as near detectors for the two NuMI options, and use muon detectors to monitor the beam for the Homestake option. The Physics working group is currently studying the impact of near detectors on the physics reach.

# Steering Committee Conclusions

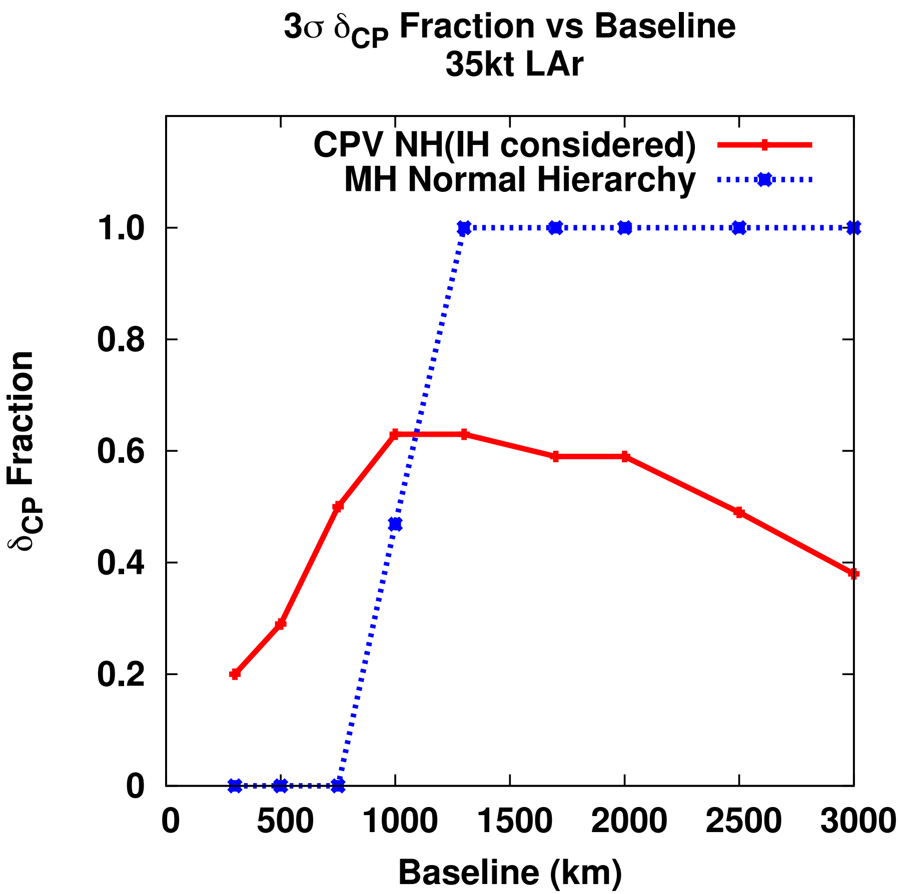
While each of these first-phase options is more sensitive than the others in some particular physics domain, the Steering Committee in its discussions strongly favored the option to build a new beamline to Homestake with an initial 10 kton LAr-TPC detector on the surface. The physics reach of this first phase is very strong; more over this option is seen by the Steering Committee as a start of a long-term world-leading program that would achieve the full goals of LBNE in time and allow probing the Standard Model most incisively beyond its current state. Ultimately this option would exploit the full power provided by Project X. At the present level of cost estimation, it appears that this preferred option may be ~10% more expensive than the other two options, but cost evaluations and value engineering exercises are continuing.

*But there are risks:*

In the next few months the LBNE collaboration and external experts will be studying the operation of LAr-TPCs on the surface to verify that the cosmic ray backgrounds are manageable. The operation on the surface may require shorter drift times than required for underground operations and the localization of the event in the TPC coincident with the ten microsecond-long beam from Fermilab. The Phase 1 experiment will use the existing detectors (MINOS near detector, MINERvA, and NOvA near detector) as near detectors for the two NuMI options, and use muon detectors to monitor the beam for the Homestake option. The Physics working group is currently studying the impact of near detectors on the physics reach.

*First studies suggest that the risks are manageable, but work continues*

# Reasons for the preference




- The long baseline neutrino physics is the highest priority because it is viewed to have a guaranteed positive scientific outcome.
- It makes no sense to give up on the comprehensive nature of LBNE since we have the technological ability to execute it.

Projects must have truly unique features or parameters that define them. These features serve the scientific program in the long run.

# Steering Committee Conclusions

*Limitations:*

Although the preferred option has the required very long baseline, its major limitation of the preferred option is that the underground physics program including proton decay and supernova collapse cannot start until later phases of the project. Placing a 10 kton detector underground instead of the surface in the first phase would allow such a start, and increase the cost by about $135M.

# Steering Committee Conclusions

*Limitations:*

Although the preferred option has the required very long baseline, its major limitation of the preferred option is that the underground physics program including proton decay and supernova collapse cannot start until later phases of the project. Placing a 10 kton detector underground instead of the surface in the first phase would allow such a start, and increase the cost by about $135M.

# Steering Committee Conclusions

*Limitations:*

Although the preferred option has the required very long baseline, its major limitation of the preferred option is that the underground physics program including proton decay and supernova collapse cannot start until later phases of the project. Placing a 10 kton detector underground instead of the surface in the first phase would allow such a start, and increase the cost by about $135M.

# Steering Committee Conclusions

*Limitations:*

Although the preferred option has the required very long baseline, its major limitation of the preferred option is that the underground physics program including proton decay and supernova collapse cannot start until later phases of the project. Placing a 10 kton detector underground instead of the surface in the first phase would allow such a start, and increase the cost by about $135M.

*Opportunities:*

Establishing a clear long-term program will make it possible to bring the support of other agencies both domestic and foreign. The opportunities offered by the beam from Fermilab, the long baseline and ultimately underground operation are unique in the world. Although the contributions from other agencies could substantially reduce the cost to the DOE or enhance the science capabilities for the first phase of the project, they are not taken into account in the present cost estimates.

# Steering Committee Conclusions

*Limitations:*

Although the preferred option has the required very long baseline, its major limitation of the preferred option is that the underground physics program including proton decay and supernova collapse cannot start until later phases of the project. Placing a 10 kton detector underground instead of the surface in the first phase would allow such a start, and increase the cost by about $135M.

*Opportunities:*

Establishing a clear long-term program will make it possible to bring the support of other agencies both domestic and foreign. The opportunities offered by the beam from Fermilab, the long baseline and ultimately underground operation are unique in the world. Although the contributions from other agencies could substantially reduce the cost to the DOE or enhance the science capabilities for the first phase of the project, they are not taken into account in the present cost estimates.

# Steering Committee Conclusions

*Limitations:*

Although the preferred option has the required very long baseline, its major limitation of the preferred option is that the underground physics program including proton decay and supernova collapse cannot start until later phases of the project. Placing a 10 kton detector underground instead of the surface in the first phase would allow such a start, and increase the cost by about $135M.*

*Opportunities:*

Establishing a clear long-term program will make it possible to bring the support of other agencies both domestic and foreign. The opportunities offered by the beam from Fermilab, the long baseline and ultimately underground operation are unique in the world. Although the contributions from other agencies could substantially reduce the cost to the DOE or enhance the science capabilities for the first phase of the project, they are not taken into account in the present cost estimates.

* Note that the cost increase of moving the detector underground is only ~15% of the total cost of the project. The cost of adding a high-performance near detector, including all civil construction, is similar.

# DOE Responds

**Department of Energy**
Office of Science
Washington, DC 20585

**Office of the Director**

June 29, 2012

Dear Pier,

I would like to thank you and your management team for your recent presentation on the revised plans for the Long Baseline Neutrino Experiment (LBNE). The steering group and project team have done an excellent job responding to our request to reconfigure the project in ways that lead to an affordable and phased approach that will enable important science results at each phase. The report of the LBNE steering group outlining the options and alternatives considered provides clear and thoughtful input to our strategic plan for the Intensity Frontier program.

We would like you to proceed with planning a Critical Decision 1 review later this year based on the reconfigured LBNE options you presented. Please work with Jim Siegrist and Dan Lehman on the timing of this review.

I am hopeful that we can put the LBNE project on a sustainable path and thereby secure a leadership position for Fermilab in the Intensity Frontier. We look forward to working with you to achieve this goal.

Sincerely yours,

W.F. Brinkman

# Phased Program

The preferred configuration would be the first step in a phased program.

In the 1st phase, LBNE would determine the sign($\Delta m^2_{32}$) and measure $\delta_{CP}$, as well as measuring other oscillation parameters: $\theta_{13}$, $\theta_{23}$, and $|\Delta m^2_{32}|$.

Subsequent phases would include:

# Phased Program

The preferred configuration would be the first step in a phased program.

In the 1st phase, LBNE would determine the sign($\Delta m^2_{32}$) and measure $\delta_{CP}$, as well as measuring other oscillation parameters: $\theta_{13}$, $\theta_{23}$, and $|\Delta m^2_{32}|$.

Subsequent phases would include:

- Build a highly capable near neutrino detector,
  - reduce systematic errors on the oscillation measurements
  - enable a broad program of non-oscillation neutrino physics.

# Phased Program

The preferred configuration would be the first step in a phased program.

In the 1st phase, LBNE would determine the sign($\Delta m^2_{32}$) and measure $\delta_{CP}$, as well as measuring other oscillation parameters: $\theta_{13}$, $\theta_{23}$, and $|\Delta m^2_{32}|$.

Subsequent phases would include:

- Build a highly capable near neutrino detector,
  - reduce systematic errors on the oscillation measurements
  - enable a broad program of non-oscillation neutrino physics.
- Increase the detector mass or increase the beam power (Project X)
  - add statistical precision to all of the neutrino measurements.

# Phased Program

The preferred configuration would be the first step in a phased program.

In the 1st phase, LBNE would determine the sign($\Delta m^2_{32}$) and measure $\delta_{CP}$, as well as measuring other oscillation parameters: $\theta_{13}$, $\theta_{23}$, and $|\Delta m^2_{32}|$.

Subsequent phases would include:

- Build a highly capable near neutrino detector,
  - reduce systematic errors on the oscillation measurements
  - enable a broad program of non-oscillation neutrino physics.
- Increase the detector mass or increase the beam power (Project X)
  - add statistical precision to all of the neutrino measurements.
- Add a large detector at the 4850 foot (4300 mwe) level at Homestake
  - enable proton decay, supernova neutrino, and other non-beam physics
  - further improve the precision of the main oscillation measurements
  - enable use of more difficult channels for a fully comprehensive program of oscillation measurements

# Phased Program

The preferred configuration would be the first step in a phased program.

In the 1st phase, LBNE would determine the sign($\Delta m^2_{32}$) and measure $\delta_{CP}$, as well as measuring other oscillation parameters: $\theta_{13}$, $\theta_{23}$, and $|\Delta m^2_{32}|$.

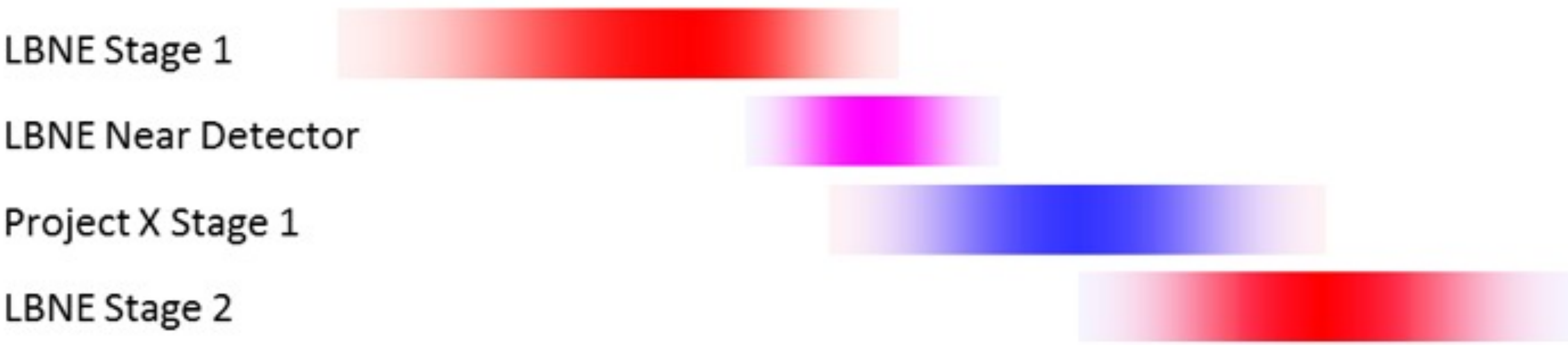
Subsequent phases would include:

- Build a highly capable near neutrino detector,
  - reduce systematic errors on the oscillation measurements
  - enable a broad program of non-oscillation neutrino physics.
- Increase the detector mass or increase the beam power (Project X)
  - add statistical precision to all of the neutrino measurements.
- Add a large detector at the 4850 foot (4300 mwe) level at Homestake
  - enable proton decay, supernova neutrino, and other non-beam physics
  - further improve the precision of the main oscillation measurements
  - enable use of more difficult channels for a fully comprehensive program of oscillation measurements

*The actual order and scope of the next phases would, of course, depend on physics, resources, and the interests of current and new collaborators.*

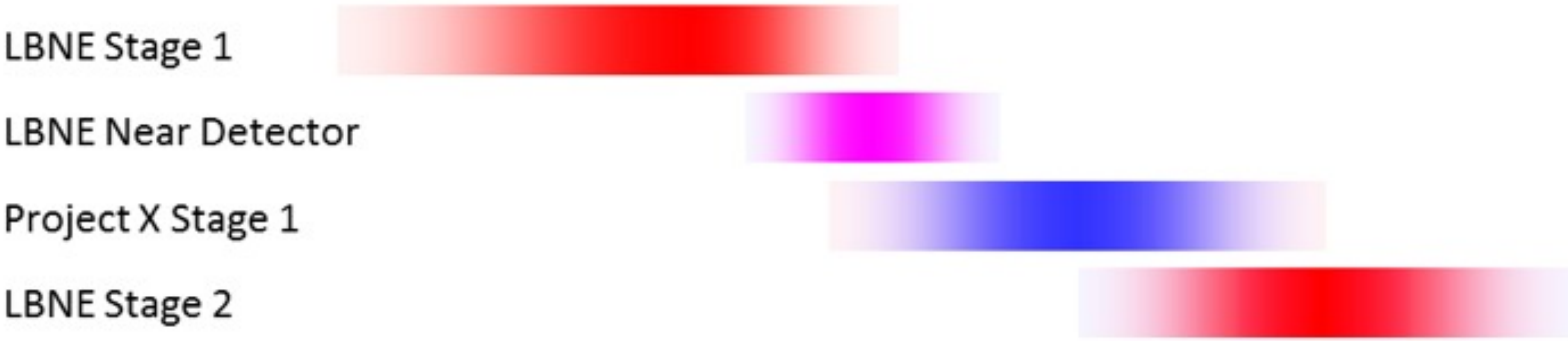
# Phased Program: Possible Example

1) 10 kt LAr detector on surface at Homestake + LBNE beamline (700 kW)
2) Near Neutrino Detector at Fermilab
3) Project X stage 1 → 1.1 MW LBNE beam
4) Additional 20-30 kt detector deep underground (4300 mwe)



# Phased Program: Possible Example

1) 10 kt LAr detector on surface at Homestake + LBNE beamline (700 kW)
2) Near Neutrino Detector at Fermilab
3) Project X stage 1 → 1.1 MW LBNE beam
4) Additional 20-30 kt detector deep underground (4300 mwe)




Additional national or international collaborators could help accelerate the implementation of the full LBNE program.

# The LBNE Project

The LBNE Project is to deliver the first phase of this program:

- A new neutrino beam at Fermilab:
  - Aimed at Homestake
  - Spectrum optimized for this distance
  - Upgradeable to ≥ 2.3 MW proton beam power
- A 10 kt LAr TPC detector on the surface at Homestake
  - In a pit just below the natural grade
  - Shielded against hadronic and EM component of CR showers

# The LBNE Project

The LBNE Project is to deliver the first phase of this program:

- A new neutrino beam at Fermilab:
  - Aimed at Homestake
  - Spectrum optimized for this distance
  - Upgradeable to ≥ 2.3 MW proton beam power
- A 10 kt LAr TPC detector on the surface at Homestake

# The LBNE Project

The LBNE Project is to deliver the first phase of this program:

- A new neutrino beam at Fermilab:
  - Aimed at Homestake
  - Spectrum optimized for this distance
  - Upgradeable to ≥ 2.3 MW proton beam power
- A 10 kt LAr TPC detector on the surface at Homestake
  - In a pit just below the natural grade
  - Shielded against hadronic and EM component of CR showers

# The LBNE Project

The LBNE Project is to deliver the first phase of this program:

- A new neutrino beam at Fermilab:
  - Aimed at Homestake
  - Spectrum optimized for this distance
  - Upgradeable to ≥ 2.3 MW proton beam power

# The LBNE Project

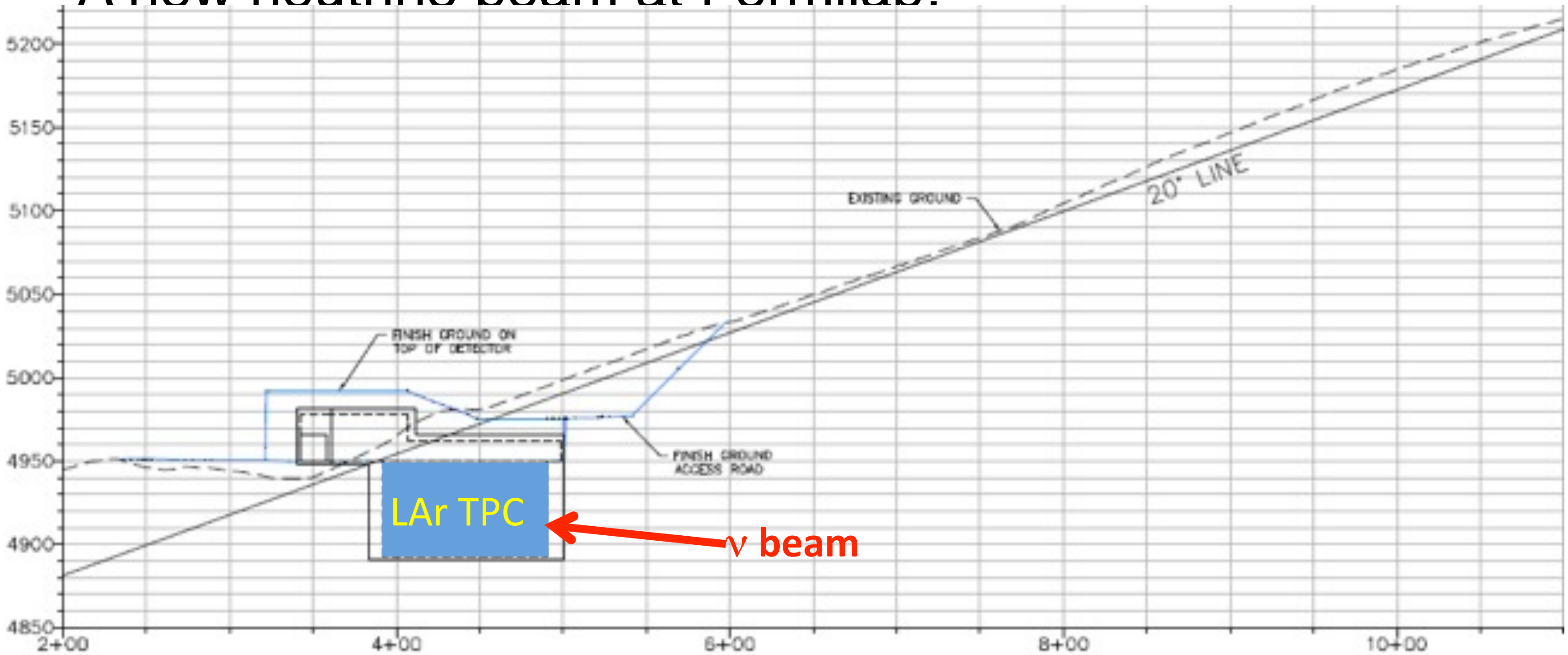
The LBNE Project is to deliver the first phase of this program:

- A new neutrino beam at Fermilab:
  - Aimed at Homestake
  - Spectrum optimized for this distance
  - Upgradeable to ≥ 2.3 MW proton beam power
- A 10 kt LAr TPC detector on the surface at Homestake
  - In a pit just below the natural grade
  - Shielded against hadronic and EM component of CR showers

# The LBNE Project

The LBNE Project is to deliver the first phase of this program:

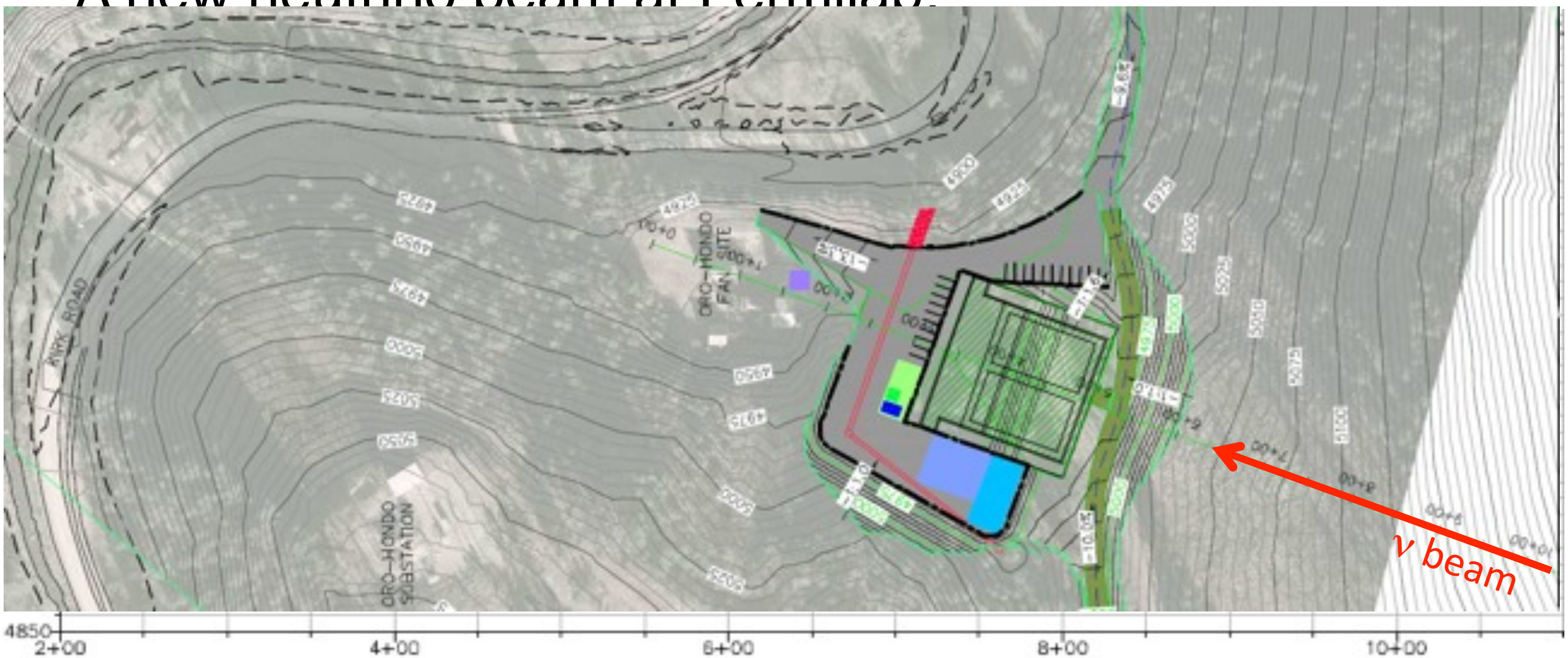
- A new neutrino beam at Fermilab:



# The LBNE Project

The LBNE Project is to deliver the first phase of this program:

- A new neutrino beam at Fermilab:



# The LBNE Project

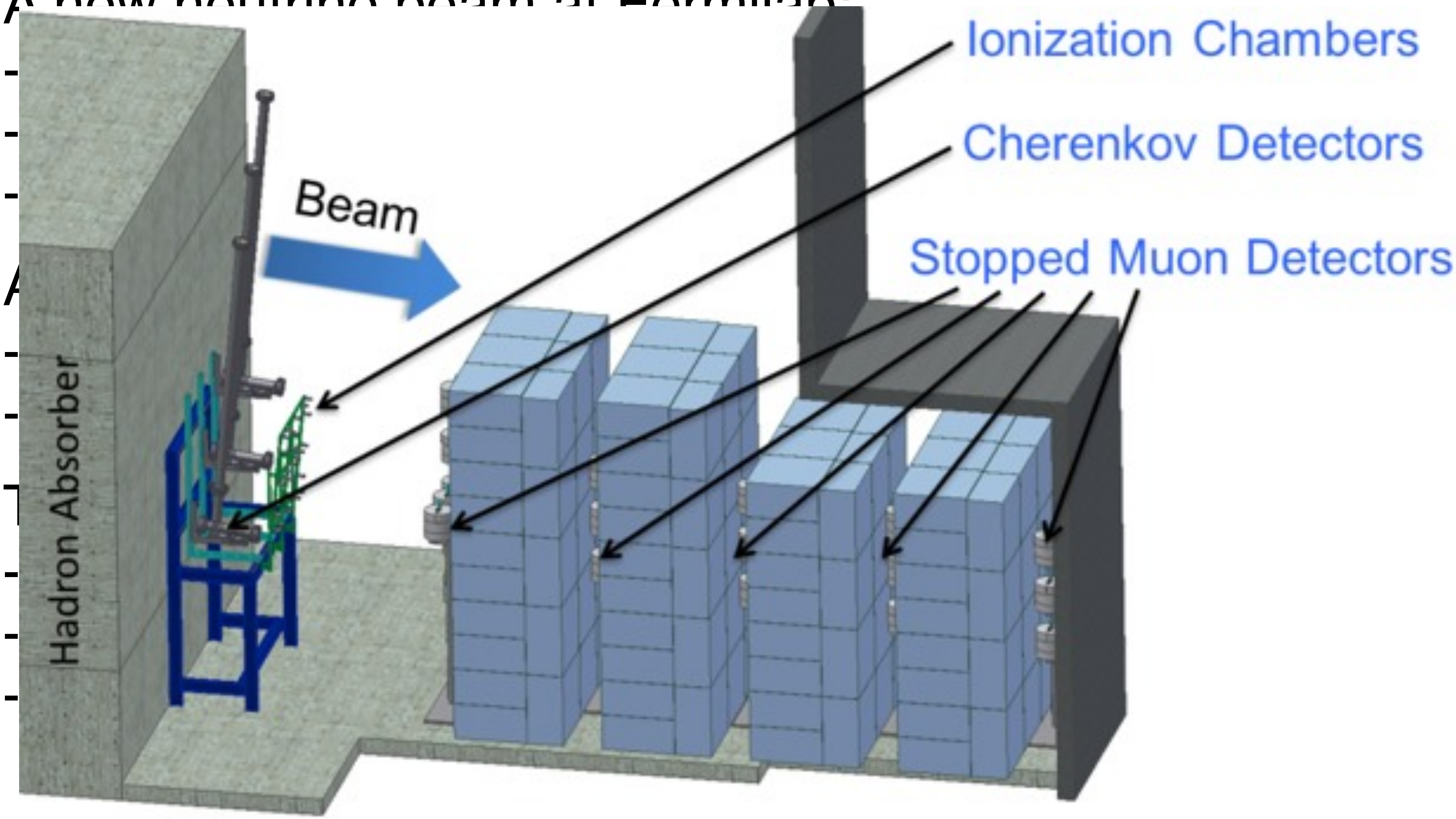
The LBNE Project is to deliver the first phase of this program:

- A new neutrino beam at Fermilab:
  - Aimed at Homestake
  - Spectrum optimized for this distance
  - Upgradeable to ≥ 2.3 MW proton beam power
- A 10 kt LAr TPC detector on the surface at Homestake
  - In a pit just below the natural grade
  - Shielded against hadronic and EM component of CR showers
- Tertiary muon detector to monitor the neutrino beam
  - ionization chambers
  - variable pressure gas Cherenkov detectors
  - stopped muon detectors

# The LBNE Project

The LBNE Project is to deliver the first phase of this program:

- A new neutrino beam at Fermilab:



# The LBNE Project

The LBNE Project is to deliver the first phase of this program:

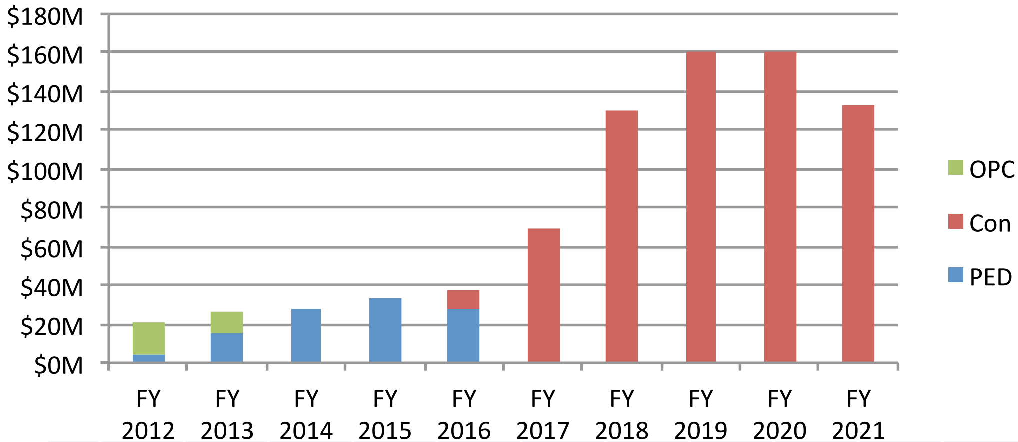
- A new neutrino beam at Fermilab:
  - Aimed at Homestake
  - Spectrum optimized for this distance
  - Upgradeable to ≥ 2.3 MW proton beam power
- A 10 kt LAr TPC detector on the surface at Homestake
  - In a pit just below the natural grade
  - Shielded against hadronic and EM component of CR showers
- Tertiary muon detector to monitor the neutrino beam
  - Ionization chambers
  - Variable pressure gas Cherenkov detectors
  - Stopped muon detectors

# The LBNE Project – Next Steps

- The next step in the DOE project approval process is "CD-1," which approves the conceptual design and overall cost scale and schedule of the Project.
- We have been encouraged by DOE to achieve this milestone by the end of December 2012.
- A prerequisite is to pass two major reviews:
  - Fermilab Director's Review 25-27 September
    - Validates the design
  - DOE ("Lehman") Review 30 October – 1 November
    - Validates the project plan
- CD-1 will allow us to move forward to complete the design and to prepare for construction.

# Current Cost/Schedule Status

- Re-estimating entire project for 10 kt surface detector at SURF with new beamline and muon detectors at Fermilab – to be complete August 30th
- Working toward matching DOE funding profile

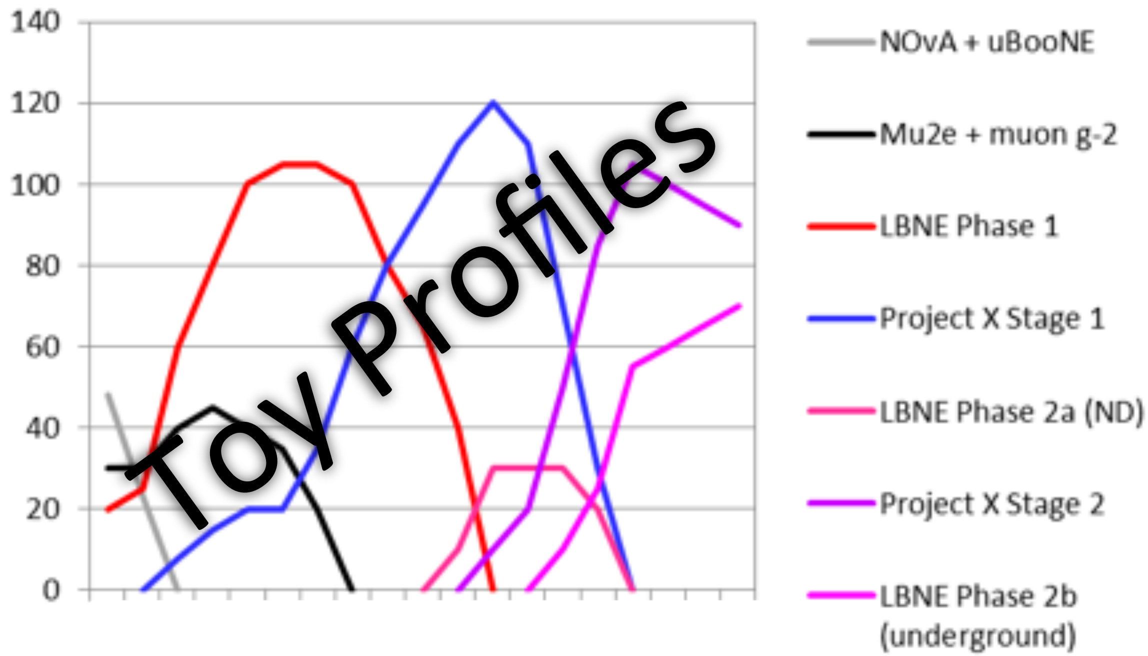



| | FY 2012 | FY 2013 | FY 2014 | FY 2015 | FY 2016 | FY 2017 | FY 2018 | FY 2019 | FY 2020 | FY 2021 | FY 2022 | Total |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| PED | 4,000 | 16,000 | 28,000 | 34,000 | 28,000 | - | - | - | - | - | - | 110,000 |
| Con | | - | - | - | 10,000 | 70,000 | 130,000 | 160,000 | 160,000 | 133,000 | - | 663,000 |
| OPC | 17,000 | 10,000 | - | - | - | - | - | - | - | - | - | 27,000 |
| Total | 21,000 | 26,000 | 28,000 | 34,000 | 38,000 | 70,000 | 130,000 | 160,000 | 160,000 | 133,000 | - | 800,000 |

- Will then prepare for September and October reviews
- Will have good handle on costs for pieces for collaborator contributions

# Phasing Possibilities: Another Example*

1) LBNE Phase 1: 10 kt LAr detector on surface at Homestake + LBNE beamline (700 kW)
2) Project X Phase 1 → 1.2 MW LBNE beam
3) LBNE Phase 2a: Near Detector
4) Project X Phase 2
5) LBNE Phase 2b: ~25 kt underground far detector




*Expanded collaboration could allow part or all of Phase 2a or 2b into Phase 1.*

*A little beyond what is in the Eng/Cost WG report

# Summary

- LBNE remains focused on its long-term goals:
  a) Comprehensive program to measure neutrino oscillations
     - determine the mass hierarchy and look for CP violation
     - precision measurement of other oscillation parameters
     - test the validity of the three-neutrino mixing model
  b) Search for baryon number violating processes
  c) Measure neutrinos from astrophysical sources, especially from a core-collapse supernova in our galaxy
- Fiscal constraints require us to approach our goals in a phased program.
- The collaboration is capable, experienced, and well organized.
- The LBNE Project will build the first phase, and is expecting DOE approval of "CD-1" this year.
- New national or international collaborators could add scope to phase 1 or accelerate the implementation of later phases.

# Summary continued

- The schedule for major projects in particle physics will remain funding limited.
- There are many demands on the same funds within and outside the field of particle physics.
- It is extremely important that the community stand behind the first step of LBNE with a view towards the future.
- There are no shortcuts to the physics we want to do.
- The US is currently the farthest along in this planning.
- Recall that the collider program took several decades and went through huge changes.

# Strategy for non-DOE participation

- Potential partners
  - NSF
    - what program? MREFC, the new MID-SCALE program
    - Midscale program is constrained to be <$100M, but probably much smaller.
  - Europe: The European strategy group meeting Sep. 10, 2012 in Warsaw. Fermilab will lead the approach.
  - Japan: HyperK collaboration is forming. Costs and expectation from the US are unknown.
  - India: Negotiations in progress towards a large contribution.
  - China:  Daya Bay II collaboration is forming. Interest in US participation unknown.

# NSF

- What are the constraints from NSF ?
- MREFC process needs NSB sign-off. Will the NSB consider LBNE a good place to invest ? LSST has been given the go-ahead; this may mean that physics does not get the next project for some time.
- Mid-scale program is new. How it gets reviewed is unclear. We have been advised to propose for this mid-scale program.

# Europe

- LHC upgrades will be the priority.
- What are the chances of getting LBNO approved ?
- LBNO will need a new beamline and considerable upgrade to the SPS.
- When will the LBNO site decision be made ?
- Is CERN willing to give up on neutrinos ?

# India

- The Indian policy-makers want to have a piece of the project that is clearly identified with India.
- Current choice is the near detector, but perhaps could include other items.
- Scale of investment is ~$100M.
- The 12th 5 year plan is online and provides considerable funding towards the sciences and particle physics. How it gets executed is unclear.

# What should we work towards ?

- NSF contribution: \$50M -\$100M
- State contribution (Illinois ?): \$80M (Fermilab works on this)
- Off project contributions from SURF ventilation and mine safety: ? (already accounted for)
- Europe: 1/2 of the far detector: \$80M
- Indian contribution: \$200M in US accounting; this contribution includes \$150M of new scope, and so \$50M of offset to our current costs.