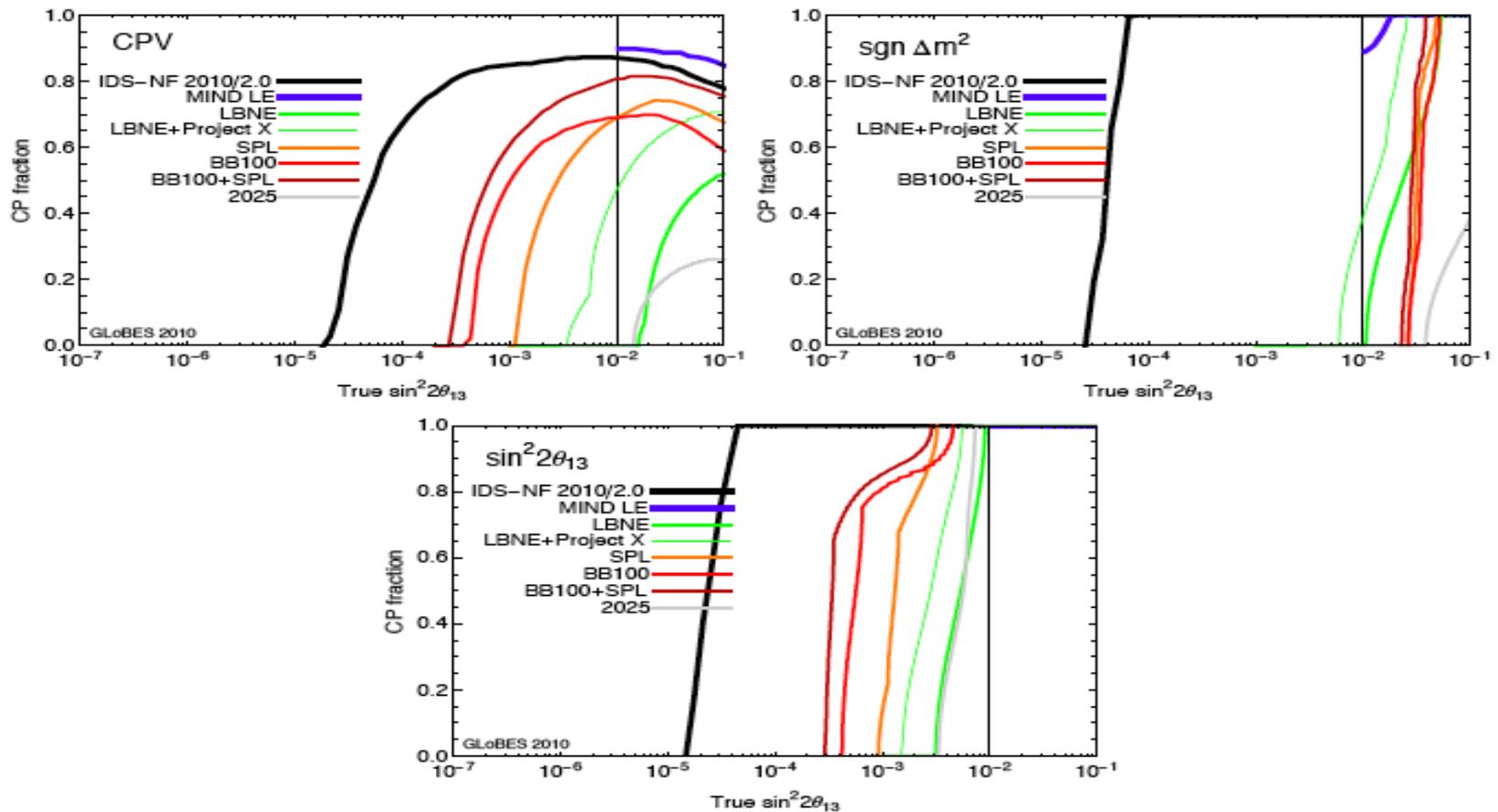
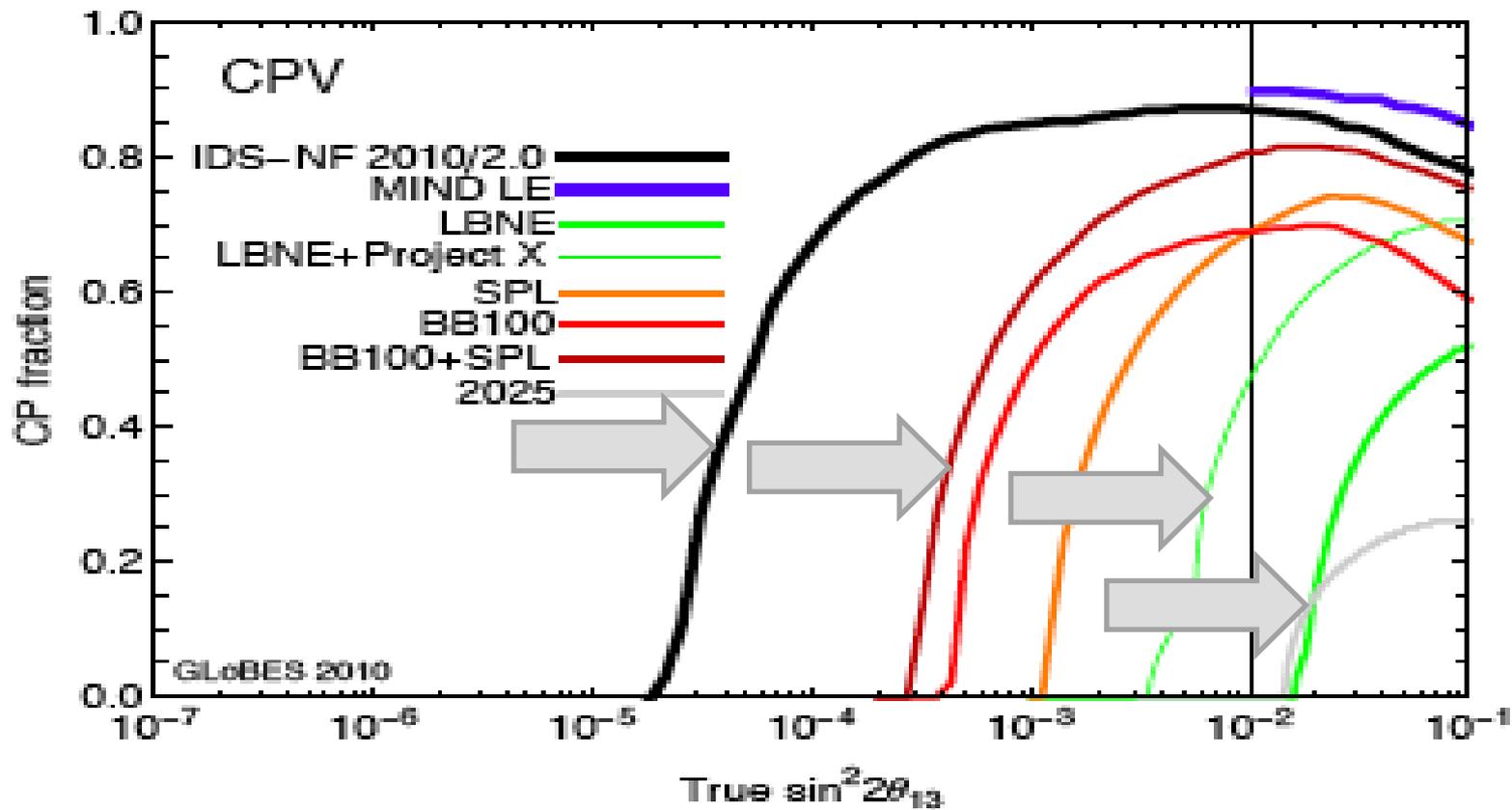


ECFA Review Panel for future accelerator based neutrino facilities

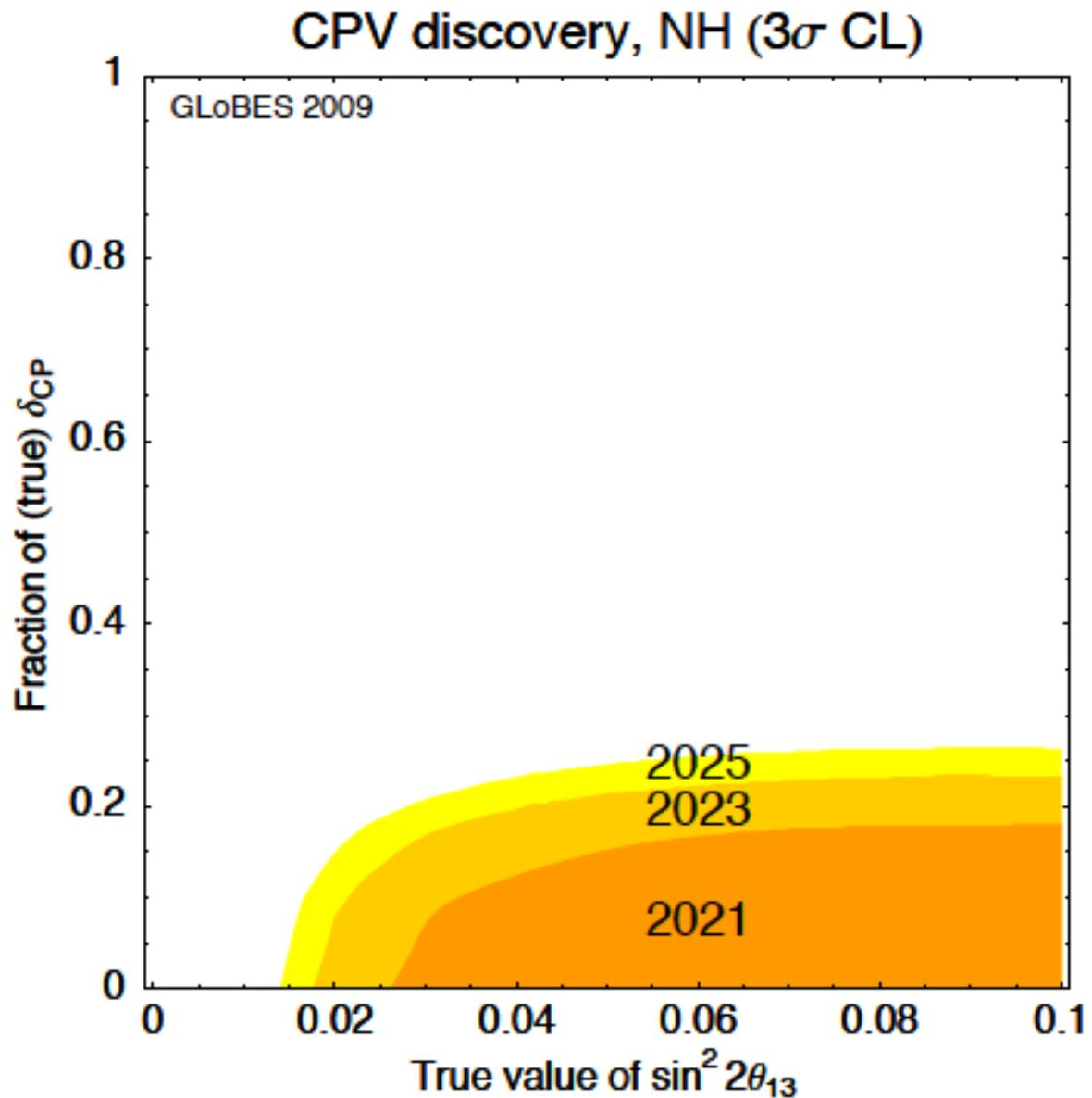


CP-violation reach



if we just run
planned facilities

includes T2K and
Project-X with
1.7 MW beams



Huber et al.

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Charge to the ECFA Review Panel for future accelerator based neutrino facilities:

to review

- **EUROnu Mid-term Report and IDS-NF Interim Design Report**

- concerning: scientific case, technical feasibility, risk and necessary R&D, cost and planning, organization and to deliver

- concise written report by the end of July 2011

- oral presentation by the panel chair at ECFA-EPS joint session on European Strategy Document Update, Grenoble, 23 July 2011 in the afternoon

ECFA neutrino review

Comparison tables		Comprehensive-ness of work done so far [1]	Technical feasibility	R&D still necessary [7]	Contingent upon	Scale of planning to make happen	Likely effort to obtain safety approval	Physics reach	Likely cost
Superbeam — CERN (a)	overall	incomplete	Challenging		CERN policy	regional	Probably manageable	Good cf. now [11]	!
Proton driver (4 MW)		Sufficient [12]	Feasible	Considerable					
Target		incomplete	Challenging	Considerable					
Horn		incomplete	Very challenging	V. extensive [10]					
Decay tunnel		Sufficient	Feasible	limited					
Dump		Sufficient	Feasible	limited					
Detector - as beta beams									
Beta beam — CERN (b)	overall	incomplete	More challenging	V. extensive [6]	CERN policy	international	Probably manageable	Better [11]	! !
Proton driver (incl. SPL)		Sufficient	Feasible	Considerable					
Ion sources		incomplete	Challenging	Very extensive					
Linac		incomplete	Feasible	limited					
RCS		incomplete	Feasible	limited					
PS		incomplete	Need to modify	Beam dynamics					
SPS		incomplete	machines [4]	Beam dynamics					
Decay ring		incomplete	Challenging [9]	Beam dynamics					
detector - water cherenkov (b')		sufficient	feasible	limited		international	managable	good (energy)	
Detector									
Neutrino factory — CERN/FNAL/RAL (c)	overall	Sufficient	Very challenging			global		Best [11]	! ! !
Proton driver (4 MW, linac+compress. or synch.)		Prob. sufficient	Challenging	extensive			considerable		
Target		incomplete	Very challenging	Very extensive					
Buncher		Prob. sufficient	Challenging	Considerable					
Rotator		Prob. sufficient	Challenging	Considerable					
Cooling channel		Prob. sufficient	Challenging	Extensive	MICE results [2]				
Linac		Sufficient	Feasible	limited					
RLAs		Sufficient	Feasible	limited					
FFAG		incomplete	V. challenging [5]	Very extensive					
Decay ring		incomplete	V. challenging [3]	Extensive [8]					
Detectors, far									
MIND (d)		sufficient	established	limited		international	managable	good	
LArTPC (e)		incomplete	Very challenging	extensive		international	considerable	best	
TASD (f)		incomplete	Challenging	considerable		international	considerable	good	
Detector, near									
option A (silicon +scifi) (g)		Prob. sufficient	Challenging	limited		regional	managable	good	

- [1] For the purpose of producing a plausible design report.
- [2] Detailed results from MICE experiment may come too late for use in Neutrino Factory Reference Design Report.
- [3] Deep and steeply sloping.
- [4] Particularly challenging for $\gamma = 350$.
- [5] Requires several orders of magnitude extrapolation from present state of the art.
- [6] Some parts of the scheme are still relatively sketchy.
- [7] Before well-informed decision could be taken as to whether practical to build or not.
- [8] Need to accommodate both beam diagnostics and engineering considerations.
- [9] RF system hardware for decay ring is very demanding.
- [10] Including materials compatibility and pulsed power issues.
- [11] Subject to parameter values, especially θ_{13}
- [12] No end-to-end design

Related research elsewhere, not reviewed here

- [a] FNAL, J-PARC
- [b] Canfranc, Fréjus, Gran Sasso, Umbria
- [b'] SuperKamiokande, LBNE
- [c] J-PARC
- [d] Minos, Nova
- [e] Icarus, Argonut, Laguna
- [f] Borexino, Kamland, Laguna
- [g] Opera, Nomad

what is in the table?

comprehensiveness of work done so far/ technical feasibility/ R&D still necessary/ contingent upon/ scale of planning to make happen/ likely effort to obtain safety approval/ physics reach/ likely cost

for the suite of components of a

Super-beam/ Beta-beam/ Neutrino Factory/
detectors

evaluation expressed in the table is explained in the core text of the report

draft report was submitted to the
community for comments via

- Ken Long (Imperial College)
- Rob Edgecock (Rutherford)

comments were received and the
document updated

A rich research program in neutrino physics exploiting particle-astrophysics, accelerator and reactor experiments has made rapid progress possible; it is vibrant to date. The pioneering phase characterized by the remarkable physics return of relatively modest experiments is concluding; increasingly complex facilities are required to fill in many aspects of our still incomplete picture of neutrino physics.

The program should aim for neutrino physics beyond the determination of θ_{13} , the angle connecting the solar and atmospheric oscillations. It will be determined or significantly limited by present experiments. An outstanding goal is the discovery of CP-violation in the lepton sector. This requires a big step in technical improvements and should not avoid the challenges of introducing new concepts in accelerator, beam and large detector technologies.

Even though it is premature to motivate future facilities on the basis of present indications (which include recent T2K and MINOS results as well as intriguing low statistics hints for new physics from short-baseline experiments and reactor data), recent developments underscore the possibility of unexpected discoveries supporting the construction of neutrino facilities with the widest science reach.

It is the committee's unanimous conclusion that both reports reviewed - the EUROnu mid-term report and the IDS-NF interim design report- made a clear case for the facilities proposed, although only the Neutrino Factory presented an end-to-end description of the road to construction. They both present a clear and in-depth description of the research and development performed so far. The community should be congratulated for the results.

From the information reviewed by the panel, it seems likely that complexity and risk, and probably cost as well, all increase in the order Super-beam to Beta-beam to Neutrino Factory. It is also reasonably clear that the physics reach of the three schemes increases in the same order — so that, for example, while the Neutrino Factory would be the most complex and risky, and probably the most expensive, nevertheless it would provide the most scientific information.

It may appear that extending the presently available technologies (for the accelerator, beam and large detector) looks faster and easier than introducing new concepts. However, to improve mature technologies substantially may well, eventually, need much more work than introducing new technologies will.

The Super-beam and Neutrino Factory proposals require high intensity, relatively low-energy proton accelerators. A common challenge for these proposals is the difficulty to handle large beam intensities with correspondingly severe high-energy losses. These create thermal dissipation and material irradiation problems for different components such as vacuum windows, targets, focusing horns or solenoid magnets.

The Beta-beam requires further development beyond the source whose design was presented. The realization of such a project is attractive from the point of view that its science reach is adequate in the presence of a large θ_{13} mixing angle and can be matched with a water Cherenkov detector that is also favored by a community of particle astrophysicists.

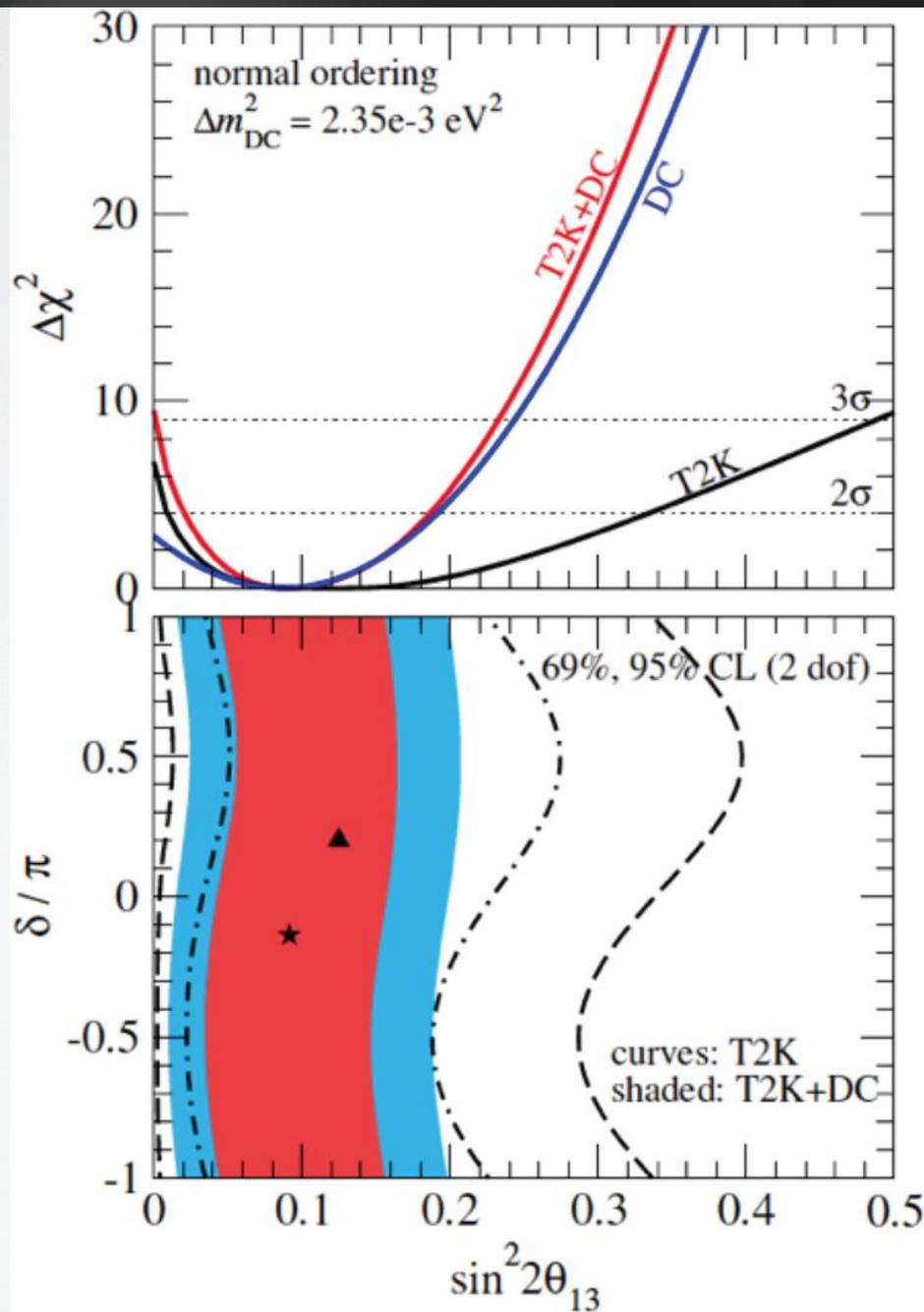
The Neutrino Factory presented an end-to-end, long-term research and development program.

It is to the advantage of both Super-beam and Beta-beam projects to develop a complete end-to-end conceptual design that can be confronted with the reality of CERN policy. This is especially the case for the Beta-beam, for which the focus of the presentations was the ion source and not the accelerator complex.

For the specific beam options presented in the reports under review, the relatively short baseline from CERN to Fréjus is not optimal.

The European neutrino-beam physics program should have synergy with astroparticle physics because of the common goal of commissioning massive detector for progress in both fields.

$$P(\bar{\nu}_e \rightarrow \bar{\nu}_e) \simeq 1 - \sin^2(2\theta_{13}) \sin^2\left(\frac{\Delta m_{32}^2 L}{4E}\right)$$



Update:

The committee is aware of the fact that the Beta-beam community has in the meantime produced a concept for the complete facility, from ion source to decay ring. It illustrates the challenges and reveals the possibility that the scope and cost may approach those of a Neutrino Factory.

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The recent “interesting indications” that θ_{13} may not vanish have created renewed interest in building a Superbeam at CERN to an underground laboratory at a favorable distance of ~ 2500 km, e.g. in Pyhäsalmi, Finland (and not Frejus). Such a plan makes the still-risky bet that θ_{13} is actually close to its present limits. If not, very large running times and/or huge detectors will be required for significant results. Alternatively, upgrading the beam to several MW will turn this into a project similar to the one described in the documents reviewed here, with a long timeline of order a decade. This approach has to confront competing projects in the US and Japan. A CERN to Pyhäsalmi baseline is also favorable for a Neutrino Factory based at CERN; it should have a better control of the systematics in studying CP-violation.

It is possible that, with time, the Beta-beam and Neutrino Factory may become true next-generation, post- θ_{13} facilities with physics goals that have to be reevaluated for a different science reach. In any case, the technical build-up will take a long time.

The committee feels that progress is difficult as long as accelerator-based neutrino facilities remain a triad with parallel development and very little confrontation. Moving forward on the best timescale will require at least common research and development and at best a common plan with priorities and timelines. In this context, progress on cost estimates is critical.

No matter how it is implemented, this neutrino program presents challenges and risks that are very significant, but the scientific rewards in terms of new physics are potentially even greater.