Feasibility Study III

U.S. My Perspective

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Outline

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Introduction

• Possibility of doing “world” Neutrino Factory Feasibility Study is under discussion
  — this is an increase in scope compared with previous Studies
    ○ and requires a corresponding increase in planning
• U.S. MC has been involved in both of the earlier Studies
  — we have some experience in organizing such an endeavor
• Comments here are my own personal views and do not purport to represent the “official” position of the MC, much less the U.S.
• I doubt I’ll tell you anything you haven’t already figured out
Previous Studies

• Study I instigated by the Fermilab Director
  — MC was invited to participate
  — basic organization and decision-making done by Fermilab editors (Holtkamp and Finley)
  — MC had “input” into planning process but no formal responsibility

• Basic desire was to focus on feasibility
  — this was the first attempt to specify a Neutrino Factory from end to end
  — approach: base design on (reasonably) well-understood technologies
  — cost estimate for the facility was a deliverable
    o but no attempt made to optimize either costs or overall performance

• Proper approach at that time, as feasibility itself was most in doubt
Previous Studies

• Led to predictable result: feasibility established, performance poor, and costs relatively high

• Examples of design choices
  — use carbon target modeled after NUMI design
  — use induction linac for phase rotation
  — use 50 GeV final beam energy

• Site-specific proton driver (8 GeV) and detector location
  — baseline of 3000 km corresponded to SLAC as detector venue
  — conventional facility costs were based on Fermilab geology

• In large measure results are generic and not dominated by site-specific parameters
Previous Studies

- Study II was done from the outset as collaboration between MC and BNL as sponsoring laboratory
  - co-led by S. Ozaki (BNL), R. Palmer (BNL-MC), M. Zisman (MC)
Previous Studies

• Relationship worked smoothly
  — Ozaki handled site-specific aspects
  — Palmer handled simulations and design concept
  — I handled technical implementation and costing

• Joint management sent clear message that MC was an equal partner in the process
  — BNL leaders were able to draw in resources from the lab that were invaluable in carrying out the study
    ◦ especially in areas of conventional construction and cost estimating
Previous Studies

• Goal: maintain convincing feasibility but improve performance substantially
  — minimizing costs was again given lower priority

• Examples of design choices
  — use Hg jet target to improve muon yield
  — use multiple induction linacs for “non-distorting” phase rotation
  — use 20 GeV final beam energy

• Site-specific proton driver (AGS, 24 GeV) and detector location
  — baseline of 3000 km corresponded to WIPP as detector venue
  — conventional facility costs were based on BNL geology
    ○ in particular, requirement to avoid penetrating water table meant we had to build a hill to house the storage ring
Previous Studies

• Results:
  — performance 6x that of Study I
    ○ $1.2 \times 10^{20}$ vs. $2 \times 10^{19}$ $\nu_e$ per year ($10^7$ s) per MW
  — cost about 75% of Study I
    ○ but this was mainly due to using 20 GeV rather than 50 GeV, saving one RLA
  — performance scalable with proton power, as jet target does not limit this parameter
    ○ should be able to operate at 4 MW
Previous Studies

• Lessons learned from the two Studies
  
  − necessary to **optimize the “front end”** (decay, bunching, phase rotation, cooling) as one **system** to get high performance

  − necessary to **simulate entire concept before starting detailed engineering** (self-consistent solution)
    
    ◦ otherwise engineers chase a “moving target”
      
      - or cost something whose parameters are incompatible with what is ultimately specified by simulations

  − also necessary to **interact with engineers during initial simulation studies** to ensure that specified parameters are achievable
    
    ⇒ it is necessary to work as partners with the key engineers to converge to a good design

  − facility as conceived is costly, $O(2B)$

  − increasing proton driver is a cost-effective way to get higher performance
Goals for Study III

• As noted, we have already covered those portions of design space representing
  — low performance, high cost
  — high performance, high cost

• What’s left?
  — high performance, optimized cost
    • note that I resisted the temptation to say “low” cost
Goals for Study III

- Based on previous work, we in MC have some ideas where to begin
  - replace induction linacs with Neuffer RF bunching and phase rotation scheme
  - replace RLA with some form of FFAG ring or possibly very fast cycling synchrotron
  - look for cost optimum between amount of cooling and acceleration system/storage ring acceptance
  - examine possibility of using cooling ring for 6D cooling
    - this would have a considerable impact on the downstream implementation
    - bunch length cannot be arbitrarily long when using a cooling ring
    - it’s time to try this in earnest
- It is recognized that others will have equally strong ideas how to proceed
Resources and Organization

• Having resources for a serious feasibility study requires the backing of a laboratory
  
  — none of the world’s Neutrino Factory R&D groups has the financial or engineering resources by itself
    
    o even in combination, their engineering resources are insufficient
  
• Given their interest in MICE, RAL is natural site to host such a study
  
  — this means that the site-specific aspects of a study reflect RAL conditions

• Resultant study will represent a much better product if all of the world’s Neutrino Factory R&D groups collaborate on it

  — this implies agreeing on the goals of the study and also agreeing on a single, optimized, scenario to examine
    
    o it’s poor strategy to consider alternative implementations in the study, as it gives the impression we cannot decide

  — this will be a difficult hurdle to clear
Resources and Organization

• Lab’s upper management must support the Study
  — key resources always over-committed
  — management approval needed to make them available
• In both our studies, request came directly from Lab Director
• Engineering resources will be needed for designing and costing
  — conventional facilities
  — power supplies
  — vacuum
  — magnets
  — RF (especially power)
  — vacuum
  — safety
Resources and Organization

• Some effort on the detector is desirable
  — identifying a viable remote site goes far in making the host lab look like a realistic candidate to host a future facility

• Proper cost optimization of a Neutrino Factory must include both the accelerator and detector

• Scale of effort: ≈20–30 person-years for a Study lasting one year

• For a world Study, leadership activities must be shared among participating groups
  — if all leadership roles taken by host lab, it will not be perceived as a shared activity
  — if none are taken by host lab, the study will likely fail

• Decision-making must likewise be shared, as in any collaboration

• We designated editors to guide major technical areas and write them up for the report, along with an overall editor
  — met in person several times and also via video conference
Summary

• Successful Study will involve effective collaboration among parties with different interests

• Upper management at host lab must support the effort, and recognize the benefits of collaboration

• One of the first issues to resolve is defining (generally site-specific) proton driver parameters

• Then define remaining “ingredients” of chosen design

• Carry out end-to-end simulations early, interacting with key engineers at this stage

• Don’t turn engineers loose on design until parameters well defined

• Exercise discipline in changing designs (“better is the enemy of good”)

• In addition to the Study report, publishing a summary paper in a journal (e.g., PRST-AB or NIM) is highly desirable

• Doing this Study well will improve the odds of someday having a Neutrino Factory…and that’s what we all want!