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High energy physics research is in crisis. There is enormous interest in advancing the energy frontier: we can say with some certainty that new, important phenomena are waiting to be discovered. But the demand for high energies has outrun the ability to construct affordable accelerators.

The accelerator technology problem appears to be formidable. Despite incremental advances in the “cost per GeV” and other measures of cost, accelerator physics has become a mature field and innovation is too slow to satisfy the demand. High energy, high luminosity machines appear to require high electrical power and as a result are expensive. Newer technologies – if they work at all – will have to overcome the same basic issues of “wall plug efficiency” and material properties (mechanical strength, thermal properties, etc.) that constrain the current technology. Superconducting technology, arguably the most important recent advance in accelerator technology, has hardly had a noticeable impact in reversing the trend towards, larger and more expensive machines.

Given that progress in particle physics is limited by accelerator technology it is somewhat surprising that it has not received more attention. Basic accelerator research that focuses on fundamental concepts with a long-term perspective is rare. Most of what passes for accelerator research is really pre-project funding whose goal is to achieve an engineering prototype to meet the requirements of a particular design. Although project-specific R&D has resulted in fundamental new knowledge, it tends to result in incremental advances in known technologies. It is not clear that a greater emphasis on basic accelerator research would lead to a technological breakthrough: the solution may very well lie in some serendipitous discovery from another field. On the other hand, the most obvious approach to the problem is to attack directly on the basis of what we already know about accelerators. Some laboratories have started programs in accelerator research that are not project specific. Fermilab has traditionally invested very little in accelerator research but has the potential to develop a first-rate program.

In the near term, however, we at Fermilab aspire to taking a leading role in the construction of an ILC. There is a strong consensus in the international community that an ILC is an important next step, and, within the U.S. community, that it would be appropriate to site it at or near Fermilab. Unfortunately, there are significant risks associated with the ILC as a future Fermilab project: it may not be realized in the near future or it may not be built near Fermilab. These risks have been recently highlighted in the remarks of Ray Orbach. While there has been a great deal of progress on the design

and building a truly international collaboration, the funding of the project remains an unsolved problem.

Of course, Fermilab is not unique in confronting the challenge of the future. How are other labs planning for the future? While SLAC maintains a strong commitment to a linear collider, its future is tied to applications of synchrotron radiation including the new LCLS, while high energy research will be based on non-accelerator techniques. DESY, while maintaining interest in a linear collider, is focusing on a free electron laser application of the linear collider technology. KEK continues work on linear collider technology, but participates in J-PARC and is likely to continue to invest in B-meson flavor physics. None of these labs seems to be basing their futures solely on the success of an ILC proposal.

It is certainly possible to take the position that a new high energy accelerator is the only worthwhile future project. If so, the current plans seem adequate. While we can certainly advocate funding and try to make Fermilab attractive as a potential site, the fact is that the fate of the ILC is largely beyond our control. Stepping up design efforts on other large projects (muon collider or a VLHC) is hardly an alternative. These projects – even if scientific and technical concerns could be addressed – are huge projects that would not be ready for construction in the near future. The question is whether there is some scientifically valuable activity or project that could serve as a core activity for the near term at Fermilab. I would think that the criteria for the project would be:

- a) Have a modest cost. This probably means something in the range of 100 M\$ (much smaller isn't the scale required to sustain a major laboratory) and less than 1 B\$ (no longer modest).
- b) Be science driven. A science driven project will stand on its own merits; technical connections to other projects (like ILC) should be considered a plus but not as the driver.
- c) Be integrated across laboratory. The project would ideally involve as many parts of the laboratory as possible. In particular, it would be desirable to have at least some of the research effort and technological development directed towards a common project.
- d) Be sufficiently flexible in the implementation plan that later stages can be abandoned in favor of a construction start on the ILC, whenever it occurs.

The only practical candidate for such a project in the near term seems to be to aggressively develop the high intensity capability of the existing accelerator complex. While we have already made steps in that direction, we could be much more aggressive. The science will certainly be less compelling than an ILC, but the cost is much lower, and perhaps a broader program than the one currently envisioned could be competitive.

The existence of Fermilab has historically been based on a large, expensive accelerator complex that demands the facilities of a large laboratory. The science program used to be based almost exclusively on the facilities on site: recent trends have been towards facilities at other sites. The trend will increase dramatically when the

Tevatron operations cease. Work at off-site facilities makes excellent sense when the infrastructure required for the core program must necessarily exist anyway. But it could very well be decided that other national laboratories or universities are better suited for this type of research. An ILC at Fermilab would, of course, be a solution to this problem. The question is whether that is the only solution.

Sincerely,

John Marriner