1 Thoughts on CDF and Fermilab Collider Physics beyond 2008-9

The reason for my concern about the future of CDF, Fermilab, and US HEP is that I believe we need to have a program of accelerator-based experiments at Fermilab for Fermilab, and thus US HEP, to survive. If this (to keep the Lab open) were the only motivation, we should close it. There is, however, a strong program of basic measurements that are unique to Fermilab, and that will not be done elsewhere in at least many years. These kinds of measurements and the innovations in hardware they may require are ideal for training students (Jack Steinberger once disingenuously asked 'why is it the US produces such good students and builds such lousy detectors?'). We need to do the R&D for the ILC, but we also need to keep the field attractive with opportunities for the best and the brightest young folks looking for fundamental measurements to make. The Tevatron is a unique source of flavor and QCD physics, and we should be thinking about how to exploit the Collider for unique and important measurements beyond 2008-9. This may mean moving to a single detector with unique capabilities such as total particle ID, a factor of 100-1000 in event/sec bandwidth, a substantially more powerful SVT, and an extended silicon/tracking reach, for example. While this is beyond the end of a spokesperson's term, and in any case is not in the job description, R&D for the future should be part of the discussion now. CDF is a huge investment in experience, talent, and money, and we should be thinking of how to best exploit these assets and sustain a US technical infrastructure.

Rather than trying to stitch a set of inter-related thoughts into a fabric, I list them linearly below:

- The schedule for the ILC will certainly stretch; the chance of Fermilab staying viable without a broad program until the ILC siting is a Sudakov-like probability- it gets smaller rapidly with the gap. If Fermilab isn't robust at the time of the ILC decision the US will find it hard to compete with a European (possibly CLC-based) proposal. At that point one has no base left on which to build new directions.

- The ILC decision will not happen until all 3 regions– Asia, Europe, and the US – are ready to agree on funding. Europe in particular has its hands full with the LHC, and there will be little incentive for CERN to push the ILC aggressively in Europe until they are ready. It could be a very long time, given the LHC, LHC-upgrades, funding cuts, etc.

- HEP is on the verge of major discoveries, we believe, and we do not know which will be the directions of the future. It may be that there are incisive measurements to be made at the Tevatron based on knowledge gained with the LHC, for example.

- The questions of "why 3 generations?", "why doublets?", "why mixing?", "why leptons and quarks?" are key questions, which need ongoing programs in both the quark and neutrino sectors. The Collider is a unique strange, charm, and bottom meson and baryon factory, which could be exploited by a 'fourth generation' detector based on upgrading either CDF or DØ.

- The lower luminosity and energy may prove to be critical to world-class precision measurements of the W and top masses, competitive or better than, and with different systematics from, those at the LHC.

- There is a wealth of QCD, soft, longitudinal (in the Peyrou plot sense), and 'engineering' measurements that will not be made elsewhere; many of these will be needed at the LHC to fully-exploit the statistical power of the LHC (up to their belly-buttons in W's and Z's and jets- it's all going to be systematics).

- A smaller setting can provide flexibility for new detector ideas, new physics ideas, other technical innovation; these are very attractive to the best incoming grad students, in fact more so than finding the Higgs as a thesis topic in competition with other students. I have found that the best students are attracted to hardware innovation over the more glamorous big but 'managed' efforts.

- While the funding for US HEP has shrunk considerably, we have not been nimble in making the appropriate increases in efficiency to compensate. We must be much more efficient, particularly in eliminating duplicate effort. Good tools, talented people, and good Lab support could make a great environment to continue doing really interesting physics beyond 2008-9.

This will not be everyone's cup-of-tea- most will go to the LHC, I expect. I would be happy to discuss the future with you, however.