MUON COLLIDER MOTIVATIONS

1. MUC could be valuable (i.e. competitive with LC) for precision measurements of $\mu^+\mu^- \to h \to b\bar{b}$ and $\Gamma_h^{\rm tot}$ if m_h near current limits.

2. Only the MUC can measure the $h \to \mu^+\mu^-$ coupling. 4% accuracy $\Rightarrow$ probe $m_{A^0} \sim 600$ GeV with no systematics. $\Rightarrow$ Crucial to testing leptonic mass generation ideas.

3. The MUC is best for discovery and study of general Higgs with no WW, ZZ coupling, especially in LHC/LC moderate $\tan\beta$ wedge. Scan or use bbstrahlung tail. (a_μ motivates light A^0.)

   Only MUC could separate H^0 and A^0 peaks near decoupling limit.

4. Only the MUC can resolve a continuum Higgs signal into separate peaks — only need to scan in $\sqrt{s}$ and use counting.

5. The MUC is the best machine for discovery/study of pseudo Nambu Goldstone bosons of TC theories.

6. The MUC is the best machine for determining the CP of Higgs bosons (especially those with no ZZ coupling) or PNGB's.

7. The MUC is the best machine for studying doubly-charged Higgs bosons (found at LHC) from a triplet representation with $\langle\Delta^0\rangle = 0$ ($\rho = 1$ is natural), especially if Majorana coupling $\propto$ mass. $\Rightarrow$ probes couplings down to see-saw level.

8. Whatever collider can reach extreme energies should be built, but MUC might turn out to be most viable.

9. MUC is again unique if there are very heavy scalar bosons (too heavy to be pair produced) with muonic couplings and no WW, ZZ couplings.

   Remember: $\alpha_s(m_Z) < 0.12$ coupling unification requires high mass SUSY; absence of FCNC's motivates super-heavy supersymmetry; bench-mark points at high mass fully viable.

10. Imagine:(a) we know that SUSY is correct, and masses of scalars suggest the possibility of large m_{A^0}; (b) at the LHC and in e^+e^- collisions we can only see the h^0, but there are some small deviations of its properties from SM predictions suggesting 300 GeV $\lesssim m_{A^0} \lesssim$ 500 GeV; (c) the SUSY $\tan\beta$ and m_{A^0} are such that the $\gamma\gamma$ collider fails to see A^0, H^0 (as is possible with current estimates if m_{A^0} is not near $2m_t$ and $\tan\beta$ is moderately larger than 5.)

    $\Rightarrow$ we would really want a MUC.

11. Suppose: μ's and e's are different in some fundamental manner not currently envisioned.

    $\Rightarrow$ we really need a MUC to discover this.

R&D to make the MUC a reality should receive sufficient funding to get the project moving as rapidly as possible