The Case for Innovative Research in Accelerator Science

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Following Snowmass 2013, the subsequent P5 report, and the resulting HEPAP subpanel review of OHEP's general accelerator research (GARD) program, we did our own review of R&D toward a new generation of discovery science facilities. We urged a deeper investment in innovative, transformational research with a strong university component. The Snowmass 2013 Accelerator Capabilities study found that the advance of accelerator science was handicapped by excessive focus by DOE/HEP on project-driven R&D. Consequently, little "free energy" remained for broad, foundational research in rf-structures, rf power sources, and accelerator theory. What was true then is worse now, especially at universities not intimately associated with a nearby national laboratory.

The financial environment of the accelerator R&D portfolio emerging from Snowmass 2020 and P5 is unlikely to improve the excessively focused R&D situation. Even the expanded "stewardship program" in OHEP is directed more at technology transfer than to revitalizing transformational work in accelerator technology.

In our review of U.S. accelerator R&D supporting high-energy physics we emphasized that going much further requires changing the capability-cost curve of accelerators. That can only happen with an aggressive, sustained, and imaginative R&D program with an eye on practicality. That certainly has not happened to the degree recommended by Snowmass 2013 or by the subsequent HEPAP sub-panel review of GARD. GARD must focus on the ability to build future accelerators at dramatically lower cost per unit energy, emphasizing practicality of concept, sound engineering and realistic costs-performance tradeoffs, while leaving sufficient room for fundamental accelerator theory that will anticipate the pathologies of machines operating in new parameter regimes. Moreover its budget must leave room for transformative research at both labs and universities.

Linear colliders: We consider cryogenically cooled copper (C^3) accelerators developed at SLAC to offer the best chance for a practical, "affordable" e^+e^- collider. The beam travels in vacuum through a structure with reasonable aperture. C^3 has no staging or emittance growth issues beyond those due to higher order modes (HOM) in the structure. Gradients of >120 MeV/m have already been demonstrated. Work on HOM is proceeding. A hot test will soon be conducted at LANL. GARD should push this option far more vigorously despite political concerns.

Meanwhile SRF for linear colliders is being "stress-tested" for "affordability" with PIP-II and LCLS-II . Should ILC not proceed in Japan, any new effort will need much higher effective gradient technology (GeV/km) than that presently attained.

Although both the PWFA and LWFA are long shots for HEP, they deserve another decade of support due to their rich intellectual content. Both techniques must still demonstrate robust staging, 6-D emittance preservation, positron acceleration and demanding beam stability to be plausible candidates for a collider. LWFAs might be promising for FELs, but that application would need its primary support from DOE/OBES.

Proton colliders: There are no new concepts for proton acceleration; and a new collider will be a synchrotron. Both CERN and China are pursuing 100 km circumference rings first for e^+e^- Higgs factories and then for 100 TeV scale pp colliders. It seems *extremely unlikely* that both will happen. As e^+e^- colliders, luminosity falls dramatically with increasing energy, and the design would have enormous synchrotron radiation loads, usually fixed by design at 50 MW/beam. For reaching 100 TeV_{cm}, a 100 km p-p machine will require dipoles well beyond the state of the art, dramatically so for operating fields exceeding 16 T. Progress has been steady but slow over the past 20 years.

The FCC report is a thorough, modern basis for programmatic decisions given strong physics justification. Serious optimization studies, including consideration of larger rings, are awaited. Unfortunately U.S. participation in the FCC effort was embarrassingly weak despite the excitement about a 100 TeV collider during Snowmass 2013. The U.S. should participate in future optimization studies of an energy frontier proton collider.

We find that OHEP could not meet the hopes and expectations of P5. A serious optimization study that includes careful analysis of $/T-m for accelerator dipole magnets is still in the future. The ILC is (to put it mildly) uncertain. GARD funding is both too small and too over-committed to push hard on new techniques: 1) Despite its clear practicality and promise, advanced normal conducting RF (e.g., C^3) has so far received insufficient support. 2) Wakefield acceleration schemes for e^+e^- colliders may seem interesting, but their wall plug efficiency is unlikely to surpass that of CLIC. Difficult challenges in plasma physics and engineering remain. 3) SRF is relatively low gradient and expensive. 4) Support for fundamental theoretical and computational accelerator physics is *grossly insufficient* to ensure a healthy, broad program of accelerator research; that is a conscious choice

Training scientists and engineers: OHEP's reduction of the opportunities for innovative accelerator research has grave implications for training accelerator physicists and engineers. It puts at grave risk the most successful programs to educate and train a new generation of accelerator scientists and technologists. Stemming from the same root cause of this decline, OHEP has increased pressures on America's regional accelerator school, transforming it from a community-wide enterprise to a single laboratory program and simultaneously decreasing both the support and flexibility of its director.

Our distilled bottom line: To make substantial advances in accelerator capabilities consistent with P5's aspirations the GARD program must have an investment budget that 1) *grows with inflation*, 2) *is not a slave to institutional priorities*, and 3) has a *vibrant university-based component*.

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