Energy Frontier Vision Building: Lessons from the Past and the Agora Events¶

Energy Frontier Workshop

April 1, 2022

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Outline

- Aspirations (others and mine)
- Snowmass 2013 and P5 2014
- A project ready for consideration
- Expectations for Snowmass21 pointing towards P5 2023

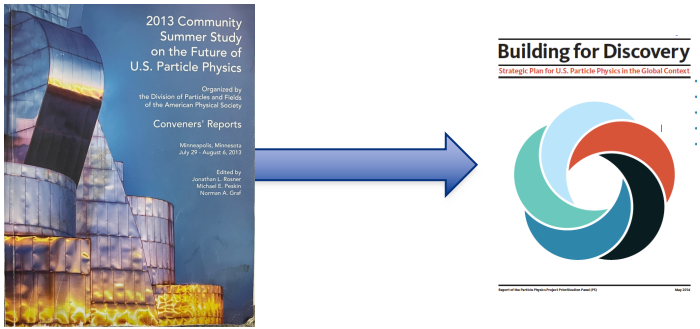
Aspirations over Time

- At Snowmass **2001**, goals were to discover the **Higgs**; detect **SUSY whose lightest members would be accessible at a 500 or 800 GeV linear collider**, explore the SUSY spectrum up to ~20TeV, where the heaviest members were expected to provide insight into the SUSY-breaking interactions.
- When the LHC was starting in 2010, **SUSY was considered the low-hanging fruit, observable in a few hundred pb^{-1}**
- By Snowmass 2013, the **Higgs was found** (7, 8 TeV), **SUSY not**!
- By **2015 -2018:** The LHC showed it **could study the Higgs at an unexpected level of precision,**
  - **still no SUSY and no new mass scale**

Current aspirations for many are to study the Higgs much better than we can at the HL-LHC and search for new particles, interactions at >~ 10 TeV

Snowmass 2013/P5 2014

- In 2013, Snowmass provided input to the High Energy Physics Advisory Panel's (HEPAP) subpanel, the Particle Physics Project Prioritization Panel, a.k.a. P5
  - Using Snowmass's scientific input and budget scenarios provided by US DOE, P5 developed and presented to DOE, via HEPAP, a 10-year **execution plan, with priorities and recommendations, for the field in the US, with an eye also towards the ten years following that**
- P5 has a broad mandate but tends to focus on large projects and facilities



Did Snowmass 2013 have an impact on the P5 outcome?

- I would say yes!
- The main recommendations (LHC, neutrinos) may have been rather obvious even at the beginning of the process, but needed justification
- There were, however, 29 recommendations, including
  - Maintain a program of projects of all scales, from the largest international projects to mid- and small-scale projects.
  - Recommendation 5: Increase the budget fraction invested in construction of projects to the 20%–25% range.
  - … The research program should provide the flexibility to support new ideas and developments
  - Select and perform in the short term a set of small-scale short-baseline experiments
  - Build DESI, Complete LSST,
  - Proceed with G2 Dark Matter programs, support one or more G3 dark Matter Programs
  - Complete Mu2e and muon g-2

.

I do not believe that all these would have been included without our strong communities developing excellent proposals

"P5" Drivers

- **From P5 report:** Snowmass, the yearlong community-wide study, preceded the formation of our new P5. A vast number of scientific opportunities were investigated, discussed, and summarized in Snowmass reports. **We distilled those essential inputs into five intertwined science Drivers for the field:**
  - Use the **Higgs** boson as a new tool for discovery
  - Pursue the physics associated with **neutrino** mass
  - Identify the new physics of **dark matter**
  - Understand cosmic acceleration: **dark energy and inflation**
  - Explore the unknown: **new** particles, interactions, and physical principles.

Please look at pages 1 and 2 of the Snowmass 2013 Report

P5 Recommendations for EF

- **Recommendation 10: The LHC upgrades constitute our highest-priority near-term large project.**
- **Recommendation 11**:Engage in **modest** and appropriate levels of **ILC** accelerator and detector **design** … Consider higher levels of collaboration if ILC proceeds.
- **Recommendation 24:** Participate in global conceptual **design studies** and critical path R&D for future very **high-energy proton-proton colliders**. Continue to play a leadership role in superconducting magnet technology
- **Recommendation 26:** Pursue accelerator R&D. Focus on outcomes and capabilities that will dramatically **improve cost effectiveness** for mid-term and far-term **accelerators.**

EF Recommendations on Muon Collider Program

- **Recommendation 25: Reassess the Muon Accelerator Program (MAP).** Incorporate into the GARD program the MAP activities that are of general importance to accelerator R&D… and consult with international partners on the early termination of MICE.

It would be wise to focus on **how circumstances have changed in physics, technology, and studies** that have made a reexamination of the status of directed muon collider R&D appropriate in 2024+

A Project Ready to "Approve"

- Is there a collider construction project ready to be presented for approval?
  - Physics Case
  - Demonstrated Technical Design
  - Host organization
  - Cost Estimate
  - Plausible demonstration of funding
  - Understanding of how risk will be "shared"
- Until something like this exists, it is not likely a project can move forward in the US

We have done a superb job of developing projects through the first two steps , as seen in this Agora series, but have not done so well at the remaining four tasks.

Towards a project

- **Given the ILC situation, there is no collider project that can be put forward as ready for approval**
- If collider physics is to continue, the field MUST have an international R&D program so we are ready to move forward
- **At least** two paths
  - **FCC-ee ➔ FCC-hh**
    - A very comprehensive, very long-term program, but with many challenges
      - FCC-ee operation, 2045-2060
        - For US contribution, funding needs may be matched with roll-off of spending on Long Baseline Neutrino
      - FCC-hh operation ~2070 – 2090++
  - **A linear collider followed by a muon collider** of 10 or preferably 20 TeV or a p-p collider of CM energy of several 10's of TeV
- **Other paths? Not too many, or none will reach a critical mass!**

Snowmass 2021/22

- **We must make strong physics cases for near-term, i.e., the HL-LHC, and far term, i.e., new colliders**
  - We have to clearly identify the physics advantages of each and how they differ or overlap
- We should acknowledge the current situation of project readiness and also the demands on funding of ongoing projects which affects timing of new projects
- **The next big decision will be based on what CERN learns about the technical feasibility, siting issues, and cost of the large tunnel and FCC-ee**
  - We need to prepare an R&D program that can support it but also can pursue the most promising directions if it cannot go forward
- We might anticipate a decision point ~ 4years from now, well before the "natural" time for the next P5
  - **We should put in place now what is needed to be ready for that**

Backup Slides

P5: 2023

- **In 2013, Snowmass provided input to the High Energy Physics Advisory Panel's (HEPAP) subpanel, the Particle Physics Project Prioritization Panel, a.k.a. P5**
  - Using Snowmass's scientific input and budget scenarios provided by US DOE, P5 developed and presented to DOE, via HEPAP, a 10-year **execution plan, with priorities and recommendations, for the field in the US, with an eye also towards the ten years following that**
    - **P5 has a broad mandate but tends to focus on large projects and facilities**

The "Snowmass Book" that we will produce and the many supporting Contributed Papers will provide critical input to P5

EF Recommendations - I

- **Recommendation 10:** Complete the LHC phase-1 upgrades and continue the strong collaboration in the LHC with the phase-2 (HL-LHC) upgrades of the accelerator and both general-purpose experiments (ATLAS and CMS). **The LHC upgrades constitute our highest-priority near-term large project.**
- **Recommendation 11**: Motivated by the strong scientific importance of the ILC and the recent initiative in Japan to host it, the U.S. should **engage in modest and appropriate levels of ILC accelerator and detector design** in areas where the U.S. can contribute critical expertise**. Consider higher levels of collaboration if ILC proceeds.**

EF Recommendations - II

- **Recommendation 24:** Participate in global **conceptual design studies and critical path R&D for future very high-energy proton-proton colliders.** Continue to play a **leadership role in superconducting magnet technology** focused on the dual goals of increasing performance and decreasing costs.
- **Recommendation 26: Pursue accelerator R&D** with high priority at levels consistent with budget constraints. Align the present R&D program with the P5 priorities and long term vision, with an appropriate balance among general R&D, directed R&D, and accelerator test facilities and among short-, medium-, and long-term efforts. **Focus on outcomes and capabilities that will dramatically improve cost effectiveness for mid-term and far-term accelerators.**

EF Recommendations - III

- **Recommendation 25: Reassess the Muon Accelerator Program (MAP).** Incorporate into the GARD program the MAP activities that are of general importance to accelerator R&D, and consult with international partners on the early termination of MICE.

P5 - I

Snowmass 2021

- Recommendation 4 : Maintain a program of projects of all scales, from the largest international projects to mid- and small-scale projects.
  - Advances in particle physics come from a combination of experimental and theoretical work, as well as from R&D for advanced accelerator and experimental techniques. Experimental research requires development, construction, operation, and scientific exploitation of projects and facilities, often of significant scale. **Unlike other regions in the world, in recent years the U.S. particle physics program has not invested substantially in construction of experimental facilities.** Addressing the Drivers in the coming and subsequent decades requires renewed investment in projects. In constant or near-constant budgets, this implies an increase in the fraction of the budget that is invested in new projects, which is currently approximately 16%.
- Recommendation 5: Increase the budget fraction invested in construction of projects to the 20%–25% range.

P5 - III

Snowmass 2021

- Recommendation 10: Complete the LHC phase-1 upgrades and continue the strong collaboration in the LHC with the phase-2 (HL-LHC) upgrades of the accelerator and both general-purpose experiments (ATLAS and CMS). The LHC upgrades constitute our highest-priority near-term large project. The minimum requirements in the text. LBNF is the highest priority large project in its timeframe.
- Recommendation 13: Form a new international collaboration to design and execute a highly capable Long-Baseline Neutrino Facility (LBNF) hosted by the U.S. To proceed, a project plan and identified resources must exist to meet the minimum requirements in the text. LBNF is the highest priority large project in its timeframe.