
Building for Discovery

Strategic Plan for U.S. Particle Physics in the Global Context

Executive Summary



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Particle physics explores the fundamental constituents of matter and energy. It reveals the profound connections underlying everything we see, including the smallest and the largest structures in the Universe. The field is highly successful. Investments have been rewarded recently with discoveries of the heaviest elementary particle (the top quark), the tiny masses of neutrinos, the accelerated expansion of the Universe, and the Higgs boson. Current opportunities will exploit these and other discoveries to push the frontiers of science into new territory at the highest energies and earliest times imaginable. For all these reasons, research in particle physics inspires young people to engage with science.

Particle physics is global. The United States and major players in other regions can together address the full breadth of the field's most urgent scientific questions if each hosts a unique world-class facility at home and partners in high-priority facilities hosted elsewhere. Strong foundations of international cooperation exist, with the Large Hadron Collider (LHC) at CERN serving as an example of a successful large international science project. Reliable partnerships are essential for the success of international projects. Building further international cooperation is an important theme of this report, and this perspective is finding worldwide resonance in an intensely competitive field.

Choices are required. Ideas for excellent new projects far exceed what can be executed with currently available resources. The U.S. must invest purposefully in areas that have the biggest impacts and that make most efficient use of limited resources. Since the 2008 Particle Physics Project Prioritization Panel (P5) report, two major U.S. particle physics facilities have terminated operations, and inflation-adjusted funding in the U.S. for particle physics has continued to decline. In addition, primarily because of earlier strong investments, landmark discoveries have been made that inform choices for future directions. A new P5 panel was therefore charged to provide "an updated strategic plan for the U.S. that can be executed over a ten-year timescale, in the context of a twenty-year global vision for the field." The Charge calls for planning under two specific budget Scenarios, reflecting current fiscal realities, as well as for an additional unconstrained Scenario.

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.**

The vision for addressing these Drivers with a prioritized set of projects, including their approximate timescales and how they fit together, was developed using a set of selection criteria. The Drivers, which are intertwined, are not prioritized. Instead, the prioritization is in the selection and timing of the specific projects, which are categorized as large, medium, or small based on the construction costs to the particle physics program.

To enable an optimal program, given recent scientific results and funding constraints, and using our criteria, we recommend some projects not be implemented, others be delayed, and some existing efforts be reduced or terminated. Having made these choices, the field can move forward immediately with a prioritized, time-ordered program, which is summarized in [Table 1](#) and includes the following features:

- The enormous physics potential of the LHC, which will be entering a new era with its planned high-luminosity upgrades, will be fully exploited. The U.S. will host a world-leading neutrino program that will have an optimized set of short- and long-baseline neutrino oscillation experiments, and its long-term focus is a reformulated venture referred to here as the Long Baseline Neutrino Facility (LBNF). The Proton Improvement Plan-II (PIP-II) project at Fermilab will provide the needed neutrino physics capability. To meet budget constraints, physics needs, and readiness criteria, large projects are ordered by peak construction time: the Mu2e experiment, the high-luminosity LHC upgrades, and LBNF.
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- The interest expressed in Japan in hosting the International Linear Collider (ILC) is an exciting development. Participation by the U.S. in project construction depends on a number of important factors, some of which are beyond the scope of P5 and some of which depend on budget Scenarios. As the physics case is extremely strong, all Scenarios include ILC support at some level through a decision point within the next 5 years.
- Several medium and small projects in areas especially promising for near-term discoveries and in which the U.S. is, or can be, in a leadership position, will move forward under all budget scenarios. These are the second- and third-generation dark matter direct detection experiments, the particle physics components of the Large Synoptic Survey Telescope (LSST) and cosmic microwave background (CMB) experiments, and a portfolio of small neutrino experiments. Another important project of this type, the Dark Energy Spectroscopic Instrument (DESI), will also move forward, except in the lowest budget Scenario.
- With a mix of large, medium, and small projects, important physics results will be produced continuously throughout the twenty-year P5 timeframe. In our budget exercises, we maintained a small projects portfolio to preserve budgetary space for a set of projects whose costs individually are not large enough to come under direct P5 review but which are of great importance to the field. This is in addition to the aforementioned small neutrino experiments portfolio, which is intended to be integrated into a coherent overall neutrino program.
- Specific investments will be made in essential accelerator R&D and instrumentation R&D. The field relies on its accelerators and instrumentation and on R&D and test facilities for these technologies.

Several significant changes in direction are recommended:

- Increase the fraction of the budget devoted to construction of new facilities.
- Reformulate the long-baseline neutrino program as an internationally designed, coordinated, and funded program with Fermilab as host.

- Redirect former Project-X activities and some existing accelerator R&D to improvements of the Fermilab accelerator complex that will provide proton beams with power greater than one megawatt by the time of first operation of the new long-baseline neutrino facility.
- Increase the planned investment in second-generation dark matter direct detection experiments.
- Increase particle physics funding of CMB research and projects in the context of continued multiagency partnerships.
- Realign activities in accelerator R&D with the P5 strategic plan. Redirect muon collider R&D and consult with international partners on the early termination of the MICE muon cooling R&D facility.

The two constrained budget Scenarios differ by approximately \$30M per year until FY2018, and thereafter have a one percent escalation difference. While seemingly small, these differences would have very large short- and long-term impacts: in the lower funding Scenario, in addition to the aforementioned loss of DESI, accelerator R&D and advanced detector R&D would be substantially reduced; research capability would be compromised due to personnel reductions; ramp up of funding for the long-baseline neutrino program would be delayed (preliminary work would still proceed immediately in both scenarios); third-generation direct detection dark matter capabilities would be reduced or delayed; and a small reprofiling of Mu2e would be necessary. Thus, the relatively small increment in funding in the higher Scenario yields a very large return on investment.

The lowest budget Scenario is precarious: it approaches the point beyond which hosting a large (\$1B scale) project in the U.S. would not be possible while maintaining the other elements necessary for mission success, particularly a minimal research program, the strong U.S. leadership position in a small number of core, near-term projects, which produce a steady stream of important new physics results, and advances in accelerator technology. Without the capability to host a large project, the U.S. would lose its position as a global leader in this field, and the international relationships that have been so productive would be fundamentally altered.

The recommendations for the unconstrained budget Scenario focus on three additional high-priority activities:

- Develop a greatly expanded accelerator R&D program that would emphasize the ability to build very high-energy accelerators beyond the High-Luminosity LHC (HL-LHC) and ILC at dramatically lower cost.
- Play a world-leading role in the ILC experimental program and provide critical expertise and components to the accelerator, should this exciting scientific opportunity be realized in Japan.
- Host a large water Cherenkov neutrino detector to complement the LBNF large liquid argon detector, unifying the global long-baseline neutrino community to take full advantage of the world's highest intensity neutrino beam at Fermilab.

With foundations set by decades of hard work and support, U.S. particle physics is poised to move forward into a new era of discovery. More generally, we strongly affirm the essential importance of fundamental research in all areas of science.

Table 1

Summary of Scenarios

Project/Activity	Scenarios			Science Drivers					Technique (Frontier)
	Scenario A	Scenario B	Scenario C	Higgs	Neutrinos	Dark Matter	Cosm. Accel.	The Unknown	
Large Projects									
Muon program: Mu2e, Muon g-2	Y, Mu2e small reprofile needed	Y	Y					✓	I
HL-LHC	Y	Y	Y	✓		✓		✓	E
LBNF + PIP-II	Y, LBNF components delayed relative to Scenario B.	Y	Y, enhanced		✓			✓	I,C
ILC	R&D only	R&D, possibly small hardware contributions. See text.	Y	✓		✓		✓	E
NuSTORM	N	N	N		✓				I
RADAR	N	N	N		✓				I
Medium Projects									
LSST	Y	Y	Y		✓		✓		C
DM G2	Y	Y	Y			✓			C
Small Projects Portfolio	Y	Y	Y		✓	✓	✓	✓	All
Accelerator R&D and Test Facilities	Y, reduced	Y, some reductions with redirection to PIP-II development	Y, enhanced	✓	✓	✓		✓	E,I
CMB-S4	Y	Y	Y		✓		✓		C
DM G3	Y, reduced	Y	Y			✓			C
PINGU	Further development of concept encouraged				✓	✓			C
ORKA	N	N	N					✓	I
MAP	N	N	N	✓	✓	✓		✓	E,I
CHIPS	N	N	N		✓				I
LAr1	N	N	N		✓				I
Additional Small Projects (beyond the Small Projects Portfolio above)									
DESI	N	Y	Y		✓		✓		C
Short Baseline Neutrino Portfolio	Y	Y	Y		✓				I

TABLE 1 Summary of Scenarios A, B, and C. Each major project considered by P5 is shown, grouped by project size and listed in time order based on year of peak construction. Project sizes are: Large (>\$200M), Medium (\$50M-\$200M), and Small (<\$50M). The science Drivers primarily addressed by each project are also indicated, along with the Frontier technique area (E=Energy, I=Intensity, C=Cosmic) defined in the 2008 P5 report.

Figure 1

Construction and Physics Timeline



FIGURE 1 Approximate construction (blue; above line) and expected physics (green; below line) profiles for the recommended major projects, grouped by size (Large [$> \$200\text{M}$] in the upper section, Medium and Small [$< \$200\text{M}$] in the lower section), shown for Scenario B. The LHC: Phase 1 upgrade is a Medium project, but shown next to the HL-LHC for context. The figure does not show the suite of small experiments that will be built and produce new results regularly.

Full List of Recommendations

For convenience, we gather here the full list of recommendations from the report, with the caveat that some meaning is lost when taken out of context. Reference is provided to the page in Section 2 upon which each recommendation appears.

Recommendation 1: Pursue the most important opportunities wherever they are, and host unique, world-class facilities that engage the global scientific community.

Recommendation 2: Pursue a program to address the five science Drivers.

Recommendation 3: Develop a mechanism to reassess the project priority at critical decision stages if costs and/or capabilities change substantively.

Recommendation 4: 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.

Recommendation 6: In addition to reaping timely science from projects, the research program should provide the flexibility to support new ideas and developments.

Recommendation 7: Any further reduction in level of effort for research should be planned with care, including assessment of potential damage in addition to alignment with the P5 vision.

Recommendation 8: As with the research program and construction projects, facility and laboratory operations budgets should be evaluated to ensure alignment with the P5 vision.

Recommendation 9: Funding for participation of U.S. particle physicists in experiments hosted by other agencies and other countries is appropriate and important but should be evaluated in the context of the Drivers and the P5 Criteria and should not compromise the success of prioritized and approved particle physics experiments.

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.

Recommendation 12: In collaboration with international partners, develop a coherent short- and long-baseline neutrino program hosted at Fermilab.

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.

Recommendation 14: Upgrade the Fermilab proton accelerator complex to produce higher intensity beams. R&D for the Proton Improvement Plan II (PIP-II) should proceed immediately, followed by construction, to provide proton beams of >1 MW by the time of first operation of the new long-baseline neutrino facility.

Recommendation 15: Select and perform in the short term a set of small-scale short-baseline experiments that can conclusively address experimental hints of physics beyond the three-neutrino paradigm. Some of these experiments should use liquid argon to advance the technology and build the international community for LBNF at Fermilab.

Recommendation 16: Build DESI as a major step forward in dark energy science, if funding permits (see Scenarios discussion below).

Recommendation 17: Complete LSST as planned.

Recommendation 18: Support CMB experiments as part of the core particle physics program. The multidisciplinary nature of the science warrants continued multiagency support.

Recommendation 19: Proceed immediately with a broad second-generation (G2) dark matter direct detection program with capabilities described in the text. Invest in this program at a level significantly above that called for in the 2012 joint agency announcement of opportunity.

Recommendation 20: Support one or more third-generation (G3) direct detection experiments, guided by the results of the preceding searches. Seek a globally complementary program and increased international partnership in G3 experiments.

Recommendation 21: Invest in CTA as part of the small projects portfolio if the critical NSF Astronomy funding can be obtained.

Recommendation 22: Complete the Mu2e and muon g-2 projects.

Recommendation 23: Support the discipline of accelerator science through advanced accelerator facilities and through funding for university programs. Strengthen national laboratory-university R&D partnerships, leveraging their diverse expertise and facilities.

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 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.

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.

Recommendation 27: Focus resources toward directed instrumentation R&D in the near-term for high-priority projects. As the technical challenges of current high-priority projects are met, restore to the extent possible a balanced mix of short-term and long-term R&D.

Recommendation 28: Strengthen university-national laboratory partnerships in instrumentation R&D through investment in instrumentation at universities. Encourage graduate programs with a focus on instrumentation education at HEP supported universities and laboratories, and fully exploit the unique capabilities and facilities offered at each.

Recommendation 29: Strengthen the global cooperation among laboratories and universities to address computing and scientific software needs, and provide efficient training in next-generation hardware and data-science software relevant to particle physics. Investigate models for the development and maintenance of major software within and across research areas, including long-term data and software preservation.

Panel Members

Steve Ritz, *chair*

University of California, Santa Cruz

Hiroaki Aihara

University of Tokyo

Martin Breidenbach

SLAC National Accelerator Laboratory

Bob Cousins

University of California, Los Angeles

André de Gouvêa

Northwestern University

Marcel Demarteau

Argonne National Laboratory

Scott Dodelson

*Fermi National Accelerator Laboratory
and University of Chicago*

Jonathan L. Feng

University of California, Irvine

Bonnie Fleming

Yale University

Fabiola Gianotti

*European Organization for Nuclear
Research (CERN)*

Francis Halzen

University of Wisconsin-Madison

JoAnne Hewett

SLAC National Accelerator Laboratory

Wim Leemans

Lawrence Berkeley National Laboratory

Joe Lykken

Fermi National Accelerator Laboratory

Dan McKinsey

Yale University

Lia Merminga

TRIUMF

Toshinori Mori

University of Tokyo

Tatsuya Nakada

*Swiss Federal Institute of Technology
in Lausanne (EPFL)*

Steve Peggs

Brookhaven National Laboratory

Saul Perlmutter

University of California, Berkeley

Kevin Pitts

University of Illinois at Urbana-Champaign

Kate Scholberg

Duke University

Rick van Kooten

Indiana University

Mark Wise

California Institute of Technology

Andy Lankford, *ex officio*

University of California, Irvine

