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

Report of the Particle Physics Project Prioritization Panel (P5)

HEPAP
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A very dedicated, hardworking panel!

Thanks!

Our community's passion, dedication, and entrepreneurial spirit have been inspirational.

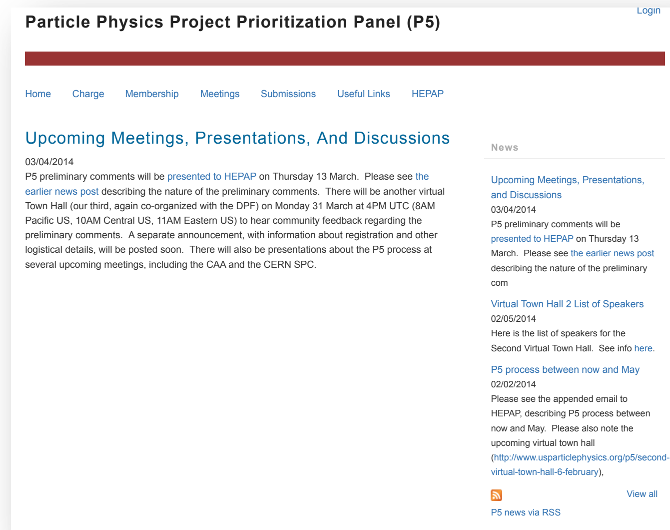
To our colleagues across our country and around the world, we say a heartfelt thank you. Every request we made received a thoughtful response, even when the requests were substantial and the schedules tight. A large number of you submitted inputs to the public portal, which we very much appreciated.

Topics

- P5 Process
- Brief recap of March Preliminary Comments presentation
- Report structure
- Introduction
  - Physics advances, global vision, changes since previous P5, Snowmass
- The Science Drivers and Main Opportunities
- Criteria
- Recommendations
  - Program-wide
  - Near-term and Mid-term High-energy Colliders
  - Neutrino Oscillation Experiments
  - Cosmic Surveys
  - Dark Matter
  - Muons and Kaons
  - Budget Scenarios (BAC)
  - Enabling R&D
  - Changes in Direction
- Benefits and Broader Impacts
- Community
- Summary and Discussion

Summary of P5 Process

- All info available on P5 website, frequently updated with News (RSS and Twitter feeds)
- Meetings:
  - Face-to-face
    - Three big, open, topical meetings: 2-4 Nov, 2-4 Dec, 15-18 Dec
    - Plus 12-14 Jan, 21-24 Feb, 5-8 April, 29-30 April
  - Frequent phone calls since September. Continuous work between meetings.
- Large Project/Activity worksheets for all phases (R&D, construction, operations) to help ensure uniformity, data quality.
- Continuous effort to maximize community interactions, including:
  - Numerous emails, outreach to younger physicists
  - Town halls at all 3 big meetings
  - Virtual town halls (with DPF) 8 Jan, 6 Feb, 31 Mar
  - Public submissions portal
  - Many ongoing discussions and consultations
- Peer review of report draft 5-10 May



http://interactions.org/p5

- Internal deliberations worked by consensus.
- No topic or option was off the table. Every alternative we could imagine was considered.

Community engagement has been essential.

March Preliminary Comments Presentation

Topics

- Review of the key elements of the charge; summary of P5 processes and activities since September
- Context:
  - The evolution of our field since the previous P5 report
  - Big scientific questions and drivers
  - The global nature of our field
- Key elements of strategic planning:
  - Opportunities to address the big scientific questions and how they fit together
  - Budgetary constraints compared with proposed programs
  - National planning in the global context
  - Balancing investments
- Discussion of prioritization criteria
- Steps to completion, and communication planning

March 2014 2

Discussed at length:

- **The 5 Science Drivers**
- **Global vision**
- **Criteria**
- **Budget scenario challenges**
- **Ongoing community interactions**

Recall, the Charge specifies three budget scenarios, with ten-year profiles:

A. *FY2013 budget baseline: flat for 3 years, then +2% per year.*

B. *FY2014 President's budget request baseline: flat for 3 years, then +3% per year.*

C. *Unconstrained:* projects "*…needed to mount a leadership program addressing the scientific opportunities…*"

Difference between scenarios integrated over the decade is ~$0.5B.

"***…consider these scenarios not as literal budget guidance but as an opportunity to identify priorities and make high-level recommendations.***"

Report Structure

- Preface
- Executive Summary

1. Introduction
   – Overview, context
   – *Brief* Summary of Science Drivers, Main Opportunities, and Enabling R&D
   – Criteria
2. Recommendations
   – Program-wide
   – Project-specific
   – Budget Scenarios
   – Enabling R&D
3. The Science Drivers
4. Benefits and Broader Impacts

Designed to move all readers quickly to the recommendations, followed by more details as needed.

Executive Summary Introduction

- 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.
  – Hosting world-class facilities and joining partnerships in facilities hosted elsewhere are both essential components of a global vision.

- 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. This global perspective is finding worldwide resonance in an intensely competitive field.
  – The 2013 *European Strategy for Particle Physics* report focuses at CERN on the Large Hadron Collider (LHC) program and envisions substantial participation at facilities in other regions.
  – Japan, following its 2012 *Report of the Subcommittee on Future Projects of High Energy Physics*, expresses interest in hosting the International Linear Collider (ILC), pursuing the Hyper-Kamiokande experiment, and collaborating on several other domestic and international projects.

Significant Developments Since the 2008 P5 Report

- Physics!
  - Higgs boson discovered at a relatively low mass, pointing the way to the next steps and informing choices for long-term planning.
  - Three Nobel Prizes related to particle physics: Quark Mixing and Symmetries, Dark Energy, Higgs Boson.
  - A key neutrino mixing parameter, $\sin^2(2\theta_{13})$, was measured to be relatively large, enabling the next steps in a campaign to understand the implications of the tiny, but non-zero, neutrino masses.
- These successes demonstrate the deep value of diversity of topic and project scale.
- New technology and innovative approaches are creating fresh opportunities that promise an even brighter future.

- Programmatic changes
  - the Deep Underground Science and Engineering Laboratory (DUSEL) did not proceed, although the Sanford Underground Research Facility (SURF) laboratory continues to develop. The Joint Dark Energy Mission (JDEM) did not proceed.
  - Tevatron collider operations and PEP-II/B-factory operations ended.
  - Inflation-adjusted funding continued to decline

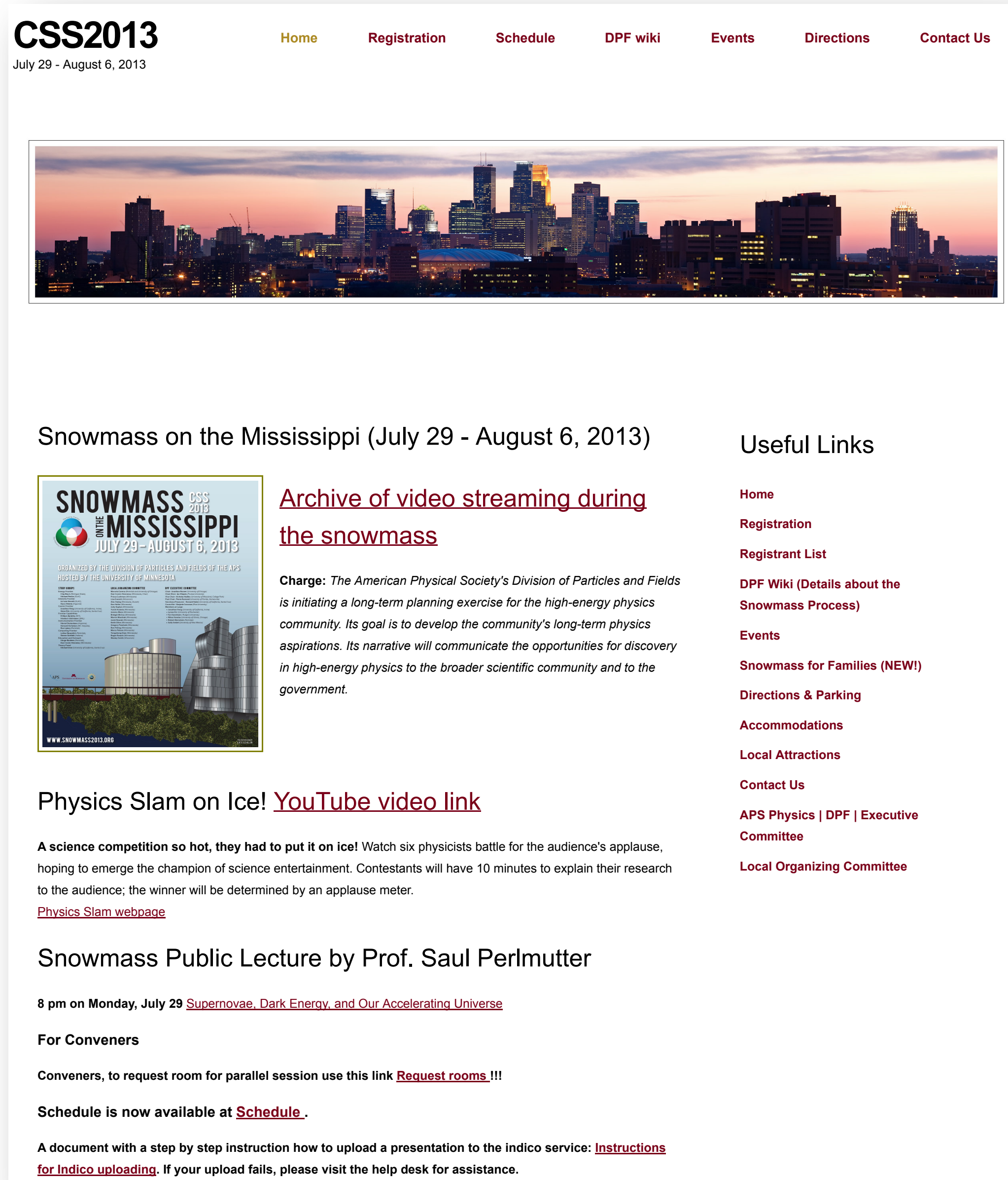
- Snowmass!

Snowmass

Snowmass, the yearlong community-wide study preceding P5, was invaluable.

A vast number of scientific opportunities were investigated, discussed, and summarized in the Snowmass reports.

See
http://www.hep.umn.edu/css2013/
and
http://www.slac.stanford.edu/econf/C1307292/



Science Drivers

- We distilled the eleven groups of physics questions from Snowmass* into five compelling lines of inquiry that show great promise for discovery over the next 10 to 20 years.
- The Science Drivers:
  - **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 Drivers are deliberately not prioritized because they are intertwined, probably more deeply than is currently understood.
- A selected set of different experimental approaches that reinforce each other is required. Projects are prioritized.
- The vision for addressing each of the Drivers using a selected set of experiments – their approximate timescales and how they fit together – is given in the report.

* See Appendix D and http://www.slac.stanford.edu/econf/C1307292/

Use the Higgs boson as a new tool for discovery

- The recently discovered Higgs boson is a form of matter never before observed.
  - What principles determine its effects on other particles? How does it interact with neutrinos or with dark matter? Is there one Higgs particle or many? Is the new particle really fundamental, or is it composed of others?
  - The Higgs boson offers a unique portal into the laws of Nature, and it connects several areas of particle physics. Any small deviation in its expected properties would be a major breakthrough.

- The full discovery potential of the Higgs will be unleashed by percent-level precision studies of the Higgs properties. The measurement of these properties is a top priority in the physics program of high-energy colliders.
  - The Large Hadron Collider (**LHC)** will be the first laboratory to use the Higgs boson as a tool for discovery, initially with substantial higher energy running at 14 TeV, and then with ten times more data at the High-Luminosity LHC (**HL-LHC)**. The HL-LHC has a compelling and comprehensive program that includes essential measurements of the Higgs properties.
  - An e^+e^- collider can provide the next outstanding opportunity to investigate the properties of the Higgs in detail. The International Linear Collider (**ILC**) is the most mature in its design and readiness for construction. The ILC would greatly increase the sensitivity to the Higgs boson interactions with the Standard Model particles, with particles in the dark sector, and with other new physics. The ILC will reach the percent or sub-percent level in sensitivity.
  - Longer-term future-generation accelerators bring prospects for even better precision measurements of Higgs properties and discovery potential.

Pursue the physics associated with neutrino mass

- Propelled by surprising discoveries from a series of pioneering experiments, neutrino physics has progressed dramatically over the past two decades, with a promising future of continued discovery.
- Many aspects of neutrino physics are puzzling. Powerful new facilities are needed to move forward, addressing:
  - What is the origin of neutrino mass? How are the masses ordered (referred to as mass hierarchy)? What are the masses? Do neutrinos and anti-neutrinos oscillate differently? Are there additional neutrino types or interactions? Are neutrinos their own antiparticles?
- The U.S. is well positioned to host a world-leading neutrino physics program, which includes an optimized set of short- and long-baseline neutrino oscillation experiments
  - The long-term focus is a reformulated venture referred to here as the Long Baseline Neutrino Facility (**LBNF**), an internationally designed, coordinated, and funded program with Fermilab as host.
  - LBNF would combine a high-intensity neutrino beam and a large-volume precision detector sited underground a long distance away to make accurate measurements of the oscillated neutrino properties. This large detector would also search for proton decay and neutrinos from supernova bursts.
- A powerful, wideband neutrino beam would be realized with Fermilab's **PIP-II** upgrade project, which provides very high intensities in the Fermilab accelerator complex.
- Cosmic surveys and a variety of other small experiments will also make important progress in answering these questions.

Identify the new physics of dark matter

- Astrophysical observations imply that the known particles make up only about one-sixth of the total matter in the Universe. The rest is dark matter (DM). The properties of dark matter particles, which are all around us, are largely unknown.
- Experiments are poised to reveal the identity of dark matter, a discovery that would transform the field of particle physics, advancing the understanding of the basic building blocks of the Universe. There are many well-motivated ideas for what dark matter could be, including
  - weakly interacting massive particles (WIMPs), axions, and new kinds of neutrinos.
- Direct detection experiments are sensitive to dark matter interactions with ordinary particles in the laboratory and will follow a progression from currently proposed second-generation (**DM G2**) experiments to much larger third-generation (**DM G3**) experiments.
- Indirect detection experiments, such as the **CTA** gamma-ray observatory, can spot the particle debris from interactions of relic dark matter particles in space. Cosmic surveys are sensitive to dark matter properties through their effects on the structures of galaxies.
- Experiments now at the LHC and eventually at future colliders seek to make dark matter particles in the laboratory for detailed studies.

Understand cosmic acceleration: dark energy and inflation

- With the telescopes that peer back in time and high-energy accelerators that study elementary particles, scientists have pieced together a story of the origin and evolution of the Universe. An important part of this story is the existence of two periods during which the expansion of the Universe accelerated.
  - A primordial epoch of acceleration, called inflation, occurred during the first fraction of a second of existence. The cause is unknown -- fundamentally new physics at ultra-high energies. A second distinct epoch of accelerated expansion began more recently and continues today, presumed to be driven by some kind of dark energy, which could be related to Einstein's cosmological constant, or driven by a different type of dark energy that evolves with time.

- Understanding inflation is possible by measuring the characteristics of two sets of primordial ripples: those that grew into the galaxies observed today, and gravitational waves, undulations in space and time that may have been observed just months ago by the BICEP2 telescope looking at the cosmic microwave background (CMB). Current CMB probes will lead to a **Stage 4 Cosmic Microwave Background (CMB-S4)** experiment, with the potential for important insights into the ultra-high energy physics that drove inflation.

- Understanding the second epoch requires better measurements:
  - The Dark Energy Spectroscopic Instrument (**DESI**) can determine the properties of dark energy to the percent level over the course of billions of years. The Large Synoptic Survey Telescope (**LSST**), measuring the positions, shapes, and distances of billions of galaxies, will perform many separate tests of the properties of dark energy.
  - Together, they can also probe the possibility that, instead of dark energy, new laws beyond those introduced by Einstein are responsible for the recent cosmic acceleration.

Explore the unknown: new particles, interactions, and physical principles

- There are clear signs of new phenomena awaiting discovery beyond those of the other four Drivers. Particle physics is a discovery science defined by the search for new particles and new interactions, and by tests of physical principles.

- Producing new particles at colliders:
  - Well-motivated extensions of the Standard Model predict that a number of such particles should be within reach of LHC. HL-LHC will extend the reach for new particles that could be missed by LHC. In the event that one or more new particles are already discovered during LHC running, HL-LHC experiments will be essential to reveal the identities and underlying physics of these particles.

- Detecting the quantum influence of new particles:
  - The existence of new particles that are too heavy to be produced directly at high-energy colliders can be inferred by looking for quantum influences in lower energy phenomena, using different kinds of particles as probes that are sensitive to different types of new particles and interactions. Some notable examples are a revolutionary increase in sensitivity for the transition of a muon to an electron in the presence of a nucleus **Mu2e** (Fermilab) and **COMET** (J-PARC), further studies of rare processes involving heavy quarks or tau leptons at **Belle II** (KEK) and **LHCb** (LHC), and a search for proton decay using the large neutrino detectors of the **LBNF** and proposed **Hyper-K** experiments.

- Future Opportunities:
  - In the longer term, **very high-energy e^+e^- colliders** and **very high-energy proton colliders** could extend the search for new particles and interactions, as well as enable precision studies of the Higgs boson and top quark properties . Upgrades at Fermilab (**PIP-II** and additional improvements) will offer further opportunities to detect the influence of new particles in rare processes.

Enabling R&D

- Advances in accelerators, instrumentation, and computing are necessary to enable the pursuit of the Drivers. Greater demands are being placed on the performance in all three areas, at reduced cost, necessitating continued investments in R&D.
- The DOE General Accelerator R&D (GARD) program and Accelerator R&D Stewardship program, as well as the new NSF Basic Accelerator Science program, form the critical basis for both long- and short-term accelerator R&D, enriching particle physics and other fields.
- **Superconducting radio-frequency accelerating cavities, high-field superconducting magnets, advanced particle acceleration techniques,** and other technologies are being developed for the required higher performance and lower cost of future accelerator concepts.
- Directed R&D programs, such as for the LHC Accelerator Research Program (**LARP**) and the Fermilab Proton Improvement Plan-II (**PIP-II**), will enable the next generation of accelerators. State-of-the-art test facilities at the national labs support activities on advanced accelerator R&D by both university and laboratory scientists.
- New particle detection techniques and instrumentation developments will provide the higher resolutions and higher sensitivities necessary to address the ever more challenging demands of future accelerator-based, underground, and cosmic particle physics experiments. Meanwhile, new computing and software techniques for acquiring, processing, and storing large data sets will empower future experiments to address not only more challenging questions, but also a broader sweep of questions.

Criteria

- Program optimization criteria
  - **Science**: based on the Drivers, assess where we want to go and how to get there, with a portfolio of the most promising approaches.
  - **International context**: pursue the most important opportunities wherever they are, and host world-leading facilities that attract the worldwide scientific community; duplication should only occur when significant value is added or when competition helps propel the field in important directions.
  - **Sustained productivity**: maintain a stream of science results while investing in future capabilities, which implies a balance of project sizes; maintain and develop critical technical and scientific expertise and infrastructure to enable future discoveries.
- Individual project criteria are
  - **Science:** how the project addresses key questions in particle physics, the size and relevance of the discovery reach, how the experiment might change the direction of the field, and the value of null results.
  - **Timing**: when the project is needed, and how it fits into the larger picture.
  - Uniqueness: what the experiment adds that is unique and/or definitive, and where it might lead. Consider the alternatives.
  - **Cost vs. value**: the scope should be well defined and match the physics case. For multidisciplinary/agency projects, distribution of support should match the distribution of science.
  - **History and dependencies**: previous prioritization, existing commitments, and the impacts of changes in direction.
  - **Feasibility**: consider the main technical, cost, and schedule risks of the proposed project.
  - **Roles**: U.S. particle physics leadership, or participation, criticality, as well as other benefits of the project.

Multidisciplinary Aspects

- Multidisciplinary connections are of great importance to particle physics. For example, the study of the particle physics of dark energy and inflation is performed by astrophysical techniques employing the detector technologies and computing techniques of particle physics. The research can also provide information on neutrino properties.
- In a different manner, studies traditionally carried out by nuclear physics to determine if the neutrino is its own antiparticle inform the particle physics campaign to address the neutrino science Driver.
- The support from different agencies, linked by the multidisciplinary nature of the science, enables new capabilities of mutual benefit.
- For multidisciplinary projects that receive particle physics funding, our criteria include a check that the distribution of support reflects the distribution of anticipated science topics and that particle physicist participation is necessary for project success. Similar criteria were developed and used by the 2009 PASAG panel.

Program-wide Recommendations (Alignment)

- The first two recommendations align the program with the global vision and science Drivers:

- **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.**

  – The Drivers themselves are not prioritized; rather the prioritization is in the selection and timing of the specific projects to address the intertwined Drivers, optimally and appropriately balanced given funding and other constraints.

Program-wide Recommendations (Costs)

- Projects are categorized [large (>$200M), medium ($50M-$200M), and small (<$50M)] by construction cost to the particle physics program.
- The range of project scales enables an uninterrupted flow of high-priority physics results throughout the P5 timeframe.
- The projects considered by P5 are at various stages of maturity; consequently, the cost estimates of many projects are conceptual and will continue to evolve. Project priority could be affected by evolution of estimated costs.
- **Recommendation 3: Develop a mechanism to reassess the project priority at critical decision stages if costs and/or capabilities change substantively.**

Program-wide Recommendations (Scales)

- Some of the biggest scientific questions driving the field can only be addressed by large and mid-scale experiments. However, small-scale experiments can also address many of the questions related to the Drivers.
  - These experiments combine timely physics with opportunities for a broad exposure to new experimental techniques, provide leadership roles for young scientists, and allow for partnerships among universities and national laboratories.
  - In our budget exercises, we maintained **a small projects portfolio** to preserve budgetary space for a number of these important small projects, whose costs are typically less than $20M. These projects individually are not large enough to come under direct P5 review. Small investments in large, multidisciplinary projects, as well as early R&D for some project concepts, were also accounted for here.

- **Recommendation 4: Maintain a program of projects of all scales, from the largest international projects to mid- and small-scale projects.**

Program-wide Recommendations (Building)

- 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% (and was even lower before).

- **Recommendation 5: Increase the budget fraction invested in construction of projects to the 20%–25% range.**

- This represents a large commitment to building new experiments, which we see as essential. Increasing the project fraction will necessarily entail judicious reductions in the fractions of the budget invested in the research program and operations. (The three main budget categories are project construction, the research program, and operations.)

- In addition, for the research program, which has seen reductions in recent years, flat-flat budgets are substantially detrimental over time due to escalation of real costs. To limit reductions in research program funding, we adopted a guideline that its budget fraction should be >40% in our budget planning exercises.

Essential Roles of Research and Theory

- The particle physics research program supports activities that give meaning to the data.

- Graduate students and postdoctoral researchers have essential roles in all aspects of this world-leading research. In turn, these young researchers obtain scientific and technical training. This develops the next generation of scientific leaders and provides to society a cadre of young people with extraordinary skills and experience.

- The U.S. has leadership in diverse areas of theoretical research in particle physics. A thriving theory program is essential for both identifying new directions for the field and supporting the current experimental program. Theoretical physicists are needed for a variety of crucial activities that include taking the lead in the interpretation and synthesis of a broad range of experimental results, progress in quantum field theory and possible new frameworks for a deeper understanding of Nature, and developing new ideas into testable models. Theoretical research both defines the physics drivers of the field and finds the deep connections among them. As experiments have confronted the Standard Model with increasing sophistication, theoretical research has provided extraordinary advances in calculation techniques, pushing the leading edge of both mathematics and high performance computing.

Program-wide Recommendations (Research)

- Particle physics is a remarkably dynamic field, with researchers nimbly changing course to invent and pursue great new opportunities. It is appropriate that priorities in the research program should be aligned with the science Drivers and the investments in projects. At the same time, it is essential to preserve a diversity of scientific approaches, support and training for young researchers, as well as leadership and forward thinking in theoretical and experimental research. It is the research program's flexibility to support new ideas and developments outside approved projects that will position the field to develop and pursue the next generation of science Drivers.

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

- The research program is the intellectual seed corn of the field. Properly cared for, the program will yield a bounty of future discoveries and innovations within and beyond particle physics. However, the community has been coping with a sequence of recent cuts in the research program budgets, and there is a strong sense that further erosion without careful evaluation will cause great damage.

- **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.**

Program-wide Recommendations (Research)

- In the constrained budget Scenarios, the funding for the research program plus operations is set by the budget fraction devoted to project construction to maintain the pace of discovery and leadership in key areas. Especially in the lowest budget Scenario, it may be unavoidable that there will be some years of flat-flat budgets for the research program. However, the effect of such declines in effort should be carefully assessed and appropriately balanced with other reductions, including those in the ongoing operations budgets, given the priorities of the science Drivers.

- **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.**

- Experiments that can provide essential information to particle physics are sometimes hosted by U.S. agencies other than the U.S. particle physics funding agencies (DOE-HEP, NSF-PHY). An important example is neutrinoless double-beta decay experiments, which address one of the most significant questions in the neutrino Driver and which are stewarded in the U.S. by the DOE Office of Nuclear Physics, with construction contributions also from NSF Particle Astrophysics. Modest levels of support by the U.S. particle physics funding agencies for particle physicist participation in such experiments, as well as in experiments hosted by other nations without major U.S. construction investments, can be of great mutual benefit.

- **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.**

Near-term & Mid-term High-energy Colliders (LHC)

- The nearest-term high-energy collider, the LHC and its upgrades, is a core part of the U.S. particle physics program, with unique physics opportunities addressing three of the main science Drivers (Higgs, New Particles, Dark Matter).
- The ongoing Phase-1 upgrade should be completed by 2018. The Phase-2 luminosity upgrade (HL-LHC) – encompassing both the general-purpose experiments (ATLAS and CMS) and the accelerator – is required to fully exploit the physics opportunities offered by the ultimate energy and luminosity performance of the LHC.
- The HL-LHC is strongly supported and is the first high-priority large-category project in our recommended program. It should move forward without significant delay to ensure that accelerator and experiments can continue to function effectively beyond the end of this decade and meet the project schedule.
  - The experiments have significant discovery potential, are complex, and operate in a very challenging environment. For these reasons, and because of the crucial roles U.S. scientists are playing in the construction, operation, and physics exploitation of both experiments, there is great value in continuing the strong U.S. participation in both the ATLAS and CMS experiments.
  - **We note that, as in the past, the contributed hardware is designed and built in the U.S.**

Near-term & Mid-term High-energy Colliders (LHC)

- The LHC program is a model for successful international science projects, and the LHC experiments are a model for international collaborations. The U.S. contingents in ATLAS and CMS form the largest national groups in both experiments and are the largest fraction of the U.S. particle physics community.
- The U.S. LHC program is a successful interagency partnership of the NSF Physics Division and the DOE Office of High Energy Physics, with each agency supporting numerous research groups in distinctive roles in the experiments, including collaboration leadership.

Near-term & Mid-term High-energy Colliders (LHC)

- The U.S. also contributed critical components and unique technical expertise to the construction of the LHC accelerator. Similarly**, the experiments and accelerator upgrades cannot occur without the unique U.S. technical capabilities (e.g. the high-field magnets necessary for the success of the project) and resources. Continuing the successful inter-agency collaboration, with their distinctive roles and contributions, in the upgrade era will bring benefits to DOE and NSF, as well as to their respective research communities.**
- In addition, the participation in the LHC continues to be a successful example of U.S. reliability in international partnerships, and it can serve as a stimulus and model of the great mutual benefits while further partnerships, such as for the U.S.-hosted neutrino program, are formulated.
- **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.**

Near-term & Mid-term High-energy Colliders (ILC)

- **The interest expressed in Japan in hosting the International Linear Collider (ILC), a 500 GeV e^+e^- accelerator upgradable to 1 TeV, is an exciting development.**
- Following substantial running of the HL-LHC, the cleanliness of the e^+e^- collisions and the nature of particle production at the ILC would result in significantly extended discovery potential as described in the Drivers sections, mainly through increased precision of measurements such as for Higgs boson properties.
  - The ILC would follow the HL-LHC as a complementary instrument for performing these studies in a global particle physics program, providing a stream of results exploring three of our Drivers for many decades.
- The U.S. has played key roles in the design of the ILC accelerator, including leadership in the Global Design Effort. Continued intellectual contributions to the accelerator and detector design are still necessary to enable a site-specific bid proposal, which would take advantage of unique U.S. accelerator physics expertise such as positron source design, beam delivery, superconducting RF, and the accelerator-detector interface. Particle physics groups in the U.S. also led the design of one of the two ILC detector concepts. The required capabilities of the detectors to perform precision measurements are challenging and need continued technology development. Support for both the accelerator and advanced detector development efforts would enhance expertise and ensure a strong position for the U.S. within the ILC global project.

Near-term & Mid-term High-energy Colliders (ILC)

- Participation by the U.S. in ILC project construction depends on a number of key 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, we plan in all Scenarios for ILC support at some level through a decision point within the next five years.
  - If the ILC proceeds, there is a high-priority option in Scenario C to enable the U.S. to play world-leading roles.
  - Even if there are no additional funds available, some hardware contributions may be possible in Scenario B, depending on the status of international agreements at that time.
  - If the ILC does not proceed, then ILC work would terminate and those resources could be applied to accelerator R&D and advanced detector technology R&D.
- **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.**

Neutrino Oscillation Experiments (Program)

- Short- and long-baseline oscillation experiments directly probe three of the questions of the neutrino science Driver:
  - How are the neutrino masses ordered? Do neutrinos and antineutrinos oscillate differently? Are there additional neutrino types and interactions?
- There is a vibrant international neutrino community invested in pursuing the physics of neutrino oscillations.
- The U.S. has unique accelerator capabilities at Fermilab to provide neutrino beams for both short- and long-baseline experiments, with some experiments underway, and a long-baseline site is available at the Sanford Underground Research Facility in South Dakota.
- Many of these current and future experiments and projects share the same technical challenges. Interest and expertise in neutrino physics and detector development of groups from around the world combined with the opportunities for experiments at Fermilab provide the essentials for an international neutrino program.

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

Neutrino Oscillation Experiments (Long Baseline)

- For a long-baseline oscillation experiment, based on the science Drivers and what is practically achievable in a major step forward, we set as the goal a mean sensitivity to CP violation of better than 3σ (corresponding to 99.8% confidence level for a detected signal) over more than 75% of the range of possible values of the unknown CP-violating phase δ_{CP}.
  – By current estimates, this corresponds to an exposure of 600 kt*MW*y assuming systematic uncertainties of 1% and 5% for the signal and background, respectively. With a wideband neutrino beam produced by a proton beam with power of 1.2 MW, this implies a far detector with fiducal mass of more than 40 kilotons (kt) of liquid argon (LAr) and a suitable near detector.
- **The minimum requirements to proceed are the identified capability to reach an exposure of at least 120 kt*MW*yr by the 2035 timeframe, the far detector situated underground with cavern space for expansion to at least 40 kt LAr fiducial volume, and 1.2 MW beam power upgradable to multi-megawatt power. The experiment should have the demonstrated capability to search for supernova (SN) bursts and for proton decay, providing a significant improvement in discovery sensitivity over current searches for the proton lifetime.**

These minimum requirements are not met by the current LBNE project's CD-1 minimum scope.

Neutrino Oscillation Experiments (LBNF)

- The long-baseline neutrino program plan has undergone multiple significant transformations since the 2008 P5 report. Formulated as a primarily domestic experiment, the minimal CD-1 configuration with a small, far detector on the surface has very limited capabilities.
- A more ambitious long-baseline neutrino facility has also been urged by the Snowmass community study and in expressions of interest from physicists in other regions.
- To address even the minimum requirements specified above, **the expertise and resources of the international neutrino community are needed.**

- **A change in approach is therefore required**: The activity should be reformulated under the auspices of a new international collaboration, as an internationally coordinated and internationally funded program, with Fermilab as host. There should be international participation in defining the program's scope and capabilities. The experiment should be designed, constructed, and operated by the international collaboration. The goal should be to achieve, and even exceed if physics eventually demands, the target requirements through the broadest possible international participation.

Neutrino Oscillation Experiments (LBNF)

- Key preparatory activities will converge over the next few years: in addition to the international reformulation described above, PIP-II design and project definition will be nearing completion, as will the necessary refurbishments to the Sanford Underground Research Facility. Together, these will set the stage for the facility to move from the preparatory to the construction phase around 2018. The peak in LBNF construction will occur after HL-LHC peak construction.

- **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.**

Neutrino Oscillation Experiments (PIP-II)

- The PIP-II project at Fermilab is a necessary investment in physics capability, enabling the world's most intense neutrino beam, providing the wideband capability for LBNF, as well as high proton intensities for other opportunities, and it is also an investment in national accelerator laboratory infrastructure. The project has already attracted interest from several potential international partners.
- **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.**

Neutrino Oscillation Experiments (Short Baseline)

- Hints from short-baseline experiments suggest possible new non-interacting neutrino types or non-standard interactions of ordinary neutrinos. These anomalies can be addressed by proposed experiments with neutrinos from radioactive sources, pion decay-at-rest beams, pion and kaon decay-in-flight beams, muon-decay beams, or nuclear reactors.
- A judiciously selected subset of experiments can definitively address the sterile-neutrino interpretation of the anomalies and potentially provide a platform for detector development and international coordination toward LBNF.
  - These small-scale experiments are in addition to the small projects portfolio described above.
  - The short-term short-baseline (SBL) science and detector development program and the long-term LBNF program should be made as coherent as possible in an optimized neutrino program.
- **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.**

Additional Project Concepts

- Concepts to address various aspects of neutrino oscillation physics via alternative approaches were considered, including
  - RADAR
  - CHIPS
  - DAEdALUS and IsoDAR
  - LAr1
  - PINGU
  - NuSTORM
- These cannot go forward as major projects at this time, due to concept maturity and/or program cost considerations. However, further development of PINGU is recommended, and IsoDAR (precursor to DAEdALUS) should be considered in the context of a short-baseline oscillation program.
- Similarly, P5 heard presentations about several other concepts for projects whose ultimate construction scope would be large but whose near-term request for R&D funding is small. These include the Storage Ring Proton EDM Experiment and NNbarX, both of which address P5 Drivers. Development has not yet advanced to a point at which it would be possible to consider recommendations to move forward with any of these projects. The R&D for these projects would fit as candidates in the small projects portfolio, with the path to eventual implementation presumably being among the evaluation criteria.

Cosmic Surveys (Optical)

- Astronomical observations have provided evidence for dark energy and inflation, physics that powered two epochs of cosmic acceleration. DESI, LSST, and CMB-S4 provide complementary, breakthrough capabilities to survey the sky with the aim of understanding these phenomena and what they say about particle physics. They also provide important probes of neutrino properties .

- The DESI project provides a major leap forward in the study of dark energy, while also making important contributions to the physics of inflation and neutrinos. An integral part of the comprehensive dark energy program, it can address the key questions with exquisite precision. DESI is technically ready to proceed, arrangements with international partners are well advanced, and it is well timed with an interagency opportunity for the use of the Mayall 4-meter facility on Kitt Peak. DESI is an important part of the particle physics program and scientifically and programmatically timely. Given this, there is great concern that DESI did not fit into the leanest budget Scenario .
- **Recommendation 16: Build DESI as a major step forward in dark energy science, if funding permits (see Scenarios discussion below).**

- The physics case for LSST is undiminished relative to its top-rank priority in the NRC Astro2010 Decadal Survey. Its breakthrough capabilities will be transformational for a broad range of science, including two of the Drivers. The project is well underway and is a good example of successful multi-agency cooperation.
- **Recommendation 17: Complete LSST as planned.**

Cosmic Surveys (CMB)

- Measurements of the cosmic microwave background (CMB) have historically been funded primarily by sources outside of particle physics. The experiments now have the capability to access the ultra-high energy physics of inflation and important neutrino properties. These measurements are of central significance to particle physics. Particle physics groups at the DOE laboratories have unique capabilities, *e.g.*, in sensor technology and production of large sensor arrays, that are essential to future CMB experiments as the technological sophistication and scale of the experiments expands. The participation of particle physicists in cases in which they contribute unique expertise is warranted. For these reasons, substantially increased particle physics funding of CMB research and projects is appropriate in the context of continued multiagency partnerships. As the scale of CMB experiments grows from Stage 3, which is of the size of an experiment in the small project portfolio, to Stage 4 (S4), which is mid-scale, increased international collaboration and coordination among major CMB projects will be needed.

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

Dark Matter (Direct)

- The experimental challenge of discovery and characterization of dark matter interactions with ordinary matter requires a multi-generational suite of progressively more sensitive and ambitious direct detection experiments.
- This is a highly competitive, rapidly evolving field with excellent potential for discovery. The second-generation direct detection experiments are ready to be designed and built, and should include the search for axions, and the search for low-mass (<10 GeV) and high-mass WIMPs.
  - Several experiments are needed using multiple target materials to search the available spin-independent and spin-dependent parameter space.
  - This suite of experiments should have substantial cross-section reach, as well as the ability to confirm or refute the current anomalous results.
  - Investment at a level substantially larger than that called for in the 2012 joint agency announcement of opportunity will be required for a program of this breadth.

- **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.**

Dark Matter (Direct)

- The results of G2 direct detection experiments and other contemporaneous dark matter searches will guide the technology and design of third-generation experiments. As the scale of these experiments grows to increase sensitivity, the experimental challenge of direct detection will still require complementary experimental techniques, and international cooperation will be warranted. The U.S. should host at least one of the third-generation experiments in this complementary global suite.

- **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.**

Dark Matter (Indirect)

- The Cherenkov Telescope Array (CTA) is the world's major step forward in ground-based gamma-ray astrophysics. Although the U.S. pioneered the detection technique, due to funding limitations the center of activity has now shifted to Europe. U.S. groups are proposing a distinctive and clever addition to the project that will significantly enhance the sensitivity to dark matter signals in important regions of parameter space. The CTA dark matter signal detection capability will be unique. While this is of direct importance to particle physics, the broader science reach of CTA transcends fields. According to our criteria, the project costs should be shared by NSF Astronomy, NSF Physics, and DOE, which is the plan presented by the proponents. The scope of the U.S. component of CTA can be reduced by up to a factor of two and still provide a valuable increase in dark matter signal sensitivity.

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

Muons and Kaons

- The Mu2e and muon g-2 projects represent a large fraction of the budget in the early years. These are immediate targets of opportunity in the drive to search for new physics, and they will help inform future choices of direction. The science case is undiminished relative to their earlier prioritization. The programmatic impacts of large changes at this point were also discussed and determined to be generally unwise, although the Mu2e profile could be adjusted by a small amount if needed.
- **Recommendation 22: Complete the Mu2e and muon g-2 projects.**

- The ORKA kaon experiment would provide an opportunity to make measurements of a process with very small theoretical uncertainties in the Standard Model with discovery potential for multi-TeV scale new physics. It has the potential for significant improvement over CERN experiment NA62, which uses a complementary technique and which has a head start. The suite of measurements with ORKA would provide excellent training for students and postdocs, and this mid-size project offers additional balance to the large-scale projects in the field. Unfortunately, due to resource constraints and anticipated conflicts with the highest priority items in the Fermilab program, P5 cannot recommend moving ahead with ORKA at this time.

Scenario B

- The Charge provides two constrained budget Scenarios, and a third, unconstrained Scenario. These Scenarios are understood not to be literal budget guidance but an exercise to help confront choices and identify priorities.
- Scenario B is defined in the Charge as a constant level of funding (“flat-flat”) for three years, followed by increases of 3% per year with respect to the FY2014 President’s budget request for HEP. Scenario A is defined in the Charge as a constant level of funding for three years, followed by increases of 2% per year with respect to the FY2013 budget for HEP. The two budgets start at somewhat different values, though they are similar in that they are flat-flat until FY2018. With the 1% difference in escalation rate and the different starting values, the two budgets differ by approximately $500M summed over a decade. The recommended programs in the three Scenarios are shown in Table 1.
- Hard choices were required. While Scenario B allows for a balanced program, based on our Criteria, excellent projects will not be fiscally possible. Moreover, the constant funding level in the early years, coupled with the urgently needed 20%–25% project construction fraction, implies an erosion of research effort. The early years are particularly constrained, given existing projects that are recommended for completion (muon g-2, Mu2e, LSST) and the urgent need to move forward with DM G2 experiments, HL-LHC upgrades, and PIP-II.
- Nevertheless, essential progress will be made on each of the science Drivers, along with some key investments in U.S. infrastructure and in future capabilities through R&D.

Scenarios B and A

- Scenario A is much more challenging. The reduction relative to Scenario B, which is approximately $30M per year until FY2018 and then grows over time, would have very large impacts:
  - DESI would not be possible
  - Accelerator R&D and advanced detector R&D would be reduced substantially
  - Extension of flat-flat research program funding would result in further personnel reductions and loss of research capability
  - Ramp up of funding for LBNF would be delayed relative to Scenario B (preliminary work would proceed immediately in both scenarios)
  - Third-generation direct detection dark matter capabilities would be reduced or delayed
  - A small change in the funding profile of Mu2e would be required.
- DESI should be the last project to be cut if moving from Scenario B toward Scenario A.
  - A small, limited-time increment above Scenario A would make this very important small project possible.

The return on the investment of the relatively small increment from Scenario A to Scenario B is large. It provides excellent science per incremental dollar by enabling the outstanding opportunity of DESI, sets a faster course for the long-baseline neutrino program, and preserves the long-term investments in R&D and the research program. As valuable as each of these items is, they simply do not fit in Scenario A.

Table 1
Summary of Scenarios

Project/Activity	Scenarios: Scenario A	Scenario B	Senario C	Science Drivers: Higgs	Neutrinos	Dark Matter	Cosm. Accel.	The Unknown	Technique (Frontier)
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 contri-butions. 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.

DRAFT FOR APPROVAL Building for Discovery: Strategic Plan for U.S. Particle Physics in the Global Context

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 [>$200M] in the upper section, Medium and Small [<$200M] 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.

Scenario A Considerations

- Scenario A 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 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.

Scenario C

- Although many projects were not possible in Scenario B, our vision for Scenario C is not a long list of projects. Instead, we focus on three high-priority opportunities that would each dramatically enhance key elements of the strategic plan recommended for Scenarios A and B.
- The U.S. could move boldly toward development of transformational accelerator R&D. There are profound questions to answer in particle physics, and recent discoveries reconfirm the value of continued investments. Going much further, however, requires changing the capability-cost curve of accelerators, which can only happen with an aggressive, sustained, and imaginative R&D program. A primary goal, therefore, is the ability to build the future-generation accelerators at dramatically lower cost.
  - For example, the primary enabling technology for pp colliders is high-field accelerator magnets, possibly with more advanced superconductors. For e^+e^- colliders, primary goals are improving the accelerating gradient and lowering the power consumption. Although these topics are R&D priorities in the constrained budget scenarios, larger investments could make these far-future accelerators technically and financially feasible on much shorter timescales.
  - Experience suggests this effort will also have large, positive impacts beyond particle physics.
  - A detailed vision and roadmap should be articulated by the upcoming HEPAP Subcommittee on Accelerator R&D.
- **As work proceeds worldwide on long-term future-generation accelerator concepts, the U.S. should be counted among the potential host nations.**

Scenario C (continued)

- Should the ILC go forward, Scenario C would enable the U.S. to play world-leading roles in the detector program as well as provide critical expertise and accelerator components.
  - The interest expressed in Japan in hosting the International Linear Collider (ILC), a 500 GeV e^+e^- accelerator upgradable to 1 TeV, is an exciting development.
  - Decisions by governments on whether or not to proceed, and the levels of participation, depend on many factors beyond the scope of P5; however, we emphasize most strongly that the scientific justification for the project is compelling.
- In addition, the U.S. could offer to host a large water Cherenkov neutrino detector to complement the LBNF liquid argon detector, unifying the global long-baseline neutrino community to take full advantage of the world's highest intensity neutrino beam. The placement of the water and liquid argon detectors would be optimized for complementarity. This approach would be an excellent example of global cooperation and planning.

Enabling R&D (Accelerator R&D)

- Together the GARD, Stewardship, and NSF programs form the critical basis for accelerator R&D, enabling particle physics and many other fields. All of these programs provide essential training for accelerator physicists and engineers. Given the substantive investments in such programs overseas, appropriate investments should be made in the U.S. to ensure a continued competitiveness by offering opportunities that attract and retain the very best and that enable development of critical technology. Historically, operation of high energy physics facilities provided research and training opportunities in accelerator science. With the termination or repurposing of these facilities, ensuring access to accelerator test facilities will help maintain the knowledge base and advance the field.

- **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.**

Enabling R&D (Future-generation Accelerators)

Far-term Future-Generation Accelerators

- The motivation for future-generation accelerators must be the science Drivers. The aforementioned R&D efforts are required to establish the technical feasibility and to make the costs reasonable. The future-generation accelerators are listed here in order of the strength of the physics case, as currently understood.
- A very high-energy proton-proton collider is the most powerful future tool for direct discovery of new particles and interactions under any scenario of physics results that can be acquired in the P5 time window. Colliders of energy up to 100 TeV, with a circumference of about 100 km with an option of e^+e^-, are presently under study at CERN, in China, and in the U.S.
  - Extensive R&D is required to make such a collider feasible at a reasonable cost. The U.S. is the world leader in R&D on high-field superconducting magnet technology, which will be a critical enabling technology for such a collider.
  - Future R&D follows naturally from the directed R&D now conducted by the LARP program for the HL-LHC.

- **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.**

Enabling R&D (MAP)

- Neutrino factories based on muon storage rings could provide higher intensity and higher quality neutrino beams than conventional high power proton beams on targets. This concept would be attractive for an international long-baseline neutrino program offering more precise and complete studies of neutrino physics beyond short-term and mid-term facilities.
- Muon colliders can reach higher energies than e^+e^- accelerators, but have many technical challenges. Addressing all of the necessary challenges would require a very strong physics motivation based on results from ongoing or future accelerators.
- The **Muon Accelerator Program (MAP)** currently aims at technology feasibility studies for far-term muon storage rings for neutrino factories and for muon colliders, including the **Muon Ionization Cooling Experiment (MICE)** at the Rutherford Appleton Laboratory.
- The large value of $\sin^2(2\theta_{13})$ enables the next generation of oscillation experiments to use conventional neutrino beams, pushing the time frame when neutrino factories might be needed further into the future, and the small Higgs mass enables study at more technically ready e+e- colliders, reducing the near-term necessity of muon colliders.

- **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.**

Enabling R&D (Accelerator R&D Alignment)

- **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.**
- A HEPAP subcommittee on accelerator R&D will provide detailed guidance on the implementation of accelerator R&D aligned with P5 priorities.

Enabling R&D (Instrumentation)

- The particle physics detector community has historically been an important contributor to broadly applicable innovation in instrumentation.
- With the recommended increase in new project construction (Recommendation 5), detector R&D activity will shift toward addressing the relatively near-term requirements of the LHC detectors and the neutrino program. This shift will enable these projects to realize their physics program in a cost-constrained environment. For the longer term, a portfolio balanced between incremental and transformational R&D is required.

- **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.**

- To alleviate the serious shortage of physicists with a background in instrumentation, workforce training at the graduate or postdoctoral level and promising career opportunities are necessary to accomplish and sustain research. University infrastructure to support teaching of instrumentation has decreased over the last decade, which has adversely affected the ability of universities to train students.

- **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 labs, and fully exploit the unique capabilities and facilities offered at each.**

Enabling R&D (Computing)

- The recent *Report from the Topical Panel Meeting on Computing and Simulations in High Energy Physics* articulated the challenges involved in meeting the increasing computational needs of the field and suggested steps to take full advantage of cost-effective computing solutions. The present practice is to handle much of the computing within individual projects. Rapidly evolving computer architectures and increasing data volumes require effective crosscutting solutions that are being developed in other science disciplines and in industry. Mechanisms are needed for the continued maintenance and development of major software frameworks and tools for particle physics and long-term data and software preservation, as well as investments to exploit next-generation hardware and computing models. Close collaboration of national laboratories and universities across the research areas will be needed to take advantage of industrial developments and to avoid duplication.

- **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.**

Significant Changes in Direction

- Increase to 20%–25% the fraction of the budget devoted to construction, and plan with care any further reductions in real funding levels for the research program. In our budget exercises, we adopted an internal guideline of >40% of the budget to be allocated to the research program.
- Change approach for the long-baseline neutrino program. The activity should be reformulated as an internationally coordinated and internationally funded program, with Fermilab as the host, to reach the science driver goals specified in the text. A new international collaboration should be formed.
- Upgrade the Fermilab proton accelerator complex to produce higher intensity beams, redirecting former Project-X activities and temporarily redirecting some existing accelerator R&D toward this effort. R&D for PIP-II should proceed immediately, followed by construction, to provide proton beams of greater than one megawatt by the time of first operation of the new long-baseline neutrino facility.
- 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.
- Provide increased particle physics funding of CMB research and projects, as part of the core particle physics program, in the context of continued multiagency partnerships.
- Re-align activities in accelerator R&D, which is critical to enabling future discoveries, based on new physics information and long-term.
  - Reassess the Muon Accelerator Program (MAP), incorporating into the general accelerator R&D program those activities that are of broad importance to accelerator R&D, and consult with international partners on the early termination of MICE.
  - In the general accelerator R&D program, focus on outcomes and capabilities that will dramatically improve cost effectiveness for mid- and far-term accelerators.

Benefits and Broader Impacts

- Particle physics shares with other basic sciences the need to innovate, invent, and develop technologies to carry out its mission. Advanced particle accelerators, cutting-edge particle detectors, and sophisticated computing techniques are the hallmarks of particle physics research.
- This dedicated research has benefited tremendously from progress in other areas of science to advance the current state of technology for particle physics. In return, developments within the particle physics community have enabled basic scientific research and applications in numerous other areas. This broad, connected scientific enterprise provides tremendous benefits to society as a whole.

- The report summarizes many topics including:
  - Materials science
  - Medical imaging and therapy
  - National Security
  - Computing
  - Bringing to life the earliest audio recordings
  - Neuroscience

- The benefits of these connections go in both directions!

Also see the report by the Task Force on *Tools, Techniques, and Technology Connections of Particle Physics*, to be posted soon on the HEPAP site

Community

A core part of our process from the start

More to come:

- Community presentation 2 June, organized by DPF
- Discussions in many venues. Will keep a list of known events on the P5 site.
- Consultations and many discussions.

Community

- Snowmass output is essential input to P5.
- Most meetings will have public components, geographically distributed.
- In addition to all the other work to set up P5, we have been talking extensively with community members about P5, the process, and the issues. This will continue.
- P5 website under construction. Will be updated frequently with news and information. In addition, an input portal is being set up.
- Community buy-in is critical to our success.
  - Process as it develops will be inclusive and clear
  - Rationale for the choices must be articulated
  - Note that it is possible to support a plan even if it doesn't match one's specific taste in physics.
  - Work will continue after the report is complete.
- HEPAP has very important roles throughout this process.

5 September 2013 S. Ritz P5 9

A huge amount of work done by the community and by the panel for P5.
THANK YOU!

"One-pager"

Building for Discovery

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

Report of the Particle Physics Project Prioritization Panel

The U.S. particle physics community has just updated its vision for the future. The P5 report presents a strategy for the next decade and beyond that enables discovery and maintains our position as a global leader through specific investments by the Department of Energy's Office of Science and the National Science Foundation Directorate for Mathematical and Physical Sciences.

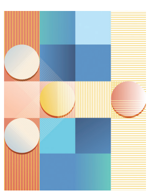
Particle physics is a highly successful, discovery-driven science. It explores the fundamental constituents of matter and energy, and it reveals the profound connections underlying everything we see, including the smallest and the largest structures in the Universe. Earlier investments have been rewarded with recent fundamental discoveries, and upcoming opportunities will push into new territory. Research in particle physics inspires young people to engage with science.

Particle physics is global. To address the most pressing scientific questions and maintain its status as a global leader, the U.S. must both host a unique, world-class facility and be a partner on the highest priority facilities hosted elsewhere.

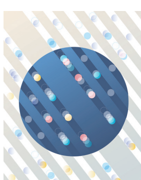
Choices were required. The updated strategy recommends investments in the best opportunities, chosen from a large number of excellent options, in order to have the biggest impact and make the most efficient use of resources over the coming decade.

Five intertwined scientific Drivers were distilled from the results of a yearlong community-wide study:

- 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

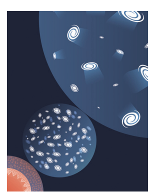


Higgs boson



Neutrino mass

Dark matter



Cosmic acceleration

Explore the unknown

The U.S. particle physics program is poised to move forward into the next era of discovery.

The P5 report recommends a prioritized and time-ordered list of experiments to address the five science Drivers optimally. These opportunities are at the small, medium, and large investment scales that, together, produce a continuous flow of major scientific results throughout a twenty-year timeframe.

- Large projects, in time order, include the Muon g-2 and Muon-to-electron Conversion (Mu2e) experiments at Fermilab, strong collaboration in the high-luminosity upgrades to the Large Hadron Collider (HL-LHC), and a U.S.-hosted Long Baseline Neutrino Facility (LBNF) that receives the world's highest intensity neutrino beam from an improved accelerator complex (PIP-II) at Fermilab.
- U.S. involvement in a Japanese-hosted International Linear Collider (ILC), should it proceed, with stronger participation in more favorable budget scenarios.
- Areas with clear U.S. leadership in which investments in medium- and small-scale experiments have great promise for near-term discovery include dark matter direct detection, the Large Synoptic Survey Telescope (LSST), the Dark Energy Spectroscopic Instrument (DESI), cosmic microwave background (CMB) experiments, and a portfolio of small projects that includes short-baseline neutrino experiments.
- Specific investments in particle accelerator, instrumentation, and computing research and development are required to support the program and to ensure the long-term productivity of the field.

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 specific activities and efforts at Fermilab to the PIP-II program of improvements to the accelerator complex, which 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.
- Re-align activities in accelerator R&D with the new strategic plan, and emphasize capabilities that will enable creating future-generation accelerators at dramatically lower cost.

Small changes in yearly budgets have large impacts on the timeline and capability of the U.S. particle physics program. A very large return on investment is ensured by the relatively small increment in funding between the constrained budget scenarios given in the P5 charge:

- A small limited-time funding increment to ensure support of the Dark Energy Spectroscopic Instrument (DESI) would yield scientific returns with high impact.
- World-leading accelerator and instrumentation development research would be retained.
- U.S. research capability would be maintained.
- The Muon-to-electron Experiment (Mu2e) at Fermilab would be completed on time.
- The long-baseline neutrino program would proceed without delays.
- The third-generation dark matter direct detection capabilities would be fully developed on time.

The lowest budget scenario given in the P5 charge is precarious. It is close to the point beyond which the U.S. would not be capable of hosting a large project while maintaining the other core program components that ensure mission success. Without this capability, the U.S. would lose its position as a global leader in this field, and highly productive international relationships would be fundamentally altered.

High-priority options for additional investments beyond our constrained scenarios are identified:

- Expand accelerator R&D to enable very high-energy future machines at lower cost, and likely provide benefits beyond particle physics.
- Play world-leading roles in the ILC detector program and provide critical accelerator components, should the ILC proceed in Japan.
- Host a large water-based neutrino detector to complement the LBNF liquid-argon detector and unify the global long-baseline neutrino community around the world's highest intensity neutrino beam provided by Fermilab.

For more information on P5 or to download a PDF copy of the report, visit usparticlephysics.org/P5

Report of the Particle Physics Project Prioritization Panel

U.S. DEPARTMENT OF ENERGY | Office of Science

NSF

Summary (1/5)

- A vision that starts from the science Drivers, with criteria to make tough choices, driven by community discussions and inputs, to develop a program.
- 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: an optimized set of short- and long-baseline neutrino oscillation experiments, with the long-term focus on LBNF. The Proton Improvement Plan (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.
- 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.

Summary (2/5)

- 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.

Summary (3/5)

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 temporarily 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.

Summary (4/5)

- 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.

Summary (5/5)

- 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 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.

P5 Panel Perspective

- This is a challenging time for particle physics. The science is deeply exciting and its endeavors have been extremely successful, yet funding in the U.S. is declining in real terms. This report offers important opportunities for U.S. investment in science, prioritized under the tightly constrained budget scenarios in the Charge.
- We had the responsibility to make the tough choices for a world-class program under each of these scenarios, which we have done. At the same time, we felt the responsibility to aspire to an even bolder future. These are not contradictory responsibilities: an annual budget is a balance sheet, but investment in fundamental research is a powerful expression that our culture and economy have greater potential in the long run. Our society's capacity to grow is limited only by our collective imagination and resolve to make long-term investments that can lead to fundamental, game-changing discoveries, even in the context of constrained budgets.
- Wondrous projects that address profound questions inspire and invigorate far beyond their specific fields, and they lay the foundations for next-century technologies we can only begin to imagine. Particle physics is an excellent candidate for such investments.
- Historic opportunities await us, enabled by decades of hard work and support. Our field is ready to move forward.

Discussion

Additional Slides

Neutrino Oscillation Experiments (Concepts)

- RADAR and CHIPS are both ideas for new detectors exploiting the existing NuMI beamline to improve knowledge of oscillation parameters. The RADAR proposal is to build a liquid argon TPC at the Ash River site, thereby offsetting R&D costs for LBNF. CHIPS proposes a large water Cherenkov detector in a water-filled mine pit, first at a NuMI off-axis location, and possibly later as an off-axis LBNF detector. Although one might gain some incremental sensitivity beyond NOvA and T2K in the shorter term with RADAR or CHIPS, the CP and mass hierarchy reach is reduced compared to that of the LBNF configuration, and these experiments are less capable for proton decay, atmospheric neutrinos, and SN burst neutrinos. A strategy focusing resources on moving ahead as fast as possible on LBNF is therefore favored.

- DAEdALUS is a different approach to the measurement of δ_{CP}, using multiple high-power cyclotrons to generate a large neutrino flux from pion decay-at-rest at a large water Cherenkov or liquid scintillator detector. The concept still requires significant development, and a suitable large-detector target has not yet been selected. IsoDAR is a proposed precursor phase to DAEdALUS with a well-defined short-baseline neutrino-oscillation physics program using cyclotron-produced ^{8}Li decay at rest. IsoDAR should be considered in the context of a short-baseline oscillation program.

Neutrino Oscillation Experiments (Concepts)

- LAr1 is a mid-scale short-baseline accelerator-based experiment to address both the neutrino and anti-neutrino SBL anomalies. An appropriate combination of smaller near-term projects may accomplish most of these goals at much lower cost, so proceeding with LAr1 is not recommended at this time.

- PINGU, an infill array concept at the IceCube facility, may also have the interesting potential to determine the neutrino mass hierarchy using atmospheric neutrinos sooner than other competing methods, as well as have sensitivity to low-mass WIMP dark matter. The details of the experiment are still under development, and we encourage continued work to understand systematics. PINGU could play a very important role as part of a larger upgrade of IceCube, or as a separate upgrade, but more work is required.

- NuSTORM is a proposal for a small muon storage ring to produce ~GeV neutrinos and antineutrinos with the advantage of a precisely known flux. The facility would also serve as an intense source of low-energy muons and serve as a technology demonstrator for a future neutrino factory. The physics reach of this program includes sensitive sterile neutrino searches and precision neutrino cross-section measurements. Although the concept is attractive as a first step towards a neutrino factory and as a means to reduce the beam-related systematic errors for LBNF, the high cost makes it impossible to pursue at the same time as PIP-II and LBNF, which are the primary objectives.