
Building for Discovery

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



Preface

Panel reports usually convey their results logically and dispassionately, with no mention of the emotional, soul-searching processes behind them. We would like to break with tradition to share some behind-the-scenes aspects and perspectives.

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.

We were given complex issues to resolve. We listened carefully

to our community, to those who charged us, and to scientists in other fields. Our community's passion, dedication, and entrepreneurial spirit have been inspirational. Therefore, 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.

In our deliberations, no topic or option was off the table. Every alternative we could imagine was considered. We worked by consensus—even when just one or two individuals voiced concerns, we worked through the issues.

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.

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Executive Summary

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.

- 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.

Introduction

1



1.1: Particle Physics is a Global Field for Discovery

This is a pivotal time for particle physics, the science of discovery and exploration of the fundamental constituents of matter and energy. Since the previous P5 report in 2008, there have been several landmark events in the field. The Higgs boson was 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) were awarded. A key neutrino mixing parameter 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.

Particle physics is global. Nations pursue particle physics because the questions are profound and provocative, and the techniques are beautiful and useful. The countries that lead these activities attract top minds and talent from around the world, inspire the next generation of scientists and technologists, and host international teams dedicated to a common purpose. The scientific program required to address all of the most compelling questions of the field is beyond the finances and the technical expertise of any one nation or region; nonetheless, the capability to address these questions in a comprehensive manner is within reach of a cooperative global program. The field is at a juncture where the major players each plan to host one of the large projects most needed by the worldwide scientific community.

Hosting world-class facilities and joining partnerships in facilities hosted elsewhere are both essential components of a global vision. Europe's future program, articulated in 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. The United States is in an excellent position to host a world-class neutrino program, while playing key roles in the LHC and the ILC, if the ILC proceeds in Japan. Strong foundations of international cooperation exist, with the LHC serving as an example of how to execute large international science projects successfully. Reliable partnerships are essential for the success of international projects. Building further international cooperation is an important theme of this report.

Inflation-adjusted funding for particle physics in the U.S. has continued to decline. In addition, since the previous P5 report, 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 ended; and PEP-II/B-factory operations ended. During the same period, however, U.S. involvement in international projects continued to be extremely productive, enabling many of the big discoveries now driving the field.

A new P5 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, given in [Appendix A](#), calls for planning under specific budget scenarios, reflecting current fiscal realities and requiring foundational choices. The U.S. must invest purposefully in areas that have the biggest impacts and make the most efficient use of limited resources. This report is intended as a roadmap for investment. Having made these choices, the field can move forward immediately with a prioritized, time-ordered program.

To develop the strategic plan, we start with the science now driving the field. Snowmass, the yearlong, community-wide study preceding P5, was invaluable. It identified a broad range of opportunities, with the understanding that careful choices from a long list of compelling options are now required. The P5 process is summarized in [Appendix C](#). Starting with Snowmass, and continuing throughout the P5 process, the community was actively engaged in a variety of venues, including three large meetings, multiple town halls, and the P5 submission portal.

Science Questions and Science Drivers

The eleven groups of physics questions from Snowmass are shown in [Appendix D](#), along with a reference to all the Snowmass documents. Based on this comprehensive work by the broad community, we have identified five compelling lines of inquiry that show great promise for discovery over the next 10 to 20 years. These are 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. For example, some of the new physics models designed to address theoretical limitations of the Standard Model¹ also predict particles that could compose the dark matter; furthermore, the Higgs boson and neutrinos may interact with the dark matter. Other connections are possible, and although the specifics are not known, there are good reasons to suspect that these deeper connections exist. A selected set of different experimental approaches that reinforce each other is therefore required. These experiments sometimes address several Drivers. For example, collider experiments address the Higgs, Dark Matter, and Exploration Drivers. Furthermore, cosmic surveys designed to address dark energy and inflation also provide unique and timely information about neutrino properties. 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 following subsection and in more detail in [Section 3](#). What is learned at each step will inform the next steps.

1.2: Brief Summary of the Science Drivers and Main Opportunities

The five science Drivers, and their associated opportunities, are summarized here. As individual projects can address multiple

Drivers, recommendations about the projects are given in [Section 2](#).

Use the Higgs boson as a new tool for discovery

The recently discovered Higgs boson is a form of matter never before observed, and it is mysterious. 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, such as a very high-energy hadron collider, 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. A diverse research program exploiting particle astrophysics, accelerator and reactor experiments has uncovered a new landscape in neutrino physics, with a promising future for continued discovery. Recent results indicate that answers to some of the most significant questions about neutrinos lie within reach of the next generation of

¹The Standard Model describes the elementary particles, which come in three distinct types: (i) the matter particles, quarks and leptons, (ii) the photon, gluons and massive W and Z , which mediate the electromagnetic, strong, and weak forces, respectively, and (iii) the Higgs boson, which gives mass to the elementary particles. The Standard Model provides a quantitative, quantum mechanical description of the interactions of these particles that has been remarkably successful.

experiments. Physicists now know that neutrinos exist in three types and that they oscillate, *i.e.*, they change type as they move in space and time. The observed oscillations imply that neutrinos have masses. Many aspects of neutrino physics are puzzling, and the experimental picture is incomplete. Powerful new facilities are needed to move forward, addressing the questions: What is the origin of neutrino mass? How are the masses ordered (referred to as mass hierarchy)? What are the masses? Do neutrinos and antineutrinos 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. Its centerpiece would be a next generation long-baseline neutrino facility (**LBNF**). 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. Short-distance oscillation experiments, 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 of the Standard Model make up only about one-sixth of the total matter in the Universe. The rest is dark matter (DM). Dark matter is presumed to consist of one or more kinds of new particles. The properties of these particles, which are all around us, are unknown. Dark matter represents a bizarre shadow world of fundamental particles that are both omnipresent and largely imperceptible. 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. These include weakly interacting massive particles (WIMPs), axions, and new kinds of neutrinos. It is imperative to search for dark matter along every feasible avenue. There are four

known experimental approaches, each providing essential clues: direct detection, indirect detection, observation of large-scale astrophysical effects, and dark matter production with particle colliders. 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 Cherenkov Telescope Array (**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

Armed with the dual tools of 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 of this inflation is unknown but may have involved fundamentally new physics at ultra-high energies. A second distinct epoch of accelerated expansion began more recently and continues today. This expansion is 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.

Resolving these mysteries requires better measurements of how rapidly the Universe was expanding during the past ten billion years. The Dark Energy Spectroscopic Instrument (**DESI**) can do this precisely enough to determine the properties of dark energy to the percent level over the course of billions of years. The matter in the Universe is gathered into patterns whose structure on large scales also carries information about the properties of dark energy. 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 using the large-scale structure.

Together, DESI and LSST can also probe the possibility that, instead of dark energy, new laws of space and time beyond those introduced by Einstein are responsible for the recent cosmic acceleration.

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.

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

There are clear indicators of new phenomena awaiting discovery beyond those motivating 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. The tools for this search are varied and include very high-energy beams of protons and electrons, intense beams of protons, and cosmic sources of ultra high-energy particles. The searches take two basic forms: producing new particles and detecting the quantum influence of new particles.

Producing new particles: The path for discovery of new particles by producing them in the laboratory is through high-energy colliders like the LHC. Well-motivated extensions of the Standard Model, such as Low-energy Supersymmetry, 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, either because they are very heavy or because they are very difficult to detect. In the event that one or more new particles are 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. There are many examples

of such experiments taking place in Europe, Japan, China, and the U.S. The global program includes projects that are complementary to one another using different kinds of particles as probes that are sensitive to different types of new particles and interactions. Some notable examples involve 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 to the accelerator complex 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 to bend and focus beams, 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 laboratories 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.

Additional important details about the science Drivers and the R&D necessary to address them can be found in [Section 3](#).

1.3: Criteria

For the prioritization process, we developed two sets of criteria: one for the optimization of the program and another for the evaluation of individual projects.

The program optimization criteria are

- 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.

The 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 multi-disciplinary/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.

Multi-disciplinary connections are of great importance to particle physics. For example, the study of the particle physics of dark energy and inflation is performed by historical 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 multi-disciplinary 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 Particle Astrophysics Scientific Assessment Group (PASAG) panel.

Recommendations

2



Having identified the science Drivers and the decision criteria, we turn to the recommendations. Program-wide recommendations are given first, followed by specific project recommendations, a discussion of the recommended programs under the three budget Scenarios, and the recommendations related to R&D.

[Table 1](#) lists the major projects considered by P5 and summarizes the program recommendations under the three budget Scenarios, noting which Drivers are primarily addressed by each project. The 2008 P5 Report defined three “Frontiers” in particle physics: Energy, Intensity, and Cosmic. The Frontiers are not lines of inquiry in the same sense as the Drivers, but they provide a useful categorization of experimental techniques and so are also indicated in [Table 1](#).

2.1: Program-wide Recommendations

The first two recommendations align the program with the global vision and science Drivers discussed in the Introduction.

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.

Projects are categorized [large (>\$200M), medium (\$50M-\$200M), and small (<\$50M)] by construction cost to the particle physics program. The large and medium projects are also ordered in time to meet the annual construction fraction guideline specified below (Recommendation 5). This is shown in [Table 1](#) and the accompanying [Figure 1](#), which displays the construction and physics activity timelines. 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.

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, multi-disciplinary 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.

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

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

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. 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. The three main budget categories are project construction, the research program, and operations.

The particle physics research program supports activities that give meaning to the data. These include analyzing the data directly, developing and refining sophisticated computer models to compare the data with theoretical expectations, synthesizing the knowledge gained from experimental discoveries and constraints, and looking forward by developing ideas that lead to new scientific opportunities.

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.

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.

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 provided by 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.

2.2: Project-specific Recommendations

Near-term and Mid-term High-energy Colliders

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 the 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.

ATLAS and CMS were constructed and are now used by international collaborations involving nearly two hundred institutions with funding from approximately forty nations. 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 consist of 600–700 scientists each, from approximately 90 universities and five DOE Office of Science national laboratories. They 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. Those roles include designing, delivering, and operating particle detectors, producing new physics results, and serving visibly in collaboration leadership. 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.

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 then 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.

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

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. A long-baseline site is also 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.

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 violation² 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 goal corresponds to an exposure of 600 kt*MW*yr 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 exposure implies a far detector with fiducial 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**

² Three of the most important symmetry operations in physics are charge conjugation, C, in which particles are replaced by their antiparticles; parity inversion, P, in which all three spatial co-ordinates are reversed; and time reversal, T. CP violation, the lack of invariance under the combined operations of C and P, is involved in the dominance of matter over antimatter in the Universe. Why there is matter but very little antimatter is still a big mystery that likely requires physics beyond the Standard Model.

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. 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.

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.

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.

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, and therefore appear separately in [Table 1](#). 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.

As discussed in [Section 3.2](#), 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.

DAE δ ALUS 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 DAE δ ALUS with a well-defined short-baseline neutrino-oscillation physics program using cyclotron-produced ^8Li decay at rest. IsoDAR 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.

LAr1 is a mid-scale short-baseline accelerator-based experiment to address both the neutrino and antineutrino 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 $\sim\text{GeV}$ 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.

Cosmic Surveys

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.

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

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 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.

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.

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.

Summary of changes in direction

Several of these recommendations represent significant changes in direction, which we highlight here:

- 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 needs (see below, Enabling R&D recommendations). Specifically, 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 Muon Ionization Cooling Experiment (MICE). In addition, in the general accelerator R&D program, focus on outcomes and capabilities that will dramatically improve cost effectiveness for mid- and far-term accelerators.

2.3: Funding Scenarios

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 and Scenario A

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, some 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.

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 to \$95M in 2024, 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. 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.

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, setting a faster course for the long-baseline neutrino program, and preserving the long-term investments in R&D and the research program. Although each is highly valuable, none can be accommodated in Scenario A.

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, <small>Mu2e small reprofile needed</small>	Y	Y					✓	I
HL-LHC	Y	Y	Y	✓		✓		✓	E
LBNF + PIP-II	Y, <small>LBNF components delayed relative to Scenario B.</small>	Y	Y, enhanced		✓			✓	I,C
ILC	R&D only	R&D, <small>possibly small hardware contributions. See text.</small>	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, <small>some reductions with redirection to PIP-II development</small>	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 [$> \$200M$] 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 C

We now turn to the unconstrained Scenario C given in our Charge. 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 a few 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 proton-proton (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. 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. Experience suggests this effort will also have large, positive impacts beyond particle physics.

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. 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.

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.

2.4: Enabling 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.

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.

A multi-TeV e^+e^- collider could be based on either the Compact Linear Collider (CLIC) or plasma-based wakefield technology. The wakefield technology would be done as an energy upgrade to the ILC, or located elsewhere.

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.

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.

Instrumentation R&D

The particle physics detector community has historically been an important contributor to broadly applicable innovation in instrumentation. A recent example is the key role of ultra-sensitive transition edge bolometers in CMB experiments. A rich spectrum of challenging physics experiments is planned that requires advances in instrumentation. The challenges include ever-greater requirements for sensitivity and performance. It is only through investments in the development of advanced, cost-effective new technologies that the science goals can be met. 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 post-doctoral 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 laboratories, and fully exploit the unique capabilities and facilities offered at each.

Computing

The recent *Report from the Topical Panel Meeting on Computing and Simulations in High Energy Physics*³ 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.

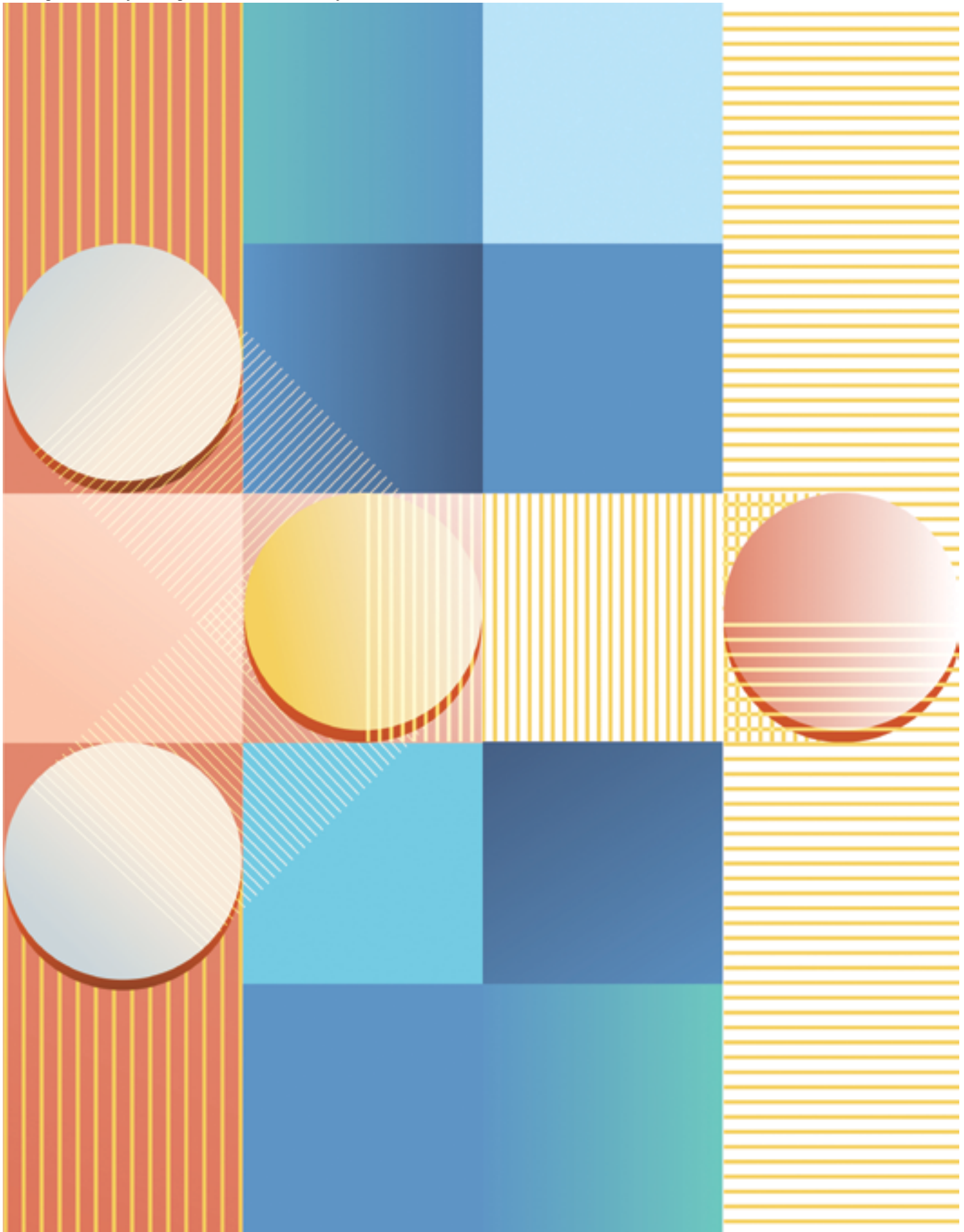
Recommendation 29: Strengthen the global cooperation among laboratories and universities to address computing and scientific software needs, and provide efficient training in next-generation hardware and data-science software relevant to particle physics. Investigate models for the development and maintenance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

The Science Drivers

3



This section provides more detailed descriptions of the five science Drivers, as well as the R&D programs needed to address the Drivers. Further information can be obtained at the Snowmass archive: <http://www.slac.stanford.edu/econf/C1307292/>, and references therein.



3.1: Use the Higgs Boson as a New Tool for Discovery

The recently discovered Higgs boson is understood to be a form of matter never before observed: a scalar boson—a fundamental particle with no spin—that generates the masses of the other fundamental particles in the Standard Model. However, the Higgs boson remains a mystery. What principles determine the values of its couplings to quarks and leptons? How is it related to neutrino masses? Does it couple to dark matter? Is there one Higgs particle or many? Is the Higgs boson really fundamental or is it composed of other constituent particles? Does it interact with particles and antiparticles in exactly the same way? This new particle offers a unique portal to understanding the laws of Nature and connects several areas of particle physics. It is a great new tool for discovery.

Now that the Higgs boson mass is known, the Standard Model predicts its interactions and properties with no free parameters. Any deviation from these predictions provides unambiguous evidence for new physics, making a rigorous study of the Higgs boson truly compelling. By measuring the Higgs boson interactions with the other Standard Model particles to percent-level precision, new physics can be explored in a general way at energy scales that are well beyond the beam energies of contemporary accelerators; the Higgs self-interactions can be studied to verify what causes the Higgs field to produce the masses of the other fundamental particles; the search for Higgs boson decays to undetected (“invisible” or “dark sector”) particles may reveal critical clues that unlock the mystery of dark matter; and vector boson production can be studied to verify the key roles the Higgs field is presumed to play in the Standard Model. Thus, the precision measurements of Higgs properties will provide a valuable look ahead to possible physics at far-future colliders, informing critical choices of direction.

Higgs boson properties: enabling discovery

To unlock its secrets, there are key properties of the Higgs boson that must be understood in detail:

Higgs boson interactions with Standard Model particles: The interaction strengths (couplings) of the Higgs boson to the other particles of the Standard Model are of particular significance.

The predictions of well-motivated scenarios of new physics generically place deviations in these couplings from their Standard Model values at the few percent level, ultimately requiring measurement of these couplings at the percent or sub-percent level to advance knowledge in many crucial avenues of exploration. The pattern of possible deviations of the couplings adds essential knowledge in discovering the underlying structure of the new mechanisms involved. In this way, knowledge can be gained about the existence of new particles that are predicted in many theories, including additional Higgs bosons that may share the role of generating masses for particles. This complements the direct search for extra Higgs bosons and other new particles at colliders. These precision measurements can also signal whether the Higgs boson is elementary or a bound state of new fundamental fermions. In particular, the top quark may have special interactions with the Higgs boson since it is the heaviest quark and its couplings may reveal new interactions. Lastly, a small admixture of CP violation may be present in the Higgs boson couplings to the Standard Model particles and this could contribute to the observed matter-antimatter asymmetry in the Universe.

Higgs boson interactions with new particles: It is possible that the Higgs boson interacts with dark matter and may provide the *only* interaction (beyond gravity) between dark matter and the Standard Model. The Higgs boson may thus serve as a valuable portal to the dark sector and the question of whether the Higgs boson also generates masses for particles in the dark sector is a crucial one. The rate for such decays (including “invisible decays”) is likely to be small and a high level of sensitivity is needed for their detection. In addition, the total decay rate, *i.e.*, the width of the Higgs boson, sums over all of its possible decay modes and is one of its most important characteristics. This quantity affects nearly all of the Higgs boson interactions and can reveal hidden decay channels.

Higgs boson interactions with itself: The Higgs boson is expected to interact with itself via couplings that are governed by the Standard Model Higgs potential. The determination of these self-couplings measures the shape of the Higgs potential and probes for the effects of new physics at very high energies. Measurements of the Higgs self-coupling are performed by observing double Higgs boson production at colliders. These

are difficult measurements, requiring high energies and high statistics.

Higgs boson interactions with neutrinos: The tiny masses of neutrinos indicate that they may interact with the Higgs sector in a special way. Either neutrinos couple to the Higgs boson very, very weakly, or neutrinos interact with a different Higgs boson, or neutrinos receive their mass from a completely different mechanism. Some models suggest that collider searches for rare Higgs decay modes and for new scalar particles that couple predominantly to leptons, together with searches for neutrinoless double-beta decay, charged-lepton flavor violation, and other related processes, could provide essential clues about how neutrinos communicate with the Higgs field.

Role of the Higgs boson in vector boson scattering: Without the Higgs boson, the probability for the scattering of two weak vector bosons exceeds unity at high energies. In the Standard Model, probability is conserved when the Higgs boson contributions to this scattering are included. This important high-energy behavior still needs to be tested experimentally and could exhibit deviations due to new particles that couple only to the electroweak gauge sector, or due to new strongly interacting Higgs-boson-like states.

Higgs Opportunities

All signs point to the important discovery potential of percent-level precision studies of Higgs properties. The determination of these properties is one of the top priorities in the physics program of high-energy colliders. The complementary aspects of hadron and electron-positron colliders are both needed to carry out this program to its fullest extent. Full exploitation of such a precision program will also require significant improvements in theoretical higher-order calculations of the Higgs boson mass, production, and decay, as well as better knowledge of key external inputs to these calculations such as the mass of the b -quark.

The **LHC** will be the first laboratory to use the Higgs boson as a tool for discovery, first with a run of 300 fb^{-1} of integrated luminosity at 14 TeV, and then with ten times more data (3000 fb^{-1}) at the **HL-LHC**. The LHC can provide a precision measurement of the Higgs mass to 100 MeV, improving to about 50 MeV

with the HL-LHC. The LHC/HL-LHC measures the product of production rates for Higgs bosons with the Higgs branching fraction into most fermions and gauge bosons. By either assuming the Standard Model predictions for some couplings, or by making some minimal model-dependent assumptions, global fits that extract the coupling values to known particles can be performed. In this manner, the LHC can measure most of the Higgs couplings to the 5–10% level, with the HL-LHC improving the precision to a few percent. Invisible decays of the Higgs boson, such as into dark matter, are detected by “observing” missing energy at colliders. In this manner, the HL-LHC can probe for invisible decays to a sensitivity of 10% for the branching fraction. The HL-LHC also provides unique capabilities to study statistically limited Standard Model decays such as $H \rightarrow \mu^+ \mu^-$ (giving access to couplings to the second-generation fermions) as well as other possible exotic decay channels. A direct demonstration of the Higgs boson’s role in the mechanism of restoration of unitarity in vector boson scattering is also a prime motivator for the HL-LHC. Indications of the Higgs self-coupling through the first observations of double-Higgs production may also be possible. The HL-LHC has a compelling and comprehensive program that will make essential measurements of the Higgs properties.

After the HL-LHC, the next opportunity to investigate the properties of the Higgs in detail is an e^+e^- collider, of which the International Linear Collider (**ILC**) is the most mature in its design and readiness for construction. The unique combination of the relatively simple Higgs boson production mechanism and the clean e^+e^- collision environment enables measurements of the branching fraction for *all* possible Higgs decay channels, including invisible decays, decay modes that are undetectable at the LHC/HL-LHC due to large backgrounds, and exotic decay topologies into new particles that may only interact with the Higgs. The ILC thus greatly increases the sensitivity to the Higgs boson couplings to particles in the dark sector and other new physics. After initial 250 GeV running, operation at 500 GeV provides increased rates of Higgs production and access to couplings via vector boson fusion and $t\bar{t}H$ production. In a completely model-independent way, most Higgs couplings can then be determined to the percent or sub-percent level, and the total width to about 5%. This level of precision is necessary to establish signals at the 3–5 sigma

level for couplings that differ from the Standard Model by a few percent and thus reaches the theoretical target. In addition, the ILC can measure the top-quark and Higgs-boson mass to better than 0.1% and determine the CP admixture of the Higgs boson at the level predicted in scenarios beyond the Standard Model, and an upgraded ILC operating at 1 TeV can measure the self-coupling of the Higgs boson to better than 20%.

The hadronic HL-LHC and e^+e^- ILC are highly complementary discovery accelerators. If new particles are observed directly at the LHC/HL-LHC, the pattern of deviations in the combined measurements of Higgs boson couplings at both the HL-LHC and ILC adds essential knowledge in determining the underlying structure of the new physics. If no new physics is found directly at the LHC/HL-LHC, the precision of the ILC measurements for the Higgs couplings can indirectly uncover new physics present at mass scales beyond that kinematically accessible at the LHC/HL-LHC.

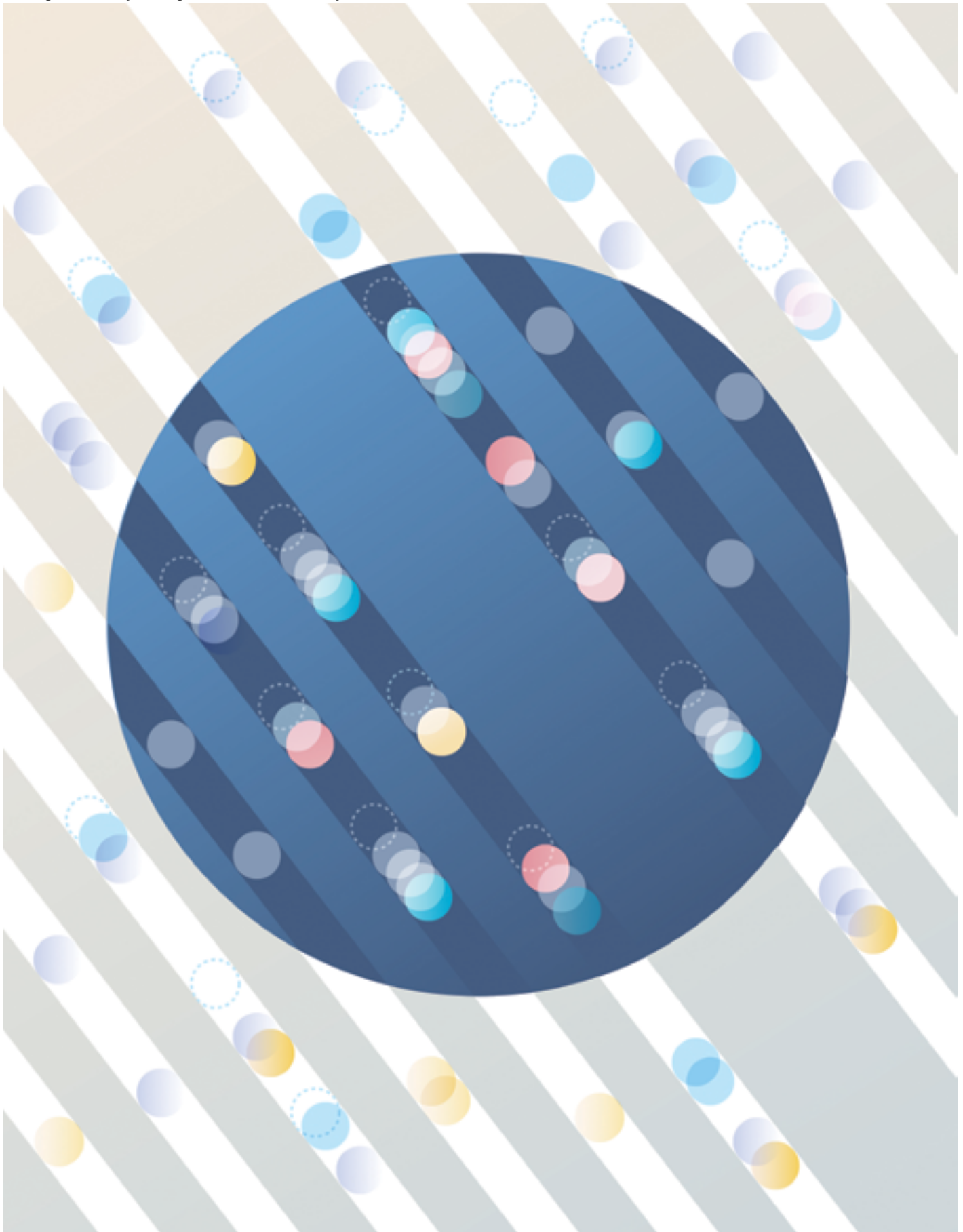
Longer-term future-generation accelerators bring prospects for even better precision in Higgs properties and hence discovery potential. Circular e^+e^- accelerators, such as the **FCC-ee** project being studied at CERN and the **CepC** project in China, generally have high instantaneous luminosity, multiple detectors, and small statistical uncertainties, and in many cases, provide the highest precision on Higgs properties. However, the options are limited in collision energy, precluding direct measurements of couplings to top quarks or self-couplings. To make significant progress in the study of the Higgs potential via self-couplings and to fully study the Higgs boson contributions to vector boson scattering, a ~ 100 TeV pp collider such as the **VLHC** and/or a multi-TeV e^+e^- linear collider such as the 3 TeV **CLIC** would be necessary. Such accelerators also have the highest discovery potential for additional, more massive, Higgs bosons.

A muon collider Higgs factory with center-of-mass collisions at the Higgs mass could be contemplated as a first stage toward a high-energy muon collider; however, the LHC is already a Higgs factory, the ILC is a precision Higgs facility with a completed Technical Design Report, and a muon collider still faces many technical challenges. Although these considerations weaken the case for the muon collider approach, we note that a muon Higgs

factory could make some unique measurements, particularly a direct measurement of the width of the Higgs boson.

Higgs Timeframes

Within the P5 timeframe, the program of Higgs boson measurements that began at the LHC will improve with the higher energy and increased integrated luminosity of the 14 TeV LHC Run 2. The HL-LHC will operate in the 2020s, increasing the precision of the available measurements as data accumulates. As the pioneering HL-LHC program ramps down in the early 2030s, the complementary ILC could launch operations. A decade or more will then be needed to achieve the target precision for Higgs boson measurements at the ILC. Together, the HL-LHC and ILC provide a stream of data using the Higgs boson as a tool for discovery for several decades.



3.2: 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. A diverse research program exploiting particle astrophysics, accelerator, and reactor experiments has uncovered a new landscape in neutrino physics with a very promising future for continued discovery. Powerful new facilities are needed to probe many aspects of the puzzling and experimentally incomplete picture of neutrino physics.

Particle physicists now know that neutrinos oscillate, *i.e.*, they change type (flavor) as they move in space and time. This, in turn, implies that neutrinos have non-zero, distinct masses and that the lepton flavors mix. A three-flavor paradigm that describes the three known Standard Model neutrinos—the flavor states—as quantum mechanical mixtures of neutrinos with well-defined masses has emerged. It describes almost all existing neutrino data successfully.

These groundbreaking discoveries open the door to addressing essential questions:

What is the origin of neutrino mass?

Speculations on the origin of neutrino masses are wide-ranging in the absence of more complete experimental information. All point to the existence of new physics, ranging from right-handed neutrinos to the violation of lepton number. One possibility is that the neutrinos interact with the Higgs field in the same way that the electron does, only much more weakly. Alternatively, neutrinos may interact with new yet-to-be-discovered Higgs fields. A third option is that neutrino masses signal a new ultra-high-energy scale that may be associated with the unification of matter and forces, or inflation. Piecing together the neutrino mass puzzle will require experimental information from several areas of particle physics research, from long-baseline neutrino experiments to high-energy colliders like the LHC, from searches for rare processes with charged-leptons to different probes of the large-scale structure of the Universe.

How are the neutrino masses ordered?

Neutrino oscillation experiments measure the neutrino mass-squared differences along with most of the parameters that characterize the relation between the neutrino flavor and mass states. The ordering of the neutrino mass differences, referred to as the mass hierarchy, remains elusive because the effects of the two independent mass-squared differences have not yet been observed in a single experiment, nor have researchers observed the oscillation-related effects of neutrinos propagating through matter (matter effects) associated with the largest of the mass-squared differences. Current- and next-generation oscillation experiments will address this. Knowledge of the mass hierarchy is necessary to reconstruct the values of the neutrino masses and to interpret the results of searches for the nature of the neutrino, as discussed below.

What are the neutrino masses?

Neutrino oscillation experiments cannot determine the absolute scale of neutrino masses. Precision measurements of the beta-decay spectrum offer the least ambiguous direct probe of the neutrino masses. Next-generation surveys are sensitive to neutrino masses at the 0.3 eV level, and new ideas for improvements are being pursued. These efforts, which access valuable information to particle physics, are primarily funded by nuclear physics agencies.

Cosmic surveys provide the most precise indirect information on the values of the neutrino masses. Neutrinos are by far the most numerous matter particles in the Universe, so they affect the distribution of galaxies and the pattern of anisotropies in the cosmic microwave background. Results from current surveys provide the tightest constraints on the sum of the neutrino masses. Next-generation cosmic surveys—LSST, DESI, CMB-S3, CMB-S4—are expected to improve on these by a factor of ten.

Do neutrinos and antineutrinos oscillate differently?

The fact that neutrino masses are non-zero allows physicists to ask whether leptons violate CP invariance. The three-flavor paradigm accommodates new (up to three) independent sources of CP violation in the form of fundamental parameters that govern how matter and antimatter behave differently. These may also play a role in understanding why the Universe is overwhelmingly made up of matter rather than antimatter.

One of these, the CP-violating phase δ_{CP} , can only be measured in oscillation experiments. Next-generation neutrino oscillation experiments, therefore, have the distinct capability of revealing whether neutrinos and antineutrinos oscillate differently.

Are there additional neutrino types and interactions?

It is common practice to analyze oscillation data assuming three flavors of neutrinos that interact as prescribed by the Standard Model, but neutrinos are a continuous source of surprises. Very large deviations are still allowed. Possible deviations from the three-flavor paradigm include non-interacting (or *sterile*) neutrinos or non-standard interactions of the ordinary neutrinos. In fact, there are hints for physics beyond the three-flavor paradigm from short-baseline oscillation experiments using reactors, accelerators and radioactive sources, and from cosmic surveys. The current data may be collectively explained by the existence of one or more sterile neutrinos with masses around 1 eV. The discovery of light sterile neutrinos would be of profound importance for fundamental physics.

While cosmic surveys are sensitive to neutrino masses, they are also powerful probes of new phenomena and speak to the question of whether currently unknown types of light neutrinos were also produced in the early Universe. There is also the possibility that the neutrino and dark matter puzzles are related: sterile neutrinos with masses in the keV mass range could be an important component of dark matter.

Are neutrinos their own antiparticles?

The very nature of neutrinos, whether they are their own antiparticles (Majorana fermions) or not (Dirac fermions), is unknown. Neutrinos are the only fundamental particles that could be Majorana fermions. Indeed, most models of the origin of neutrino masses predict that this is the case. The answer to this question is intimately tied to the status of lepton number conservation—lepton number violation implies that the neutrinos are Majorana fermions. The observation of lepton number violation, similar to that of baryon number violation, would have a dramatic impact on fundamental physics. On the flip side, if neutrinos are Dirac fermions, lepton number conservation is a fundamental law of nature. Addressing this question is mandatory in order to understand the origin of neutrino masses, and experiments to determine the nature

of the neutrino also inform the program to measure the absolute neutrino mass.

The most powerful probe of lepton number conservation, and whether neutrinos are Dirac or Majorana, is the observation of neutrinoless double-beta decay. These are questions and experiments of the greatest interest to particle physics. Although construction of these experiments is primarily funded by DOE Nuclear Physics, which stewards this line of research, NSF Particle Astrophysics (PA) funds some construction and supports research groups on these experiments. DOE HEP also supports some research groups. Next-generation neutrinoless double-beta decay experiments are currently being planned and prioritized by the Nuclear Science Advisory Committee. These experiments are intended to observe a signal if the neutrino mass hierarchy is inverted and neutrinos are Majorana fermions. Next-generation experiments will continue to benefit from strong HEP and PA participation.

Opportunities in Neutrino Oscillation Physics

Neutrino oscillation rates depend not only on the fundamental parameters, mass and mixing, but also on experimental parameters: the baseline, the neutrino energy, and the event rate driven by beam power and detector mass. The combination of all of these determines the strength of the oscillation signals observed and therefore the sensitivity of the experimental efforts.

The current long-baseline experiments, T2K in Japan and the NOVA experiment in the U.S., have some sensitivity to the neutrino mass hierarchy, depending on the value of the CP-violating phase δ_{CP} . Other near-term proposals, including JUNO and PINGU, may provide valuable information. Both are very challenging. The PINGU proposal, an infill of the IceCube experiment at the South Pole, will attempt to reveal the mass hierarchy via the measurement of a very large sample of atmospheric neutrinos. JUNO, instead, hopes to reveal the mass-hierarchy through the precision measurement of the reactor neutrino flux some 50 km from the source; it should also provide precision measurements of other neutrino oscillation parameters. There is no current or planned experiment in this decade with significant sensitivity to leptonic CP violation.

The U.S. is well positioned to host a world-leading neutrino

physics program. Its centerpiece would be a next-generation long-baseline neutrino facility (LBNF). LBNF will combine a high-intensity wideband beam, very long baseline, and large-volume precision detector to make an accurate measure of the oscillated neutrino spectrum.

LBNF's 1.2 MW wideband neutrino beam will be realized with the Fermilab's PIP-II upgrade project, which provides very high intensities in the Fermilab accelerator complex. The construction of PIP-II and the beamline for LBNF will bring major advances in accelerator technology in the areas of SCRF and targetry and lay the foundation for a possible future neutrino factory.

A large-volume neutrino detector based on LAr technology capable of detecting tracks from charged particles at an extremely low threshold will record signals and reject backgrounds across a wide range of energies in LBNF. The oscillated spectra can be compared to the un-oscillated spectra by combining these far detector measurements with those in a near detector.

With these ingredients, combined with a baseline greater than 1000 km, LBNF can, with a single experiment, measure evidence for CP-violation in the lepton sector and provide a definite determination of the mass hierarchy, independent of the value of δ_{CP} . In addition, LBNF has the potential to isolate a sample of tau-neutrinos that may play a significant role in testing the three-flavor paradigm and revealing the existence of new phenomena.

Hyper-Kamiokande (Hyper-K) is a proposed megaton-class water Cherenkov detector to be exposed to an off-axis narrowband neutrino beam in Japan. Hyper-K would use neutrinos from an upgraded MW-class J-PARC accelerator complex at a baseline of 295 km, combined with atmospheric neutrinos. The project shares many of the goals of LBNF, albeit with a different detector technology, different beam characteristics, and a different baseline. Within the context of a cooperative global program of large-scale projects, the formulation of an optimized, coherent, long-baseline neutrino program could be explored to strengthen the overall global particle physics program.

Alternatives to LBNF, and alternative routes to LBNF, have

been proposed. RADAR and CHIPS are both ideas for new detectors exploiting the existing NuMI beamline to improve knowledge of oscillation parameters. RADAR proposes to build a LAr TPC at the Ash River site, thereby combining a physics program with detector R&D towards LBNF. CHIPS proposes to deploy 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. These could provide some incremental sensitivity beyond NOVA and T2K for mass hierarchy and CP violation. DAE δ ALUS 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. It potentially has CP reach comparable to that of LBNF but is insensitive to matter effects.

Going Underground

Underground operation of a massive LAr detector will support an enriched physics program that complements and enhances the neutrino-physics reach of the accelerator-based program and mitigates significant background risks associated with operation on the surface. An underground large-volume LAr detector will perform precision measurements of atmospheric neutrinos, search for nucleon decay, and possibly detect a neutrino burst from a core-collapse supernova explosion in the Milky Way galaxy, which is predicted to occur with a frequency of about three per century. The atmospheric neutrino measurements complement and enhance the oscillation program. LBNF's LAr detector technology is most sensitive to nucleon decay into kaons, providing a complementary window to searches more sensitive to decays into pions. The significance of nucleon decay and its impacts are described in [Section 3.5](#).

Supernova neutrinos are messengers both of the mechanism of stellar core collapses and of neutrino properties. Supernovae release 99% of their vast energy in the form of neutrinos and antineutrinos of all flavors. Supernova neutrinos oscillate while propagating through the star's ejecta, carrying information on neutrino properties, including the mass hierarchy, as well as on the astrophysics of the collapse. Their propagation is modified by never-observed effects due to neutrino-neutrino interactions, and encodes potential signatures of new physics such as new neutrino states and interactions, and neutrino magnetic

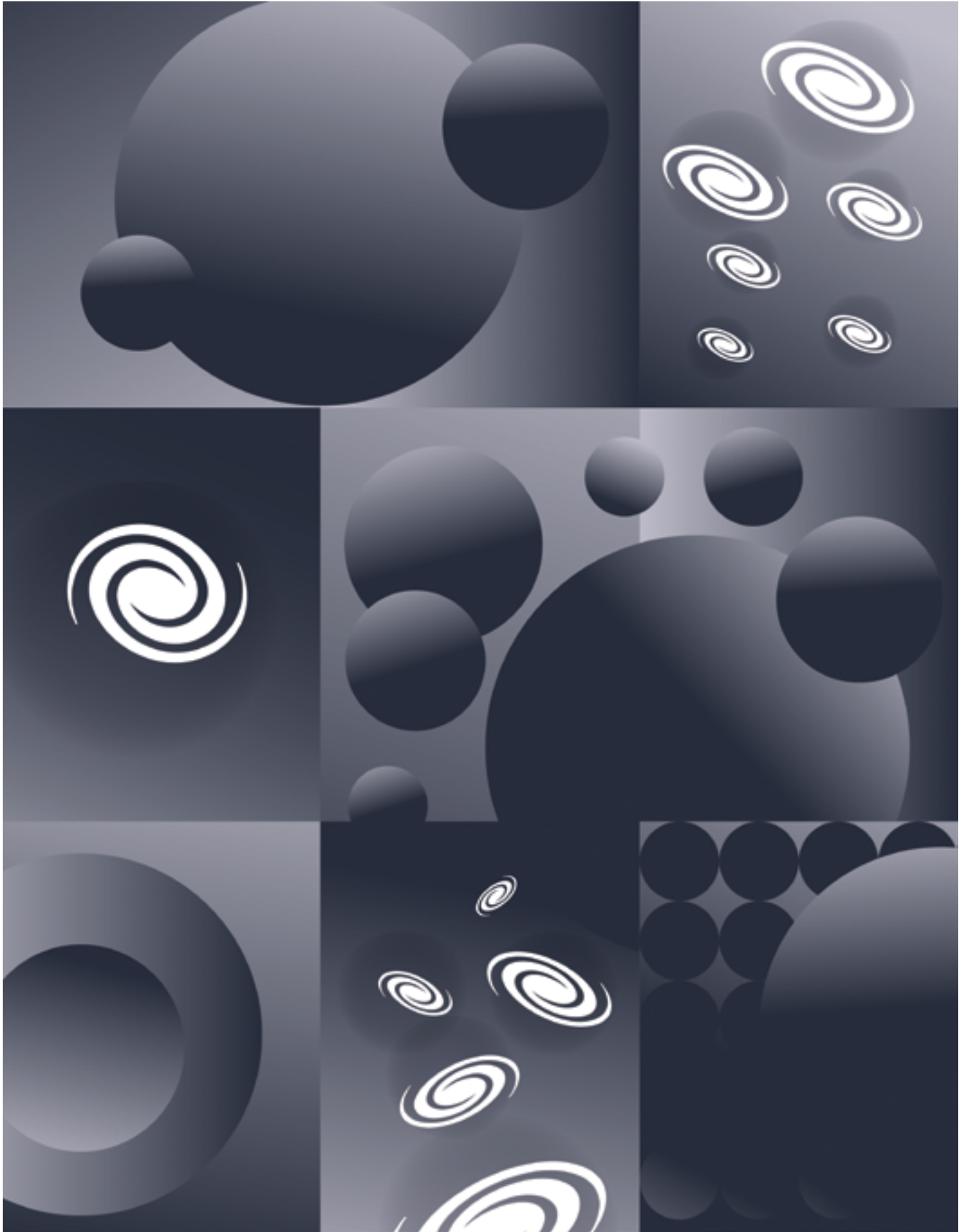
moments. Precision measurements of supernova-neutrino fluxes are also sensitive to new light particles. Measurements of the energy spectrum and time structure of the burst with broad flavor sensitivity are critical to the extraction of neutrino physics. Although existing and proposed supernova-neutrino detectors worldwide are primarily sensitive to electron-*antineutrinos*, the LBNF LAr detector has exquisite sensitivity to the electron-*neutrino* flavor component, which carries unique physics and astrophysics information, including effects from the formation of the remnant neutron star and electron-neutrino-specific oscillation effects.

Short-Baseline Neutrino Oscillations

The hints from short-baseline experiments suggesting new physics can be addressed by several experiments currently under construction or in the proposal stage. These would make use of neutrinos from radioactive sources, pion decay-at-rest beams, pion and kaon decay-in-flight beams, muon-decay beams, and nuclear reactors. Different subsets of these efforts have the potential of definitively addressing the sterile neutrino interpretation of the short-baseline anomalies. The short-baseline accelerator experiments at Fermilab also provide a platform for LAr detector development toward LBNF. Most of these proposals are small-scale—ICARUS++, IsoDAR, LAr1-ND, MicroBooNE, OscSNS, PROSPECT—while LAr1 and NuSTORM are mid-scale and large-scale, respectively.

Diversity and balance in the neutrino program

The U.S. neutrino program envisioned in this report encompasses both small and large experiments in the near- and far-term to address fundamental questions in particle physics. Development of software and hardware for different experiments complement and enhance one another. Data from near-term experiments produce physics results while construction for next-generation experiments is underway. This provides a diversity and balance essential for the field.



3.3: Identify the New Physics of Dark Matter

Astrophysical observations imply that the known particles of the Standard Model make up only about one-sixth of the total matter in the Universe. The rest is dark matter (DM), presumed to be particles that are all around us and are passing through Earth and everything else at one-thousandth the speed of light, similar to the Earth's speed orbiting the galaxy. Dark matter represents a bizarre shadow world of fundamental particles that are both omnipresent and largely imperceptible. In the coming decade, dark matter experiments will improve upon the sensitivities of current searches by several orders of magnitude and together they are poised to reveal the identity of the dark matter. This discovery 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. These include weakly interacting massive particles (WIMPs), gravitinos, axions, sterile neutrinos, asymmetric dark matter, and hidden sector dark matter. The masses and interaction strengths of these candidates span many orders of magnitude, and, of course, the dark matter could be composed of more than one type of particle. WIMPs, interacting through the exchange of particles with mass on the order of the weak scale, can experience thermal⁴ freeze-out at densities within the correct order of magnitude to constitute the dark matter. This tantalizing possibility makes the WIMP an especially well-motivated dark matter candidate.

Although there is abundant evidence for the gravitational interactions of dark matter from astronomical observations on a variety of length and time scales, no other interactions of dark matter have been conclusively detected, although there are some unconfirmed anomalies. Several different approaches are required to establish and corroborate a dark matter signal and to extrapolate from a discovery to understanding the properties of dark matter in the Universe. It is therefore imperative to search for dark matter in every feasible avenue. There are four known experimental approaches to carry out the search for dark matter: direct detection, indirect detection, particle colliders, and astrophysical evidence of non-gravitational interactions.

Direct Detection

In direct detection experiments, physicists search for dark matter particles that routinely pass through, but occasionally interact with, ordinary matter. A confirmed direct detection signal would prove that dark matter particles exist and would provide information on their fundamental nature and interactions, paving the way to understand the new physics known to be lurking beyond the Standard Model. Direct detection experiments are based on observation of elastic recoils of nuclei from WIMP collisions, or axion conversion in a magnetic field.

Weakly interacting, asymmetric, and hidden sector dark matter particles

These dark matter particles can occasionally interact with ordinary matter, producing rare nuclear recoil events that can be detected using ultra-sensitive, low-background experiments. The U.S. program is positioned to make a major dark matter discovery. Over the past 30 years, the sensitivity of direct searches for DM-nucleon scattering has improved by over five orders of magnitude. This rate of progress is expected to continue and perhaps even accelerate over the next decade, presenting a tremendous opportunity for the discovery of dark matter interaction with ordinary matter. Many direct detection techniques were invented by U.S. groups, leading experiments have major U.S. participation, and more than 300 researchers in the U.S. are working on direct detection experiments. There is a wide range of direct detection technologies, and this field is a hotbed of technological innovation. Technologies with major U.S. participation include: two-phase xenon; single- and two-phase argon; cryogenic germanium and silicon; bubble chambers; sodium iodide crystals; and directional time-projection chambers. The preeminent challenge in this field is the elimination of backgrounds, with approaches including the use of low background materials, self-shielding, particle identification, and astrophysical rate modulation. A continued R&D effort will enable optimal sensitivity of future DM detection experiments over the full expected mass range and will develop techniques that can indicate the direction of incoming dark matter particles.

Through the Snowmass process, a community consensus emerged on criteria for new DM direct searches: any new experiment should either provide at least an order of magnitude improvement in cross section sensitivity for some range of DM

⁴In most WIMP models, pairs of dark matter particles are produced and annihilated in the hot early Universe in thermal equilibrium with ordinary matter. Those that survive annihilation “freeze out” and persist to the present day. WIMP candidates that annihilate to the correct relic density to be dark matter typically have average annihilation cross-section (multiplied by the relative velocity of the annihilating WIMPs) of $\langle\sigma v\rangle = 3 \times 10^{-26} \text{ cm}^3 \text{ s}^{-1}$.

masses and interaction types, or demonstrate the capability to confirm or deny an indication of a DM signal from another experiment. Currently under review is a suite of Generation 2 (G2) discovery experiments. These experiments should span a broad mass range (1 GeV to 100 TeV) and use multiple target materials to search the available spin-independent and spin-dependent parameter space⁵ with an emphasis on cross-section reach and discovery potential, as well as the ability to confirm or refute the current anomalous results. Because there is significant international competition, and a discovery could happen at any time, proceeding with the G2 experiments as quickly as possible is especially valuable.

Following either a G2 discovery or further constraints, there should be at least one U.S.-led, internationally attractive Generation 3 (G3) direct detection experiment with maximal discovery potential. In addition, the U.S. should participate in other global advanced direct detection experiments at the G3 scale. If no G3 discovery is made, DM searches will become limited by solar, atmospheric, and diffuse supernova neutrino coherent scattering backgrounds, so the goal of G3 experiments will be to explore DM parameter space comprehensively down to the limits set by neutrino backgrounds. This provides a well-defined target for this field over the next 20 years.

Axions

The axion is another well-motivated candidate for the dark matter. Its existence would confirm a long-standing hypothesis that an additional symmetry in Nature is the reason that CP-violating terms are small in the strong interaction, and it also has the required properties to be the dark matter. The upcoming ADMX experiment is projected to be sensitive to axion masses up to 40 μeV . With continuing R&D, future experiments should probe higher masses, within the allowed range available within astrophysical constraints. Data from cosmology will provide additional constraints on the axion parameter space in the region currently being probed by axion searches.

Indirect detection

In many models, dark matter particles can interact with each other, converting their large rest mass into other particles, including gamma rays, neutrinos, and charged particles. Indirect detection of dark matter refers to searches of anomalous fluxes

of these high-energy particles, which provide a unique test of the particle nature of dark matter, *in situ* in the cosmos. Indirect detection could discover or confirm a new particle and unambiguously identify it with dark matter. Large concentrations of dark matter have collected in gravitational wells including the inner galaxy and nearby dwarf galaxies, the latter of which have low astrophysical backgrounds.

The rate from any of these sources depends on the annihilation cross section, which also governs the relic abundance of the dark matter. Therefore, there is a robust and well-defined target in all indirect detection experiments: the value of the cross section that leads to the observed dark matter abundance. Successful confirmation of an indirect detection signal would open up a program of fundamental measurements of the properties of the dark matter particle(s), including masses and branching ratios. Relevant limits on the annihilation of dark matter particles into photons, electrons, and antiprotons have been obtained from the PAMELA, AMS, and Fermi satellites, and from the VERITAS and H.E.S.S. ground-based experiments. Although excess positrons have been detected, the constraints from gamma rays have already ruled out the most straightforward dark matter interpretation of this excess. Future experiments sensitive to antideuteron fluxes at low energies may provide incisive tests of some WIMP dark matter candidates.

The next major step for the indirect dark matter program will be to search for dark matter using the Cherenkov Telescope Array (CTA). CTA will search for dark matter in both the inner Galaxy and in dwarf galaxies. U.S. groups in CTA are developing innovative dual-mirror technology with the goal of providing superior event reconstruction, background rejection, and resolution of astrophysical point sources to distinguish them from diffuse emission and dark matter signals. CTA will have sufficient sensitivity to reach the WIMP thermal annihilation cross section up to WIMP masses of ~ 10 TeV, assuming conventional dark matter halo profiles. Strong U.S. participation in CTA would enable such sensitivity with significantly shorter observing time, continuing a leading U.S. program of indirect dark matter searches.

The IceCube, Super-Kamiokande, and PINGU experiments can detect the neutrinos produced by dark matter annihilation. WIMPs trapped in the sun annihilate, producing high-energy

⁵In the non-relativistic limit, WIMP-nucleon couplings are usefully classified as “spin-dependent” when the scattering rate depends on the DM particle’s spin, or “spin independent” when the DM particle’s spin does not affect the rate.

neutrinos, an unambiguous signal with no astrophysical backgrounds. PINGU is projected to be sensitive to spin-dependent WIMP-proton interactions at dark matter masses as low as 5 GeV. The sensitivity of PINGU to low WIMP masses is interesting since this region is intrinsically difficult to probe with current direct detection experiments. Using IceCube as a veto for incoming tracks, PINGU can view the galactic center more efficiently than before, greatly improving the sensitivity to dark matter in that important region of the sky.

Sterile neutrino dark matter can decay to Standard Model neutrinos plus a mono-energetic x-ray photon that may be detected with x-ray telescopes. Ideal places to look are nearby galaxies or clusters of galaxies.

Accelerators

Dark matter can be produced directly in high-energy colliders, providing the opportunity to discover and study its properties in a controlled laboratory environment. The detection of dark matter production relies on the signature of “missing energy,” an observed imbalance of energy and momentum, and can be classified by the visible particles that are produced in association. Collider searches for such direct production are typically sensitive to low-mass dark matter due to the large production rate. In some scenarios, the LHC can provide the strongest WIMP search reach for dark matter up to roughly 25 GeV, and this sensitivity can be extended to a few hundred GeV at the HL-LHC for specialized models. The LHC best probes dark matter candidates that have couplings to quarks, while the ILC mainly probes dark matter candidates with leptonic couplings. In addition, many theories beyond the Standard Model predict a spectrum of new particles, with large production rates, that subsequently decay into dark matter particles. This provides an additional avenue to search for dark matter at colliders, extending the sensitivity to WIMP masses up to the TeV scale at the LHC.

It is possible that the Higgs boson interacts with dark matter particles and may generate a fraction of their mass. Collider searches for “invisible” decays of the Higgs boson, which are detected by missing energy signatures, are of the utmost importance to exploit this potential window into the dark sector. The HL-LHC has sensitivity to such decays at the 10% level, and the ILC extends this reach to the sub-percent level, providing a crucial probe of mass generation in the dark sector.

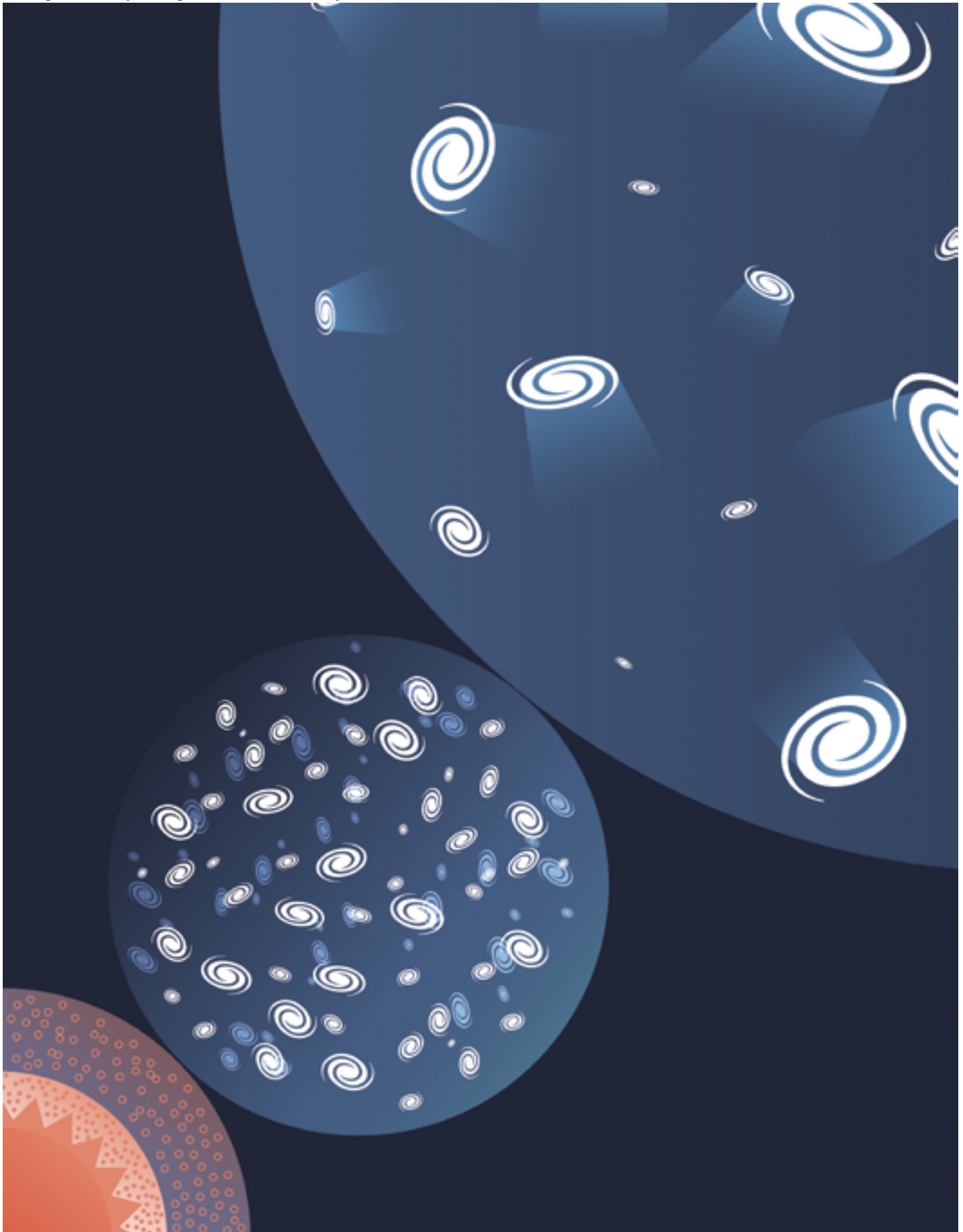
The dark matter may be composed of ultra-light (less than a GeV), very weakly interacting particles. Searches for such states can be carried out with high-intensity, low-energy beams available at Jefferson Lab or with neutrino beams aimed at large underground detectors.

Astrophysical probes

Although models of cold, collisionless dark matter agree well with cosmological observations, these models may break down at galactic or smaller scales. Simulations of dark matter structure formation suggest that the density in the inner cores of galaxies should be much higher than is currently observed. These simulations also predict the existence of many small subhalos, which could be identified with small galaxies orbiting the Milky Way; again the predictions for the number of satellites deviate from the observed number. It is possible that astrophysical effects explain these small-scale problems, but it is also possible that they are pointing to interesting properties of the dark matter particles. For example, dark matter with additional self-interactions (such as through heavy photon exchange) can give the same large-scale behavior as collisionless dark matter but different behavior at small scales. Warm dark matter, with mass ~ 1 keV, can suppress structure formation at small length scales compared to cold dark matter. Optical surveys (such as DES, LSST, and DESI) may be used to address these issues.

Goals and Timeframes

- Probe dark matter interactions with ordinary matter over a range of dark matter masses and interaction types, with large and important discovery reach (2015–2024).
- If dark matter-ordinary matter interactions are discovered, measure the dark matter particle mass and all properties of its interactions with an ambitious international effort. Otherwise, continue to probe lower cross sections down to the background limits due to neutrino-nucleon coherent scattering (2021–2030).
- Perform indirect searches for dark matter annihilation in the Sun, galactic center, and dwarf galaxies (2018–2028).
- Search for dark matter production in high-energy colliders over a broad range of masses and cross sections (2015–2035).
- Elucidate the nature of dark matter interactions using cosmic surveys (2016–2025).



3.4: Understand Cosmic Acceleration: Dark Energy and Inflation

The light from distant objects arrives at Earth after long travel times, so we observe objects as they were long ago. Observations of deep space, therefore, enable the study of the Universe when it was very young. At early times, the typical energies of particles were much higher than today. For example, the energies of the particles when the Universe was a trillionth of a second old were comparable to those of the ultra-high energy particles produced at the LHC. Armed with the dual tools of telescopes that peer back in time and high-energy accelerators that study the elementary particles and their interactions, scientists have pieced together a deeper understanding 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 phenomenon that requires new physics. That understanding can now be used to learn about the new physics responsible for the apparent early epoch of acceleration, inflation, and the latest epoch, driven presumably by dark energy.

The key questions surrounding dark energy and inflation include: What is driving the accelerated expansion of the Universe today? Is it caused by the energy in the vacuum, the so-called cosmological constant? If so, why is the value of the cosmological constant, which is but one form of dark energy, so small? Is cosmic acceleration driven by a different type of dark energy that evolves with time, or is it due to a modification of Einstein's theory of gravity, general relativity? What drove the apparent accelerated expansion during the inflationary epoch at ultra-high energies?

Observing three fundamental characteristics of the Universe—expansion history, structure, and the nature of the primordial ripples—will help answer these questions. The properties of the dark energy driving the current phase of acceleration can be determined by measuring the *expansion history* of the Universe. This history is inferred from the brightness of distant supernovae and from a distinctive pattern in the way galaxies are distributed in space, the so-called Baryon Acoustic Oscillation (BAO) signature.

Matter is not distributed uniformly in the Universe: like the

population of the United States, it has formed patterns, called *structure*. How this structure grows over time and space is the second key characteristic. Measurements of structure can distinguish two competing explanations for the current cosmic acceleration: dark energy and modified gravity. The two explanations can be tailored to produce identical expansion histories, but they predict different growth of structure in time and distance. Three measurements from which the structure can be inferred are (i) gravitational lensing—the distortions in the observed shapes of galaxies due to the bending of light by (mostly dark) matter between us and the galaxies; (ii) galaxy cluster abundance—how the number of very large objects changes over time; and (iii) Redshift Space Distortions (RSD)—the velocities of galaxies near over-dense regions.

To learn about the physics of inflation, the structure in the Universe today must be studied to uncover the nature of the first ripples from which this structure emerged. These *primordial ripples*—in both the density and in space-time via gravitational waves—were caused by quantum mechanical fluctuations during inflation and therefore provide information about the physics that drove that early epoch of acceleration. This third characteristic can be probed by measuring structure in galaxy surveys and, even more cleanly, by measuring specific properties of the Cosmic Microwave Background (CMB). Indeed, the amplitude of the primordial gravitational wave signal may have been measured by the BICEP2 experiment and, if confirmed, sets the energy scale of inflation to be 2×10^{16} GeV.

Goals and Timeframes

- Measure the parameters that characterize dark energy to 5% precision (2020) and then improve to 1% (2025) over the entire history from the decelerating epoch to accelerating epoch.
- Distinguish dark energy from modified gravity as an explanation of the current epoch of acceleration by measuring structure to 10% (2020), ultimately reaching percent precision over a wide range of distance scales and times (2030).
- Confirm or refute the BICEP2 detection of primordial gravitational waves from inflation (2015–17). Depending on the outcome, either measure the amplitude of this signal to the percent level or constrain the spectrum to sub-percent accuracy to distinguish between models of inflation (2025).

Opportunities

The current Dark Energy Survey (DES), eBOSS, and Stage-3 CMB experiments will begin delivering results by the end of the decade. Then the next generation of experiments, together with complementary efforts in Europe and Asia, will push the techniques to their limits.

The Dark Energy Spectroscopic Instrument (**DESI**) can provide very accurate distances to 25 million galaxies and quasars. The expansion history constraints from DESI BAO can be measured at the percent level, looking back over ten billion years. DESI can also measure RSD, providing a unique view of structure growth over time.

The Large Synoptic Survey Telescope (**LSST**) will precisely measure the positions and shapes of four billion galaxies along with estimates of their distances (using *photometric redshifts*), 100,000 massive galaxy clusters, and 250,000 Type Ia Supernovae, providing an order-of-magnitude improvement relative to current-generation experiments with structure measurements complementing the precise expansion history from DESI. The huge amount of information from LSST will enable detailed tests of the standard cosmological paradigm (cosmological constant and cold dark matter) over a wide range of time and space.

Together DESI and LSST can attack the question of what is driving cosmic acceleration with multiple observations that measure both expansion history and growth of structure. The precise expansion history measurements from DESI will leave competing models little freedom when confronting the measured structure of the Universe. By 2030, physicists may well have a definitive answer to the question of whether the current epoch of acceleration is driven by a new substance (dark energy) or by a new theory of space and time (a modification to general relativity).

Both surveys can also tighten current constraints on the shape of the primordial density spectrum generated during inflation. The shape is quantified with a slope, and these surveys will increase the accuracy on the slope by a factor of five or more. A Stage 4 Cosmic Microwave Background experiment (**CMB-S4**) aims at measuring the amplitude of the gravitational waves produced during inflation with percent level precision. Together

with the galaxy surveys' improved constraints on the shape of the density spectrum, CMB-S4 will constrain the ultra-high energy physics responsible for inflation.

These opportunities also address another key Driver, as all three projects will constrain the sum of the neutrino masses. CMB-S4, armed with the expansion history from DESI, projects to detect the tiny effect of neutrino masses in the cosmos with an uncertainty in the sum of the neutrino masses that is less than a third of the minimum value determined from laboratory measurements of neutrino properties. DESI alone can reach comparable limits depending on systematics with a completely different approach. CMB-S4 will also constrain the number of relativistic degrees of freedom, which can be altered if light sterile neutrinos or other new particles are produced in the early Universe.

Conversely, a comprehensive neutrino program that identifies an ultra-high energy scale as the source of neutrino mass will complement the cosmic probes of these scales. Searches for proton decay are also sensitive to physics at very high energies and will provide clues about theories that unify the electromagnetic, weak and strong forces. Together with information about inflation gleaned from CMB-S4, DESI, and LSST, these measurements offer the hope of piecing together a picture of physics at energies more than a trillion times larger than those probed directly at the LHC.

Candidates for the **Small Projects Portfolio** can dramatically leverage investments in DESI and LSST. With Integral Field Spectrographs, the large samples of both nearby and distant supernovae found by, *e.g.*, DES and LSST can be studied in detail to make supernova-based measurements as precise as the complementary DESI BAO measurements. With focused spectroscopic follow-up of the LSST galaxies, the galaxy-based measurements from LSST can be calibrated much more precisely. Proposals to develop novel Microwave Kinetic Inductance Detectors would allow the billions of galaxies found by LSST to be used for wider field/lower resolution RSD. Novel probes to search for the new force introduced by explanations of acceleration that modify Einstein's theory of gravity were identified at Snowmass.

The Future

Confounding expectations, cosmic surveys have repeatedly uncovered evidence for new physics, although to date having mapped only a small fraction of the Universe. The upcoming generation will constrain this physics in ways described above, but it also may well continue to produce the physics surprises that have appeared with almost every new generation of cosmology experiments. If so, we can anticipate the new physics serving as the focus of the ensuing suite of cosmic experiments well beyond 2030.



3.5: Explore the Unknown: New Particles, Interactions, and Physical Principles

In addition to the specific new phenomena in the other four Drivers, there are other clear indicators of physics beyond the Standard Model. The matter-antimatter asymmetry of the Universe is not explained by the measured CP-violating phase in the weak interactions of quarks. Furthermore, as previously mentioned, the relatively low value of the Higgs mass and the perplexing value of the dark energy density are strong hints that new physics awaits discovery.

It is important that we employ a broad-based strategy to search for new particles and interactions. In the next subsections we outline the most promising opportunities to reveal new physics using complementary approaches: High Energy Colliders, Precision Physics and Rare Processes, Cosmic Particles, and Low-mass Particles. In addition we comment on the roadmap towards the future. It is most important to remember that these experiments explore the unknown, and surprises are likely.

High Energy Colliders

The discovery of the Higgs boson at the LHC has opened a new era for particle physics at high energy. Its couplings to the W and Z boson are Standard Model-like, confirming that it is responsible for giving mass to these particles. Measurements of the angular distributions of its decay products indicate its scalar nature. The prediction of the existence of the Higgs boson is a triumph of the Standard Model, and its experimental observation a remarkable accomplishment of the high energy physics community.

However, the smallness (for example, compared to the Planck mass) of the Higgs boson mass suggests that there must be new particles and interactions that can be discovered at very high-energy colliders like the LHC that explore the TeV energy scale. The most studied extension of the Standard Model that explains the smallness of the Higgs boson mass relies on the existence of a new space-time symmetry called Supersymmetry. If Supersymmetry explains the smallness of the mass of the Higgs boson then it implies that there are many new particles (called superpartners) that can be observed in high-energy colliders.

Stable weakly interacting massive particles (WIMPs) typically have the right thermal-relic abundance to compose the dark matter. Extensions of the Standard Model, such as Supersymmetry, that explain the smallness of the Higgs boson mass often have a WIMP dark matter candidate. At the LHC, missing energy signatures can be used to search for the particle that makes up the dark matter, and in particular WIMPs.

The top quark couples much more strongly to the Higgs particle than do the other quarks and leptons. This may indicate a special role for the top quark and makes it very important to study its interactions more precisely at high-energy colliders, for example, through searches for exotic decays.

Direct access to new particles and new interactions is a crucial component of a comprehensive program to explore the unknown forces and particles of nature. In the near term, the LHC upgrade to higher luminosity is critical to continued exploration of the physics associated with the TeV energy scale. Beyond the HL-LHC, a 100 TeV hadron collider (VLHC) could discover and study new particles and their interactions up to an energy scale of about 50 TeV. It would also allow complete elucidation of the Electroweak Symmetry Breaking mechanism.

The ILC is a 500 GeV e^+e^- linear collider (upgradable to 1 TeV) that will complement the LHC program. It can discover new weakly interacting particles that may escape detection at the LHC. The cleaner environment can also allow more precise measurement of the interactions of Standard Model particles and of new particles. A multi-TeV scale e^+e^- collider would extend the reach of this program to higher energies.

Precision Physics and Rare Processes

High precision and rare decay experiments often do not require the highest energy accelerators but instead rely on very intense beams to produce large numbers of particles that can be studied with great precision. This provides unique sensitivity to physics at energy scales far higher than can be accessed directly at colliders. Proposed extensions of the Standard Model, *e.g.*, Supersymmetry, extra spatial dimensions, or further unification of forces not only relate parts of the Standard Model but also predict new particles and interactions whose effects may be seen in precision measurements and in rates of rare processes.

Such experiments therefore have an opportunistic component, based on what can be measured and clearly interpreted, and are performed at a variety of facilities. They include:

- *Heavy quarks and τ leptons:* The study of decays of hadrons containing a heavy quark (charm, bottom) and decays of the τ lepton at Belle II and LHCb will provide more precise tests of the validity of Standard Model predictions (including the possible sources of CP non-conservation), increasing the sensitivity to the mass scale of new physics by dramatically increasing the sample of such decays by roughly two orders of magnitude.

- *Rare Kaon decays:* The cleanliness of the Standard Model prediction of the branching ratios for the rare kaon decays $K^+ \rightarrow \pi^+ \nu \nu$ and $K_L \rightarrow \pi^0 \nu \nu$ make them an especially attractive place to search for new physics. The CERN NA62 experiment plans to increase the existing sample of $K^+ \rightarrow \pi^+ \nu \nu$ by at least an order of magnitude, and further improvement could be made by the Fermilab experiment ORKA. The KOTO experiment at J-PARC in Japan aims for first observation of the CP-violating decay $K_L \rightarrow \pi^0 \nu \nu$.

- *Rare Muon decays and processes:* Observation of charged lepton flavor violation (*e.g.*, a muon changing to an electron) would be a signature of new physics. In the muon sector, a dramatic order of magnitude increase in sensitivity to the scale of such new physics should come from experiments on the decays $\mu \rightarrow e \gamma$, $\mu \rightarrow e e^+ e^-$, and muon conversion to an electron in the presence of a nucleus. These experiments will be performed at J-PARC, PSI, and Fermilab.

Of these three processes, muon conversion to an electron in the presence of a nucleus will give the greatest increase in mass reach for new physics. Very ambitious next-generation experiments aim to be sensitive to conversion rates four orders of magnitude beyond the existing bounds, allowing them to reveal the presence of new particles with masses up to thousands of TeV, well beyond the reach of the LHC. Worldwide, there are two planned muon-to-electron conversion experiments: COMET at J-PARC and Mu2e at Fermilab. Phase I of COMET is designed to achieve a 3×10^{-15} single-event sensitivity. Phase II of COMET, not yet approved, and Mu2e plan to improve this sensitivity by two more orders of magnitude in a similar time frame.

- *Muon magnetic moment:* The prediction for the anomalous magnetic moment, $g-2$, of the muon differs from its measured value by three-and-a-half standard deviations. A new experiment, Muon $g-2$ at Fermilab, will significantly improve the accuracy of the measurement, and combining this with further improvements in the theoretical prediction for it may sharpen this discrepancy and point the way to new physics. A different approach to measuring $g-2$, using ultra cold muons, has been proposed at J-PARC.

- *Baryon number violation:* Most of the mass of ordinary matter lies in the nucleus of atoms, which are composed of protons and neutrons (baryons). After several decades of challenging experiments, stringent limits have been placed on the proton lifetime, showing it to be much longer than the age of the Universe. In fact, for some decay modes the limit on the lifetime is so strong that a single proton decay of that type would not be expected to occur in a gallon of water even after waiting a million years. Still, many extensions to the Standard Model predict an unstable proton. Notable among these are the models that unify the strong, weak, and electromagnetic interactions—in supersymmetric extensions of the Standard Model this unification occurs at an energy scale around 10^{16} GeV. The search for proton decay is an avenue to probe this ultra-high energy physics scale. Cosmic surveys that search for distortions of the microwave background also probe physics at this same energy scale. Large underground detectors built for the study of neutrino oscillations and measurements of supernova neutrinos can simultaneously be used to search for proton decay. The search for proton decay is part of the planned program for the neutrino experiments Hyper-Kamiokande and LBNF. They offer the opportunity over the next 20 years to increase the sensitivity to proton decay by an order of magnitude.

Baryon number violation by two units may be related to Majorana neutrino masses, which violate lepton number conservation by two units. The NNbarX experiment proposes to search for neutron-antineutron oscillations to increase the sensitivity to this process by three orders of magnitude.

- *Electric dipole moments:* Besides the phase in the weak interactions of quarks, potential sources of CP non-conservation include the strong CP phase θ_{QCD} , and the phase(s) in the weak

interactions of neutrinos. Many extensions of the Standard Model, including Supersymmetry, have additional sources of CP non-conservation. Among the most powerful probes of new physics that does not conserve CP are the electric dipole moments (edm's) of the neutron, electron and proton. Searches for the edm's of neutrons and electrons are already sensitive to contributions from new particle masses at the 10–100 TeV scale, with substantial improvements in reach expected over the next decade. A new direct neutron edm experiment is planned at Oak Ridge National Laboratory. At Fermilab, a direct measurement of the electric dipole moment of the proton, with sensitivity three orders of magnitude better than the present limit, may be possible. The experiment would be sensitive to contributions from new particles with masses well in excess of 100 TeV.

Looking farther into the future, progress in precision physics and rare processes will be shaped partly by what particle physicists learn in the coming decade. Upgrades to the accelerator complex at Fermilab (PIP-II and additional improvements) will offer opportunities to further this program. For example, combined with modest upgrades to Mu2e, improvements in the Fermilab accelerator complex potentially could provide increased sensitivity (by a factor of ten) to muon-to-electron conversion and allow one to search for this very rare process in different nuclei. This will provide crucial clues on the nature of the new physics revealed in the event of an observation in the next-generation experiments.

Cosmic Particles

A suite of experiments is observing extra-terrestrial gamma rays, neutrinos, and cosmic rays produced by cosmic accelerators. Interestingly, their sources have not all been identified, and how they accelerate particles to the very large energies observed is still a matter of debate. Although delivering the highest energy photons (tens of TeV), neutrinos ($>10^3$ TeV), and hadrons ($>10^8$ TeV), the main scientific interest in these experiments is currently more astrophysics than particle physics because no new particle physics is required to explain them.

There is a set of cosmic-particles projects, however, that are of clear significance for particle physics: the searches for ultra-high energy cosmic neutrinos. A new generation of radio detection

experiments is under development to detect ultra-high-energy neutrinos with energies up to 10^7 TeV. Using directional information these experiments will be able to determine the interaction strengths of neutrinos at center of mass collision energies around 10 TeV, probing a new regime of weak interaction physics at energies inaccessible to current or planned future colliders. Leading tests of fundamental physical principles have been performed by neutrino and gamma-ray experiments. Cosmic particle detectors, including CTA, PINGU, and General Antiparticle Spectrometer (GAPS), also have sensitivity to indirect dark matter signals.

Low-mass Particles

Current data still allow the existence of new, light particles that couple only very weakly to ordinary matter. These “hidden sector” possibilities include axions, axion-like particles, hidden-sector photons, millicharged particles, and other exotica, some of which have independent physical motivations. Significant regions of physically viable parameter space for many of these particles can be searched with relatively modest-scale experiments. For example, “dark photons” (new gauge bosons that have small “kinetic mixing” with photons, resulting in very weak interactions with charged particles) will have distinctive kinematic signatures in high-intensity electron beam dump experiments at Jefferson Lab and can also be searched for in accelerator-based neutrino experiments.

3.6: Enabling R&D and Computing

Accelerators that provide the required particle beams, instrumentation that reveals the particle interactions, and computing that supports both the machines and the physics experiments, lie at the foundation of particle physics. These areas—accelerators, instrumentation, and computing—together with the theoretical predictions, enable research and connect particle physics with other fields of science and with society. To address the science Drivers, increasing demands for higher performance at lower cost are being placed on all three areas. This necessitates the ongoing pursuit of innovation.

Innovation relies on continued long-range investments in basic R&D and in the training of students and postdocs. These require, among other things, state-of-the-art facilities at the national laboratories and universities, support for researchers to use them effectively, and the creation of opportunities that attract young talent.

Accelerator Research

The future of particle physics depends critically on transformational accelerator R&D to enable new capabilities and to advance existing technologies at lower cost. The program is driven by the physics goals, but future physics opportunities will be determined by what is made possible. Currently, the U.S. has a world-leading program in the critical technologies of superconducting radio frequency (SCRF) cavities, superconducting magnets, and high-power targets, which are required for the next generation of accelerators. The U.S. has unique facilities and has made great progress in the development of advanced accelerators thanks to strategic investments at national laboratories and several universities.

The DOE office of High Energy Physics (HEP) sponsors both the General Accelerator R&D (GARD) program and more directed R&D aimed at a specific accelerator or technology. The GARD program ranges from accelerator theory and computation to advanced technology development, including high-field and high-temperature superconductors, SCRF, and novel acceleration ultra-high gradient techniques. GARD tends to support relatively small university and laboratory efforts and larger lab-based projects of a scale of \$10M. Both GARD and

the projects have time horizons ranging from almost immediate (*e.g.*, LHC Accelerator Research Program [LARP], which provides essential R&D for the current LHC and for the HL-LHC) to efforts with scales longer than an individual career. GARD programs typically have broad applicability, including light sources, nuclear physics, and medical and industrial accelerators. Recently, a DOE Stewardship program was formed specifically to support topics with broad applicability.

Universities play a unique role in advancing accelerator science. NSF has recently launched a program in accelerator science “to seed and support fundamental accelerator science at universities as an academic discipline... [and to support the] training of the next generation of accelerator scientists, including students, postdoctoral researchers, and junior faculty, who will lead innovations in the field and will form the backbone of the nation’s highly trained accelerator workforce.” This program enables universities to advance accelerator science and technology by leveraging their multidisciplinary expertise and infrastructure.

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.

General Accelerator R&D programs

The GARD and Stewardship programs enable a comprehensive set of efforts supporting current operating accelerators, developing well-understood technologies for mid-term accelerators, and creating novel acceleration concepts. For example, GARD thrusts in normal and superconducting RF acceleration are relevant to the Linac Coherent Light Source II (LCLS-II), PIP-II, ILC and other industrial and medical accelerators, while the unique position that the U.S. holds in high field superconducting

magnets supports the HL-LHC upgrade and future pp colliders. High power targets, as noted at Snowmass, will address critical needs and should be included in GARD. HEP also supports test facilities and projects that conduct work more focused on particular future accelerators.

There is a critical need for technical breakthroughs that will yield more cost-effective accelerators. For example, ultrahigh gradient accelerator techniques will require the development of power sources (RF, lasers, and electron beam drivers) compatible with high average power and high wall plug efficiency, and accelerating structures (plasmas, metallic, and dielectric) that can sustain high average power, have high damage threshold, and can be cascaded. Engagement of the national laboratories, universities, and industry will be essential for comprehensive R&D to meet these challenges. Advancing these technologies will benefit many other areas of science and technology.

The effectiveness of the HEP programs can be improved by strengthening and making more explicit the linkages between the GARD programs and specific transformative outcomes. When applicable, justifications for research directions should include, and be evaluated on, specific target applications relevant to HEP (including high-level accelerator parameters) through a coordinated national and international effort. GARD contains several core competencies and critical efforts that match the spirit of the Stewardship program and could be pursued through that program or other sources.

Directed Accelerator R&D

The U.S. LHC Accelerator Research Program (**LARP**) funds R&D aimed at the LHC, with activities principally at FNAL, BNL, LBNL, and SLAC. LARP includes SC magnet R&D, a variety of beam physics activities, and the Toohig Fellowship program. Most LARP research is currently aimed at the HL-LHC upgrade and includes Nb_3Sn low-beta interaction quadrupoles, crab cavities, and a high bandwidth feedback system.

The Proton Improvement Plan-II (**PIP-II**) will increase Fermilab's capabilities to deliver 1.2 MW average beam power to the Long-Baseline Neutrino Facility production target, simultaneously establishing a platform for subsequent upgrades to multi-MW capability. The central PIP-II element is a new 800

MeV superconducting linac operated at low duty factor but constructed to be capable of continuous operation. Upgrades to a number of systems in the Booster, Recycler, and Main Injector will also be required. Power upgrades beyond those envisioned for PIP-II will require R&D for high average power proton linacs and target systems.

It is appropriate for the PIP-II effort to be supported partially by temporary redirection of GARD funding of SCRF R&D and facilities at Fermilab.

FACET-II would be an upgrade to the SLAC user test facility FACET for beam-driven plasma-based wakefield acceleration of electrons—and, uniquely, positrons—that has attracted proposals from more than 50 users from across the accelerator community. **BELLA-II** would be a demonstrator facility at LBNL for high average power operation of laser and laser plasma accelerator technology in the kW-class at kHz repetition rate.

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 $\sin^2(2\theta_{13})$ extends the time frame for when neutrino factories might be needed, and the small Higgs mass with positive implications for its study at more technically ready e^+e^- colliders reduces the near-term necessity of muon colliders.

The U.S. has played a leadership role in the design and construction of novel advanced technological systems for the International Linear Collider (**ILC**). While high-level discussions between the U.S., Japan, and global partners are unfolding, pre-project funding would help convert the Technical Design Report (TDR) to a site-specific design incorporating design improvements and will enable re-engagement of the U.S. community.

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 practical. The future-generation accelerators are discussed in [Section 2](#).

Instrumentation R&D

The particle physics detector community has historically been an important contributor to broadly applicable innovation in instrumentation. A recent example is the key role of ultra-sensitive transition edge bolometers in CMB experiments. A rich spectrum of challenging physics experiments is planned that requires advances in instrumentation, with increasing requirements on sensitivity and performance. It is only through investments in the development of advanced, cost-effective new technologies that science goals can be met. 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.

Computing

Computing cuts across all activities in particle physics, and these activities spur innovation in computing. The field played leading roles in developing and using high-throughput and distributed/grid computing, online (real-time and near-real-time) data processing, high-performance computing, high-performance networking, large-scale data storage, large-scale data management and analysis, and the World Wide Web. Particle physics projects successfully manage software development and operations on a global scale.

Computing in particle physics continues to evolve based on needs and opportunities. For example, the use of high-performance computing, combined with new algorithms, is advancing full 3-D simulations at realistic beam intensities of nearly all types of accelerators. This will enable “virtual prototyping” of accelerator components on a larger scale than is currently possible. The physics data from the LHC experiments stresses both computing infrastructure and expertise, and the LHC operations in the next decades will likely result in order-of-magnitude increases in data volume and analysis complexity. Experiments exploring the cosmos will greatly extend their data needs as vast new surveys and high-throughput instruments come on line. Cosmological computing is making significant progress in connecting fundamental physics with the

structure and evolution of the Universe at the necessary level of detail. Theory computations will continue to increase in importance, as higher fidelity modeling will be required to understand the data.

Benefits and Broader Impacts

4



Particle physics shares with other basic sciences the need to innovate, invent, and develop technologies to carry out its mission to explore the nature of matter, energy, space and time. 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.⁶ This broad, connected scientific enterprise provides tremendous benefits to society as a whole.

The particle physics effort to advance knowledge is most significantly exemplified by the drive for sophisticated accelerator technology to create state-of-the-art high-energy particle beams and their associated experiments. Beyond advancing the technology for particle physics, this dedicated research and development has enabled basic scientific research and applications in areas beyond high-energy physics, including materials science, medical imaging and therapy, and national security. New facilities, such as advanced light sources, are enabled because of the decades of development by particle physics. Some technologies advanced by particle physics have opened up commercial markets that now play a significant role in the U.S. economy and fabric of life.

The web of connections between the tools and techniques of particle physics, other fields of science, industry, and society is extensive. Particle physics' role in the creation of the World Wide Web has been called out in a report from the President's Council of Advisors on Science & Technology (PCAST). The development of distributed "grid" computing technology is a response to the need for increased data analysis power that can be made available by accessing remote computer installations in widely dispersed institutes. By launching grids in Europe and the U.S. to support the LHC, particle physics demonstrated the capability to unite globally distributed computing resources into a single coordinated computing service. These grids are now used by a wide spectrum of sciences ranging from archeology and astronomy to computational chemistry and materials science.

The particle physics and nuclear physics communities studying quantum chromodynamics using numerical lattice algorithms have been avid users of advanced computing platforms and major drivers and contributors to early developments in the supercomputing industry. Their connections with advanced computing continue today with collaborations of both particle and nuclear physics researchers with industry. Sophisticated simulation packages developed by the particle physics community are being used in many areas, including radiation therapy, medical imaging, nuclear physics, accelerator modeling, materials science, and aerospace.

Theoretical and mathematical techniques developed in particle physics have found applications in other scientific fields, and *vice versa*, most notably those that employ similar mathematical language and focus on quantum mechanics, quantum field theory, and renormalization group flows. Those research fields, including atomic, molecular and optical physics, astrophysics, condensed matter physics, nuclear physics, and quantum information science, enjoy a particularly close relation with particle theory. Condensed matter physics, for example, intrinsically involves physical lattices and exhibits a rich variety of non-perturbative phenomena, whose properties are often controlled by gauge symmetries. Science has been enriched through the exchange of theoretical advances and computational techniques between particle physics theory and connected disciplines.⁷

Instrumentation developed to enable particle physics experiments has sometimes been enthusiastically adopted in other fields. For example, semiconductor based charged particle track detection technology from collider experiments has become a key tool at synchrotron radiation and FEL facilities; new crystal growth methods developed for particle detectors later found use in a large commercial market for these crystals in medical imaging; and the application of core particle physics techniques led to the discovery of new retinal functions of the eye. These same techniques are currently being used more generally in neuroscience. These adaptations sometimes have broad cultural importance. For example, optical methods for the accurate placement of silicon detectors designed and built in the U.S. for LHC experiments were recently adapted to enable non-destructive playback of the earliest audio recordings,

Further information can be obtained in the *Tools, Techniques, and Technology Connections of Particle Physics* report: <http://science.energy.gov/hep/news-and-resources/reports/>

Further information can be obtained in the March 14, 2014, presentation to HEPAP, *Connections of Particle Physics with Other Disciplines*, by C. Callan and S. Kachru: <http://science.energy.gov/hep/hepap/meetings/>

bringing back to life a treasure trove of sounds and voices from that pioneering period in the late 19th century.

Particle physics also provides facilities used by a diverse community. Environmental scientists use particle beams in climate studies, and geoscientists gather valuable data using neutrino detectors. In some cases, unlikely connections have been established through the construction of particle physics facilities. For example, the deployment of photodetectors at a depth of 2500 m in the ice of the Antarctic for the study of neutrinos provided the most clearly-resolved measurements of Antarctic dust strata during the last glacial period and are being used to reconstruct paleo-climate records in exceptional detail. The particle physics community also developed and operates cameras, such as the Dark Energy Camera and the Sloan Digital Sky Survey, that serve the broader particle-astronomy community. Muon spin spectroscopy, a very sensitive probe for condensed matter studies, is made possible through the accelerator-based production of muon beams.

Accelerators are critical to many areas beyond their traditional role in particle physics and they influence our lives in many ways. In recognition of particle physics contributions to technology, the DOE Office of Science Accelerator R&D Stewardship program was recently initiated to serve as a catalyst in transitioning accelerator technologies from particle physics to applications that will create benefits for science and society at large. Modern medicine is a prime beneficiary of particle physics research, using accelerators to diagnose and treat patients. Proton therapy centers provide increasing access to conformal radiation therapy, and ion beam technology is anticipated to allow significant advances in patient treatment. In addition, research and development investments by particle physics in superconducting radio frequency cavities has led to their planned use in advanced light sources, enabling a broad spectrum of studies in an extensive range of disciplines.

Many of the developments noted above have been informed by other science and technology advances, strengthening research connections and productivity. Understanding and expanding these connections will enable the particle physics community to continue to discover innovative experimental pathways and create novel detector devices to explore the

Universe and, if history is a guide, have impacts on society beyond our direct science mission.

Appendices

Appendix A: Charge



U.S. Department of Energy
and the
National Science Foundation



Professor Andrew Lankford
Chair, HEPAP
University of California at Irvine
4129H Frederick Reines Hall
Irvine, CA 92697

Dear Professor Lankford:

Much has changed since the last long-range planning document for high energy physics was endorsed by HEPAP (the Particle Physics Project Prioritization Panel (P5) report, submitted in 2008). It is therefore an opportune time to revisit this guidance to the DOE and the NSF. To that end, we ask that you constitute a new P5 panel to develop an updated strategic plan for U.S. high energy physics that can be executed over a 10 year timescale, in the context of a 20-year global vision for the field.

In developing this plan, we would like you to take account of two particularly relevant considerations. First, there is a need to understand the priorities, options, impacts and scientific deliverables for the U.S. program under more stringent budgets than were considered by the previous P5 panel. Second, the recent discovery of what appears to be the long-sought Standard Model Higgs boson and the observation of mixing between all three known neutrino types at unexpectedly large rates have opened up the possibility of new experiments and facilities that can address key scientific questions about the fundamental nature of the universe in new and incisive ways. Other factors that should inform development of the plan include a fuller understanding of the nature of the physics to be explored at the Large Hadron Collider (LHC), and the global coordination required to realize proposed major new scientific facilities.

To better understand this picture, we request an assessment of the current and future scientific opportunities over the next 20 year period. In addition, we request a critical examination of the investments that would be needed to ensure the vitality, scientific productivity, and discovery potential of U.S. high energy physics research during this timeframe. Specifically, we request that HEPAP examine current, planned, and proposed U.S. research capabilities and assess their role and potential for scientific advancement; assess their uniqueness and relative scientific impact in the international context; and estimate the time and resources (the facilities, personnel, research and development and capital investments) needed to achieve their goals. In developing its recommendations, the committee should consider the budgetary constraints indicated below, as well as the technical readiness and feasibility of these efforts. We also request that HEPAP consider the appropriate balance of small, mid-scale, and large experiments and identify, where possible, multiple or complementary pathways to address the important scientific questions. We expect the "Snowmass" reports and the previous HEPAP study of future

facilities will be useful inputs, and that you will make efforts to maximize community input and participation in your process.

Your evaluation should examine the need to maintain a healthy and flexible domestic infrastructure so that the U.S. high-energy physics program can deliver science results regularly throughout the coming decade. Your report should include an explicit discussion of the extent to which it is necessary to construct, maintain and/or upgrade leading domestic HEP facilities in order to maintain a leadership position in this global scientific effort, while at the same time maintaining a healthy balance that preserves essential roles and contributions for national laboratories and universities and enables opportunities for global coordination of large initiatives.

Your report should provide recommendations on the priorities for an optimized high energy physics program over the next ten years (FY 2014-2023), under the following three scenarios:

- a constant level of funding for three years, followed by increases of 2.0% per year with respect to the appropriated FY 2013 budget for HEP; and
- a constant level of funding for three years, followed by increases of 3.0% per year with respect to the FY 2014 President's Budget Request for HEP; and
- unconstrained budget. For this scenario, please list, in priority order, specific activities, beyond those mentioned in the previous budget scenario, that are needed to mount a leadership program addressing the scientific opportunities identified by the research community.

You should consider these scenarios not as literal budget guidance but as an opportunity to identify priorities and make high-level recommendations. The programs you recommend should be (to some significant extent) implementable under reasonable assumptions. At the same time the budget scenarios should not drive the prioritization to the degree that projects are promoted solely for their ability to fit within an assumed profile.

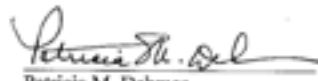
The report should articulate the scientific opportunities which can and cannot be pursued and the approximate overall level of support that is needed in the HEP core research and advanced technology R&D programs to achieve these opportunities in the various scenarios.

The report should also provide a detailed perspective on whether and how the pursuit of possible major international partnerships (such as LHC upgrades, Japanese-hosted ILC, LBNE, etc.) might fit into the program you recommend in each of the scenarios. Given the long timescales for such major initiatives, we expect the funding required to enable the priorities you identify may well extend past the next 10 years, but any new projects recommended should be technically and fiscally plausible to execute in a 20 year time frame.


Finally, effective communications about the excitement, impact and vitality of high-energy physics that can be shared with non-scientific audiences will be critical in making the case for the new strategic plan. We would find it useful if your report can update the discussion of the scientific questions that drive the field, in a manner that is accessible to non-specialists (e.g., a science discussion at the level of the *Quantum Universe* report); and also crisply articulate the value of basic research and the broader impacts of high-energy physics on other sciences and on society, including the impacts of training of particle and accelerator physicists.

We would appreciate the committee's preliminary comments by March 1, 2014 and a final report by May 1, 2014. We understand this is a difficult task; however your considerations on these issues will be an essential input to planning at both the DOE and NSF.

Sincerely,



Patricia M. Dehmer
Acting Director, Office of Science
U.S. Department of Energy



Dr. F. Fleming Crim
Assistant Director
Directorate for Mathematical and
Physical Sciences
National Science Foundation

Appendix B: 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

Appendix C: Process and Meetings

The P5 process had several components, all of which were designed with community engagement in mind:

- A website was maintained, with information, frequent news, meetings, and a submissions portal with a public archive. <http://interactions.org/p5>
- There were three large public meetings, whose agendas are appended. All talks are posted online.
- There were three physical town halls and three virtual town halls. The virtual town halls were particularly effective for hearing younger voices.
- A special effort was made to reach out to younger colleagues, with emails to Snowmass Young mailing lists and to PIs urging them to inform their students and post-docs about the process, and a Twitter feed.

Each of the major activities considered was given a standard form to fill in, with cost profiles and FTE estimates for each phase of the project (R&D, construction, operations), separated by funding agency, along with information about project level of maturity, contingency, etc. From these, and agency inputs, detailed spreadsheets were developed and used to support the budget exercises.

The panel worked by consensus. There were full-panel phone calls approximately weekly throughout the process, as well as many subgroups to work on tasks in parallel. The panel had additional face-to-face meetings on the following dates in 2014: 12–14 January, 21–24 February, 5–8 April, and 29–30 April. At most meetings, there were sessions without agency personnel in the room.

There were HEPAP presentations and discussions in September 2013, December 2013, March 2014, and May 2014. Preliminary comments were presented and discussed at the March meeting, and the Report was presented, discussed, and approved at the May 2014 HEPAP meeting.

The strategic plan and recommendations contained in this report, after adoption by HEPAP, are advisory input to the Department of Energy and the National Science Foundation. The actual design and implementation of any plan in these agencies is the responsibility of program management.

The following members of the scientific community served as peer reviewers:

Sally Dawson
Brookhaven National Laboratory

Persis Drell
SLAC National Accelerator Laboratory

David Gross
University of California, Santa Barbara

Klaus Honscheid
Ohio State University

Boris Kayser
Fermi National Accelerator Laboratory

Josh Klein
University of Pennsylvania

Dan Marlow
Princeton University

Michael Maloney
National Academy of Sciences

David Schmitz
University of Chicago

Abe Seiden
University of California, Santa Cruz

Marjorie Shapiro
University of California, Berkeley

We thank them very much for their thoughtful comments, which were provided on a tight schedule.

Our report benefited enormously from professional editing help by Jim Dawson. AAAS Science & Technology Policy Fellow Michael Cooke was very helpful in numerous aspects of press and outreach.

Agency staff and contractors were ever-responsive to logistical requests. We especially thank Christie Ashton, Donna Nevels, Linda Severs, and Kathy Yarmas. National lab staff also solved myriad logistical problems—they made it look easy, and we know it was not!

P5 Workshop on the Future of High Energy Physics #1

Fermi National Accelerator Laboratory, Nov. 2–4, 2013
<https://indico.fnal.gov/conferenceOtherViews.py?confId=7485>

Snowmass inputs and discussion

Energy Frontier

Michael Peskin
SLAC National Accelerator Laboratory

Intensity Frontier

Hendrik Weerts
Argonne National Laboratory

Cosmic Frontier

Jonathan Feng
University of California, Irvine

Capabilities Frontier

William Barletta
Massachusetts Institute of Technology

Instrumentation Frontier

Ronald Lipton
Fermi National Accelerator Laboratory

Computing Frontier

Lothar Bauerdick
Fermi National Accelerator Laboratory

Education and Outreach

Marjorie Bardeen
Fermi National Accelerator Laboratory

International context talks

Accelerator-based Particle Physics Program in Europe

Rolf Heuer
CERN

Particle Physics Program in Japan

Atsuto Suzuki
KEK

Strategic Planning for the Particle Physics Program in China

Yifang Wang
IHEP

Accelerator-based neutrino program

LBNE Science and International Collaboration

Robert Wilson
Colorado State University

LBNE Beam Planning and Optimization

Vaia Papadimitriou
Fermi National Accelerator Laboratory

LBNE Project Cost, Scope, Schedule

James Strait
Fermi National Accelerator Laboratory

Hyper-K

Masato Shiozawa
University of Tokyo

Tsuyoshi NAKAYA
Kyoto University

DAEδALUS and IsoDAR

Mike Shaevitz
Columbia University

Additional Water Cherenkov

Robert Svoboda
University of California, Davis

Additional Interim Accelerator-based Neutrino Projects

David Schmitz
University of Chicago

NuSTORM

Alan Bross
Fermi National Accelerator Laboratory

Non-accelerator neutrino program

Overview of Future Reactor Experiments

Karsten Heeger
Yale University

Overview of Future Neutrino Mass and Characteristics Experiments

Hamish Robertson
University of Washington

Town Hall

P5 Workshop on the Future of High Energy Physics #2

SLAC National Accelerator Laboratory, Dec. 2–4, 2013
<https://indico.bnl.gov/conferenceDisplay.py?confId=688>

Dark Matter

Dark Matter Overview

Tim Tait
University of California, Irvine

Direct WIMP Detection

Harry Nelson
University of California, Santa Barbara

CTA

David Williams
University of California, Santa Cruz

PINGU

Doug Cowen
Pennsylvania State University

Theory

Theory Study

Michael Dine
University of California, Santa Cruz

Computing

Topical Panel on DOE HEP Computing

Salman Habib
Argonne National Laboratory

Science Connections

Science Connections Panel

Shamit Kachru
Stanford University

Curt Callen
Princeton University

Town Hall

International Context: Astroparticle Physics Planning in Europe

Particle Astrophysics Planning in Europe

Stavros Katsanevas
U. Paris VII Denis-Diderot and ApPEC

Cosmic Surveys: Dark Energy and CMB

Particle Physics from Cosmic Surveys: Overview of Opportunities

Klaus Honscheid
Ohio State University

LSST

Bhuv Jain
University of Pennsylvania

DESI

Michael Levi
Lawrence Berkeley National Laboratory

CMB

John Carlstrom
University of Chicago

HE Cosmic Particles and Additional Topics

Cosmic Particles Overview of Opportunities

Jordan Goodman
University of Maryland

P5 Workshop on the Future of High Energy Physics #3

Brookhaven National Laboratory, Dec. 15–18, 2013
<https://indico.bnl.gov/conferenceDisplay.py?confId=680>

LHC Upgrades

Introduction to HL-LHC

Beate Heinemann

*University of California, Berkeley
and Lawrence Berkeley National Laboratory*

Joseph Incandela

University of California, Santa Barbara

Plans for LHC Accelerator Upgrades

Giorgio Apollinari

Fermi National Accelerator Laboratory

CMS/US CMS Detector Upgrade Plans

Jeff Spalding

Fermi National Accelerator Laboratory

ATLAS/US ATLAS Detector Upgrade Plans

Hal Evans

Indiana University

ILC

Overview, Physics and Detectors

Jonathan Bagger

Johns Hopkins University

ILC the Machine

Mike Harrison

Brookhaven National Laboratory

Fermilab Proton Accelerator Complex and Opportunities Overview

Fermilab Proton Accelerator Complex and Opportunities

Steve Holmes

Fermi National Accelerator Laboratory

Overview of Physics Opportunities with High-intensity Proton Beams

Andreas Kronfeld

Fermi National Accelerator Laboratory

Proton-driven Rare Process/Precision Experiments

ORKA

Doug Bryman

University of British Columbia

NNbarX

Yuri Kamyshkov

University of Tennessee

Muon Campus

Chris Polly

Fermi National Accelerator Laboratory

g-2

David Hertzog

University of Washington

Mu2e

Ron Ray

Fermi National Accelerator Laboratory

COMET

Yoshitaka Kuno

Osaka University

Proton EDM

Yannis Semertzidis

CAPP/IBS

Young Physicists Forum

HE Vision Machines

Overview of Physics Opportunities at Very High Energy Machines

Sally Dawson

Brookhaven National Laboratory

The Global FCC Effort

Michael Benedikt

CERN

Muon Collider and Neutrino Factory

Mark Palmer

*Fermi National Accelerator Laboratory***CLIC**

Steiner Stapnes

*CERN and University of Oslo***Town Hall****Accelerator R&D****Superconducting RF and Normal Conducting RF**

Hasan Padamsee

*Cornell University***High-field SC Magnets**

David Larbalestier

*Florida State University***High-power Targets**

P. Hurh

*Fermi National Accelerator Laboratory***Plasma and Dielectrics**

Jean-Pierre Delahaye

*SLAC National Accelerator Laboratory***Instrumentation R&D****Report from the Technology Connections Panel**

Marcel Demarteau

*Argonne National Laboratory***Additional Topics****LHCb and Belle II**

Hassan Jawahery

University of Maryland

Appendix D: Snowmass Questions

How do we understand the Higgs boson? What principle determines its couplings to quarks and leptons? Why does it condense and acquire a vacuum value throughout the Universe? Is there one Higgs particle or many? Is the Higgs particle elementary or composite?

What principle determines the masses and mixings of quarks and leptons? Why is the mixing pattern apparently different for quarks and leptons? Why is there CP violation in quark mixing? Do leptons violate CP?

Why are neutrinos so light compared to other matter particles? Are neutrinos their own antiparticles? Are their small masses connected to the presence of a very high mass scale? Are there new interactions that are invisible except through their role in neutrino physics?

What mechanism produced the excess of matter over antimatter that we see in the Universe? Why are the interactions of particles and antiparticles not exactly mirror opposites?

Dark matter is the dominant component of mass in the Universe. What is the dark matter made of? Is it composed of one type of new particle or several? What principle determined the current density of dark matter in the Universe? Are the dark matter particles connected to the particles of the Standard Model, or are they part of an entirely new dark sector of particles?

What is dark energy? Is it a static energy per unit volume of the vacuum, or is it dynamical and evolving with the Universe? What principle determines its value?

What did the Universe look like in its earliest moments, and how did it evolve to contain the structures we observe today? The inflationary Universe model requires new fields active in the early Universe. Where did these come from, and how can we probe them today?

Are there additional forces that we have not yet observed? Are there additional quantum numbers associated with new fundamental symmetries? Are the four known forces unified at very short distances? What principles are involved in this unification?

Are there new particles at the TeV energy scale? Such particles are motivated by the problem of the Higgs boson, and by ideas about space-time symmetry such as supersymmetry and extra dimensions. If they exist, how do they acquire mass, and what is their mass spectrum? Do they provide new sources of quark and lepton mixing and CP violation?

Are there new particles that are light and extremely weakly interacting? Such particles are motivated by many issues, including the strong CP problem, dark matter, dark energy, inflation, and attempts to unify the microscopic forces with gravity. What experiments can be used to find evidence for these particles?

Are there extremely massive particles to which we can only couple indirectly at currently accessible energies? Examples of such particles are seesaw heavy neutrinos or grand unified scale particles mediating proton decay. How can we demonstrate that these particles exist?

Appendix E: 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. ([p. 8](#))

Recommendation 2: Pursue a program to address the five science Drivers. ([p. 8](#))

Recommendation 3: Develop a mechanism to reassess the project priority at critical decision stages if costs and/or capabilities change substantively. ([p. 8](#))

Recommendation 4: Maintain a program of projects of all scales, from the largest international projects to mid- and small-scale projects. ([p. 8](#))

Recommendation 5: Increase the budget fraction invested in construction of projects to the 20%–25% range. ([p. 8](#))

Recommendation 6: In addition to reaping timely science from projects, the research program should provide the flexibility to support new ideas and developments. ([p. 9](#))

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. ([p. 9](#))

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. ([p. 9](#))

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. ([p. 10](#))

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. ([p. 10](#))

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. ([p. 11](#))

Recommendation 12: In collaboration with international partners, develop a coherent short- and long-baseline neutrino program hosted at Fermilab. ([p. 11](#))

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. ([p. 12](#))

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. ([p. 12](#))

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. ([p. 12](#))

Recommendation 16: Build DESI as a major step forward in dark energy science, if funding permits (see Scenarios discussion below). ([p. 13](#))

Recommendation 17: Complete LSST as planned. ([p. 14](#))

Recommendation 18: Support CMB experiments as part of the core particle physics program. The multidisciplinary nature of the science warrants continued multiagency support. ([p. 14](#))

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. ([p. 14](#))

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. ([p. 14](#))

Recommendation 21: Invest in CTA as part of the small projects portfolio if the critical NSF Astronomy funding can be obtained. ([p. 15](#))

Recommendation 22: Complete the Mu2e and muon g-2 projects. ([p. 15](#))

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. ([p. 19](#))

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. ([p. 20](#))

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. ([p. 20](#))

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. ([p. 20](#))

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. ([p. 20](#))

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. ([p. 21](#))

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. ([p. 21](#))

Credit

Michael Branigan and his crew at Sandbox Studio, Chicago worked swiftly and expertly on the art direction, design and typography. Anastasia Kozhevnikova created the beautiful, abstract illustrations for the chapters dividers and science Drivers.

HEPAP P5 Report Transmittal Letter

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May 28, 2014

Dr. Patricia M. Dehmer
Acting Director, Office of Science
U.S. Department of Energy

Dr. F. Fleming Crim
Assistant Director
Directorate for Mathematical and Physical Sciences
National Science Foundation

Dear Dr. Dehmer and Dr. Crim:

The Report of the Particle Physics Project Prioritization Panel, "Building for Discovery: Strategic Plan for U.S. Particle Physics in the Global Context", was presented to HEPAP at its meeting on May 22, 2014. This report addresses your charge "to develop an updated strategic plan for U.S. high energy physics that can be executed over a 10 year timescale, in the context of a 20-year global vision for the field." At the meeting, P5 Chair Steve Ritz reviewed the report and its recommendations, and responded to questions from HEPAP members.

Following discussion and deliberation, HEPAP unanimously approved the P5 report. HEPAP also separately approved each of the report's recommendations. HEPAP members commended P5 for the success with which, starting with the input of the particle physics community, it developed a strategic plan for the field. They also noted the quality of the report in addressing the charge, and expressed their deep appreciation to the members of P5 for the enormous effort that subpanel devoted to this process. In conclusion, HEPAP voiced its strong endorsement of the strategic plan presented by the P5 report, and supports its immediate implementation.

With this letter, on behalf of HEPAP, I respectfully submit for your consideration the final report of P5.

Sincerely yours,

Andrew J. Lankford
Chair, High Energy Physics Advisory Panel

On behalf of the members of HEPAP:

Ursula Bassler
Mary Bishai
Ilan Ben-Zvi
James Buckley
Bruce Carlsten
John Carlstrom
Mirjam Cvetič
Robin Erbacher

Karsten Heeger
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Murdoch Gilchriese

Tao Han
Leslie Rosenberg
Gabriella Sciolla
Ian Shipsey
Thomas Shutt
Paul Steinhardt
Robert Tschirhart

Conflict of Interest Resolution During HEPAP Approval Process

In promulgating this report to the Department of Energy and the National Science Foundation, HEPAP recognizes that there are particular recommendations that could affect the interests of several organizations and facilities engaged in particle physics research. We further recognize that some of the members of HEPAP are members of those identified organizations or work at those facilities and have interests which could be affected by the recommendations that are being forwarded. Prior to the review of and voting on this report, and in accordance with advice from the DOE General Counsel's office, those individuals have been identified and recused from participating in discussions associated with their home organizations or from voting on recommendations associated with those institutions, as follows:

For Recommendations 12-15, Prof. Gerber, Dr. McBride, and Dr. Tschirhart did not participate in discussions or voting on these recommendations;

For Recommendation 16, Dr. Gilchriese and Dr. Ligeti did not participate in discussions or voting on this recommendation;

For Recommendation 18, Prof. Carlstrom did not participate in discussions or voting on this recommendation;

For Recommendation 19, Dr. Gilchriese, Prof. Rosenberg, and Prof. Shutt did not participate in discussions or voting on this recommendation;

For Recommendation 21, Prof. Buckley did not participate in discussions or voting on this recommendation;

For Recommendation 22, Prof. Gerber, Dr. McBride, and Dr. Tschirhart did not participate in discussions or voting on this recommendation;

For Recommendation 25, Dr. Bishai, Dr. Ben-Zvi, Prof. Gerber, Dr. McBride, and Dr. Tschirhart did not participate in discussions or voting on this recommendation.



Report of the Particle
Physics Project
Prioritization Panel (P5)



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