

**Testimony of**

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Chairman Lummis, Ranking Member Swalwell, and Members of the Subcommittee, thank you for inviting me to this important hearing. Particle physicists have come together to make a recommended plan that is driven by the science and meets tight fiscal constraints. The plan enables leadership by the United States in the global context, resolves key issues for the field, and envisions a continuous flow of exciting and important results while making essential investments in the future. I had the privilege of chairing the Particle Physics Project Prioritization Panel (P5), which developed and articulated this plan, and I am grateful for the opportunity to discuss with you our process and results. Much of the text below comes from our report, which HEPAP (the FACA panel advising the DOE and NSF) considered carefully and voted unanimously to approve on 22 May 2014.

As you know, particle physics explores the fundamental constituents of matter and energy, revealing profound connections underlying everything we see, including the smallest and largest structures in the Universe. The field is highly successful: investments have been rewarded recently with the discoveries of the heaviest elementary particle (the top quark), the tiny masses of neutrinos, the accelerating expansion of the Universe, and the Higgs boson. Since 2008, three Nobel Prizes related to particle physics were awarded. 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 these reasons, and more, research in particle physics inspires young people to engage with science.

Particle physics is global. The countries and regions that lead the field 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. Addressing 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 is within reach of a cooperative global program. The U.S. 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 and clearly defined roles and responsibilities are essential for success. Building international cooperation is an important theme of our report, and this perspective is finding worldwide resonance in an intensely competitive field.

The field has a vibrant, entrepreneurial spirit, with great ideas for excellent new projects, but these far exceed what can be executed with currently available resources. Tough choices were required. Our panel understood that an important part of our job was to recommend ways for the U.S. to invest purposefully in areas that have the biggest impacts and that make most efficient use of limited resources.

Since the 2008 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, with ten-year profiles reflecting current fiscal realities:

A: FY2013 budget baseline flat for three years, then escalating at 2% per year

B: FY2014 President’s budget request baseline flat for three years, then escalating at 3% per year

as well as for an unconstrained Scenario C. As the Charge states, these were considered “...not as literal budget guidance, but as an opportunity to identify priorities and make high-level recommendations.”

We started with the science. A yearlong community-wide study, called “Snowmass”, preceded the formation of our new P5. A vast number of scientific opportunities were investigated, discussed, and summarized in Snowmass reports. Based on this comprehensive work by the broad community, we 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 solve other problems in particle physics 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 there are good reasons to suspect that these deeper connections exist. Indeed, discovering those deep connections is a primary goal of the field. 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 limited set of experiments—their approximate timescales and how they fit together—is given in the report. What is learned at each step will inform the next steps.

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 an explicit set of selection 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 could move forward immediately with a prioritized and time-ordered recommended program, which is summarized in the report 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, would be fully exploited. The U.S. continues to play essential roles in LHC construction, operations, and physics analysis, and U.S. scientists have very visible leadership roles. As in the past, the provided hardware would be designed and built in the U.S.
- The U.S. would host a world-leading neutrino program with an optimized set of short- and long-baseline neutrino oscillation experiments. The long-term focus of the program would be the Long Baseline Neutrino Facility (LBNF). The Proton Improvement Plan-II (PIP-II) project at Fermilab would provide the world's most powerful neutrino beam.
- Large projects are ordered by peak construction time, based on budget constraints, physics needs, and readiness criteria, as follows: completion of the Mu2e experiment at Fermilab, the high-luminosity LHC upgrades, and LBNF. Figure 1 in the report shows this time ordering, as well as the continuity of physics results across the program throughout the timeframe considered by P5.
- 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 in a strong leadership position, would 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 a small neutrino experiments portfolio, which is intended to be integrated into a coherent overall neutrino program.

- Specific investments would 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 investment in construction of new facilities. In constrained budget scenarios, this implies an increased fraction of the budget devoted to construction, and this will necessarily entail some judicious and painful reductions in the fractions of the budget invested in the research program and in operations. This represents a large commitment to building new experiments, which we see as essential. Particle physics is a dynamic field, with researchers nimbly changing course to invent and pursue great new opportunities.
- Reformulate the long-baseline neutrino program as an internationally designed and funded program, with Fermilab as host.
- Upgrade the Fermilab proton accelerator complex to produce the world's most powerful neutrino beam, redirecting former Project-X activities and temporarily redirecting some existing accelerator R&D toward this effort.
- 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.
- Based on new physics information, 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. In the general accelerator R&D program, focus on outcomes and capabilities that will dramatically improve cost effectiveness for mid- and far-term accelerators.

As discussed in the report, budget Scenario B allows for a balanced program. Scenario A differs from B by approximately \$30M per year until FY2018, and thereafter has a one percent per year escalation difference. While seemingly relatively small, these differences would have very large short- and long-term impacts. Relative to Scenario A, Scenario B would enable the large scientific returns of DESI, world-leading accelerator and instrumentation development research would not be curtailed, U.S. research capability – including a thriving theory program – would be maintained, the Mu2e experiment at Fermilab would be completed on time, the long-baseline neutrino program would proceed without delays, and third-generation dark matter direct detection capabilities would be fully developed on time. As valuable as each of these items is, they simply do not fit in Scenario A. The bang for the buck of the incremental investment would be really big.

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 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 C focus on three additional high-priority activities: develop a greatly expanded accelerator R&D program that would emphasize the ability to build future-generation accelerators 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; and 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.

I'd like to add a few words about our process, which is also described in Appendix C of the report. The work by P5 grew directly from the preceding community-wide study, and there was a continuous effort on many fronts throughout the P5 process to maintain direct community engagement, including workshops, physical and virtual town halls, consultations, presentations, and a public submissions portal. We had a deeply engaged panel, consisting of leaders from the U.S. and abroad, who looked beyond their own subfields to craft an optimal plan for the whole field. In our deliberations, no topic or option was off the table. Every alternative we could imagine was considered. We operated by consensus: even when just one or two individuals voiced concerns, we worked through the issues. Toward the end of the process, a draft of the report was sent to eleven community members for peer review, and their thoughtful and frank comments helped to improve the quality of the report considerably.

In conclusion, the P5 report offers important opportunities for U.S. investment in science, prioritized under the tightly constrained budget scenarios in the Charge. 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. Historic opportunities await us, enabled by decades of hard work and support. The U.S. particle physics community is ready to move forward.

Thank you very much for your interest in this work and the opportunity for me to share these results.