

Planning the Future of U.S. Particle Physics

Report of the 2013 Community Summer Study of the APS Division of Particles and Fields

University of Minnesota
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Foreword

Particle physics research in the United States has been a vibrant field since the middle of the past century. In the past thirty years the particle physics community gathered periodically at Snowmass, Colorado, to take stock of its progress and chart its future. The last such meeting was held in 2001.

In 2011, with the expected discovery of the Higgs Boson and the likelihood of advances in neutrino physics, the Division of Particles and Fields of the American Physical Society decided that the time was ripe for another “Snowmass” gathering. The meeting was held on the campus of the University of Minnesota, July 29 – August 6, 2013. Preparatory meetings during 2012 and 2013 were launched by a Community Planning Meeting at Fermilab, October 11–13, 2012.

“Snowmass on the Mississippi” (the 2013 Community Summer Study) was designed to address the questions the particle physics community wishes to answer over the next two decades, and how we plan to answer them. While we did not prioritize activities, our aim was to ask and answer hard questions. A subsequent prioritization panel with broad community representation is placing these questions and answers within realistic budgetary scenarios. Our aim has been to produce a resource book, of length and emphasis similar to the Physics Briefing Book (2012) of the European Strategy Group, which such a panel can find useful in its deliberations. We also hope to convey the health and diversity of the U.S. program, in a global context, to our colleagues and fellow citizens.

Although we found it convenient to retain the “frontier” categories of the previous P5 (“Particle Physics Project Prioritization Panel”), whose last report was issued in 2008, the division of the field into such categories should not obscure our focus on fundamental questions of physics, which, by their nature, cross such frontiers. These inter-frontier discussions were a major component of the meeting in Minneapolis.

This volume is presented in such a way that it can be read at various levels. An Executive Summary lays out the broad topics treated in more detail in a summary chapter. Each Frontier (Intensity, Energy, Cosmic, Theory, Capabilities, Instrumentation, Computing, and Communication) has its own chapter containing further details. Reference is made to submissions by each Frontier’s subgroups, and to contributed White Papers. The full set of Frontier reports, subgroup reports, and contributed papers can be found at:

<http://www.slac.stanford.edu/econf/C1307292/>

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For the Conveners and the DPF Executive Committee,

Jonathan L. Rosner	DPF Chair
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Executive Summary

The 2013 Community Summer Study, known as “Snowmass,” brought together nearly 700 physicists to identify the critical research directions for the United States particle physics program. Commissioned by the American Physical Society, this meeting was the culmination of intense work over the past year by more than 1000 physicists that defined the most important questions for this field and identified the most promising opportunities to address them. This Snowmass study report is a key resource for setting priorities in particle physics.

Particle physicists seek to understand the structure of the universe. We address two main questions: What are the most elementary constituents of nature, and what are the forces that cause them to interact? These questions are fundamental, and the desire to explore them is a defining characteristic of the human spirit. At the same time, finding the answers has practical value: It helps drive technical innovation in instrumentation, computing, and accelerators, and leads to the development of a skilled technical workforce. The development of new technologies, from industrial techniques, to medical imaging, high-performance computing, and beyond, has continually improved the quality of human life.

The discovery of the Higgs boson in 2012 was a remarkable achievement made possible by decades of worldwide collaboration. The existence of the Higgs boson was predicted in the 1960s. By last year it was the sole missing piece of the theory we call the Standard Model. This theory provides a coherent picture of the strong, weak, and electromagnetic interactions, with the latter two unified in an “electroweak” theory. The Standard Model contains quarks, leptons, force carriers, and now the Higgs boson.

However, the Standard Model still leaves significant questions unanswered. What is the nature of the Higgs boson? What can we learn from discovering that neutrinos have mass? Can the known forces be further unified? The particles of the Standard Model make up only 5% of the universe — what is the other 95%? Why is the universe almost all matter and no antimatter?

Many different techniques are needed to answer these questions. Particle physics explores three basic frontiers, often called cosmic, energy, and intensity. We continue that designation in this report. Each frontier uses its own tools and techniques, but all ultimately address the same fundamental questions.

The experiments that address these questions are ambitious, large-scale projects. Mounting them requires long-term vision. We are fortunate that our priorities are shared by physicists in other regions of the world, so that these experiments can be realized as global partnerships. The U.S. brings crucial leadership, design talent, technology, and resources that will be essential to these experiments wherever they are located.

The outline that follows introduces the future directions necessary for further progress in our understanding of nature at its most fundamental level. The order does not reflect prioritization.

Particle Physics Frontiers

Intensity Frontier: Experiments at the “Intensity Frontier” explore fundamental questions by using precision measurements to search for extremely rare processes and for tiny deviations from expectations. They can reveal new laws of physics at very high energies, in many cases exploring beyond the direct reach of high-energy accelerators. They often require the greatest possible beam intensities and ultra-sensitive detectors. The Snowmass study identified facilities and experiments that will ensure the global leadership of the U.S. in Intensity Frontier science.

Neutrinos are the most elusive of the known fundamental particles. We know of three species, which can change (“oscillate”) into one another under the right conditions. We have recently discovered that one of the neutrino oscillation parameters is large enough to let us measure two fundamental properties of neutrinos.

First, we can hope to determine which of the three neutrinos is heaviest and which is lightest (the mass hierarchy). Second, we can determine if leptons violate CP (charge-parity) symmetry, one of the most important symmetries of nature. We have known for almost fifty years that CP is violated by quarks; now the focus has shifted to leptons.

The Long-Baseline Neutrino Experiment (LBNE) will measure the mass hierarchy and is uniquely positioned to determine whether leptons violate CP. Future multi-megawatt beams aimed at LBNE, such as those from Project X at Fermilab, would enable studies of CP violation in neutrino oscillations with conclusive accuracy. An underground LBNE detector would also permit the study of atmospheric neutrinos, proton decay, and precision measurement of any galactic supernova explosion. This represents a vibrant global program with the U.S. as host.

The Snowmass study discussed further opportunities to learn about neutrinos. An upgrade of the IceCube experiment, involving photodetectors buried in Antarctic ice, could provide a promising approach to measure the mass hierarchy using atmospheric neutrinos. Next-generation neutrinoless double-beta decay experiments could reach the sensitivity necessary to determine whether neutrinos are their own antiparticles, and are a critical component of a strong neutrino program.

Transitions among different types, or “flavors,” of quarks and leptons provide essential probes of new physics, and are a central element of the Intensity Frontier. Substantial progress toward understanding these transitions can be made in this decade with experiments utilizing the Fermilab accelerator complex. These include a new measurement of the anomalous magnetic moment of the muon, a sensitive search for muon-to-electron conversion, and a proposed experiment to probe rare K meson decays to a new level of precision.

If particles have a property known as electric dipole moments (EDMs), they will violate CP symmetry. Finding EDMs would have profound implications, allowing us to check models of CP violation and to help understand the preponderance of matter in the universe. Project X could provide the means to make incisive EDM measurements of unprecedented precision.

The U.S. has also made substantial investments in other flavor physics experiments. Snowmass studies showed that the U.S. can capitalize on them, achieving unprecedented sensitivities with the capabilities offered by the LHCb experiment at CERN, Belle-II and J-PARC in Japan, and BESIII in China. Each probes a different aspect of new physics in a unique way. Project X will complement and extend results of these experiments.

New light, weakly coupled particles appear in many theoretical models, especially those of the cosmic dark matter. Some searches for these particles are feasible with intense beams and comparatively modest detectors at existing facilities. The Snowmass studies identified a rich, diverse, and low-cost program with a potential for high-impact discoveries, illustrating the importance of modest-scale experiments to complement large-scale efforts.

Energy Frontier: Experiments at the “Energy Frontier” make use of high-energy colliders to directly produce heavy elementary particles and to search for new ones. The properties of the Higgs boson raise crucial questions that guide large parts of the future particle physics program. The Higgs boson discovery calls for a three-pronged research program at high-energy accelerators, to: (1) determine the properties of the Higgs boson as accurately as possible, (2) make precise measurements of the heavy particles W , Z , and the top quark, which can carry the imprint of the Higgs field, and (3) search for new particles predicted by models of the Higgs boson and electroweak symmetry breaking. These topics also overlap with those in other frontiers. The expectation of particles with masses at or above one TeV directly motivates searches for dark matter particles and flavor-changing rare decays.

For at least the next fifteen years, the experiments at the Large Hadron Collider (LHC) at CERN will drive the Energy Frontier program forward. The LHC experiments are successful global collaborations. U.S. leadership and contributions to detector and accelerator components, technology, and physics insight have been indispensable to that success.

Following the Higgs boson discovery, the LHC moves into a phase of precision study of the properties of this particle. The high-luminosity LHC will measure Higgs boson interactions at the few-percent level. LHC operation at higher energy over the next ten years, and, later, at higher luminosity, will continue the search for new particles produced through the strong or electroweak interactions. It will probe for new dynamics of the W , Z , and Higgs bosons at TeV energies and study rare decays using billions of top quarks.

Compelling science motivates continuing this program with experiments at lepton colliders. Experiments at such colliders can reach sub-percent precision in Higgs boson properties in a unique, model-independent way, enabling discovery of percent-level deviations from the Standard Model predicted in many theories. They can improve the precision of our knowledge of the W , Z , and top quark well enough to allow the discovery of predicted new-physics effects. They search for new particles in a manner complementing new particle searches at the LHC. A global effort has completed the technical design of the International Linear Collider (ILC) accelerator and detectors that will provide these capabilities in the latter part of the next decade. The Japanese particle physics community has declared this facility as its first priority for new initiatives.

The Snowmass study considered many other options for high-energy colliders that might be realized over a longer term. These included higher-energy linear colliders, circular e^+e^- colliders, muon colliders, and photon colliders, all of which merit continued study. The Snowmass study identified, in particular, the promise of a 100 TeV-class hadron collider (VLHC), which would provide a large step in energy with great potential for new insights into electroweak symmetry breaking and dark matter. The feasibility of such a machine should be clarified through renewed accelerator R&D and physics studies over the next decade.

In all of the above projects, U.S. leadership in developing detector and accelerator technologies is playing a critical role. These U.S. initiatives are essential to meet the world-wide scientific goals in particle physics.

Cosmic Frontier: We now know that 95% of the universe is in the form of dark matter and dark energy. These components are responsible for the structures and accelerated expansion of the universe observed today, but their fundamental nature is almost completely unknown. “Cosmic Frontier” experiments are designed to determine the nature of dark matter and dark energy and to use the universe as a laboratory to search for new fundamental particles and interactions. Along with the other frontiers, the Cosmic Frontier provides particle physics with clear evidence for physics beyond the Standard Model, profound questions of popular interest, frequent new results and surprises with broad impacts, many opportunities for discovery with unique probes, important cross-frontier topics, and a full range of project scales providing flexible programmatic options.

Our studies showed how different approaches to dark matter — direct detection; indirect detection of gamma rays, neutrinos, and antimatter; accelerator-based searches; simulations; and astrophysical surveys — provide unique and necessary information. A census of current and proposed experiments and their capabilities shows that new direct and indirect detection experiments will probe dark matter masses inaccessible to colliders and provide leaps in sensitivity at moderate cost. For many leading dark matter candidates, including well-known examples of weakly-interacting massive particles and axions, this progress will lead to the first non-gravitational signals of the dark universe and open the door to the identification of dark matter, with far-reaching implications for both particle physics and astronomy.

Snowmass studies also strongly reinforced the roles that cosmic surveys play in particle physics. Current and upcoming dark energy imaging and spectroscopic surveys will shrink the errors, as recommended in previous community studies, but they will do even more for particle physics. The richness of the data and detailed

attention to the reduction of systematic uncertainties will enable many new tests of the behavior of dark energy and general relativity over a wide range of cosmic distance and time scales.

The universe appears to have undergone an enormous expansion in the very first moments after the Big Bang. This phenomenon, known as inflation, helps us understand many subsequent details of how the universe developed. Cosmic microwave background (CMB) experiments will probe the physics of inflation with enough sensitivity to test many of our ideas about the birth of the universe.

Cosmic Frontier experiments can also help us understand neutrinos. Studies of the CMB, measurements of the distribution and apparent shapes of galaxies, and polar-ice neutrino experiments will provide precise information about neutrinos, including the mass hierarchy, the sum of their masses, and the number of light neutrinos. These experiments provide information complementary to laboratory studies of neutrinos, and both types combine to create a powerful means for detailed neutrino investigations.

Finally, the Snowmass process reiterated the unique information we can gain from studies of cosmic particles and the detection of significant numbers of the highest-energy cosmic rays produced in nature. These studies provide a window on proton interactions at energies beyond the reach of the LHC and include the detection of extremely high-energy neutrinos produced in the interactions of cosmic rays with CMB photons, which will enable the study of neutrino interactions at center-of-mass energies up to 100 TeV.

For all of these areas, the Snowmass study identified essential technologies and facilities, the advances required in theoretical models, and experiments with great promise. The largest projects are, appropriately and necessarily, international. The U.S. is still the leader in this quickly evolving area, but other regions with intensive interest in this physics are advancing rapidly.

Theoretical Physics: Progress in science is based on the interplay between theory and experiment, between having an idea about nature and testing that idea in the laboratory. Neither can move forward without the other. The U.S. has been a world leader for many decades in particle theory, and a sustained strong and vibrant program remains essential for the success of U.S. particle physics. Theoretical physicists have been a driving force in both the development and testing of the Standard Model, including the discovery of the Higgs boson. They play crucial roles in formulating the big questions in the field, setting out hypotheses that address them, and proposing experimental strategies to confirm or refute them. At the same time, theorists seek new structures that might provide unanticipated results. In contrast to experiment, theory depends less on facilities and equipment; the success of the U.S. program rests mainly on principal investigators in universities and national labs, working with postdoctoral fellows and graduate students, and collaborating with both U.S. and international researchers.

Enabling Frontiers

Accelerator Capabilities: Accelerator-based experiments continue to be the mainstay of transformational physics on both the Energy and Intensity Frontiers. Progress in these frontiers demands advancing the capabilities of accelerator facilities. The U.S. has been a leader in accelerator physics, its critical and supporting technologies, and the innovative design of research facilities. Its continuing leadership is necessary for future discoveries.

The LHC incorporates major U.S. contributions, including high-field superconducting magnets that focus the beams into collision. For future Energy Frontier facilities, U.S. laboratories have pioneered the technology of Nb₃Sn magnets that will permit higher fields than possible with the present LHC technology. The U.S. LHC Accelerator Research Program (LARP) gives the U.S. a world-leading ability to develop high-field superconducting accelerator magnets — a capability central to the LHC luminosity upgrade and for a future proton collider with far greater energy than the LHC. A 100 TeV-class hadron collider (VLHC) is within the

development reach of existing materials for a tunnel of 100 km or larger. A collider at even higher energy may require new classes of superconducting magnets and novel ways of handling synchrotron radiation.

The ILC, as described in its Technical Design Report, is ready to proceed to construction. Its design incorporates U.S. contributions in accelerator theory, damping ring design, superconducting accelerator technology, and beam control and delivery. Concepts for multi-TeV lepton colliders include the CLIC two-beam accelerator, plasma wakefield accelerators driven either by beams or lasers, and a muon collider.

Accelerators for proposed Intensity Frontier experiments need to deliver multi-megawatt proton beams with flexible, experiment-dependent timing structures — demands beyond the capabilities of any existing accelerator. Multi-megawatt beams are the focus of vigorous R&D for both the Project X superconducting linac and DAEδALUS cyclotrons. A broad range of highly flexible timing structures is being pursued within Project X.

Managing huge stored energy and controlling beam losses to minuscule levels will be essential to operation of all frontier physics accelerators. Specific challenges include generating high-quality beams, modeling beam dynamics, and managing material damage in high-power targets. Maximizing the potential and minimizing the risks in future accelerator projects requires integrated multi-laboratory programs to increase the readiness of critical technologies. Yet engineering-intensive programs must not squeeze out visionary, innovative research in basic accelerator science.

A strong U.S. research program of accelerator stewardship benefits all areas of science and industry that use accelerator technology. The broad application of superconducting technology to accelerator-based science exemplifies the payoff of long-range investment that transcends individual projects. Investment in national laboratories and research universities, with their broad expertise and technical infrastructure, will yield new generations of accelerators capable of higher energies, more intense beams, and more efficient operation.

Underground Laboratory Capabilities: Many experiments searching for dark matter, proton decay, or seeking to determine the properties of neutrinos must be located underground to shield the sensitive experiments from cosmic ray backgrounds. Underground facilities are located or proposed in North and South America, Europe, Asia and in the Antarctic ice. The scope of underground capabilities in all regions is expected to increase by the end of the decade to accommodate the experimental demand. Locating LBNE underground would allow this experiment to realize its full scientific potential and could make it an anchor for a future domestic underground laboratory, bringing to the U.S. a broad range of compelling experiments and researchers from around the world.

Instrumentation: Instrumentation enables experiments to answer the science questions facing particle physics. Particle physics has a long and distinguished history of inventing, designing, and building the specialized instrumentation required for its experimental research. The physics requirements of many experiments in the Energy, Intensity and Cosmic Frontiers entail very large-scale detectors, but the cost involved in simply scaling up existing technologies is becoming prohibitive. In order for the field to stay competitive, new innovative technologies will need to be developed. Investment in the development of these new technologies is not a luxury but a prerequisite for the sustainability of the field. It has therefore become very important for the particle physics community to establish a mechanism for developing and implementing a coherent vision for the future direction for particle physics instrumentation.

The Snowmass study has formulated a vision for a U.S. instrumentation program for particle physics. We identified key barriers to answering the science questions and recognized select technologies for further investment enabling the U.S. to retain a leadership position in a broad global experimental program.

Accelerator experiments will require fast, radiation-hard, low-mass, highly segmented, intelligent and sophisticated trackers and vertex detectors; cost-effective, highly segmented, radiation-hard calorimeters with

excellent energy resolution; and high-speed data acquisition systems. Experiments studying particle interactions with small cross sections or rare decays typically need cost-effective large volume detectors with efficient background rejection using large area charged particle detection and/or optical readout systems. Some of these experiments need materials and sensors with ultra-low intrinsic radioactivity while others need high-granularity detectors with fast timing and good energy resolution. Many experiments in the cosmic frontier utilize large arrays of ultra-low-noise electromagnetic radiation detectors.

Executing a strategy to develop innovative technologies requires integrating the diverse capabilities and resources of universities, national laboratories, other branches of science, and industry into detector R&D collaborations, emphasizing the importance of innovation through a domestic instrumentation development program. A coordinating panel for advanced detectors would help articulate the mission of this program and facilitate its implementation. The goals of the program are to develop both incremental and transformational cost-effective technologies with maximal scientific reach, based on the technological strengths in the U.S. A stable and adequately funded generic instrumentation program will ensure that particle physics invests in its future and establishes a foundation for a competitive, healthy long-term program.

Computing: Computing is essential to all particle physics experiments and in many areas of theoretical physics. Although some hardware is customized, most of it comes from commercial vendors. Thus, selecting the right hardware and using it efficiently are essential to achieve our scientific goals.

We have to train the personnel required to develop and support the parallel programs needed now and in the future. Increased parallelism is required because of changes in chip technology and the evolution of high-performance systems to include multi-core chips and accelerators. It is also important to support the development and implementation of new algorithms in several theoretical areas.

Particle physicists should help in the planning of U.S. and international networks, as LHC upgrades will place more demands on the distributed computing systems for ATLAS and CMS. Failure to do so could lead to bottlenecks from wide-area networks, hampering the analysis of results from those and future experiments. Funding agencies should encourage enhanced coordination of software development across all frontiers. The needs of Intensity Frontier experiments are not yet at the level of LHC experiments, but will grow.

Future experiments will demand better storage capacity and bandwidth. Disk prices may not drop as rapidly as they have in the past. Scientists working on the cutting edge of computing must also continue to detail their needs to the national supercomputing centers. The funding agencies should seek community input on the appropriate mix of resources dedicated to a particular project and shared computing resources.

Early attention to these issues can increase efficiency, reduce costs, enable significantly more realistic theoretical calculations, and avoid computing bottlenecks that could limit scientific progress.

Communication, Education, and Outreach

The particle physics community recognizes the critical importance of consistent and coherent communication, education, and public outreach (CE&O). These foster nationwide support for the field and develop the next generation of scientists, engineers, and scientifically literate citizens. More of us should engage in CE&O activities to translate the American public's fascination with particle physics research into the support necessary to enable the field to answer its biggest questions. Existing activities should be augmented with dedicated personnel who will enhance these efforts, provide nationwide coordination, and spearhead new initiatives. These personnel would develop materials for use in informing the public about direct and indirect applications of particle physics research. They would put in place sustainable methods to collect statistics on workforce development and technology transfer. They could provide professional development opportunities for educators, as well as creating new learning opportunities for students of all ages.

Conclusion

With the completion of the Standard Model, particle physicists now turn their attention to still deeper questions about the nature of matter and the constituents of the universe. This report proposes an ambitious array of new experiments. We consider it realistic to carry out these experiments through a long-term plan and through global partnerships. Particle physicists have been the pioneers of large-scale scientific projects. We have constructed facilities of unprecedented scale, including the Tevatron and the Large Hadron Collider, through decades-long programs requiring world-wide collaboration. These led to discoveries that are the foundation of our current success.

Several strategic goals have emerged from the Snowmass study.

- Probe the highest possible energies and distance scales with the existing and upgraded Large Hadron Collider and reach for even higher precision with a lepton collider; study the properties of the Higgs boson in full detail.
- Develop technologies for the long-term future to build multi-TeV lepton colliders and 100 TeV hadron colliders.
- Execute a program with the U.S. as host that provides precision tests of the neutrino sector with an underground detector; search for new physics in quark and lepton decays in conjunction with precision measurements of electric dipole and anomalous magnetic moments.
- Identify the particles that make up dark matter through complementary experiments deep underground, on the Earth's surface, and in space, and determine the properties of the dark sector.
- Map the evolution of the universe to reveal the origin of cosmic inflation, unravel the mystery of dark energy, and determine the ultimate fate of the cosmos.
- Invest in the development of new, enabling instrumentation and accelerator technology.
- Invest in advanced computing technology and programming expertise essential to both experiment and theory.
- Carry on theoretical work in support of experimental projects and to explore new unifying frameworks.
- Invest in the training of physicists to develop the most creative minds to generate new ideas in theory and experiment that advance science and benefit the broader society.
- Establish a nationally coordinated communication, education and outreach effort, supported by a dedicated team, to convey the excitement and value of our field to others.

In pursuit of these projects, we have developed a community that links together scientists from all regions of the world pursuing common goals. Our community is ready and eager to carry out the next steps in humankind's quest to understand the basic workings of the universe.

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Summary of the 2013 Community Summer Study

1.1 Introduction

The 2013 Community Summer Study, known as “Snowmass,” sought to identify the critical research directions for the United States particle physics program. This meeting was the culmination of intense work over the past year to define the most important questions for this field and identify the most promising opportunities to address them. The resulting study report, presented here, is a key resource for setting priorities in particle physics.

Through the previous six decades of precision and discovery-level particle physics, we have learned much about the basic laws that govern the Universe. We have uncovered the laws that describe the subnuclear forces and, with the discovery of the Higgs boson, the agent that we believe should give mass to all elementary particles. However, there is still much that we do not understand. The advances in our knowledge of elementary particle physics have sharpened the questions in that domain. Recent discoveries about the matter and energy content of the Universe have brought new questions that are equally fundamental, and equally vexing.

One of the goals of Snowmass was to develop a framework of scientific questions that can form the basis for a future program in particle physics, and to survey experiments that would address these questions. To introduce a summary of the results of Snowmass, we propose a basic set of questions about particle physics whose answers are not yet known. The search for these answers will be carried out with a broad range of experimental methods, cutting across the frontiers around which the Snowmass study was organized.

1. 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?
2. 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?
3. 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?
4. What mechanism produced the excess of matter over anti-matter that we see in the Universe? Why are the interactions of particles and antiparticles not exactly mirror opposites?
5. 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?

6. 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?
7. 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?
8. 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?
9. 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?
10. 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?
11. 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?

The search for answers to these questions is intimately tied to the development of technology. Particle physics experiments and accelerators put extraordinary demands on sensors, precision engineering, and data management, incorporated into devices of very large scale. Our community invents new technologies to address these needs and develops them into usable form. The progress of our field requires both technology development directed at the problems of specific experiments and the development of new technologies that provide higher performance or decreased cost for devices with broad application. This technology development for accelerators and detectors ultimately benefits all of physical science.

In many areas of physics experimentation, there are specific technological developments that would be of enormous benefit. Existing technologies are unlikely to meet the science needs of future particle physics experiments. New technologies need to be explored that could lead to transformative advances, enabling cost-effective particle physics experiments but also new initiatives of broad importance.

In developing such technologies, we need to address several questions.

1. Experiments continue to reach for rarer processes, more precise measurements, higher energies and luminosities, and more inclusive observations. How do we achieve the finer granularity, larger volume, more radiation-hard, lower-cost, and higher-speed detectors that will in large part determine our experimental reach?
2. Paradigm-altering technology developments are occurring in electronics and materials design, potentially offering breakthrough capabilities. How can these advances be incorporated into new detectors with improved overall performance? How do we make best use of the resources available in universities, national laboratories, and industry to develop new detector systems?
3. What technologies will be needed to acquire, analyze, and store the enormous amounts of data from future experiments? Can local intelligence be incorporated to manage data flow? How will we fully and efficiently utilize data stored in large databases?

4. Scaling of current accelerator designs to higher energy leads to machines of very large size, cost, and power demand. Can new technologies lead to more practical strategies? Is there an ultimate highest energy for colliders?
5. Proposed experiments at a range of energy scales call for particle beams of extreme brightness. Are there technologies to achieve high beam power in a better-controlled and more cost-effective way?

It is important for particle physicists to share the excitement and benefits of our field with a broader public. To that end:

1. How do we engage particle physicists in communication, education and outreach activities so as to convince policy makers and the public that particle physics is exciting and worth supporting?
2. How do we educate a talented and diverse group of students who choose to enter particle physics and other STEM careers, including science teaching?

In the following chapters, we discuss these issues in more detail and explain how they will be addressed in the coming decades by new initiatives in particle physics. The discussion is organized along the lines of the physics frontiers. The next sections contain the summary of each main program element and the conclusions for each of the frontiers.

1.2 Intensity Frontier

All frontiers of particle physics aim to discover and understand the constituents of matter and their interactions at the highest energies, at the shortest distances, and at the earliest times in the Universe. The Standard Model (SM) fails to explain all observed phenomena: New interactions and yet unseen particles must exist. They may manifest themselves either directly, as new particles, or by causing reaction rates to differ from the often very precise predictions of the SM. The Intensity Frontier explores these fundamental questions by searching for new physics in extremely rare processes or those forbidden in the SM. This requires the greatest possible beam intensities, as well as massive, ultra-sensitive detectors. Many of these experiments are sensitive to new physics at higher mass scales or weaker interaction strengths than those directly accessible at high-energy colliders, thus providing opportunities for paradigm-changing new discoveries complementary to Energy and Cosmic Frontier experiments.

The range of experiments encompassing the Intensity Frontier is broad and diverse. Intense beams of neutrinos aimed over long distances at very large detectors will allow us to explore the neutrino mass hierarchy, and search for CP violation and non-standard interactions. The very large detectors, if located underground, will provide increased sensitivity to proton decay. Multi-ton detectors searching for neutrinoless double-beta decay will determine whether neutrinos are their own antiparticles. Intense beams of electrons will enable searches for hidden-sector particles that may mediate dark matter interactions. Extremely rare muon and tau decays, if seen, will signal violation of charged lepton quantum numbers. Measurements of intrinsic lepton properties, such as electric and magnetic dipole moments, are another promising thrust. Rare and CP-violating decays of bottom, charm, and strange particles, measured with unprecedented precision, will clarify the new physics underlying discoveries at the Large Hadron Collider (LHC). In any new physics scenario, Intensity Frontier experiments with sensitivities to very high mass scales will be crucial for exploration.

At Snowmass, the Intensity Frontier program was defined in terms of six areas that formed the basis of working groups, with experiments that probe neutrinos, baryon number violation, charged leptons, quark flavor physics, nucleons, nuclei, and atoms, and new light, weakly-coupled particles.

The working group reports provide a clear overview of the science program within each area of the Intensity Frontier. They present discovery opportunities for facilities that will be available this decade or will come online during the next decade. Here, we briefly summarize the findings from each working group.

Neutrinos: Decades of experimental and observational scrutiny have revealed less than a handful of phenomena outside the SM. These include the dark energy and dark matter puzzles, and the existence of non-zero neutrino masses. Neutrino masses represent one of the few experimental pointers towards a new underlying theory. While many experiments continue to look for other new phenomena and deviations from SM predictions, it is clear that continued detailed study of the neutrino sector is of the utmost importance.

Compared to the other fermions, the elusive neutrinos have been extremely difficult to study in detail. Despite the challenges, neutrino physics has been tremendously successful over the past two decades. From almost complete lack of knowledge about neutrino mass and mixing twenty years ago, we now have a robust, simple, three-flavor paradigm describing most of the data.

However, key questions in the three-flavor sector remain: We do not know the mass ordering or the value of neutrino masses, nor whether neutrinos violate CP symmetry, nor whether the neutrino is its own antiparticle, and we have only just begun to test the three-flavor paradigm. A precision neutrino oscillation program is required to carry out such measurements. Furthermore, some experiments have uncovered intriguing anomalies that merit additional study, and could lead to the discovery of states or interactions beyond the SM. Advances in detector technology and analytical techniques for the next generation of neutrino experiments are well underway. We have clear experimental paths forward for building on our success, for precision testing of the three-flavor paradigm, for the exploration of anomalies, and for the measurement of fundamental neutrino properties and interactions.

The next decade promises significant experimental progress around the world. In the search for neutrinoless double-beta decay, a number of experiments rely on complementary isotopes and experimental techniques. The next generation of 100-kg-class neutrinoless double-beta-decay search experiments should have sensitivity to effective masses in the 100 meV range; beyond that, there are opportunities for ton-class experiments that will reach sub-10 meV effective mass sensitivity, pushing below the inverted hierarchy region. The next generation of tritium-beta-decay experiments will directly probe neutrino masses a factor of 10 smaller than the best current bounds. Innovative ideas may help to go beyond these sensitivities.

The neutrino mass hierarchy can be unambiguously resolved using accelerator neutrino oscillation experiments with baselines around 1000 km (or longer) and detector masses of order tens of kilotons. Precision measurements of atmospheric neutrino oscillations with megaton-scale underground detectors or detectors under ice can also resolve the mass hierarchy. The discovery of a non-zero θ_{13} mixing angle enables long-baseline neutrino experiments to search for leptonic CP violation in appearance experiments. The search for CP violation in the neutrino sector is a top priority for particle physics efforts worldwide, and vigorous planning for the next-generation large-scale neutrino oscillation experiment is underway internationally. Regardless of the experimental approach, high-power proton beams (greater than 1 MW) coupled with massive detectors (of order 100 kiloton), are needed to study CP violation in neutrino oscillations. The U.S., with the Long-Baseline Neutrino Experiment (LBNE) and a future multi-megawatt beam from Project X at Fermilab, is uniquely positioned to lead an international campaign to measure CP violation and push the limits of the three-flavor paradigm. An underground location for a far detector significantly enhances the physics reach. LBNE represents a vibrant global program with the U.S. as host.

Given the challenges associated with precision measurements in the neutrino sector, complementary baselines, sources, and detector techniques will be required to bring the picture into focus. New accelerator technologies, such as neutrino factories and cyclotron-based-sources, may eventually take measurements to the next level. Smaller experiments will also play a key role in addressing some of the remaining anomalies and hints for physics beyond the three-neutrino paradigm, and study neutrino-matter interactions in detail.

The diversity of physics topics that can be probed through the neutrino sector is very significant, and the interplay between neutrino physics and other fields is rich. Neutrinos can and will provide important information on structure formation in the early Universe; Earth, Sun, and supernova physics; nuclear properties; and rare decays of charged leptons and hadrons. *The neutrino sector sits at the nexus of a worldwide effort that crosses the frontiers of particle physics.*

Baryon number violation: Within the SM, protons are stable, as baryon number is assumed to be conserved. However, baryon number is not a fundamental symmetry of the SM and is not conserved in many of its extensions. In particular, baryon number violation is an essential ingredient for the creation of the observed asymmetry of matter over anti-matter in the Universe. Grand unified theories (GUTs) predict that the proton decays with a lifetime in excess of 10^{30} years, with the decay being mediated at scales of order 10^{16} GeV. Two important decay channels in GUTs are $p \rightarrow e^+\pi^0$ and $p \rightarrow \bar{\nu}K^+$; several other modes are also possible. The current limits on the proton lifetime in these two channels are roughly 10^{34} years and 6×10^{33} years, respectively, a factor of 5 to 10 below predictions in certain well-motivated GUT models.

The search for proton decay is carried out in detectors containing enough protons, and placed underground to reduce backgrounds. Large neutrino oscillation detectors are ideal for this task, and proton decay is an important piece of their physics portfolio. The largest existing underground neutrino experiment is the 22.5-kiloton Super-Kamiokande water Cherenkov detector. Future underground neutrino experiments, such as a 34-kiloton liquid argon time-projection chamber (LBNE) or a 560-kiloton water Cherenkov detector (Hyper-Kamiokande) can measure lifetimes on the order of GUT expectations with exposures of roughly 10 years. Typical exposure of these experiments could reach a sensitivity of $\tau(p \rightarrow e^+\pi^0) < 10^{35}$ years and $\tau(p \rightarrow \bar{\nu}K^+) < 3 \times 10^{34}$ years.

Neutron-antineutron oscillations would violate baryon number by two units. They are searched for with a beam of free neutrons, in which a neutron would transform into an antineutron that annihilates in a distant detector. Such oscillations are expected in theories where baryogenesis occurs near or below the electroweak scale. A proposed experiment at Project X at Fermilab, using free neutrons from a 1 MW spallation target, could discover this phenomenon or improve existing limits on the oscillation probability by four orders of magnitude.

Charged leptons: The charged lepton experimental program offers significant discovery opportunities in this decade's experiments and in even more sensitive experiments possible with future facilities such as Project X at Fermilab. Extremely sensitive searches for rare decays of muons and tau leptons, together with precision measurements of their properties, will elucidate the scale and dynamics of flavor generation or limit the scale of flavor generation to well above 10^4 TeV. Any indication of charged lepton flavor violation (CLFV) would be an indisputable discovery of new physics. Precision measurements of lepton flavor-conserving (LFC) processes can be used to verify predictions of the SM and look for signs of new physics.

The experimental program consists of a large and diverse set of opportunities and includes multi-purpose experiments that utilize the large tau production rates at high-luminosity B factories, as well as highly optimized single-purpose experiments that explore muon transitions.

Significant advances in studying CLFV in the muon sector can be achieved this decade. For the rare decay $\mu \rightarrow e\gamma$, the MEG upgrade at the Paul Scherrer Institute (PSI) can reach branching fractions up to 6×10^{-14} .

The Mu3e collaboration at PSI plans to improve their sensitivity to $\mu \rightarrow 3e$ by approximately four orders of magnitude.

Observation of the direct conversion of a muon to an electron in the field of a nucleus would provide a powerful window to physics beyond the SM. Current limits for $\mu N \rightarrow e N$ conversion are at the level of 10^{-12} to 10^{-13} from experiments at PSI. Later this decade, COMET at J-PARC plans to improve these bounds by two orders of magnitude. A separate proposal at J-PARC, DeeMe, would use a different technique to reach a similar sensitivity. Before the end of the decade, Mu2e at Fermilab, followed soon by COMET, will begin operations and improve the existing search reach by four orders of magnitude. This would reach sensitivity to signals from supersymmetric grand unified models. If no signal is observed, this would set constraints on CLFV physics at the scale of 10^4 TeV. Future experiments beyond these are being considered in conjunction with more intense muon beams that could be available with new facilities at J-PARC or Project X at Fermilab.

The muon's magnetic moment is predicted very precisely in the SM. New physics contributes via radiative corrections. The present level of sensitivity was obtained by the E821 experiment at Brookhaven National Laboratory, with a difference between the measurement and the SM theoretical prediction of 3.6σ . A new experiment, E989 (Muon $g-2$) at Fermilab, will re-use the E821 muon storage ring at Fermilab with the same experimental technique. E989 is expected to increase the statistics by a factor of 20 with a corresponding reduction of systematic uncertainties, resulting in an overall reduction in the experimental error by a factor of roughly 4. An alternate approach at J-PARC using lower-energy muons is expected to have the same precision as E989 at Fermilab but very different systematics. A world-wide effort is underway to reduce the theoretical uncertainty in the SM prediction with new data from e^+e^- machines and breakthroughs in lattice gauge theory.

New physics effects usually scale as a function of the lepton mass, and hence τ observables can be very sensitive to new contributions. Important observables in τ leptons are CLFV decays, CP violation, the electric dipole moment, and the anomalous magnetic dipole moment. The large τ production rates possible at the future SuperKEKB facility in Japan could achieve an order of magnitude improvement in CLFV branching fractions over current results from BABAR and Belle.

The charged lepton sector has significant potential to reveal more information on the fundamental principles of nature, and the U.S. has the opportunity to play a leading role with facilities planned for this decade.

Quark flavor physics: The study of strange, charm, and bottom quark systems has a long and rich history in particle physics. Measurements of rare processes in the flavor sector have led to startling revelations and played a critical role in the development of the SM. The constraints on physics beyond the SM from flavor physics considerations are powerful. The current quark flavor data set is mostly in agreement with SM expectations with a handful of 3 to 4σ anomalies. New corrections to the SM at the level of tens of percent are still allowed by the data. Contributions to flavor processes from many theories beyond the SM arise at this level, and thus more precise measurements may observe new physics. If new massive states are observed at the LHC, detailed measurements of the quark flavor sector will be necessary to determine the underlying theory and its flavor structure. If such states are not discovered in high-energy collisions, then precision quark flavor experiments, with their ability to probe mass scales far beyond the reach of the LHC, provide the best opportunity to set the next energy scale to explore. Depending on the strength of new physics interactions, this program already indicates that the new physics scale is above 1 TeV and in some scenarios above 10^5 TeV. Proposed experiments can probe even further. Continued investigations of the quark flavor sector are thus strongly motivated.

A well-planned program of flavor physics experiments has the potential to continue this history of advances. Such a program exists worldwide with the LHCb experiment at the LHC, an upgraded SuperKEKB facility

in Japan, BESIII in China, and future rare kaon decay experiments at CERN, J-PARC and potentially Fermilab. These facilities will carry out a rich multi-purpose program in the strange, charm, and bottom sectors and perform numerous crucial measurements of rare decays and CP-violating observables. The proposed experiment ORKA at the Fermilab Main Injector would probe rare kaon decays to unprecedented precision and would retain the U.S. capability to perform quark flavor experiments. In the longer term, Project X at Fermilab could become the dominant facility in the world for rare kaon decays. The expected sensitivities for these future programs are detailed in the full report, and are at the level which could discover new physics. It is important to note that these results are not predicated on future theoretical progress, although theoretical advancements will strengthen the program by increasing the set of observables that can reveal new physics. U.S. contributions and support for quark flavor experiments are necessary in order for the U.S. HEP program to have the breadth to assure meaningful participation in future discoveries.

Nucleons, nuclei, and atoms: The use of nucleons, nuclei, and atoms as laboratories for the study of fundamental interactions is entering a new era. These systems have sensitivity to physics beyond the SM and provide important tests of vital symmetries through measurements of electric dipole moments (EDMs), weak decays of light hadrons, weak neutral currents, and atomic parity violation.

Observation of an EDM would signify both parity and time-reversal symmetry violation, and would probe the physics of CP violation. The SM predictions (via multi-loop contributions) for the EDMs of the electron, neutron, and nucleus are 10^{-38} , 10^{-31} , and 10^{-33} e-cm, respectively. EDM measurements are challenging and the present experimental sensitivity is approximately 10^{-27} , 3×10^{-26} , and 3×10^{-29} e-cm for the electron, nucleon, and ^{199}Hg nucleus, respectively. Experiments searching for the electron EDM typically use the polar molecules YbF and ThO and ultimately expect to reach a level of 3×10^{-31} e-cm. Several current or planned experiments searching for the neutron EDM are expected to reach a sensitivity of 5×10^{-28} e-cm, a factor of 100 below current limits. For atoms, future experiments using mercury, radon, and radium expect sensitivities at the level of 10^{-32} e-cm. This would require upgraded facilities such as FRIB at Michigan State University or Project X at Fermilab. These future programs will be sensitive to signals predicted to appear in several theories beyond the SM.

The weak decays of light hadrons provide precision input to the SM and are a sensitive test of new interactions. The ratio of decay channels $e\nu/\mu\nu$ for pions and kaons affords a precise test of lepton universality, probing new physics up to 10^3 TeV. For pions, experiments at TRIUMF and PSI will improve the measurement error by a factor of five. Neutron beta decay provides an accurate determination of the CKM element V_{ud} , enabling strong tests of CKM unitarity and constraining new physics up to scales of ~ 10 TeV. Several programs are underway to measure observables of the neutron lifetime and decay asymmetries with improved precision. Measurements of parity-violating asymmetries in fixed-target scattering with polarized electrons allows for a precision determination of the weak mixing angle at low values of momentum transfer, which in turn constrains new parity-violating effects up to 2 – 3 TeV. An improved polarized Møller scattering experiment at upgraded JLab facilities expects to determine the weak mixing angle to an accuracy of 0.1%, comparable to measurements at the Z pole. Parity violation in atomic transitions also yields valuable measurements of the weak mixing angle. New techniques requiring intense sources are being developed; Project X at Fermilab would provide more rare isotopes for this program than any other facility.

New light, weakly coupled particles: New light particles that couple very weakly to the SM fields are a common feature of extensions beyond the SM. They are motivated by both theoretical and observational considerations, including the strong CP problem and the nature of dark matter and dark energy. Examples of such particles include axions, hidden-sector photons, milli-charged particles, and chameleons. These hidden-sector particles typically couple weakly to the photon via mixing. Intense sources are hence required to produce them at rates sufficient to enable their discovery. The parameters relevant for searches of such hidden-sector particles are their mass and coupling strength to the photon. A variety of experiments constrain part of this parameter space, but much territory is still open for exploration. The current constraints arise

from astronomical observations, cosmological arguments, and a variety of laser, heavy-flavor, and fixed-target experiments. Regions that may signal dark matter detection or annihilation and areas that offer an explanation for the present result on the anomalous magnetic moment of the muon have yet to be probed.

Numerous laboratory experiments are either in progress or proposed. Two microwave cavity searches for axions will be underway soon in the U.S., but require further developments to increase their mass reach. The light-shining-through-walls technique, where photons are injected against an opaque barrier, continues to explore open regions of parameter space. More advanced technology is needed to make progress in the mid-term. Axion helioscope searches were first carried out using borrowed magnets, and now require a custom-built magnet to improve sensitivity. Collider searches for hidden-sector particles can be performed via the reaction $e^+e^- \rightarrow \gamma\ell^+\ell^-$ at high luminosity e^+e^- factories or in the decays of gauge bosons at the LHC. Fixed-target experiments using both electron and proton beams are a promising place to search for hidden-sector particles. The electron beam experiments APEX at the Jefferson National Accelerator Facility (JLab) and A1 at the University of Mainz have recently performed short test runs and have plans for more extensive runs this decade. HPS has been approved by JLab and will run after the 12 GeV upgrade. DarkLight proposes to use the free-electron laser beam at JLab. Proton fixed-target experiments have the potential to explore regions of parameter space that cannot be probed by any other technique. Neutrino experiments, such as MiniBooNe, can widen the search to smaller couplings. The intense proton source at Project X could also provide a powerful extension to the search reach. Impressively large regions of parameter space are currently unexplored and are ripe for the discovery of light, weakly-coupled particles.

Conclusions: The above program exhibits the broad spectrum of science opportunities attainable at the Intensity Frontier. While each subfield is at a different stage of maturity in terms of testing the SM, the proposed experiments in each area are poised to have major impact. The programs involving transitions of heavy quarks, charged leptons, and nucleons, nuclei, and atoms are advanced, with the most precise SM predictions and a well-developed experimental effort that has spanned decades. In this case, the next level of experimental precision would reach the point where effects of new TeV-scale interactions are expected to be observable. More sensitive searches for proton decay and new light, weakly-coupled particles can cover a large range of parameter space that is consistent with grand unified theories and cosmological observations. Neutrino physics is just beginning the era of precision measurements where it is possible to probe basic neutrino properties and answer principal questions. Neutrino physics holds great promise for discovery.

Such an extensive program is necessary to address the unresolved fundamental questions about nature. The knowledge we seek cannot be gained by a single experiment or on a single frontier, but rather from the combination of results from many distinct approaches working together in concert. The full report from the Intensity Frontier provides a reference for the captivating science that can be carried out in this decade and next. The Intensity Frontier program has the potential to make discoveries that change paradigms and alter our view of the Universe.

1.3 Energy Frontier

Experiments at the Energy Frontier make use of high-energy accelerators to produce and study heavy elementary particles and to search for new ones. The Energy Frontier includes experiments at the Large Hadron Collider at CERN and those at future colliding-beam accelerators proposed for lepton-lepton and proton-proton collisions.

The first run of the LHC has closed a nearly half-century-old chapter in the story of elementary particle physics. We have discovered a most unusual new particle with properties very similar to those expected of the Standard Model Higgs boson. The appearance of this particle — and further confirmation of its identity —

ends one era and opens another. On one hand, the Standard Model of particle physics is complete. We know all of the particles in this model and how they interact with one another and we have at least a basic idea of their properties. On the other hand, we also know that the Standard Model is incomplete in important ways. It challenges us to uncover the physics behind its apparently ad hoc structure. We are certain that a host of observed anomalous phenomena and set of confusing conceptual questions have explanations that require new physics outside the Standard Model.

The LHC and the CMS, ATLAS, and LHCb detectors have brought to bear impressive capabilities for exploring the answers to these new questions. The LHC accelerator is expected to dramatically increase its ability to deliver beams in the period between now and 2030, increasing its energy by almost a factor of two and its integrated luminosity by a factor of 100. The detectors will improve their ability to collect enormous data sets and to discriminate the properties of events with increasing precision. Around the world, other new accelerators are being considered that will give us additional power in understanding the heaviest particles of the Standard Model and exploring for new ones. In this report, and in the detailed working group reports, we trace out the programs of these accelerators and present their most important goals.

Importance of the TeV energy scale: Our successful theory of weak interactions is based on the idea of an underlying symmetry that is spontaneously broken. The symmetry of the theory of weak interactions dictates the couplings of the quarks and leptons to the W and Z bosons. Its predictions have been confirmed by high-precision experiments. However, this symmetry forbids the quarks, leptons, and vector bosons from having mass. To reconcile the symmetry of weak interactions with the reality of particle masses, one more unexpected element is required. This is a field or set of fields that couples to all types of particles and forms a condensate filling the Universe. The discovery of the Higgs particle establishes that this condensate exists and is the origin of particle masses.

This is an historic achievement. It is not an end but a beginning. It highlights many questions that the Standard Model leaves unanswered. These require new, equally bold ideas. Two of these questions — the nature of the Higgs field and the composition of dark matter — give particularly strong motivations for collider experiments.

The Standard Model does not explain the underlying structure of the Higgs field or the reason why it condenses. It does not explain the magnitude of the condensate, which sets the mass scale of all known elementary particles. The fact that the observed Higgs particle is a scalar particle makes it very difficult to understand why this scale is smaller than other basic mass scales of nature such as the Planck scale. There are no simple models that answer this question. New fundamental structures are needed. The Higgs field must be a composite of more basic entities, or space-time itself must be extended, through supersymmetry or through extra dimensions of space. These ideas predict a rich spectrum of new elementary particles, typically including a larger set of Higgs bosons, with masses at the TeV energy scale.

The Standard Model also does not account for the dark matter that makes up most of the matter of the Universe. The simplest and most compelling model of dark matter is that it is composed of a stable, weakly interacting, massive particle (WIMP) that was produced in the hot early Universe. To obtain the observed density of dark matter, this model requires the WIMP interactions to be roughly at the TeV energy scale. If this model is correct, it may be possible to study dark matter under controlled laboratory conditions in collider experiments.

Compelling ideas about fundamental physics predict new particles at the TeV energy scale that should be discoverable in experiments at the LHC and planned future accelerators. These experiments will provide the crucial tests of those ideas. Furthermore, if such particles are discovered, they can be studied in detail in collider experiments to determine their properties and to establish new fundamental laws of nature.

The past successes of particle physics and its current central questions then call for a three-pronged program of research in collider experiments:

1. We must study the Higgs boson itself in as much detail as possible, searching for signs of a larger Higgs sector and the effects of new heavy particles.
2. We must search for the imprint of the Higgs boson and its possible partners on the couplings of the W and Z bosons and the top quark.
3. We must search directly for new particles with TeV masses that can address important problems in fundamental physics.

The Energy Frontier study pointed to all three of these approaches as motivations for further experiments at colliders. The results of the study confirmed that the existing LHC detectors and their planned upgrades, together with proposed precision lepton collider experiments, will be nimble and sensitive enough to carry this three-fold campaign forward into the next two decades.

The Energy Frontier study was organized into six working groups — on the Higgs boson, the W and Z bosons, quantum chromodynamics (QCD), the top quark, new particles and forces, and flavor interactions at high energies. Each working group was asked to evaluate the future program for its topic both from a high-level perspective and from the viewpoint of supplying motivation for experiments at a range of proposed accelerators. In the remainder of this section, we present the conclusions of these reports, first by physics topic, then by facility.

Higgs boson: A new bosonic resonance at 125 GeV was discovered at the LHC only one year ago. Many properties of this particle have now been measured and, up to this point, are consistent with those of the Higgs boson of the minimal SM. The couplings of this boson roughly scale with mass. The specific form of the coupling to the Z boson indicates that the particle has spin-parity 0^+ and that the corresponding field has a nonzero vacuum expectation value.

However, we cannot be complacent about the identity and role of this particle. On one hand, the idea that a single scalar field is solely responsible for the generation of all particle masses is just one possibility among many and needs explicit verification. On the other hand, models with additional Higgs bosons and related new particles, and models in which the Higgs boson is composite, are hardly tested. Deviations from the minimal Higgs boson properties due to new particles with mass M are suppressed by a factor $(m_h/M)^2$, so to the extent that the LHC has set lower limits on the masses of new particles at many hundreds of GeV, we would not yet have expected to see the modifications to the Higgs properties caused by those particles.

An experimental program to probe the Higgs boson contains several elements. The first is to search for deviations from the minimal SM expectation that the Higgs boson couples to each particle species according to its mass. Such deviations are expected in almost all models of new physics. However, the effects are expected to be small, at the few-percent level if induced by new particles that will not be directly detected at the LHC. There is a characteristic pattern of deviations for each new physics model. The High-Luminosity LHC (HL-LHC) is expected to measure these couplings with precisions of several percent, varying from coupling to coupling. Lepton collider experiments have the potential to push these precisions to the sub-percent level, which would be needed to uncover deviations from Standard Model predictions with significance high enough to claim evidence of new physics.

Such a program of precision measurements of Higgs couplings requires a parallel concerted effort in precision theory. It also requires improvement of our knowledge of crucial input parameters such as α_s and m_b , which can be provided by lattice gauge theory computations. Collider experiments can also probe the nonlinear

Higgs field self-coupling to the 10–20% level, thereby testing the critically important question of the shape of the Higgs potential.

Future experiments should also improve our knowledge of the Higgs boson mass and quantum numbers. The spin of the observed resonance should already be clear from LHC data in this decade. A more subtle question is whether this particle contains a small admixture of a CP-odd state, signaling CP violation in the Higgs sector and confirmation of at least one additional Higgs-like particle. We discuss probes for this effect at various colliders.

Finally, it is important to search directly for additional Higgs bosons. The LHC can probe to masses of 1 TeV with model-dependent limits. Lepton colliders can make more model-independent searches to masses close to the collider beam energy.

W and Z boson, QCD, and the top quark: The study of W and Z bosons includes both the extension of the program of precision electroweak measurements, and the search for new interactions in the three- and four-vector boson couplings.

The minimal SM makes precise predictions for the well-studied precision observables M_W and $\sin^2 \theta_W$. At the moment, the observed values are within 2σ of the predictions; the deviations are consistent with the effects of new particles in a range of new physics models. Better precision in this program is clearly needed. Future experiments will sharpen our knowledge of these quantities and potentially expose inconsistency with the SM. The LHC, especially in its high-luminosity phase, has the potential to reduce the error on the W mass to ± 5 MeV. This requires a factor of 7 decrease in the current error due to parton distribution functions and is a challenge to QCD researchers. Lepton colliders can make further improvements, to an error of ± 2.5 MeV, with a dedicated measurement of the WW threshold. A linear collider with beam polarization running at the Z resonance to produce 10^9 Z bosons (Giga- Z) is expected to reduce the error on $\sin^2 \theta_W$ by a factor of 10. Finally, a circular e^+e^- collider operating in a 100 km tunnel can potentially push both errors down by another factor of 4. All of these precision measurements challenge the inflexible correlations among the SM particles and their respective forces. Such precision measurements of electroweak observables could become discoveries of new physics if the tight constraints within the SM begin to unravel.

The second theme of W and Z boson studies is the search for anomalous nonlinear couplings of the vector bosons. Collider experiments with enough energy to produce pairs of W and Z bosons are sensitive to three-gauge-boson couplings. At the LHC, we will be sensitive, for the first time, to non-standard four-boson interactions, which would indicate new interactions in vector boson scattering. Lepton collider experiments have the potential to push current uncertainties on three-boson couplings down by an order of magnitude, into the region in which new physics effects are predicted in models in which the Higgs boson is composite. Both hadron and lepton colliders can access vector boson scattering, but the total center-of-mass energy available in a scattering process is a crucial factor. The high-luminosity LHC will be sensitive to vector boson or Higgs resonances with masses well above 1 TeV.

QCD is well established as the correct theory of the strong interactions. Nevertheless, advances in QCD are needed to achieve the goals of future experiments, especially at hadron colliders. These experiments require improved knowledge of the parton distribution functions. That can be achieved with data expected from the LHC on the rapidity distributions of W , Z , and top quark production. In addition, precision cross-section computations, to the NNLO level, are needed for many two- and three-particle production processes, especially those involving the Higgs boson. This will require advances in the theoretical art of QCD computation. Finally, it is important to push the error on the value of α_s below the 0.5% level. Lattice gauge theory seems to be a promising avenue for achieving this goal.

The top quark was discovered at the Fermilab Tevatron and studied there with samples of tens of thousands of $t\bar{t}$ pairs. The LHC experiments will produce and study billions of top quarks. At future lepton colliders,

we will use the electroweak couplings of top quarks as a production mode and probe these with polarization observables. Both methods will transform our knowledge of this quark, whose properties are intimately connected to the mysteries of flavor and mass generation. To this day, we are surprised at the high mass of this presumably fundamental particle and its proximity to the value of the Higgs vacuum expectation value.

The top quark mass is not only an important puzzle in itself but also is an important input parameter for particle physics. The strongest demands on precision in the top quark mass come from the precision electroweak program, where interpretation of a 5 MeV error in m_W requires a 500 MeV error on m_t . This mass must be a theoretically well-defined quantity, convertible to a short-distance parameter such as the $\overline{MS}$ mass. There are strategies applicable at the LHC that allow the measurement of a well-defined top quark mass to this 500 MeV accuracy. At lepton colliders, measurement of the cross section at the top quark pair production threshold gives the $\overline{MS}$ mass to 100 MeV, as required for the more accurate precision electroweak program available at these machines.

Top-quark couplings will be studied with high accuracy both at hadron and at lepton colliders. New physics from top quark and Higgs compositeness can create few-percent corrections to the gluon, photon, and, especially, Z boson couplings. These effects can be observed as corrections to the pair-production cross sections relative to the predictions of the SM. The top-quark coupling capabilities of a lepton collider are especially strong, with accuracies possible at the sub-percent level. The billions of top quarks produced at the high-luminosity LHC allow very sensitive studies of rare flavor-changing top decays, to a level that complements searches at low energy for flavor-changing quark decays.

Models of the Higgs potential and its symmetry breaking typically require new particles that are partners, in some way, of the top quark. The LHC, especially in its high-luminosity stage, will have the capability for extensive searches for supersymmetric partners of the top quark, heavy vector-like top quarks that appear in models with Higgs and top quark compositeness, and heavy resonances that decay to $t\bar{t}$, which appear in models with new space dimensions.

Searches for new particles and interactions: High-energy colliders can search for new particles with a very broad range of properties. These particles, with masses near the 1 TeV scale, are required in models of electroweak symmetry breaking. Other questions also call for new particles accessible to high energy colliders. A large class of models of dark matter place the dark matter particle as the lightest particle of a TeV mass spectroscopy. Grand unification requires new particles near the TeV scale, including partners of known particles and perhaps also new vector bosons associated with enhanced gauge symmetry. CP violation in the Higgs boson sector is required in models that generate the matter-antimatter asymmetry at the electroweak phase transition. More generally, new particles can bring new sources of flavor and CP violation that might be reflected in the discovery of new flavor-changing reactions at low energy.

The LHC has already, in only its first run, increased the reach and power of searches for new particles over a broad scope. We expect that this power will increase dramatically in the next decade, as the LHC experiments acquire 300 fb^{-1} of data at 14 TeV. This extension probes deeply into the region expected for the masses of new particles in all classes of models of electroweak symmetry breaking. The high-luminosity stage of the LHC, up to 3000 fb^{-1} , will provide a further, very significant, extension of the search region. This extension is particularly powerful for states produced through electroweak interactions, for which a factor of 2 increase in the mass reach is available in some cases.

Lepton colliders would bring new and complementary capabilities. They would carry out model-independent searches for states such as dark matter candidate particles whose signatures are especially difficult to observe at hadron colliders. Lepton colliders would uncover new decay modes and measure branching ratios and quantum numbers for any new particle within their energy range.

Physics opportunities for colliders: The physics opportunities described above are reflected as motivations for current and future high-energy colliders. Our study considered a wide range of proposed machines. The full report from the Energy Frontier presents the cases for these machines in some detail.

We first point out the opportunity provided by the 14 TeV run of the LHC scheduled for the next decade. This will provide robust searches for new particles over a broad front, with great promise of the discovery of the TeV particle spectrum motivated at the beginning of this section. Any plan for high energy physics in the longer term must include the possibility of discovering new particles in this period and exploiting that discovery at the facilities that will follow.

We find the case for the high-luminosity stage of the LHC compelling. This plan to deliver 3000 fb^{-1} has been listed in the European Strategy for Particle Physics as the highest priority accelerator project in Europe for the 2020's. We find that it will provide a significant additional step in the search for new particles, and that it will provide other important capabilities. The most important of these is the beginning of the era of precision Higgs boson measurements, to few-percent precision. It is likely to give the first evidence of the Higgs boson self-coupling. It will provide a program of precision measurement in the SM that will dramatically tighten our knowledge of the W boson and the top quark, with measurements sensitive to the predictions of a variety of new physics models. We have already noted that the additional luminosity will significantly enhance the capability of the LHC to search for new heavy particles.

We considered the scientific case for the International Linear Collider (ILC). This next-stage lepton collider has recently completed its Technical Design Report and was judged in the Snowmass study to be ready for construction. This facility is named as the highest priority for new initiatives by the Japanese particle physics community. We find that this machine is strongly motivated. It will reach sub-percent accuracy in the study of the Higgs boson, allowing discovery of percent-level effects in the Higgs couplings predicted in new physics models. It will measure the Higgs width in a model-independent way. It will give the capability to observe all possible Higgs modes, including decays to SM modes not observable at the LHC, to dark matter, and to other invisible and exotic states. It will extend our knowledge of the top quark and the W and Z bosons well beyond the precision achievable at the LHC, setting up a confrontation with models that include Higgs boson and top quark composite structure.

The Energy Frontier study considered many other accelerator facilities for construction over longer time scales. These included higher-energy linear colliders, circular e^+e^- colliders, muon colliders, and photon colliders. We present a detailed discussion of the physics motivations for these facilities in our full report. There was particular interest in a proton collider of energy 100 TeV (VLHC), which would come close to the capability of covering the full model space for models of “natural” electroweak symmetry breaking and WIMP dark matter. Our study developed materials and resources to begin a more complete survey of physics at such a high-energy collider. This study, and a parallel development of magnet technology for higher-energy proton colliders, should be pursued over the next decade.

Conclusions: Previous surveys of the prospects for high-energy accelerator experiments have spoken in terms of reducing the space of parameters — couplings, mixings, masses — as if that were the goal. Now, more than ever, the momentum points not toward exclusion, but toward the discovery of new states. Many possible directions are open and must be pursued.

The Higgs boson discovery changes everything. It transforms the research agenda for particle physics, giving us a set of sharp questions that we cannot ignore. It motivates more strongly the exploration of the TeV energy scale, where the solution to the mystery of dark matter and other key problems might also be found. The study of the Higgs particle to high precision, together with high-precision studies of the W , Z , and top quark and searches for new states, provide us with complementary routes to fully explore the particles and

forces in this range of energies. The current LHC detectors and their planned upgrades are well suited to carry on this program. Future accelerators will bring new capabilities to pursue it further.

High-energy colliders provide manifest opportunities to discover new fundamental interactions of broad consequence. U.S. physicists have been leaders in Energy Frontier experiments up to now and are well positioned to take a leading role in the discoveries of the coming decades.

1.4 Cosmic Frontier

Investigations at the Cosmic Frontier use the Universe as a laboratory to learn about particle physics. Our understanding of the Universe has been transformed in recent years. In particular, experiments at the Cosmic Frontier have demonstrated that only 5% of the contents of the Universe are well understood, with the rest composed of mysterious dark matter and dark energy. As a result, the Cosmic Frontier now plays a central role in the global particle physics program, providing overwhelming evidence for new particles and new interactions, as well as powerful, unique opportunities to address many of our most fascinating questions: What is dark matter? What is dark energy? Why is there more matter than antimatter? What are the properties of neutrinos? How did the Universe begin? What is the physics of the Universe at the highest energies?

To identify outstanding scientific opportunities for the coming 10 to 20 years, the Cosmic Frontier Working Group was organized into six subgroups: Weakly-interacting massive particle (WIMP) dark matter direct detection, WIMP dark matter indirect detection, non-WIMP dark matter, dark matter complementarity, dark energy and CMB, and cosmic particles and fundamental physics. In several cases, these subgroups were further divided into topical working groups.

The Λ CDM standard model of cosmology provides the backdrop for much of Cosmic Frontier research. In this model, the Universe underwent a very early epoch of accelerated expansion (inflation), which was followed by eras in which the Universe was dominated successively by radiation, cold dark matter (CDM), and dark energy (Λ). At present, the known particles make up only 5% of the energy density of the Universe, with neutrinos contributing at least 0.1%. The rest is 25% dark matter and 70% dark energy. Remarkably, incisive measurements that explore all of the key components of the model are now within reach. The leaps in sensitivity of the new facilities bring us to a time with strong discovery potential in many areas. Further surprises are likely in this rapidly advancing area, with potentially far-reaching consequences.

Dark matter: The work of Snowmass highlighted the coming decade as one of particular promise for the goal of identifying dark matter. Evidence for particle dark matter has been building for 80 years through the study of galaxy clusters, galactic rotation curves, weak lensing, strong lensing, hot gas in galaxy clusters, galaxy cluster collisions, supernovae, and the cosmic microwave background (CMB). However, all evidence so far is based on dark matter's gravitational interactions, and its particle identity remains a deep mystery.

Among the many dark matter candidates, one well-known possibility is weakly-interacting massive particles with masses in the 1 GeV to 100 TeV range. Particles with these properties appear in many models designed to address the gauge hierarchy problem. In cosmology, particles with these properties may obtain the correct relic density either through thermal freeze-out or through an asymmetry connecting their number density to that of baryons.

WIMP direct detection experiments search for the interactions of WIMPs with normal matter. WIMPs may scatter elastically off nuclei, producing recoil energies in the 1–100 keV range, which can be detected through phonons, ionization, scintillation, or other methods. There are daunting backgrounds, and direct detection experiments must be placed deep underground. In the last several years, however, this field has

seen a burgeoning of innovative approaches to discriminate signal from background, including experiments incorporating dual-phase media, self-shielding, pulse shape discrimination, and threshold detectors.

The first two decades of direct detection experiments have yielded a diverse and successful program, resulting in “Moore’s Law”-type progress, with sensitivities doubling roughly every 18 months. In the coming decade, this rate of progress is expected to continue or even accelerate for both spin-independent and spin-dependent interactions. Upcoming second-generation (G2) experiments will improve sensitivities by an order of magnitude, probing the Higgs-mediated cross sections expected for well-known supersymmetric and extra-dimensional candidates, and also extending the sensitivity to both $\sim \text{GeV}$ low-mass WIMPs, where possible signals have been reported, and $\sim \text{TeV}$ masses that are beyond the reach of colliders. Following these experiments, multi-ton-scale third-generation (G3) experiments are expected to improve current sensitivities by up to three orders of magnitude and will either find dark matter or detect background events from solar, atmospheric, and diffuse supernovae neutrinos. Probing beyond this sensitivity will require either background subtraction or techniques such as directional detection or annual modulation. The Snowmass process produced a detailed census of present and proposed direct detection facilities, with uniform treatment of their capabilities and issues, along with a survey of promising technologies.

WIMPs may also be found through indirect detection, in which pairs of WIMPs annihilate, producing SM particles, including gamma rays, neutrinos, electrons and positrons, protons and antiprotons, and deuterons and antideuterons. Detection of these particles may be used to constrain or infer dark matter properties. The expectation that WIMP annihilation in the early Universe determines the dark matter abundance sets a natural velocity-averaged annihilation cross section of $\langle \sigma_{\text{an}} v \rangle \sim 3 \times 10^{-26} \text{ cm}^3 \text{s}^{-1}$ for indirect detection experiments.

Gamma rays from dark matter annihilation may be detected by both space- and ground-based experiments. In space, the Fermi-LAT has recently demonstrated the promise of this approach, excluding the natural cross section $\langle \sigma_{\text{an}} v \rangle$ for dark matter masses below 30 GeV, given certain halo profile and annihilation channel assumptions. The reach is expected to be extended significantly with additional data. On the ground, VERITAS and other atmospheric Cherenkov telescopes have set significant limits on dark matter properties by looking for gamma rays from dark matter-rich dwarf galaxies. Moving forward, the atmospheric Cherenkov telescope community has coalesced to build the Cherenkov Telescope Array (CTA), with sensitivity at the natural cross-section scale for dark matter masses from 100 GeV to 10 TeV, far beyond current or planned colliders. These projections require U.S. involvement in CTA, which will double the planned mid-sized telescope array and enable critical improvements in sensitivity and angular resolution.

Neutrinos also provide promising means for indirect detection of dark matter. High-energy neutrinos from the core of the Sun would be a smoking-gun signal of dark matter particle annihilation. The signal depends primarily on the spin-dependent WIMP-nucleon scattering cross section, which determines the capture rate. Current bounds from Super-K in Japan and IceCube at the South Pole already provide leading limits on this cross section, and PINGU, an infill array upgrade to IceCube, will extend the sensitivity to lower masses. In the coming decade, IceCube and PINGU, along with Hyper-Kamiokande, will probe cross sections one to two orders of magnitude below current bounds, with sensitivities competitive with those of planned G2 direct detection experiments.

Antimatter signals of dark matter are pursued in a variety of ways. Recent measurements of cosmic-ray positrons by the AMS-02 magnetic spectrometer confirm and improve with excellent precision earlier measurements by PAMELA and Fermi. The rising positron fraction could be indicative of positrons created in the decay or annihilation of dark matter. In the near future, AMS-02 will extend its determination of the positron fraction to energies close to 1 TeV, and add important information on cosmic-ray propagation. Given the possibility of astrophysical sources of primary positrons, however, it may be very difficult to definitively attribute the excess positrons to dark matter. Antideuterons provide a signal that is potentially

more easily discriminated from astrophysical backgrounds. With a long-duration balloon flight, the General Antiparticle Spectrometer (GAPS) detector could provide sensitivities comparable to AMS-02. Last, the production of positrons and electrons from dark matter annihilation also produces secondary radiation. Detection of signals with radio to X-ray frequencies has the potential to probe the WIMP parameter space.

The Snowmass process also evaluated the prospects for non-WIMP candidates, which could be some or all of the dark matter. The axion is particularly well-motivated, as it arises from the leading solution to the strong CP problem of the SM. RF-cavity and solar searches for axions, such as ADMX and IAXO, will probe a large range of axion parameter space, including the cosmologically favored region, and have strong discovery potential. Sterile neutrinos are also highly motivated by the observed non-zero masses of active neutrinos. In the mass range where sterile neutrinos are dark matter candidates, their radiative decays produce a monoenergetic photon, which may be detected with X-ray telescopes. Many other dark matter candidates were also surveyed, including asymmetric dark matter, primordial black holes, Q-balls, self-interacting dark matter, superheavy dark matter, and superWIMP dark matter.

How do the diverse strategies for identifying dark matter fit together? The Snowmass process produced a clear articulation of how the different approaches — including the direct and indirect detection experiments mentioned above, but also particle colliders and astrophysical probes — each provide unique and necessary information. This complementarity was examined in two theoretical frameworks. First, the discovery prospects were examined in complete supersymmetric models, with randomly selected parameters in the phenomenological MSSM framework. Second, the possibility that only the dark matter particle is kinematically accessible was considered using the framework of dark matter effective theories. In both cases, the complementarity of different approaches was evident at all levels, both to establish a compelling dark matter signal and, just as importantly, after discovery, to determine the detailed properties of the particle or particles that make up dark matter.

Dark energy and CMB: Cosmic surveys — optical imaging and spectroscopic surveys and detailed measurements of the CMB — precisely map the Universe on many different angular scales and over wide ranges of cosmic time. They provide unique information about cosmology and new physics, including inflation, dark matter, dark energy, and neutrino properties. These measurements are challenging, requiring advances in instrumentation and excellent control of systematic effects. Fortunately, these advances are now within reach, thanks to decades of investment and close collaborations between particle physicists and astrophysicists. The payoffs for this effort are large.

Measurements of the distance-redshift relation, first using supernovae and then additional complementary techniques, revealed the expansion history of the Universe, particularly over the past several billion years, and yielded the surprising discovery that the expansion rate has been increasing instead of decreasing. Now we must determine what is causing the cosmic acceleration. This “dark energy” must produce negative pressure to be responsible for the observed effect. One important clue is whether the negative pressure has been constant in time or is evolving. The stage III (the DES and HSC imaging surveys, and the PFS and eBOSS spectroscopic surveys) and stage IV (LSST imaging survey and DESI spectroscopic survey on mountaintops; Euclid and WFIRST-AFTA in space) dark energy facilities will constrain both the value and the evolution of the value with much higher precision, as recommended in previous community studies, but they will also do much more. We must also check whether our description of gravity is correct, and this is where measurements of the growth of structure, over a wide range of distance scales using both imaging and spectroscopic surveys, are needed.

There are several alternatives to general relativity (GR) that can accurately describe the observed distance-redshift relation, but they also modify the behavior of gravity over different distance scales. The alternative models therefore predict structure growth rates that are different from those in the standard theory. Measuring the structure growth rate over many different distance scales will test GR and the alternative models.

Deviation from expectation on just one of these scales will signal new physics. In other words, the upcoming dark energy facilities, particularly at stage IV, where systematic error management is built deeply into the design, will provide many precise tests and will characterize the behavior of dark energy beyond merely a single parameter value and its evolution with time. We will know the strength of the effects in a two-dimensional parameter space of distance and cosmic time, as well as any deviations from expectations in their correlations. Further surprises may await us.

Inflation is the leading paradigm for the dynamics of the very early Universe, and current observations of large-scale structure lend support to this intriguing idea. The most direct available probes of inflation come from CMB observations, and the overall agreement is remarkably good. However, it has not been possible to explore the underlying physics of inflation until now: The coming generations of CMB experiments will have sufficient sensitivity to falsify large classes of models. The signal is a characteristic pattern with non-zero curl (called “*B* mode”), faintly imprinted on the polarization of the CMB fluctuations, due to gravitational waves produced during the epoch of inflation. The shape of the potential of the scalar field driving inflation directly affects the spectrum of gravitational waves and hence the strength of the imprint, r (the ratio of tensor to scalar power), over characteristic angular scales on the sky. The current generation of experiments is sensitive to $r \sim 0.1$, but over the next 10 to 20 years, improvements of two orders of magnitude are possible by scaling the number of detectors by similar factors, from $\sim 10^3$ (current) to $\sim 10^4$ (generation III) to $\sim 5 \times 10^5$ (generation IV). This would require a change from the way things have been done in the past. Groups would merge into one coordinated effort, tapping national lab facility design, integration, computing, and management capabilities.

In addition, future optical and CMB cosmic surveys, as well as future polar-ice neutrino projects (see below), will provide precise information about neutrino properties, including the mass hierarchy, the number of light neutrinos, and the sum of the neutrino masses. Combining this with information from accelerator- and reactor-based neutrino experiments, as well from experiments searching for neutrinoless double-beta decay, will accelerate our understanding of fundamental neutrino properties and enable us to understand the implications of apparent inconsistencies.

Snowmass provided an excellent opportunity to address common problems and to develop a common vision for the potential of cosmic surveys to advance particle physics. Highlights included developing detailed strategies to distinguish dark energy from modified gravity; exploiting the complementarity of probes for determining the key cosmological parameters; understanding more deeply the strengths and ultimate limitations of the different techniques; and discussing the planned facilities, which are the result of intensive community processes over many years. The group articulated a set of goals: (1) remain a leader in dark energy research, (2) build a generation IV CMB polarization experiment, and (3) extend the reach of cosmic surveys with targeted calibration campaigns, targeted R&D, and support for work at the interface of theory, simulation, and data analysis.

Cosmic particles: Measurements of fluxes of cosmic particles (charged particles, photons, and neutrinos) also address many topics in particle physics beyond indirect dark matter searches. Recent results include the detection by IceCube of very high-energy neutrinos that are likely to be from astrophysical sources; the observation of the GZK suppression in the cosmic-ray flux above 3×10^{19} eV; the measurement of the positron fraction up to 300 GeV, suggesting the existence of primary sources of positrons from astrophysical processes and/or dark matter interactions; and confirmation that supernova remnant systems are a source of galactic cosmic rays. These and other discoveries were made by the current generation of experiments.

Goals for the coming decade include determining the origin of the highest energy particles in the Universe, measuring interaction cross sections at energies unattainable in terrestrial accelerators, detecting the GZK neutrinos that arise from the interactions of ultra-high-energy cosmic rays with the CMB, determining the neutrino mass hierarchy, and searching for other physics beyond the Standard Model.

To meet these goals, the group recommends: significant U.S. participation in the Cherenkov Telescope Array (CTA), which is the next-generation ground-based gamma-ray facility; simultaneous operation of Fermi, HAWC, and VERITAS, the current generation of space- and ground-based U.S.-led gamma-ray facilities; construction of the PINGU neutrino detector to lower the energy threshold to a few GeV and enable the determination of the neutrino mass hierarchy using atmospheric neutrinos; continued operation of the Auger and Telescope Array air shower arrays with upgrades to enhance the determination of the composition of the flux of cosmic rays around the GZK suppression region; construction and deployment of the JEM-EUSO mission aboard the International Space Station to extend observations of the cosmic ray flux and anisotropy well beyond the GZK region; and construction of a next-generation ultra-high-energy GZK neutrino detector, which will either detect GZK neutrinos (and constrain the neutrino-nucleon cross section at ultra-high energy) or exclude all but the most unfavorable parts of the allowed parameter space. A detailed census of present and proposed cosmic particle measurement facilities was produced during the Snowmass process.

Conclusions: In synergy with the other frontier areas, the Cosmic Frontier provides to particle physics clear evidence for physics beyond the Standard Model; profound questions of popular interest; frequent new results; surprises, with broad impact; a large discovery space with unique probes; important cross-frontier topics; and a full range of project scales, providing flexible programmatic options. For each area of the Cosmic Frontier, the Snowmass study identified essential technologies and facilities, the advances required in theoretical models, and experiments with great promise. The largest projects are, appropriately and necessarily, international. The U.S. is still the leader in many areas of the Cosmic Frontier, but this field is evolving quickly and other regions with intensive interest in this physics are advancing rapidly.

1.5 Theory

This section summarizes the report of the Snowmass Theory Panel. The DPF constituted this panel with the goal of understanding both the scientific problems and opportunities of the next decade, and the challenges involved in sustaining a first-class theory program in the U.S.

Theoretical physics has played a crucial role in particle physics since its earliest days. Theorists developed the basic framework in which we understand elementary particles: quantum field theory. This framework embodies Einstein's principles of special relativity and locality of interactions within the laws of quantum mechanics. It is extraordinarily successful. Theorists appreciated the role of symmetries as organizing principles for understanding data and clues to the nature of physical law. They developed calculational methods for quantum field theories, permitting the computation of scattering amplitudes, bound state masses, and numerous other quantities, often with extremely high precision. These developments combined to both produce and test the Standard Model.

The discovery of a scalar particle at the LHC may well mark the completion of the Standard Model. This object is likely the Higgs boson of the simplest version of the theory. Theorists have played and will continue to play essential roles in firmly establishing the identity of this object. Its study at colliders requires not only great experimental ingenuity and persistence, but also an array of theoretical tools for calculating the rates for its production and decay. Just as crucial are techniques for the calculation of the large backgrounds arising from other Standard Model processes.

While theoretical studies of quantum field theory and the SM have a long history, two areas have seen extensive progress in the last decade, and will continue to be the subjects of intense development. The first of these is perturbative methods for the calculation of scattering amplitudes. In recent years, calculations essential for collider physics, and previously believed essentially impossible, have been carried through using a range of new methods. These computations played a crucial role in the discovery of the Higgs boson, and

are vitally important in searches for new physics. The development of new methods, and their application in LHC and other experiments, as well as in theoretical investigations, will remain a major activity in the field in the coming years.

Another area of striking progress has been lattice gauge theory. This is the principal tool we have for the quantitative study of the strong interactions in processes at low energies. It is now possible to compute the spectrum of hadrons with high accuracy, and lattice computations have been crucial in the measurement of the properties of heavy quarks. Continuing improvements in calculational methods are anticipated in coming years. At Snowmass we heard about new applications of lattice methods, such as prospects for computations essential to any convincing interpretation of results from the upcoming muon $g - 2$ experiment.

When we say the Standard Model may now be “complete” we mean that the theory is consistent to much higher energies. But there are strong reasons to believe that, at energies not much higher than those we probe today, there should be new phenomena. Theorists have been the drivers in formulating these questions, and proposing possible solutions. Among the questions are:

1. Why are there vastly disparate mass scales in nature, such as the Planck mass and the weak scale?
2. Why are neutrinos light?
3. Are neutrinos their own antiparticles?
4. What is the origin of the asymmetry between matter and antimatter?
5. What is the identity of dark matter?
6. What is the identity of dark energy?
7. What is the origin of the curious pattern of quarks and leptons, and their masses?
8. Do the forces unify?
9. What modifications of our basic understanding are required to reconcile quantum mechanics and gravity?

For each of these questions, theorists have proposed answers. Many of these are the subject of present or planned experimental searches.

The Planck scale and the weak scale differ by at least 15 orders of magnitude. The difficulty of explaining the existence of such widely different scales is called the “hierarchy problem.” The large ratio of scales might be viewed as simply a “fact,” but within quantum field theory, this sort of hierarchy is generally quite unstable. (This is usually referred to as the “fine tuning” or “technical naturalness” problem.) Proposals to solve this problem all suggest physics at or near the TeV scale. Among the most explored of these is supersymmetry, a possible new symmetry of nature, which connects fermions and bosons. The LHC is actively searching for the new particles predicted by the supersymmetry hypothesis, and has excluded many popular models. Alternative proposals include the possibility that the Higgs particle is composite, or associated with phenomena in dimensions of space-time beyond the usual four. Over the next decade theorists will continue to explore these and other models, incorporating the constraints from experiment, or the results of discoveries.

We know that the Universe consists largely of forms of energy not contained within the SM: dark matter and dark energy. The case for dark matter, a form of matter which behaves, essentially, like dust and interacts extremely weakly with ordinary matter, has become compelling in the past decade. Theorists

have proposed several persuasive ideas for what the dark matter might be and how it was produced in the big bang. Supersymmetric models, in fact, naturally yield candidates (so-called weakly interacting massive particles, or WIMPs), which are automatically produced in roughly the right quantities. These particles are the subject of active experimental search at accelerators, deep underground, and in space. Theorists are working actively to survey the possible models and to understand exclusions and possible signals in ongoing and future experiments.

But there are other candidates for the dark matter. Perhaps the most prominent of these is the axion. This particle was proposed to explain perhaps the largest remaining puzzle of the strong interactions: the conservation of CP. The equations of QCD include a parameter, θ , a pure number, which violates CP. Exquisite experiments set a limit on the size of any electric dipole moment of the neutron, and this, in turn, requires $\theta < 10^{-10}$. One possible explanation for this small number is a new particle, called the “axion”, whose dynamics adjust θ to a value close to zero. It turns out that if the axion exists, it is also a candidate for the dark matter. Theorists continue to refine the axion theory, exploiting developments in field theory and in string theory, and to explore its properties. The ADMX experiment at the University of Washington is currently searching for this particle and has a good chance to find it if it exists.

Dark energy is equally mysterious. Representing about 70% of the energy budget of the Universe, this substance has *negative* pressure. Most theorists suspect that this is Einstein’s “cosmological constant”, and the data to date are consistent with this interpretation. But its value is very puzzling. Conventional ideas of quantum field theory suggest that there should be much more of it, and even more puzzling is the fact that its density is just such that it is becoming important in the current epoch of the Universe. These questions occupy the attention of many theorists, and there are a number of proposed answers, but it is safe to say that there is no compelling picture, and that this will certainly remain an active area of theoretical investigation for some time.

Neutrinos are now known to have mass, and we know some features of their masses (mass matrix). Neutrinos are far lighter than other particles, and their masses are not accounted for within the SM itself. Theorists have identified two possible mechanisms to generate neutrino mass. One is associated with new particle. These particles might have enormous masses. In this case, neutrinos are their own antiparticles. Alternatively, there might be extremely light additional degrees of freedom. This is a question that can be tested experimentally, and which has several theoretical consequences. Understanding the neutrino masses and mixings will be a central part of both the experimental and theoretical particle physics programs over the next decade.

Within the 5% of the energy budget which consists of ordinary protons and neutrons (baryons), there is a further puzzle: why is there matter at all, i.e., why didn’t the Universe emerge from the Big Bang with equal amounts of matter and antimatter? With the discovery of CP violation 50 years ago, it was recognized (first by Andrei Sakharov) that this is a question that can be addressed by science. Theorists have understood that the SM, however, does not violate CP sufficiently to account for the observed asymmetry; additional degrees of freedom (particles) are an essential component. They have put forward a number of proposals for how the asymmetry might arise. Some of the most compelling lie within the frameworks of theories of lepton mass (“leptogenesis”) and supersymmetry. These ideas might have observable consequences for the cosmos, such as the emission of gravitational waves, and for experiments at accelerators.

Much of the history of particle physics has been tied up with the problem of “flavor”: the existence of three generations of quarks and leptons, and the features of their masses and mixings. Theorists have been central to this subject, proposing the idea of quarks and explaining the problem of mixing. In recent decades, they have developed theoretical tools to understand the behavior of heavy quarks, and a range of ideas for how the repetitive structure of quarks and leptons might emerge. These include ideas involving new symmetries, grand unification, string theory and extra dimensions. While many of these ideas are plausible, none are, as

of yet, compelling in themselves, and these issues — dealing with the dynamics of heavy quarks and seeking an understanding of the basic issues of flavor — will be the focus of important activities in the next decade.

The unification of forces is a long-standing dream. In the past few decades, theorists have put forward concrete ideas about how this might arise and proposals for experiments that could test the possibility. With supersymmetry, quite remarkably, the gauge couplings of the SM unify at a high energy scale, and many proposals have been put forward for an underlying explanation. A simple and compelling set of ideas of this type go by the name “grand unified theories.” These elegantly enlarge the structure of the SM. The most dramatic consequence of all of these proposals is the prospect of proton decay, which has been the subject of extensive experimental search. Ideas of unification have also led to a rich set of theoretical questions, including the existence of magnetic monopoles.

Beyond grand unified theories, the most ambitious attempt to unify the forces is associated with “superstring theory.” What is called string theory is part of a larger, only partly understood, structure which unifies Einstein’s general relativity and the other known forces in a quantum mechanical framework. While many questions are not yet answered, string theory has provided insight into longstanding questions in particle physics, including the unification of forces, the strong CP problem, dark matter, and dark energy. It has also inspired much interesting phenomenology, such as that associated with large extra dimensions. This is an area that will continue to occupy a significant fraction of the community in the coming years, and which is likely to see significant additional progress.

Much of the panel’s effort was devoted, as required by its charge, to examining structural issues in theoretical physics. The panel’s report deals at some length with questions of funding. The panel was concerned that the current budgetary climate at both NSF and DOE puts in jeopardy the research program that we have outlined above. Most prominent among our concerns was support for postdoctoral fellows and students. Students and postdocs are important drivers of research, and clearly represent the future of the field. The panel recommends keeping the current level of support for productive research groups, of roughly one postdoc and one graduate student per two PIs.

The panel supports the program of comparative reviews, recently introduced by the DOE. This permits the agency to look critically at the support levels of individual theory groups, moving away from a model where funding levels were usually determined by making modest adjustments to historical levels of support. It permits the funding of new research groups and dropping groups which have become less productive (as the NSF has done historically). This is essential to adapting to the present funding climate.

The panel understands the need to increase the fraction of the DOE budget devoted to projects, but argues that this has particularly severe consequences for theoretical physics. We proposed that the DOE consider a project category aimed at theory, and in particular designed to sustain a suitable population of postdoctoral fellows. One suggestion is the creation of “theory networks,” loosely modeled on networks established in Europe. The DOE would call for proposals to compete to establish such networks, with a lifetime of three to five years. The central topics of investigation would be determined by the institutions, but we envision that they might range from intense, phenomenological efforts in areas like neutrino physics, to investigations of more foundational issues in field theory and string theory.

1.6 Accelerator Capabilities

The Accelerator Capability study is a synthesis of individual workshops of six working groups plus the collective Snowmass meeting of all interested participants. Each group addressed major challenges foreseen

for their respective class of accelerators in a pre-Snowmass meeting. The groups also considered a set of big questions regarding accelerator capabilities for the long-term future of high energy physics:

1. How can one build a collider at the 10 – 30 TeV constituent mass scale?
2. What is the furthest practical energy reach of accelerator-based particle physics?
3. How would one generate ten or more megawatts of proton beam power?
4. Can multi-megawatt targets survive? If so, for how long?
5. Can plasma-based accelerators achieve energies and luminosities relevant to particle physics?
6. Can accelerators be made an order of magnitude cheaper per GeV and/or per MW?

The results of the workshops formed the basis for draft reports from the working groups that were discussed in the general Snowmass meeting to form this consensus summary.

Hadron colliders: This working group focused on the evolution of the LHC and possible designs for a (much) higher energy proton collider (VLHC). The group considered: (1) how high a luminosity is possible for the LHC, (2) what are available increasing integrated luminosity without compromising experiments or detector survival, (3) how high an energy is possible in the LHC tunnel, (4) what impediments exist to designing a 100 TeV collider, and (5) what the associated accelerator research roadmap should be for hadron colliders.

The priority recommendation of our study is full exploitation of the LHC. Doing so requires a strong LHC Accelerator Research Program sponsored by the Office of High Energy Physics that transitions to a US-LHC high luminosity construction project. During the project period we recommend continuing a focused, integrated, laboratory program that emphasizes the engineering readiness of technologies suitable for a 26 TeV upgrade of the LHC or a machine of higher energy in a larger tunnel. The most critical technology development toward higher-energy hadron colliders is the next-generation high field Nb₃Sn magnets (limited to 15 Tesla) and adequate beam control technology to assure machine protection.

The reach of an LHC energy upgrade is constrained by the limits of Nb₃Sn technology and by the absence of engineering materials with high-field properties beyond those of Nb₃Sn. Moreover, even doubling the LHC energy in the present tunnel introduces substantial issues of synchrotron radiation management. Radiation management will become very difficult as the synchrotron power on the beam tube reaches 5 W/m.

To achieve energies beyond those of the LHC, the multi-laboratory study of VLHC remains valid. Snowmass has stimulated renewed effort on the VLHC in both the U.S. and Europe. American participation in the CERN-led international study for colliders in a large tunnel that will begin in 2014 will inform decisions to expand the reach of U.S. technology and guide research investments. The areas in which U.S. accelerator scientists can make the most valuable contributions are beam dynamics, superconducting magnets, vacuum systems, and machine protection.

Long-term, innovative research will expand the technical options for any future hadron collider. Dipoles with operating fields beyond 15 T need new conductor elements such as small-filament, high-temperature superconductors in continuous kilometer lengths. Better conductors, innovative stress management, and novel structural materials will enable even higher-field magnets with greater temperature margin. With ever more stored energy in the beams, better understanding and modeling of beam dynamics is essential to control beam halos and lost beam particles. Machine protection and design of beam abort dumps for multi-GJ beams will be challenging. Other issues for research include effects of marginal synchrotron radiation

damping, beam physics of the injection chain, effects of noise and ground motion, and options for interaction region design.

Energy-frontier lepton and photon colliders: Our study welcomes the initiative for the International Linear Collider (ILC) in Japan. The ILC would begin as a 250 GeV Higgs factory with future expansion to 500 GeV. The U.S. accelerator community is capable of contributing to the ILC as part of a balanced U.S. particle physics program. As described in its Technical Design Report (TDR), the ILC is technically ready to proceed to construction. The TDR incorporates leadership U.S. contributions to machine physics and technology in superconducting RF (SRF), high-power targets for positron production, beam delivery, damping ring design, and beam dynamics such as electron cloud effects. Extending the ILC to 1 TeV would require lengthened linac tunnels and added cryomodels. It would use the original ILC sources, damping rings, beam delivery systems, and beam dumps.

The excitement surrounding the Higgs boson discovery stimulated consideration of alternatives to a SRF Higgs factory. Concepts include a linear collider using copper linacs, a large-circumference e^+e^- ring, a compact muon collider ring, a photon collider, and e^+e^- linear colliders based on wakefield acceleration techniques. These concepts span a broad range of technical readiness (from requiring demonstration of feasibility to having a detailed conceptual design) and timescales upon which a machine could be constructed. They also have varying energy reach from the hundreds of GeV scale to the multi-TeV regime.

It is natural to investigate whether a 250 GeV Higgs factory could fit in the LHC tunnel. This option is undesirable because it interferes with LHC operations and because the beam physics is highly constrained. Assessment of a circular collider in a very large (of the order of 100 km) tunnel, with an energy reach up to about 400 GeV, will be part of the CERN-led study of large colliders mentioned above. Such a machine is a substantial extrapolation from existing and past storage rings, albeit from a large experience base. Beamstrahlung at the interaction point strongly couples energy reach and luminosity. The luminosity would be largest at the Z peak, but fall rapidly as the center of mass energy increases. Should the ILC not go forward over the next decade and should the renewed interest in a very large circumference hadron collider be sustained, the possibility of a circular Higgs factory deserves extensive consideration.

In a Higgs factory photon collider, two electron beams are accelerated to 80 GeV and converted to 63 GeV photon beams via inverse Compton scattering against low-energy (3.5 eV), high-intensity (5 J) laser pulses. The high-energy photon beams then collide to generate Higgs bosons through the s -channel resonance, $\gamma\gamma \rightarrow H$. A photon collider has the distinct advantage of requiring only an 80 GeV electron beam energy. Photon colliders could accompany proposed linear or circular colliders or be stand-alone facilities. The laser technologies overlap with those for laser wakefield accelerators.

Muon accelerators could provide world-leading experimental capabilities at energies from the Higgs s -channel threshold at 126 GeV up to the multi-TeV scale. A circular muon collider, if feasible, could reach such energies, because the larger mass of the muon suppresses synchrotron radiation. As muons at rest have a lifetime of 2.2 μ s, they will decay in flight. The short muon lifetime demands that beam creation, manipulation and acceleration to high energy be done rapidly; high-gradient acceleration is essential. An Energy Frontier muon collider would necessarily be relatively compact. Even a 5 TeV collider would fit on the Fermilab site. Critical beam physics issues are: (1) cooling the muon phase volume by 10^6 , and (2) accumulating 10^{12} μ^+ and μ^- bunches in the collider. A vigorous, integrated R&D program toward demonstrating feasibility of a muon collider (Muon Accelerator Program) is highly desirable. The current funding level is, however, insufficient for timely progress. Development of a muon collider capability would be closely connected with Intensity Frontier accelerators such as intense neutrino sources.

The Compact Linear Collider (CLIC) concept is based on 100 MeV/m copper linac technology; it would stretch 50 km for a 3 TeV collider. CLIC would be powered by two high-current drive beams running parallel

to the colliding beams through a sequence of power extraction and transfer structures, where they produce short, high-power RF pulses that are transferred into the accelerating structures. The practical energy reach depends on control of wakefields and on the accelerating gradient in industrialized accelerator sections. U.S. national laboratories have substantial expertise in CLIC technologies.

Yet another approach for multi-TeV energies proposes to use wakefields in plasmas or dielectric structures driven either by beams or lasers to achieve accelerating fields of 10 to 100 GeV/m. Many feasibility and practicality issues remain: positron acceleration, multi-stage acceleration, control of beam quality, and plasma instabilities at tens of kHz repetition rate. All variants require an integrated proof-of-principle test. The U.S. is a world leader in these strong physics programs at the frontier of accelerator science.

High-intensity proton sources for neutrinos, muons, and rare processes: Requirements for Intensity Frontier experiments are more diverse than for the Energy Frontier. Therefore, this study addressed a set of structured questions: (1) What secondary beams are needed for Intensity Frontier experiments? (2) What proton beams could generate such beams? (3) Can these proton beams be made by existing machines? (4) What new capabilities are needed? (5) What accelerator and target research is needed to realize the new capabilities? The study surveyed particle physics requirements for secondary beams, including beams of neutrinos, kaons, muons, and neutrons. Experiment advocates supplied nineteen secondary beam requests. From these the study group derived primary proton beam characteristics.

The common characteristics required are average beam power, with more than 1 MW delivered, and a flexible, experiment-dependent time structure. Beam requirements were compared with 20 existing proton beam-lines and 14 planned upgrades. The overarching conclusion is that the next generation of intensity frontier experiments requires beam intensities and timing structures beyond the capabilities of any existing accelerator.

Fermilab's proposed multi-stage Project X would yield a world-leading facility based on a modern multi-MW superconducting proton linac capable of injecting into the Fermilab Main Injector. The linac would deliver a flexible on-demand beam structure that could serve multiple experiments over an energy range 0.25–120 GeV. The linac would provide a platform for future muon facilities including nuSTORM, a neutrino factory, and a muon collider. A complete, integrated Reference Design Report identifies technical risks that will be mitigated in a structured research program already underway.

The DAE δ ALUS collaboration proposes multiple sources of decay-at-rest anti-neutrinos for short-baseline oscillation experiments. This project has narrower experimental scope than Project X. DAE δ ALUS would use three multi-MW H_2^+ cyclotrons and target stations located about 2 to 20 km from a large hydrogenous detector. The experiment would measure CP violation in a way that is complementary to the LBNE experiment. The first stage of DAE δ ALUS would be IsoDAR, a compact 60 MeV cyclotron located only 15 m from the KamLAND detector, that would make a definitive search for one or two sterile neutrinos. This international collaboration has strong connections with commercial cyclotron industries.

Another possibility is nuSTORM, neutrinos from STORed muons. This would be a first step toward a long-baseline neutrino factory capability. The nuSTORM muon storage ring would send well-characterized neutrino beams to detectors at 50 m and 1900 m for a sterile neutrino search and neutrino cross-section measurements.

A common research issue for Intensity Frontier capabilities is the injection system, composed of low-emittance, high-current ion sources with effective beam choppers. Control of space-charge forces is important for preserving beam quality. Understanding and limiting beam loss is a dominant operational issue requiring adequate simulation of halo formation, efficient beam collimation, and very high-efficiency extraction.

High-power targets are a difficult challenge that limits facility performance. The principal underlying damage mechanisms of the target materials are atom displacements and gas production. Particulars depend on primary beam characteristics, target material, operating temperature, and the duty factor of the accelerator. Unfortunately, one cannot directly translate experience with nuclear reactors to estimate the performance of targets with high-energy beams from experience with nuclear reactors. Details of target behavior and failure mechanisms are a mesoscale problem that is difficult to simulate. Computed radiation effects in inhomogeneous materials subject to time-varying irradiation need validation with controlled, instrumented in-beam tests.

High-intensity electron and photon beams: This working group addressed two major questions: (1) What capabilities at heavy flavor factories are required to realize the full range of physics opportunities? (2) What are new physics opportunities using high power electron and positron beams? The relevant technologies exploit strong synergy with light sources and damping rings.

SuperKEKB, a super-high-luminosity B -factory, is an upgrade to the KEKB B -factory currently under construction in Japan with commissioning to commence in January 2015. To achieve the target luminosity of $8 \times 10^{35} \text{ cm}^{-2}\text{s}^{-1}$, a forty-fold increase over that of KEKB, the SuperKEKB beam currents will be approximately twice as high as used at KEKB, and vertical bunch sizes at the collision point about 20 times smaller than those achieved at KEKB. Greater U.S. collaboration would strengthen the SuperKEKB project and might enable even higher luminosity well in excess of $10^{36} \text{ cm}^{-2}\text{s}^{-1}$.

Two super tau-charm factories have been proposed: one at Frascati (Tor Vergata) in Italy and one at Novosibirsk in Russia. Both machines are two-ring, symmetric-energy machines, with provisions for longitudinally polarized beams.

The DarkLight experiment will use the high-intensity electron beam of the JLab FEL, impinging on a hydrogen target, to search for gauge bosons associated with “dark force” theories. It might also be possible to use an intense, low-emittance positron beam, impinging on a plasma target, to generate sufficient muon/anti-muon pairs to provide a source beam for a future muon collider without the need for a separate muon cooling stage.

Electron-ion colliders: Several future electron-ion colliders have been studied in recent years. All would be based at an existing accelerator facility. The collider configurations include both ring-ring and linac-ring options. Center of mass energies range from 14 GeV to 2000 GeV. Most of the collider concepts share several enabling technologies. SRF cavities must be able to operate with high average and high peak beam currents, providing effective damping of high-order modes. The cryomodule design must be consistent with containing high beam power. Hadron beam transverse and longitudinal emittances must be small to achieve high collider luminosity. Therefore, the designs with medium hadron energy call for the application of powerful cooling techniques.

The low β^* interaction region designs for all proposed colliders require strong focusing of beams at the collision point and fast separation of beams after the collision. The synchrotron radiation fan produced by electrons in the focusing magnets must be kept from hitting the the beam pipe in the vicinity of the detectors and inside superconducting magnets.

The linac-ring designs utilize a polarized electron source, with an average current ranging from 6 mA to 50 mA. The linac-ring scheme introduces non-standard beam-beam effects, which must be explored to understand the limits on the luminosity and the beam parameters. Other shared technologies include techniques to preserve beam polarization. Spin matching and the harmonic correction techniques have to be investigated for ring-ring colliders to minimize the beam depolarization due to synchrotron radiation, especially in the presence of spin rotators and solenoidal detector magnets.

Accelerator technology test beds: We identified a broad range of existing and needed test capabilities for proposed frontier accelerators. The first category of test facilities permits testing beam physics or accelerator components essential to manage technical risks in planned projects. A second category would integrate accelerator systems to provide proof-of-practicality tests. The third category provides tests of physics feasibility of concepts and/or components. This study identified 35 existing facilities in the U.S. and overseas, both with and without beam testing capability. Although these facilities provide substantial readiness to move forward with the highest priority accelerators for particle physics, the long range future of particle physics needs a few additional dedicated test capabilities in the near term.

1.7 Underground Laboratory Capabilities

Some of the most compelling experiments in particle physics can only be done at underground facilities. Searches for dark matter and neutrinoless double beta decay and neutrino experiments using solar, reactor, atmospheric, and supernova neutrinos and neutrinos from accelerators all require underground facilities and capabilities.

Underground facilities are located in North America, Europe, Asia, and Antarctica (in ice). New underground facilities have become operational in all of these regions in the last few years. The world-wide particle physics community plans to expand underground capabilities over the next years, primarily outside the United States. If all of these plans are realized, general-purpose space for underground experiments will roughly double by the end of the decade. The expansion would include major new facilities to host reactor experiments at moderate depths, and a new class of very large facilities for long-baseline and atmospheric neutrino experiments, proton decay, and other physics.

Plans for expansion or continuation of underground facilities in the United States are less developed. Currently, there are no plans with approved federal funding for expansion of underground capabilities at the four underground sites located in the United States. The Long-Baseline Neutrino Experiment (LBNE) has provisional approval to be located on the surface at the Sanford Underground Research Facility (SURF) in South Dakota, but design work is underway in anticipation of achieving a global collaboration to allow LBNE to be sited deep underground at SURF. The LBNE physics community expressed strong support for the deeper site during the Snowmass process.

All of the next generation (G2) dark matter experiments can be accommodated by existing or planned underground facilities, assuming no reduction in these facilities for the rest of the decade. Only one of these experiments is planned to be located at a U.S. facility. Several neutrinoless double-beta-decay experiments are already under construction at existing underground facilities, one of which is in the U.S. Next-generation (ton scale) neutrinoless double-beta-decay experiments can likely be accommodated by existing and planned facilities, but will face competition for underground laboratory space from dark matter experiments.

Detectors for reactor experiments with baselines greater than 100 m require medium-depth underground laboratories. Future reactor experiments are being planned overseas based on funding commitments from the host countries.

The flagship of the international neutrino effort is the search for CP violation in the lepton sector, which requires a massive detector and a very intense neutrino beam. There are other motivations for constructing this massive detector underground. The search for nucleon decay is one of the most important topics in particle physics. Atmospheric neutrinos, observable in a large underground detector, may be sensitive to all of the currently poorly known neutrino oscillation parameters. The spectacular neutrino burst from a nearby supernova event would be detected at no additional cost if the detector is underground, but such detection

is very difficult on the surface. Some of the same detectors that would be used for long-baseline neutrino experiments could be used to advance the search for CP violation, nucleon decay, the study of atmospheric neutrinos, and other physics if the detector is located underground. This is the plan for Hyper-K (Japan) and LBNO (Europe). It would be a lost opportunity if this condition cannot be satisfied with LBNE.

Experimental needs for materials assay and storage outstrip the capability of existing facilities, and space for such work should be reserved at new facilities. In addition, underground space should be reserved for small prototype testing and generic R&D. There is enough space at U.S. facilities to meet future needs if the existing underground labs are maintained.

As the scale and cost of underground experiments grows it will become even more important to maintain open competitive access to underground laboratories. The best way for the governments to support the international system of underground experiments is for each major country (or region) to support at least one major underground laboratory capable of hosting forefront experiments. It is not clear whether it would be possible to sustain this international support if one country chose to take a major role in the research without supporting any facility.

Our conclusions are:

1. We should locate LBNE underground to realize its full science potential. This step would also provide a natural base for additional domestic underground capabilities at SURF in the future.
2. The U.S. has leading roles in many of the future dark matter, neutrinoless double-beta-decay and neutrino experiments.
3. More coordination and planning of underground facilities (overseas and domestic) is required to maintain this leading role, including use of U.S. infrastructure.
4. Maintaining an underground facility that can be expanded to house the largest dark matter and neutrinoless double-beta-decay experiments would guarantee the ability of the U.S. to continue its strong role in the worldwide program of underground physics.

1.8 Instrumentation

The search for answers to fundamental questions in the field of particle physics has always been intimately tied to the development of innovative technologies or significant advancements in existing technologies. The particle physics community has a long history of inventing detectors based on new technologies to address the science needs and advancing these technologies to large-scale reliable use. For many decades the instrumentation needs of particle physics have motivated university faculty, national laboratory staff, and industrial scientists to develop the technological foundations and invent the detectors responsible for many of the important particle physics discoveries. The field of particle physics is generally regarded as an incubator of innovation in instrumentation. Moreover, driven by the needs of particle physics, the technology developed for accelerators and detectors has historically benefited many other fields of the physical and applied sciences and medicine.

Modern particle physics experiments and high-energy accelerators put extraordinary demands on sensors, sensor readout electronics, precision engineering, and data acquisition and management, often incorporated into detector systems of very large scale. In addition, the scientific approaches are broadening to include lower-energy, high-intensity and ultra-low-background experiments, including experiments deploying very

large volume detectors to study very rare processes, and also experiments that study fundamental properties of the cosmic energy and matter with greater precision.

Focusing only on scaling up existing technologies to larger experiments or carrying out detector R&D only when it is needed is tempting in tight budgetary times, but this is counter to the successful approach that has been followed up to now. Instead, more fundamental innovation and development of new approaches are necessary to make experiments feasible or economically viable. An appropriate investment in a detector R&D program will be required to enable the science goals and meet the budgetary challenges. This program would develop, over intermediate and long time frames, new tools and technologies that are both cost-effective and have an enhanced physics reach. It will allow the field to continue to carry out flagship domestic experimental research and have leadership roles in off-shore experimental projects, while the development of new, transformative detection capabilities will ensure an affordable and healthy experimental particle physics research program in the future. The major challenge for instrumentation is to structure the current advanced detector R&D program such that it will enable the United States to continue to maintain scientific leadership in many key areas of a broad international experimental program in particle physics.

The instrumentation needs of planned and proposed future experiments across the field were surveyed in two joint preparatory meetings, in several dedicated topical workshops and in joint sessions with the Energy, Cosmic, and Intensity Frontiers during the Snowmass study. In addition, nearly one hundred white papers on instrumentation were submitted, covering a broad range of topics. In all cases instrumentation development was considered central to progress. An overview of the current and planned programs of experiments at the various physics frontiers and specific detector needs is provided in the full Instrumentation report. This survey of the whole experimental program of particle physics identifies key issues in instrumentation within the next decade and beyond. It also gives a picture of the opportunities that exist to establish an instrumentation program that can satisfy both the needs of particle physics and at the same time create an environment of innovation to benefit other fields of science. This summary articulates a vision for instrumentation that will enable execution of a broad, targeted experimental program within the fiscal realities of our time and identify areas where the U.S. can take a leadership position.

Ideally, the program of detector R&D should include a range of projects with various levels of risk and time scales for full development. Detector R&D carried out within existing experiments is, by necessity, project-driven and provides a low-risk path to relatively incremental improvements to existing technologies. Detector R&D that is motivated by common needs among various experiments is generally longer-term, can be higher-risk, and adds value to multiple experimental areas at the same time. This type of R&D can lead to incremental or significant improvements in cost reduction, scientific reach, or both. At the highest level of impact and risk is long-term detector R&D leading to transformative changes in cost reduction, increase in scientific reach or both, across a significant part of the experimental program. This is the kind of high-risk, high-reward detector R&D that has the potential to lead to scientific breakthroughs. Underpinning these R&D efforts is the urgent need for training the next generation of instrumentation experts, without which there can be no long-term future.

Major technological advances based on a better understanding of the underlying science are also occurring in other scientific disciplines such as materials science, photonics and nanotechnology. Many of these advances have the potential to lead to transformational new technologies for particle physics detectors. Particle physics should continue to exploit and pursue the technology advances in other experimental scientific disciplines, which could contribute to opportunities for innovation and the development of transformative technologies for particle physics.

A healthy national instrumentation program must provide a balance between evolutionary and revolutionary detector development while training the next generation of experts. We have five recommendations for critical elements of this national program:

1. Support detector R&D with clearly identified areas of detector development based on the strengths of the community.
2. Achieve an appropriate balance between evolutionary and revolutionary detector R&D, i.e., an appropriate “portfolio of risk,” and build expertise in innovative technologies that can be applied to the design and construction of novel, cost-effective particle physics detectors.
3. Develop a process for optimizing the use of existing university, national laboratory, and industrial resources, to grown and retain local technical expertise at universities and laboratories, and to identify incentives and mechanisms for improving detector R&D collaborations and equipment sharing among universities, national laboratories and industry.
4. Create opportunities for attracting, and providing careers for, particle physicists with interest in, and outstanding capability for, innovative detector design and development. This community of experimental physicists will preserve the background and skills needed to design and build future generations of particle physics experiments.
5. Provide mechanisms for identifying and transferring appropriate technologies developed in other scientific disciplines to particle physics and for transferring applicable technologies developed in particle physics to other science disciplines, such as nuclear physics, basic energy sciences, and related branches of science, medicine, and national security.

The Snowmass process provided broad input and guidance from the particle physics community in identifying these crucial elements of a national technology and instrumentation program. A previous DPF Task Force on Instrumentation led to the creation of CPAD, the Coordinating Panel for Advanced Detectors, which is intended to act as the advocate for the detector development program, to promote its merits, and to provide venues for regular presentation of results. CPAD can bring different groups of technical experts together to make the community aware of developments elsewhere. CPAD can also coordinate between the funding agencies and the instrumentation developers by providing information about instrumentation needs and ongoing activities.

A crucial enabling element for an instrumentation program is the development of expert physicist manpower. An investment in younger physicists who work on instrumentation is critical to support a long-term particle physics program. It is noted that only under exceptional circumstances are doctorate degrees awarded by U.S. physics department for a Ph.D. thesis based on instrumentation, whereas this is commonplace in Europe. Furthermore, early specialization in instrumentation is strongly disadvantaged given the emphasis on physics analysis to obtain faculty positions. This has to change for the field to remain viable.

Other essential enablers are national laboratory resources, unique facilities such as test beams, and targeted funding aimed at a specific problem. The U.S. national laboratories are a resource of unique importance. Their breadth, because of their multidisciplinary scientific nature, provides cross-fertilization of useful technologies from nuclear physics, basic energy science, materials research, engineering, chemistry, and computer science. Many U.S. national laboratories have close links to top-ranked universities, which are also centers for multi-disciplinary innovation and ideas. University groups have greater difficulty maintaining long-term technical and engineering resources, while the national laboratories naturally maintain these as consequence of their missions. By combining the intellectual and manpower capabilities of universities with the resources of the national laboratories and the product development capabilities of industry, the U.S. can continue to confront and overcome many of the technological challenges of future particle physics experiments.

The United States has several complementary test beam and irradiation facilities. These facilities are a critical component of instrumentation work, and there is a separate Snowmass report about them. The Snowmass process did not allow for a proper evaluation of all facilities for instrumentation, and it is

recommended that CPAD finalize this process. Adequate support for these facilities is essential for a healthy detector R&D program.

Another enabler to encourage transformative innovation in instrumentation would be to initiate a new program, outside the existing funding, for long-term investment in more speculative but potentially high impact research motivated by a set of “grand challenges.” Once identified, such grand challenges would be effective in focusing the creative power of the instrumentation community on problems that have the potential for large payoff. Areas where existing technologies would be cost-prohibitive for meeting the goals of future experiments are good candidates for new initiatives. These grand challenges should be issued nationally, and cross-disciplinary collaboration should be strongly encouraged. This would allow the program to take advantage of the tremendous progress and breakthrough advances that have been made in areas of science outside the field of particle physics that could prove very valuable for the development of future instrumentation. Because of the cross-disciplinary aspect, close collaboration between universities and national laboratories will be a key component. Funding would be subject to proposal review, but should be at a substantial level for a period of at least three years. CPAD could be engaged to identify the set of grand challenges that would define the program.

During the Snowmass Instrumentation discussions, a number of instrumentation areas were recognized as of strategic importance. These areas all focus on major technological barriers that stand in the way of reaching the science goals. Some of them have the potential to deliver very cost-effective instrumentation methods and provide breakthrough new technology. The choice of these areas was guided by their physics impact and existing strengths and capabilities in the country. Consideration was given to the technology’s usefulness to other branches of science. Although some of these instrumentation themes seem very challenging, it is likely that many, if not all, can be realized with a dedicated instrumentation effort. The main strategic areas that have been identified are described below.

ASICs: The use of Application Specific Integrated Circuit (ASIC) electronics and interconnect development is often critical to enable an experiment. A number of factors make ASICs essential to particle physics, such as small physical size, high channel density, ability to integrate a variety of function blocks, low power dissipation and radiation tolerance. Examples of areas where ASIC-related R&D is required are high-speed waveform sampling, pico-second timing, high-rate radiation tolerant data transmission, low temperature operation, low power and 2.5D and 3D assemblies. The field of particle physics has spearheaded the use of ASICs, but there is a growing need and adoption by other disciplines. ASIC development provides an excellent opportunity to work more closely with other branches of science by trying to address their instrumentation needs. A report summarizing a workshop held earlier in 2013 to look at the ASIC needs for particle physics was submitted to the Snowmass proceedings.

Calorimetry: The measurement of the total energy of electrons and jets lies at the core of experiments at the Energy and Intensity Frontiers. Projects designed to search for very rare processes or to make precision measurements are in need of more precise, faster, and more cost-effective methods to perform these calorimetric measurements.

High-speed data acquisition: Experiments are required to handle huge interaction rates to acquire, transport, process and retain the events of interest, preserve the accuracy of the measurements of intrinsic particle properties, and uncover signatures of new physics. More intelligent trigger and data acquisition systems are needed to enable higher statistics experiments.

Large-volume detectors: The study of neutrino properties and their interactions and the search for dark matter require large-volume detectors at underground facilities. Innovative technologies that allow scaling in a cost-effective way with increased sensitivity are required to enable the spectroscopy of these fundamental particles. A coherent research program in low-radioactive materials and assay is required.

Photodetectors: A multitude of physics processes can be studied by measuring photons with wavelengths ranging from mm to nm. Instruments used to study these photons are based on a range of materials ranging from superconductors to semiconductors, from alkali metals to crystals. The development of large arrays with improved spectral sensitivity, energy and time resolution, and excellent background rejection would truly revolutionize future experiments.

Pixelated sensors: High granularity has become a requirement for many of our detectors. Often the higher density results in performance compromises. The development of new technologies designed to deal with the higher density, while avoiding these compromises and improving overall performance, is essential for future experiments. These include sensors with a greater degree of pixelation, radiation hardness, high speed, and built-in intelligence to carry out a number of operations, including hit time-stamping, clustering and recognizing hit correlations, that can affordably be deployed in large areas.

Power and mass: Especially at the Energy Frontier, experiments are characterized by high radiation, huge interaction rates, and serious constraints on power and mass budget. Better low-mass structural materials that are strong and stable, including materials with ultra-low intrinsic radioactivity, would benefit a broad spectrum of future experiments. The design of electrical power distribution and cooling systems seems mundane, but it can severely limit the physics reach of current experiments. These systems must deliver services with low mass in a high radiation and magnetic field environment. Innovative solutions are critically important for next generation experiments.

The particle physics technology and instrumentation program described here requires a multi-year commitment from the funding agencies. The funding required to meet short-term financial obligations to sustain an existing particle physics research program puts enormous pressure on funds earmarked for long-term, generic detector development. In spite of these pressures, a stably and adequately funded generic instrumentation program will ensure that the field invests in its future and establishes a foundation for a competitive, healthy program in the long term.

1.9 Computing

Computing has become a major component of all particle physics experiments and in many areas of theoretical particle physics. The Computing study group established subgroups covering user needs and infrastructure. The study considered user needs for experiments at the Energy and Intensity Frontiers, and the combined needs of Cosmic Frontier experiments, astrophysics and cosmology. Theory subgroups covered accelerator science, astrophysics and cosmology, lattice field theory, and perturbative QCD. Four infrastructure groups examined trends in computing to predict how technology will evolve and how it will affect future costs and capabilities. These groups focused on distributed computing and facility infrastructures, networking, data management and storage, and software development, personnel, and training. They identified critical technology needs for particle physics that might require the DOE or NSF to fund research in computer science and technology.

During the period between the Community Planning Meeting at Fermilab and the Community Summer Study meeting at the University of Minnesota, the Computing groups were actively engaged with the other frontiers to learn of their plans and estimate their computing needs. The infrastructure groups engaged with vendors, computer users, providers, and technical experts to predict trends in computing, networking, storage, and software development, including considerations of costs, capacities, and speeds. Two days of parallel sessions at the Minnesota meeting were devoted to discussions across the subgroups, to finalize subgroup findings, and to identify common trends and needs.

Progress in particle physics experiment and theory will require significantly more computing, software development, storage, and networking. Different projects stretch future capabilities in different ways, but there are many common needs among the different areas of particle physics. In the future more commonality and community planning would aid in moving ahead in the most efficient manner. This requires careful and continuing review of the topics we studied, in particular, user needs and capabilities of current and future technology. For many years, the particle physics community has been a great source of computing innovation and expertise. It is essential to leverage those assets through wider sharing of knowledge throughout the experimental and theoretical communities. We should be open to bi-directional sharing of expertise with the entire scientific community.

The experimental program relies for the most part on distributed high-throughput computing (HTC). Simulation, data analysis, and reconstruction of individual events are independent of each other, so that groups of events can be assigned to hardware in different locations. Results are combined when all event groups are done. This distributed computing model was pioneered by the Energy Frontier experiments. It relies on a distributed infrastructure of computing centers as part of the Open Science Grid in the U.S. and extending across the globe. Theoretical computing and simulation needs are more commonly addressed by high-performance computing (HPC), in which thousands to hundreds of thousands of tightly coupled CPUs are working simultaneously on a single problem. These resources are provided mostly through DOE and NSF supercomputing centers.

One issue for those applications that traditionally rely on HTC for their data-intensive computing is to what degree they can or should use national supercomputer centers, which have traditionally been designed for HPC usage. Work is proceeding to make these HTC applications run on HPC, and to interface HPC centers to the HTC workload and data management infrastructures. Also, traditional HPC applications are developing in the direction of more data-intensive science, which, however, is currently not a good match to existing and next-generation HPC architectures. Computational resources will have to address the demands for greatly increasing data rates, and the increased needs for data-intensive computing tasks like data analytics, for comparing large samples of simulations and observational data.

Another pressing issue facing both HTC and HPC communities is that processor speeds are no longer increasing exponentially, as they were for at least two decades. Instead, new chip architectures provide multiple cores. Thus, we cannot rely on new hardware to run serial codes faster, and we must parallelize codes to increase application performance. In addition to multi-core chips, there are accelerators such as graphical processing units (GPUs) and many-core chips such as the Intel Xeon Phi. Computing resource needs for Energy Frontier experiments used to scale roughly with the rate that processor speeds increased, following Moore's law. Future advances will require full use of multiple-core and many-thread architectures. Scaling of disk capacity and throughput is of significant concern in the storage area, since per-unit capacities are no longer increasing as rapidly.

These changes in chip technology and high-performance system architectures require us to develop parallel algorithms and codes, and to train personnel to develop, support, and maintain them. Different subgroups are at different stages in their efforts to port to these new technologies. Lattice QCD, for example, started its GPU porting efforts in 2008 and has had code in production for some time, particularly for matrix inversions of the lattice Dirac operator; however, there are other parts of the code that are still only running on CPUs. Cosmological simulations have exploited GPUs since 2009 and some codes have fully incorporated GPUs in their production versions, running at full scale on hybrid supercomputers. Accelerator science is also actively porting codes to GPUs. Some of the solvers and particle-in-cell infrastructures have been ported and very significant speed-ups have been obtained. The perturbative QCD community has also started using GPUs.

These trends lead to vastly increasing code and system complexities. For example, only a limited number of people in the field can program GPUs. In this and other highly technical areas, developing and keeping

expertise in new software technologies is a challenge, because well-trained personnel and key developers are leaving to take attractive positions in industry. Continued training is an important aspect. Training materials are now provided by some of the national supercomputing centers, and by summer schools organized by, among others, the Virtual School of Computational Science and Engineering. We must examine whether these provide the right training for our field and whether the delivery mechanisms are timely. On-line media, workbooks, and wikis are suggested to enhance training. Another area of common concern is the career path of those who become experts in software development and computing. It is useful to help young scientists learn computing and software skills that are marketable for non-academic jobs, but it is also important that there be career paths within particle physics, including tenure-track jobs, for those working at the forefront of computation.

Energy Frontier experiments already experience computing limitations that limit the amount of physics data that can be analyzed. The planned upgrades to the LHC energy and luminosity are expected to result in a ten-fold increase in the number of events and a ten-fold increase in event complexity. Effort has begun to increase code efficiency and parallelism in reconstruction software and to explore the potential of GPUs. The experiments are considering saving more raw events to tape and only reconstructing them selectively. The LHC produces about 15 PB of raw data per year now, but by 2021 the rate may rise to 130 PB. Attention needs to be paid to data management and wide-area networking, to assure that network connectivity does not become a bottleneck for distributed event analysis. It is important to monitor storage cost and throughputs. More than half of the computing cost is now for storage, and in the future it may become cost-effective to recompute certain derived quantities rather than storing them.

Intensity Frontier experiments have combined computing requirements on the scale of a single Energy Frontier experiment, but they form a more diverse set than those of the Energy Frontier. Our survey found that there is significant overlap in different experiments' needs. Sharing of resources across experiments, as in the Open Science Grid, is a first step in addressing peak computing resource needs. Continued coordination of software development between these experiments will allow for efficiently developed coding infrastructure. Leveraging the data handling experience and expertise of the Energy Frontier experiments for the diverse Intensity Frontier experiments would significantly improve their ability to reconstruct and analyze data.

Cosmic Frontier experiments will greatly expand their storage needs with the start of new surveys and the development of new instruments. Current data sets are about 1 PB, and the total data set is expected to be about 50 PB in ten years. Beyond that, in 10–20 years data will be collected at the rate of 400 PB/yr. On the astrophysics and cosmology theory side, some of the most challenging simulations are being run on supercomputers. Current allocations for this effort are approximately 200M core-hours annually. Very large simulations will require increasing computing power. Comparing simulations with observations will play a crucial role in interpreting experiments, and simulations are also needed to help design new instruments. There are very significant challenges in dealing with new computer architectures and very large data sets, as described above. Growing archival storage, visualizing simulations, and allowing public access to data are also issues that need attention.

Accelerator science is called on to simulate new accelerator designs and to provide near-real-time simulation feedback for accelerator operation. Research into new algorithms and designs has the potential to bring new ideas and capabilities to the field. It will be necessary to include additional physics in codes and to improve algorithms to achieve these goals. Production runs can use from 10K to 100K cores. Considerable effort is being expended to port to new architectures, especially to address the real-time requirements.

Lattice gauge theory calculations rely on national supercomputer centers and hardware purchased for the USQCD Computing Project. Allocations at supercomputer centers have exceeded 500 M core-hrs this year, and resource requests will go up by a factor of 50 by the end of this decade. This program provides essential input for interpretation of a number of experiments, and increased precision will be required in the

future. For example, the b quark mass and the strong coupling α_s will need to be known at the 0.25% level, a factor of 2 better than now, to compare upcoming ILC Higgs observations with SM predictions. Advances in the calculation of hadronic contributions to the muon's anomalous magnetic moment will be needed for interpretation of the planned experimental measurement at Fermilab.

Perturbative QCD is essential for theoretical understanding of collider physics rates. Experts in perturbative QCD computation ported codes to the HPC centers at NERSC and OLCF, and to the Open Science Grid. They have also been benchmarking GPU codes and finding impressive speed-up over a single core. A repository of codes has been established at NERSC. A long-term goal is to make it easy for experimentalists to use these codes to compute Standard Model event rates for the processes they need.

The **Distributed computing and facilities infrastructures** subgroup looked at the growth trends in distributed resources as provided by the Open Science Grid, and the national high performance computing centers. Most of the computing by experiments is of the HTC type, but HPC centers could be used for specific work flows. Using existing computing centers could save smaller experiments from large investments in hardware and personnel. Distributed HTC has become important in a number of science areas outside particle physics, but particle physics is still the biggest user and must continue to drive the future computing development. HPC computing needs for theoretical physics will require an order of magnitude increase in capacity and capability at the HPC centers in the next five years, and two orders of magnitude in the next ten years.

The **Networking** subgroup considered the implications of distributed computing on network needs, required R&D and engagement with the National Research and Education Networks (which carries most of our traffic). The group formulated a number of research questions that need to be answered before 2020. Expectations of network performance should be raised so that planning for network needs is on par with that for computing and storage. The gap between peak bandwidth and delivered bandwidth should be narrowed. Wide-area network performance should not be an insurmountable bottleneck in the next five to ten years as long as investments in higher performance links continue. However, there is uncertainty as to whether network costs will drop at the same rate as they have in the past.

The **Software development, personnel and training** subgroup proposed a number of recommendations to implement three main goals. The first goal is to use software development strategies and staffing models that result in software that is more widely useful to the HEP community. The second goal is to develop and support software that will run with optimal efficiency on future computer architectures. The third goal is to ensure that developers and users have the training necessary to deal with the increasingly complex software environments and computing systems that will be used in the future.

The **Storage and data management** subgroup found that storage continues to be a cost driver for many experiments. It is necessary to manage the cost to optimize the science output from the experiment. Tape storage continues to be relatively inexpensive and should be utilized more within the storage hierarchy. Disk storage is likely to increase relatively slowly in capacity per unit cost, due to a shrinking consumer market and technology barriers. Operating distributed data management systems can be costly for experiments, and continued R&D in this area would benefit a number of experiments.

To summarize, the challenging resource needs for the planned and proposed physics programs require efficient and flexible use of all resources. HEP needs both distributed HTC and HPC. Emerging experimental programs might consider a mix to fulfill demands. Programs to fund these resources need to continue. Sharing and opportunistic use help address resource needs, from all tiers of computing, eventually including commercial providers. There is increasing need for data-intensive computing in traditionally computation-intensive fields, including at HPC centers, for data analytics, combining simulations and observational data, etc.

In order to satisfy our increasing computational demands, the field needs to make better use of advanced computing architectures. With the need for more parallelization, the complexity of software and systems continues to increase, impacting architectures for application frameworks, workload management systems, and also the physics code. We must develop and maintain expertise in all areas of the field, and re-engineer frameworks, libraries, and physics codes. Unless corrective action is taken to enable us to take full advantage of the new hardware architectures, we could be frozen out of cost-effective computing solutions within 10 years. There is a large code base that needs to be re-engineered, and we currently do not have enough people trained to do it.

The continuing huge growth in observational and simulation data drives the need for continued R&D investment in data management, data access methods, and networking. Continued evolution of data management and storage systems will be needed in order to take advantage of new network capabilities, ensure efficiency and robustness of the global data federations, and to contain the level of effort needed for operations. Significant challenges with data management and access remain, and research into these areas could continue to bring benefit across the frontiers. We expect solutions that will be based on content delivery approaches, dynamic data placement, and remote data access.

Network reliability is essential for data-intensive distributed computing. Emerging network capabilities and data access technologies improve our ability to use resources independent of location. This will enable use of diverse compute resources. These include dedicated facilities, university computing centers, and opportunistic use of shared resources between PIs. They will expand to commercial clouds and eventually also make use of leadership-class HPC centers relevant for data-intensive computing. The computing models should treat networks as a resource that needs to be managed and planned for.

Computing will be essential for progress in theory and experiment over the next two decades. The field continues to learn how to do more science with constrained resources, requiring us to be more flexible and perhaps tolerate higher levels of risk. The advances in computer hardware that we have seen in the past may not continue at the same rate in the future. The issues identified in this report require continuing attention. Addressing them will increase efficiency, reduce costs, and enable us to meet the experimental and theoretical goals identified through the Snowmass process.

1.10 Communication, Education, and Outreach

Broad societal support for particle physics research will be required to achieve the many scientific and technological goals identified by the U.S. particle physics community through the Snowmass process. Building and sustaining this support will require the particle physics community to unite behind a common plan that emerges from the Snowmass/P5 process and to communicate enthusiasm for the future of the field and its societal impacts to a wider audience of policy makers, opinion leaders, scientists in other fields, educators, and students.

Federally supported research in particle physics and related fields has led to an impressive list of Nobel Prize-winning discoveries: the first detailed study of the cosmic microwave background, the discovery of neutrino masses and mixing (and earlier work on solar neutrinos many years earlier), the discovery of the accelerating expansion of the Universe, the understanding of the strong force, and the discovery that the CKM matrix explains CP violation. The most recent Nobel Prize was awarded for the Higgs boson, whose discovery in 2012 was made possible by scientific talent, technology and leadership from the United States.

The American public is fascinated by these discoveries, and by the full breadth of current and future particle physics projects. The saga of the Large Hadron Collider and the Higgs boson discovery reached audience

levels unprecedented for a particle physics event. Public lectures and other events on particle physics topics draw crowds. Milestones, discoveries and even proposals for projects in particle physics routinely make headlines.

Translating this public excitement into greater support for the field requires existing communication, education and outreach (CE&O) activities to be augmented and enhanced. We need national coordination and training, additional resources, and the commitment by the particle physics community to convey consistent, coherent and compelling messages about the importance of particle physics research and its value to society.

Many individuals, groups, and institutions in the U.S. particle physics community already reach out to members of the public, decision makers, teachers, and students through a wide variety of effective activities. However, there is room for improvement in the nationwide coordination of these activities, in the mobilization of the entire U.S. community to take part in the activities, and in efforts to use varied activities to convey consistent and compelling messages to stakeholders.

The following is a survey of existing CE&O activities targeted at four audiences: policy makers and opinion leaders, scientists in other fields, the general public, and educators and students.

For policy makers and opinion leaders: The U.S. particle physics community engages in a number of efforts to build support for research among policy makers and opinion leaders. User groups make annual visits to Washington, D.C. and, with scientific societies, conduct email and letter-writing campaigns at key points in the budget cycle. Scientists participate in Washington, D.C., events organized by the American Association for the Advancement of Science (AAAS), the American Physical Society (APS), the National User Facilities Organization (NUFO), and other organizations. Scientific and industrial societies, national laboratories, and individual scientists engage in direct advocacy with legislators. Scientists and media relations professionals at universities and labs work to place particle physics stories and physicists in influential media outlets.

For the scientific community: Past and current outreach activities of the particle physics community targeted at colleagues in the broader science community include colloquia and seminars at university departments and at national laboratories, and plenary sessions at APS and AAAS meetings. Particle physicists publish their results and pedagogical review articles in journals such as *Science* and *Nature*. They write articles for *Scientific American*, *Popular Science* and similar magazines and for online forums and science blogs, publish popular science books, and write reports commissioned by labs and agencies. Examples of the last category include *Quantum Universe* and *Discovering the Quantum Universe*, prepared by HEPAP for the DOE and NSF.

For the general public: Existing activities that reach the general public are extremely broad but have varying levels of support. The single most common activity is public talks. Scientists frequently participate in open houses and related events such as science festivals, lab and department tours, physics shows, alumni weekends, and workshops for the public. They contribute to external publications and shows by writing magazine articles and op-ed pieces in newspapers, participating as consultants to radio and television programs and movies, and working with the news media. They produce outreach materials such as books, brochures, posters, web-based materials, and multimedia products. They engage in social media activities such as blogs, Facebook pages, Twitter feeds, and the creation of YouTube videos.

For educators and students: The particle physics community in the United States and abroad has succeeded in increasing student interest and achievement in STEM fields, including particle physics, through a variety of efforts. The 2013 APS Excellence in Physics Education Award was presented to leaders of University of Illinois undergraduate physics education research. The QuarkNet long-term teacher development program has changed how many teachers view science and education by putting cosmic-ray detectors, online analysis tools, and LHC data into their hands. Netzwerk Teilchenwelt adapted the QuarkNet model for students, teachers, and physicists in Germany. The International Particle Physics Outreach Group (IPPOG)

sponsored 161 master classes in 37 countries in 2013, including 29 masterclasses in 9 countries in the Fermilab-based portion of the program. National laboratories run successful long-term education programs, many with particle physics content and partnerships with particle physics groups. The Contemporary Physics Education Project brings together physicists and educators to develop wall charts, posters, websites and activities.

As a result of the Snowmass process, the community has recognized that more physicists must engage in CE&O activities. The quality and coordination of such activities must be improved in order to increase public support for the field, develop the next generation of physicists, and ensure scientifically literate and engaged citizens.

A survey of 641 members of the particle physics community conducted in the spring of 2013 indicated that while about 60% of physicists engage in outreach to the general public and 50% reach K-12 teachers or students, only 30-35% engage in activities targeted to scientists in other fields or policy makers. The survey also identified the greatest barriers to participation in CE&O activities, including lack of time, little reward in career advancement, and a lack of resources to communicate the broader societal impacts of particle physics research.

At the Snowmass meeting, a number of prominent voices called for renewed commitment to CE&O:

- “We must educate our representatives in Congress, our fellow citizens, the business community and the scientific agencies.” — D. Gross
- “You are underselling yourselves...you are technology incubators for other fields of science.” — R. Roser
- “The media missed the substantial impact of the U.S. on the Higgs discovery.” — J. Incandela
- “You need to appeal to varied stakeholders to convince them that you do valuable science with a sensible plan. Illustrate the benefits of particle physics to society.” — G. Blazey

The CE&O group developed the following goals, strategies, and recommendations with input from particle physicists and education and outreach professionals. The recommendations support a proactive, coordinated CE&O effort from the entire U.S. particle physics community.

As overarching goals for U.S. particle physics communication, education and outreach, we recommend:

1. Ensuring that the U.S. particle physics community has the resources necessary to conduct research and maintain a world leadership role.
2. Ensuring that the U.S. public appreciates the value and excitement of particle physics.
3. Ensuring that a talented and diverse group of students enters particle physics and other STEM careers, including science teaching.

We recommend five-year CE&O implementation recommendations that cut across all audiences:

1. Augment existing efforts with additional personnel and resources dedicated to nationwide coordination, training and support.
2. Develop a comprehensive central communication, education and outreach resource for physicists, with initial content available before the end of the 2013/2014 P5 process.

3. Provide communication training to the U.S. particle physics community.
4. Work with DPF and HEPAP to develop a sustainable process for collecting statistics on workforce development and technology transfer and with APS to investigate a U.S. economic impact study for physics research that includes particle physics.

We further recommend strategies for specific audiences:

For policy makers and opinion leaders: (1) Empower and enable members of the particle physics community to communicate and advocate coherently, consistently, and effectively on behalf of their science. (2) Develop an enduring process to track, update, and disseminate statistics on the impact of particle physics on society. (3) Put informed third-party advocates to work raising the profile of and informing key stakeholders about the importance of particle physics, physics, and basic science to the United States.

For the scientific community: (1) Foster more dialog and understanding between subfields of science. (2) Identify areas of common cause and unite in support of them. (3) Develop consensus in our field that we need to prioritize, buy into the mechanism of prioritization, and then support the resulting plan.

For the general public: (1) Engage the public in a wide range of outreach activities. (2) Make the public aware of direct and indirect applications of research, both historical and potential. (3) Communicate the role and stories of U.S. physicists in particle physics, particularly in major discoveries and in the context of our international collaborations.

For educators and students: (1) Directly engage with students and educators. Invite educators and students into our unique community. (2) Offer long-term professional development and training opportunities for educators (including pre-service educators), aligned with current and appropriate standards and enabling educators to explore best-practice teaching methods. Make an effort to collaborate with local schools of education whenever possible. (3) Create learning opportunities for students of all ages, including classroom, out-of-school and online activities that allow students to explore particle physics to construct their own understanding and develop the skills and habits of mind necessary to perform research.

Resources: An overarching recommendation that supports all goals and strategies is the augmentation of existing efforts with additional personnel and resources dedicated to nationwide coordination, training, and support for particle physics education, outreach, and communication activities. Such a team would enhance existing efforts and spearhead new initiatives, such as the development of a comprehensive central communication, education, and outreach resource for physicists, the development of sustainable methods to collect statistics on workforce development and technology transfer, materials designed to inform the public about direct and indirect applications of particle physics, the creation of professional development opportunities for educators, and new learning opportunities for students of all ages.

1.11 Conclusion

Here we recapitulate the Conclusion given previously in our Executive Summary. With the completion of the Standard Model, particle physicists now turn their attention to still deeper questions about the nature of matter and the constituents of the universe. This report proposes an ambitious array of new experiments. We consider it realistic to carry out these experiments through a long-term plan and through global partnerships. Particle physicists have been the pioneers of large-scale scientific projects. We have constructed facilities of unprecedented scale, including the Tevatron and the Large Hadron Collider, through decades-long programs requiring world-wide collaboration. These led to discoveries that are the foundation of our current success.

Several strategic goals have emerged from the Snowmass study.

- Probe the highest possible energies and distance scales with the existing and upgraded Large Hadron Collider and reach for even higher precision with a lepton collider; study the properties of the Higgs boson in full detail.
- Develop technologies for the long-term future to build multi-TeV lepton colliders and 100 TeV hadron colliders.
- Execute a program with the U.S. as host that provides precision tests of the neutrino sector with an underground detector; search for new physics in quark and lepton decays in conjunction with precision measurements of electric dipole and anomalous magnetic moments.
- Identify the particles that make up dark matter through complementary experiments deep underground, on the Earth's surface, and in space, and determine the properties of the dark sector.
- Map the evolution of the universe to reveal the origin of cosmic inflation, unravel the mystery of dark energy, and determine the ultimate fate of the cosmos.
- Invest in the development of new, enabling instrumentation and accelerator technology.
- Invest in advanced computing technology and programming expertise essential to both experiment and theory.
- Carry on theoretical work in support of experimental projects and to explore new unifying frameworks.
- Invest in the training of physicists to develop the most creative minds to generate new ideas in theory and experiment that advance science and benefit the broader society.
- Establish a nationally coordinated communication, education and outreach effort, supported by a dedicated team, to convey the excitement and value of our field to others.

In pursuit of these projects, we have developed a community that links together scientists from all regions of the world pursuing common goals. Our community is ready and eager to carry out the next steps in humankind's quest to understand the basic workings of the universe.

Intensity Frontier

Conveners: J. L. Hewett and H. Weerts

K. S. Babu, J. Butler, B. Casey, A. de Gouvêa, R. Essig, Y. Grossman, D. Hitlin, J. Jaros, E. Kearns,
K. Kumar, Z. Ligeti, Z.-T. Lu, K. Pitts, M. Ramsey-Musolf, J. Ritchie, K. Scholberg, W. Wester,
G. P. Zeller

2.1 Introduction

Particle physics aims to understand the universe around us. The Standard Model (SM) of particle physics describes the basic structure of matter and the forces through which matter interacts, to the extent we have been able to probe thus far. However, it leaves some big questions unanswered. Some are within the SM itself, such as why there are so many fundamental particles and why they have different masses. In other cases, the SM simply fails to explain some phenomena, such as the observed matter-antimatter asymmetry in the universe, the existence of dark matter, and the presence of non-zero neutrino masses. These gaps lead us to conclude that the universe must contain new and unexplored elements of nature.

These questions are best pursued with a variety of approaches, rather than with a single experiment or technique. Particle physics uses three basic approaches, often characterized as exploration along the Cosmic, Energy and Intensity Frontiers. Each employs different tools and techniques, but they ultimately address the same fundamental questions. This allows a multi-pronged approach, where attacking basic questions from different angles furthers knowledge and provides deeper answers, so that the whole is more than a sum of the parts.

The Intensity Frontier explores fundamental physics with intense sources and ultra-sensitive detectors. It encompasses searches for extremely rare processes and for tiny deviations from Standard Model expectations. Intensity Frontier experiments use precision measurements to probe quantum effects. They typically investigate new laws of physics that manifest themselves at higher energies or weaker interactions than those directly accessible at high-energy particle accelerators. This is illustrated in Fig. 2-1. These experiments require the greatest possible beam intensities of neutrinos, electrons, muons, photons or hadrons, as well as large detectors. This provides an opportunity for substantial new discoveries complementary to Energy and Cosmic Frontier experiments.

For neutrinos, large underground detectors are central and necessary to understand the physics beyond the Standard Model that has been revealed by the flavor oscillation of massive neutrinos. Coupled with intense neutrino beams, they provide the means to establish the neutrino mass hierarchy and determine the CP-violating phase inherent in the predominant three-flavor paradigm, which is still to be robustly confirmed. This design also empowers searches for unexpected phenomena such as non-standard interactions or sterile neutrinos. Super-massive, underground detectors continue the search for baryon number violation by increasing the sensitivity to the proton lifetime by an order of magnitude, moving deep into the territory motivated by current thinking on the unification of forces. High-resolution, low-background underground detectors search for exotic nuclear decays to determine whether neutrinos are their own antiparticles.

Intense beams of muons are required to search for charged-lepton flavor-violating processes, such as muon to electron conversion, $\mu \rightarrow e\gamma$ or $\mu \rightarrow eee$ decays, which are extremely rare in the Standard Model, but may be detectable in scenarios with new physics. Study of the intrinsic properties of muons, such as the magnetic and electric dipole moments, become possible with intense muon beams. Large samples of tau leptons, produced at high luminosity e^+e^- storage rings, can search for similar observables in the third generation.

In the quark sector, much more precise bottom, charm, and strange particle decay measurements will increase by over an order of magnitude the sensitivity to deviations from the Standard Model, as well as allow for the precision measurements of flavor-changing neutral-current and CP-violating decays. This suite of measurements will be crucial to understand the detailed nature of any new phenomena the Large Hadron Collider (LHC) at CERN may uncover, and will strongly constrain new physics scenarios.

Intense beams of electrons can enable searches for hidden-sector force particles, which may mediate dark matter interactions. Intense photon beams may produce para-photons, as well as axions, axion-like particles, or scalars. Some of these may be connected to dark matter or dark energy. The Intensity Frontier can shed light on whole new worlds of light, weakly-coupled particles, which have so far remained hidden from observation.

The intensity frontier program was defined in terms of six areas that formed the basis of working groups: experiments that probe (1) neutrinos[1], (2) baryon number violation[2], (3) charged leptons[3], (4) quark flavor[4], (5) nucleons, nuclei, and atoms[5], and (6) new light, weakly-coupled particles[6]. The working groups began their efforts well in advance of the Summer Study, holding regular meetings and soliciting written contributions. The 2013 Community Summer Study followed the 2011 workshop on *Fundamental Physics at the Intensity Frontier* held in Rockville, MD. The 2011 workshop was sponsored by the Office of High Energy Physics in the U.S. Department of Energy Office of Science and was charged with identifying the most compelling science opportunities at the Intensity Frontier. The report of the Rockville meeting[7] defined the scope of the Intensity Frontier, which was sustained for the 2013 Summer Study. An additional resource detailing the piece of the Intensity Frontier program that is connected to Project X at Fermilab is the report of the 2012 Project X Physics Study[8].

The working group reports[1, 2, 3, 4, 5, 6] provide a clear overview of the science program within each area of the Intensity Frontier. The discovery opportunities are presented for facilities that will be available this decade or will come online during the next decade. Here, we summarize the findings from each working group from the 2013 Community Summer Study, affectionately called “Snowmass”.

2.2 Neutrinos

2.2.1 Overview and summary

Decades of experimental and observational scrutiny have revealed less than a handful of phenomena outside the Standard Model, among them evidence for dark energy and dark matter, and the existence of non-zero neutrino masses. While many experiments continue to look for other new phenomena and deviations from Standard Model predictions, it is clear that continued detailed study of the neutrino sector is of the utmost importance.

Compared to the other fermions, the elusive nature of the neutrinos has made them extremely difficult to study in detail. In spite of the challenges, **neutrino physics has advanced quickly and dramatically**

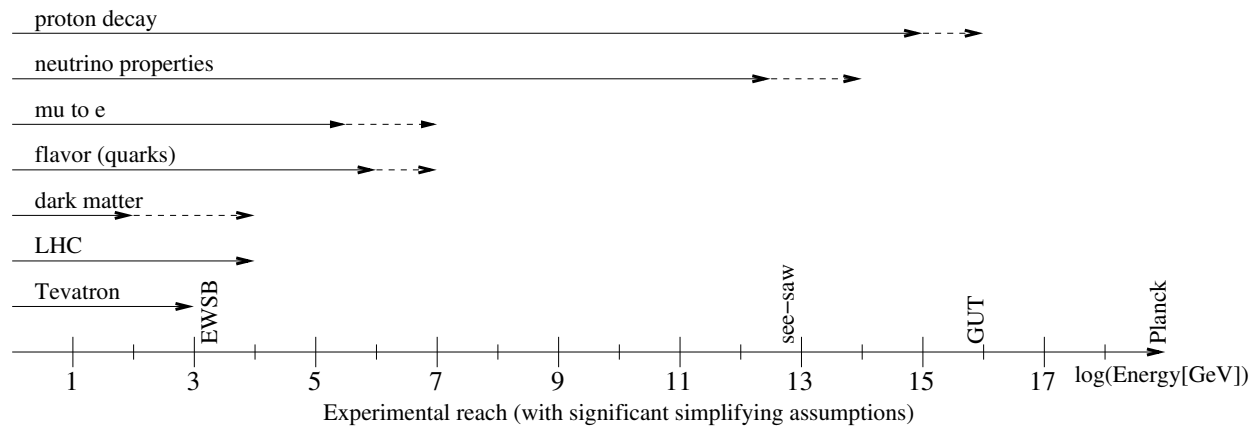


Figure 2-1. The energy scale of new physics that is probed by various experimental programs as indicated. The solid line represents the present level of experimental sensitivity, while the dashed line indicates the expected sensitivity of proposed facilities. The Intensity Frontier programs (proton decay, neutrino properties, μ to e , flavor) provide indirect probes of new physics effects. In contrast, the results presented for the Cosmic (dark matter) and Energy (14 TeV LHC, Tevatron) Frontier programs represent the direct search for the production of new particles. Indirect searches are also possible at these facilities, and increase their sensitivity to high energy scales by roughly an order of magnitude. The vertical text shows the energy scale at which Electroweak Symmetry Breaking (EWSB), the neutrino see-saw mechanism, and Grand Unified Theories (GUT) occur, and where the quantum effects of gravity become strong (Planck).

since the end of the last century. Thanks to a remarkable suite of experiments and associated theoretical work, two previously unknown and closely related features of neutrinos now stand out clearly: neutrinos have mass, and neutrinos of different generations mix with each other. Starting from almost no knowledge of the neutrino masses or lepton mixing parameters twenty years ago, we have built a robust, simple, three-flavor paradigm which successfully describes most of the data.

Experiments with solar and atmospheric neutrinos, as well as neutrinos produced in reactors and accelerators, have established, beyond reasonable doubt, that a neutrino produced in a well-defined flavor state (e.g., a muon-type neutrino ν_μ) has a nonzero probability of being detected in a different flavor state (e.g., an electron-type neutrino ν_e). This flavor-changing probability depends on the neutrino energy and the distance traversed between the source and the detector. The only consistent explanation of nearly all neutrino data collected over the last two decades is a phenomenon referred to as “neutrino mass-induced flavor oscillation.”

In two different oscillation sectors, similar parallel stories unfolded: Hints of neutrino flavor change in experiments studying naturally-produced neutrinos were confirmed, and later refined, by experiments with human-made neutrinos. The disappearance of atmospheric ν_μ was unambiguously confirmed by several beam ν_μ disappearance experiments, which have now achieved high precision on the driving “atmospheric” mixing parameters, *i.e.*, the mass-squared difference $|\Delta m_{32}^2|$ and the mixing angle θ_{23} . The observation of the disappearance of ν_e from the Sun, a decades-old mystery, was definitively confirmed as evidence for flavor change using flavor-blind neutral-current interactions. This “solar” oscillation was further confirmed, and the pertinent “solar” mixing parameters (θ_{12} and the mass-squared difference Δm_{21}^2) were precisely measured using reactor antineutrinos and further solar data. This complementarity illustrates the importance of exploring the diverse neutrino sources available (see Fig. 2-2).

The current generation of detectors is now exploring oscillations in a three-flavor context, with both accelerator and reactor tour-de-force experiments having now measured, with good precision, the value of the third mixing angle, θ_{13} , via positive searches for $\nu_\mu \rightarrow \nu_e$ appearance and $\bar{\nu}_e$ disappearance.

Furthermore, while most of the data fit the three-flavor paradigm very well, some experiments have uncovered intriguing anomalies that do not fit this simple picture. These exceptions include apparent short-baseline $\nu_\mu \rightarrow \nu_e$ and $\bar{\nu}_\mu \rightarrow \bar{\nu}_e$ transitions, and the anomalous disappearance of reactor and radioactive source electron-type antineutrinos and neutrinos. Although these hints currently have only modest statistical significance, if confirmed they would be evidence for states or interactions present in theories beyond the SM.

The observation of neutrino oscillations implies that neutrinos have nonzero masses, a discovery of fundamental significance. We do not know the mechanism responsible for the generation of neutrino masses, but we can state with some certainty that new degrees of freedom are required. The number of options is enormous. The current data do not reveal, for example, whether the new physics scale is very low (~ 1 eV) or very high ($\sim 10^{15}$ GeV). The origin of neutrino masses is one of the biggest puzzles in particle physics today, and will only be revealed, and perhaps only indirectly, with more experimental information from different probes in the different frontiers of particle physics research. Furthermore, the pattern of lepton mixing is very different from that of quarks. We do not yet know what that means, but precision studies of lepton mixing via neutrino oscillations may reveal crucial information regarding the long-standing flavor puzzle.

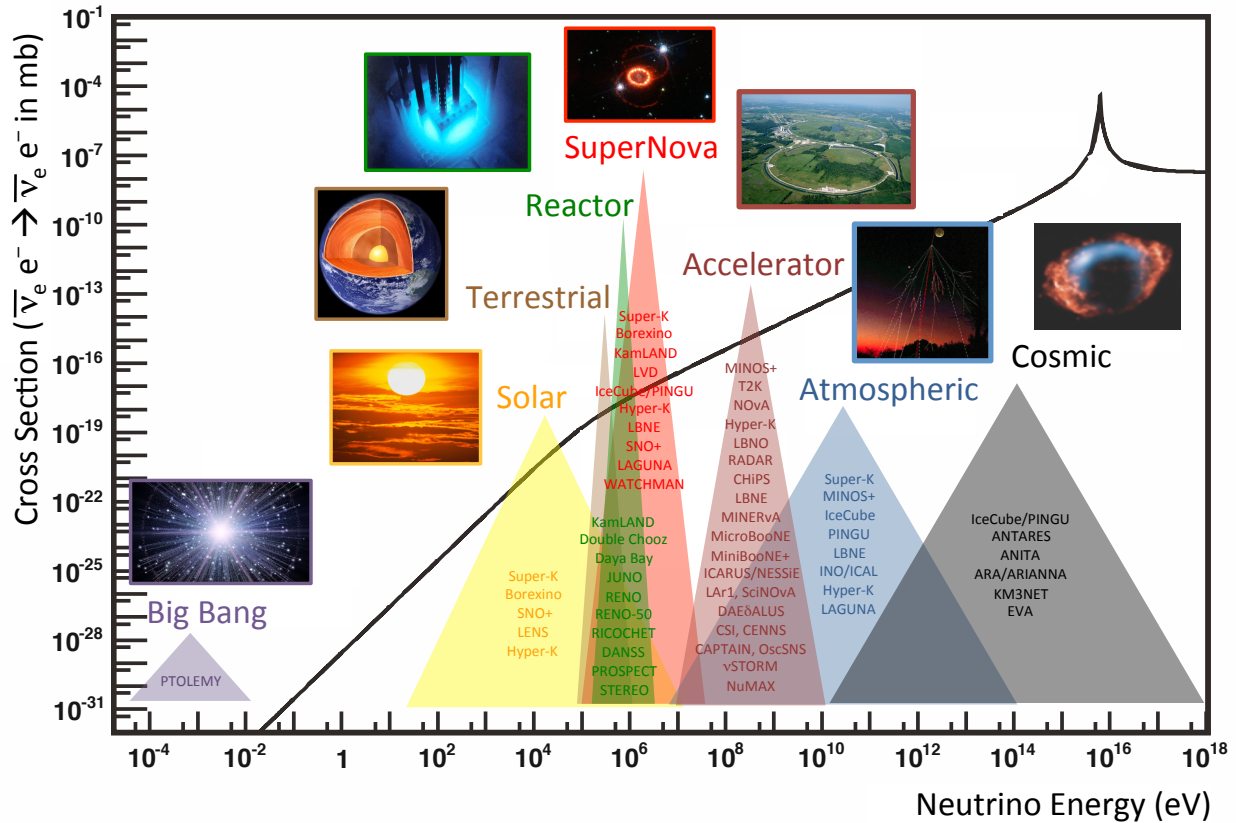


Figure 2-2. Neutrino interaction cross section as a function of energy, showing typical energy regimes accessible by different neutrino sources and experiments. The curve shows the scattering cross section for $\bar{\nu}_e e^- \rightarrow e^- \bar{\nu}_e$ on free electrons, for illustration. Figure is modified from [9]

2.2.2 The big questions and physics opportunities

We are now poised to answer some of the most fundamental and important questions of our time. **There is a clear experimental path forward**, which builds heavily on the recent successful history of this rapidly-evolving field of particle physics. We now list these questions in turn and review the proposed experimental program that will address them.

What is the pattern of neutrino masses? Is there CP violation in the lepton sector? To what extent does the three-flavor paradigm describe nature?

The current neutrino data allow for very large deviations from the three-flavor paradigm. New neutrino-matter interactions as strong as the SM weak interactions are not ruled out. In fact, mechanisms for generating neutrino mass not only allow, but sometimes predict, the existence of new “neutrino” states with virtually any mass.

Even in the absence of more surprises, we still do not know how the neutrino masses are ordered: Do we have two “light” and one “heavy” neutrino (the so-called normal mass hierarchy) or two “heavy” and one “light” neutrino (the inverted hierarchy)? The resolution of this issue is of the utmost importance, for both practical and fundamental reasons. As will become more clear below, resolving the neutrino mass hierarchy will allow us to optimize the information obtained from other neutrino experimental probes, including searches for leptonic CP-invariance violation, searches for the absolute value of the neutrino masses, and searches for the violation of lepton number via neutrinoless double-beta decay. In addition, the mass hierarchy will also reveal invaluable information concerning the origin of neutrino masses. If the mass hierarchy is inverted, we learn many things. For example, the inverted hierarchy implies that at least two of the three neutrino masses are quasi-degenerate, a condition that is not observed in the spectrum of charged leptons or quarks.

Experimental neutrino oscillation data revealed that CP invariance can be violated in the lepton sector. In particular, neutrino oscillation studies have the opportunity to explore a brand new source of CP violation. The lepton sector accommodates up to three new sources of CP violation in the neutrino mass matrix – one Dirac phase, which is analogous to the CKM phase in the quark sector, and two Majorana phases. Neutrino oscillations make it possible to explore the Dirac phase. The Majorana phases are non-zero only if neutrinos are their own antiparticles, and can be deduced (depending on the physics of lepton-number violation) from the rate of neutrinoless double-beta decay, a determination of the mass hierarchy, and a direct measurement of the neutrino mass. Currently, two sources of CP violation are known: the CP-odd phase in the CKM matrix, and the QCD $\bar{\theta}$ parameter. The former is known to be large, while the latter is known to be at most vanishingly small. Exploring CP violation in the lepton sector is guaranteed to significantly increase our understanding of this phenomenon. It is also likely that information regarding CP violation in the lepton sector will play a key role when it comes to understanding the mechanism for baryogenesis.

These questions can only be addressed directly by neutrino oscillation experiments. The current generation of oscillation experiments, including Double Chooz, RENO, Daya Bay, T2K, and NO ν A, will start to resolve the neutrino mass hierarchy. Combining their data samples may provide a first glimpse at CP violation in the lepton sector. These will also provide improved measurements of almost all neutrino oscillation parameters. Next-generation experiments, coupled with very intense proton beams, will definitively resolve the neutrino mass hierarchy and substantially improve our ability to test CP invariance in the lepton sector. Next-generation experiments with reactor neutrinos with intermediate baselines (around 50 km), as well as atmospheric neutrino experiments, may also independently resolve the neutrino mass hierarchy. The former will also provide precision measurements of the “solar” parameters, Δm_{21}^2 and θ_{12} . Different experiments with different energies, baselines, and detector technologies will allow for strong tests of the three-flavor paradigm. In the farther future, a more definitive probe of the three-flavor paradigm and precision measurements of CP

violation in the lepton sector (or lack thereof) will require long-baseline experiments with different neutrino beams. Leading candidates include neutrinos from pion decay at rest produced by high-intensity cyclotron proton sources, and neutrinos from muon storage rings. A muon-storage-ring facility should be able to measure the Dirac CP-odd phase with a precision on par with what has been achieved in the quark sector, and provide the most stringent constraints on the three-flavor paradigm, thanks to its capability to measure several different oscillation channels with similar precision.

Comprehensive and detailed studies of neutrino-matter scattering not only serve as tests of the SM and probes of nuclear structure but are also a definite requirement for precision neutrino oscillation experiments. The convolution of an uncertain neutrino flux with imprecise scattering cross sections and roughly estimated nuclear effects can result in large, even dominant, systematic errors in the measurement of neutrino oscillation parameters. More generally, we need to fully characterize neutrino-matter interactions (requiring dedicated experimental and theoretical efforts) to enable deeper understanding of neutrino oscillations, supernova dynamics, and dark matter searches.

Are neutrinos Majorana or Dirac particles?

Massive neutrinos are special. Among all known fermions, neutrinos are the only ones not charged under the two unbroken gauge symmetries: electromagnetism and color. This implies that, unlike all other known particles, neutrinos may be Majorana fermions. The existence of Majorana neutrinos would imply, for example, that neutrino masses are a consequence of a new fundamental energy scale in physics, potentially completely unrelated to the electroweak scale. Dirac neutrinos, on the other hand, would imply that $U(1)_{B-L}$, or some subgroup, is a fundamental symmetry of nature, with deep consequences for our understanding of the laws of physics.

If neutrinos are Majorana fermions, lepton number cannot be a conserved quantum number. (Conversely, lepton number violation indicates that massive neutrinos are Majorana fermions.) Hence, the best (perhaps only) probes for the hypothesis that neutrinos are Majorana fermions are searches for lepton number violation. By far, the most sensitive probe of lepton-number conservation is the pursuit of neutrinoless double-beta decay ($0\nu\beta\beta$), $(Z) \rightarrow (Z+2)e^-e^-$, where (Z) stands for a nucleus with atomic number Z . Independent from the strict connection to the nature of the neutrino, the observation of $0\nu\beta\beta$ would dramatically impact our understanding of nature (similar to the potential observation of baryon number violation) and would provide clues concerning the origin of the baryon asymmetry.

In many models for the origin of lepton-number violation, $0\nu\beta\beta$ is dominated by the exchange of virtual massive Majorana neutrinos, in such a way that the amplitude for this process is proportional to $m_{ee} \equiv \sum_i U_{ei}^2 m_i$, $i = 1, 2, 3, \dots$ under the assumption that all neutrinos are light (which holds for the three SM neutrinos). Under these circumstances, the observation of $0\nu\beta\beta$ would not only reveal that neutrinos are Majorana fermions, it would also provide information concerning the absolute values of the neutrino masses. Conversely, if we know the neutrino mass-squared differences and the magnitude of the relevant elements of the mixing matrix, we can predict the rate for $0\nu\beta\beta$ as a function of the unknown value of the lightest neutrino mass. In particular, if the neutrino mass hierarchy is inverted, there is a lower bound to $|m_{ee}| \gtrsim 20$ meV.

The current generation of 100-kg-class $0\nu\beta\beta$ search experiments should reach effective masses in the 100 meV range; beyond that, there are opportunities for multi-ton-class experiments that will reach sub-10 meV effective mass sensitivity, pushing below the inverted hierarchy region. In order to fully exploit the relation between $0\nu\beta\beta$ and nonzero Majorana neutrino masses, it is, however, imperative to understand in detail the associated nuclear matrix elements. These require detailed theoretical computations beyond those carried out to date.

What is the absolute neutrino mass scale?

While the values of the neutrino mass-squared differences are known, their absolute mass values remain elusive. In order to properly understand particle physics in general, and neutrinos in particular, it is clear that knowledge of particle masses – not just mass-squared differences – is mandatory. The current neutrino data still allow for the possibility that the lightest neutrino mass is vanishingly small, or that the masses of all three known neutrinos are quasi-degenerate. These two possibilities are qualitatively different and point to potentially different origins for the nonzero neutrino masses.

Neutrino masses can only be directly determined via non-oscillation neutrino experiments. The most model-independent observable sensitive to sub-eV neutrino masses is the shape of the end-point of beta decay spectra. Precision studies of tritium beta decay provide the most stringent bounds, and are expected to play a leading role in next-generation experiments. KATRIN, the most ambitious current-generation tritium-beta-decay experiment, will directly probe neutrino masses a factor of 10 smaller than the best current bounds. Innovative ideas are needed to go beyond this level of sensitivity.

Other probes of the absolute value of the neutrino masses include $0\nu\beta\beta$, discussed above, and analysis of the large-scale structure of the Universe. Both are, in their own way, much more model-dependent than precision studies of beta decay. Today, cosmological observables provide the most stringent bounds on the absolute values of the neutrino masses, constraining their sums to be below several tenths of an eV. The prospects for the next several years are very exciting.

Are there already hints of new physics in existing data?

There are intriguing anomalies that cannot be accommodated within the three-flavor paradigm, and suggest new physics beyond it. In particular, there is marginal ($2 - 4\sigma$) yet persistent evidence for oscillation phenomena at short baselines that is not consistent with the well-established oscillation lengths associated with the “solar” and “atmospheric” mass-squared differences. These anomalies, which are not directly ruled out by other experiments, include the excess of $\bar{\nu}_e$ events observed by the LSND experiment, the ν_e and $\bar{\nu}_e$ excesses observed by MiniBooNE (particularly at low-energies), the deficit of $\bar{\nu}_e$ events observed by reactor neutrino experiments, and the deficit of ν_e events observed in the SAGE and GALLEX radioactive source experiments. Although there may be several possible ways to explain these anomalies by introducing new physics, the most credible ones, while not ruled out, do not provide a good fit to *all* available neutrino data. Combined, the anomalies are often interpreted as evidence for one or more additional neutrino states, known as sterile neutrinos. The $3 + N$ sterile neutrino model, in which there are three light mostly-active neutrinos and N mostly-sterile neutrinos that mix with the active flavors, is often used to fit the existing data and gauge the reach of proposed next-generation experiments. For $N > 1$, these models allow for CP-violating effects in short-baseline appearance experiments.

Beyond particle physics, cosmology hints at the existence of additional neutrinos. Fits to astrophysical data sets – including the cosmic microwave background, large scale structure, baryon acoustic oscillations, and Big Bang nucleosynthesis – are sensitive to the effective number of light degrees of freedom (N_{eff}). In the SM, N_{eff} is equivalent to the effective number of neutrino species, although in principle this could include other types of light, weakly-coupled states. The recent Planck data are consistent with $N_{\text{eff}} = 3$ but still allow $N_{\text{eff}} = 4$. Potential connections between this hint and the short-baseline anomalies above are tantalizing but neither established nor excluded.

All these anomalies in the neutrino sector may disappear with more data; but if they are confirmed, the consequences would open up a whole new sector to explore experimentally and theoretically. The discovery of new neutrino states, for example, would revolutionize our understanding of particle physics. Definitive tests are clearly needed and concrete efforts are already underway. The MicroBooNE experiment, for example,

aims at addressing the low-energy excesses observed at MiniBooNE. A variety of neutrino sources and flavor-changing observables are being pursued as potential means to address the different anomalies.

What new knowledge will neutrinos from astrophysical sources bring?

Neutrinos come from natural sources as close as the Earth and Sun, as far away as distant galaxies, and even as remnants from the Big Bang. They range in kinetic energy from less than one meV to greater than one PeV. As weakly-interacting particles, they probe otherwise inaccessible properties of astrophysical sources; astrophysical neutrino sources shed light on the nature of neutrinos themselves, and on cosmology.

At the very lowest energies, we can access information about the $T_\nu = 1.95$ K Big Bang relic neutrinos via cosmological observables; direct detection of these particles is extremely challenging but nevertheless can be pursued.

In the few to few-tens-of-MeV energy range, large underground liquid scintillator, water Cherenkov, and liquid argon detectors are the instruments of choice. Solar neutrinos may have more to tell us about neutrino oscillations and other neutrino properties, and about solar physics. Neutrinos from stellar core collapse have the potential not only to shed light on the astrophysics of gravitational collapse, but also to provide a unique probe of neutrino properties. It is now even possible to study the Earth via MeV geoneutrinos from terrestrial radioactivity.

In the TeV, and higher, energy region, fluxes of atmospheric neutrinos start to diminish, and neutrinos of astrophysical origin begin to dominate the flux. Unlike photons and charged particles, these cosmic neutrinos travel unimpeded from their sources and will bring information on the origin of ultra-high-energy cosmic rays. Due to the very small expected fluxes, the construction of neutrino telescopes requires the instrumentation of enormous natural reservoirs of water. The recent detection of the first ultra-high-energy astrophysical neutrinos by IceCube has opened a critical new window of investigation into the study of nature's highest-energy particle accelerators.

2.2.3 The path forward

Table 2.1 gives a summary of the many current and proposed experiments, in the U.S. and abroad, designed to address the physics questions posed in this section. The number of possibilities is endless, nonetheless we now describe a specific path forward, both in the U.S. and in an international context. Neutrino physics is a broad subfield of fundamental particle physics, and requires a multi-pronged approach in order to address all the outstanding questions and fully explore the new physics revealed by neutrino oscillation experiments. Investment in a range of large, medium and small-scale neutrino experiments (as well as in detector R&D and theory) will ensure a healthy program.

Comprehensive test of the three-flavor paradigm, via long-baseline, precision neutrino oscillation experiments: The next-generation experiments will take full advantage of conventional neutrino beams from pion decay in flight. These will begin to over-constrain the parameter space, and will start to seriously explore CP-violating phenomena in the lepton sector. The U.S., with the Long-Baseline Neutrino Experiment (LBNE) and a future multi-megawatt beam from Project X, is uniquely positioned to lead an international campaign to measure CP violation and aggressively test the three-flavor paradigm. Complementary experiments with different energies, baselines, and detector technologies (*e.g.*, Hyper-K in Japan) are required in order to fully exploit conventional neutrino beams. The accompanying very-large detectors, if placed underground, also allow for the study of atmospheric neutrinos, nucleon decay, and precision measurements of neutrinos from a

galactic supernova explosion. PINGU, an upgrade of IceCube, provides a promising opportunity to measure the mass hierarchy using atmospheric neutrinos.

Further in the future, experiments will require more intense, and better understood neutrino beams. Promising possibilities include neutrinos from muon storage rings (e.g., NuMAX), and neutrinos from very intense cyclotron-based sources of pion decay at rest (e.g., DAEδALUS). Muon-based neutrino beams in particular have strong synergies with Project X and provide a necessary step in the R&D for a high-energy muon collider. While these large, ambitious projects are vigorously developed, the following medium and small-scale neutrino activities need to be pursued.

- *Precision measurement of neutrino cross sections and a detailed understanding of the neutrino flux from pion-decay-in-flight neutrino beams.* These activities can be pursued in the “near-detectors” associated with the large, long-baseline projects or alongside R&D projects related to next-next generation neutrino beams, as well as by small-scale dedicated experiments. A well-considered program of precision scattering experiments in both low- and high-energy regimes, combined with a renewed dedicated theoretical effort to develop a reliable, nuclear-physics based description of neutrino interactions in nuclei is mandatory. Scattering measurements may also be of intrinsic interest.
- *Definite resolution of the current short-baseline anomalies.* These will (probably) require neutrino sources other than pion decay in flight and the pursuit of different flavor-changing channels, including $\nu_{e,\mu}$ disappearance and $\nu_\mu \rightarrow \nu_e$ appearance, using a combination of reactor, radioactive source, and accelerator experiments. In addition to small-scale dedicated experiments, such experiments can be carried out as part of R&D projects related to next-next generation neutrino beams (e.g., nuSTORM, IsoDAR).
- *Vigorous pursuit of R&D projects related to the development of next-next generation neutrino-beam experiments.* Medium and small experiments associated with such projects could also address several key issues in neutrino physics.

Searches for neutrinoless double-beta decay: The current generation of experiments is pursuing various detector technologies with different double-beta decaying isotopes. The goals of these experiments are to (1) discover neutrinoless double-beta decay, which is guaranteed if the neutrinos are Majorana fermions and their masses are quasi-degenerate (*i.e.*, masses of order 0.1-0.5 eV), and (2) provide information regarding the most promising technique for the next-generation of ton-scale experiments. Next-generation experiments aim at discovering neutrinoless double-beta decay if neutrinos are Majorana fermions and if the neutrino mass hierarchy is inverted. In the case of a negative result, assuming oscillation experiments have revealed that the neutrino mass-hierarchy is inverted, these experiments will provide strong evidence that the neutrinos are Dirac fermions. As with precision measurements of beta decay (see below), the information one can extract from the current and the next generation of neutrinoless double-beta decay experiments increases significantly if indirect evidence for neutrino masses is uncovered, for example, with cosmological probes.

Determination of the absolute values of the neutrino masses: Precision measurements of beta decay remain the most promising model-independent probes. While the KATRIN experiment is taking data, vigorous R&D efforts for next-generation probes (e.g., ECHo, Project 8, PTOLEMY) are required in order to identify whether it is possible to reach sensitivities to the effective “electron-neutrino mass” below 0.05 eV. Nontrivial information is expected from different cosmological probes of the large-scale structure of the Universe.

The allure and relevance of neutrino science and technology extends well beyond the fundamental research community. The neutrino signal itself may be useful for monitoring reactors in the context of international nuclear nonproliferation. The essential building blocks of neutrino science – detectors and accelerators – have important spin-off applications for medicine and in industry. Finally, ever since neutrinos were first

postulated and discovered, their unusual, ghostlike properties and non-intuitive behavior have fascinated the general public. The success of our field depends in no small part on our ability to effectively convey both the mystery and utility of neutrino science to the public, Congress, policy-makers, and funding agencies.

The diversity of physics topics that can be probed through the neutrino sector is enormous and the interplay between neutrino physics and other fields is rich. Neutrinos have and will continue to provide important information on structure formation in the early universe, Earth, solar and supernova physics, nuclear properties, and rare decays of charged leptons and hadrons. Conversely, information regarding neutrino properties and the origin of neutrino masses is expected from the Energy and Cosmic Frontiers, and from other areas of Intensity Frontier research. In other words, the neutrino sector sits at the nexus of the worldwide effort in Energy, Intensity and Cosmic Frontier physics.

2.3 Baryon number violation

2.3.1 Overview and summary

The stability of ordinary matter is attributed to the conservation of baryon number, B . The proton and the neutron are assigned $B = +1$, while their antiparticles have $B = -1$, and the leptons and antileptons all have $B = 0$. However, many leading theoretical ideas suggest that baryon number may be only an approximate symmetry, one that is violated by small amounts. Experimental observation of baryon number violation would have a profound impact on our understanding of the evolution of the Universe and the basic forces of nature. Baryon number violation is an essential ingredient for the creation of the observed asymmetry of matter over antimatter from the symmetrical Universe that emerged from the Big Bang. And baryon number violation is a hallmark prediction of grand unified theories.

2.3.2 Theoretical motivation

Grand unified theories (GUTs) are well-motivated on several grounds. They unify the strong, weak and electromagnetic forces into a single unified force. There is evidence from data on the force couplings at low energies that these couplings may unify to a single value at high energies. The three gauge couplings of the SM appear to take on a common value at a scale of about 10^{16} GeV, depending on the new physics that lies at the intermediate energies above the electroweak scale. Such a large energy scale (amounting to distance scale of order 10^{-30} cm) is well beyond the reach of any conceivable particle accelerators.

GUTs are based on symmetries and organize quarks and leptons into simple unified representations of symmetry groups that naturally explain the equality of the magnitude of the electric charge of the electron to that of the proton. GUTs provide an understanding of the quantum numbers of the quarks and leptons, as well as an explanation for the miraculous cancellation of chiral anomalies within each generation. GUTs, notably those based on the gauge symmetry $SO(10)$, require the existence of right-handed neutrinos, one per family, which enable the seesaw mechanism to generate small Majorana masses for the ordinary neutrinos. These right-handed neutrinos are prime candidates for the generation of the baryon asymmetry of the Universe via leptogenesis.

Grouping of quarks with leptons, and particles with antiparticles, into a common GUT multiplet leads to the violation of baryon number and thus proton decay. Super-heavy so-called X and Y gauge bosons mediate proton decay through dimension-6 operators, as illustrated in the left panel of Fig. 2-3. The minimal version

Table 2-1. Summary of the very many current and proposed experiments, in the U.S. and abroad, designed to address various physics questions. Rows refer to neutrino sources and columns refer to categories of physics topics these sources can address. The intent is not to give a “laundry list”, but to give a sense of the activity and breadth of the field. Some multipurpose experiments appear under more than one physics category. Experiments based in the U.S. (or initiated and primarily led by U.S. collaborators) are shown in blue and underlined (note that many others have substantial U.S. participation or leadership). Proposed and future experiments are in bold; current experiments (running or with construction well underway) are in regular font. More details and references can be found in the subsections of the Neutrino Working Group report [1]. Below, DAR refers to decay at rest and DIF to decay in flight.

Source	3-flavor osc.	Maj./Dirac	Abs. Mass	Interactions	Anomalies/Exotic ²	Astro/Cosmo
Reactor	KamLAND, Double Chooz, Daya Bay, JUNO , RENO, RENO-50			<u>RICOCHET</u>	DANNS, STEREO <u>PROSPECT</u> <u>RICOCHET</u> <u>US Reactor</u>	
Solar	Super-K, Borexino, SNO+, Hyper-K , <u>LENS</u>					Super-K, Borexino, SNO+, Hyper-K , <u>LENS</u>
Supernova ¹	Super-K, Borexino, KamLAND, LVD, <u>IceCube/PINGU</u> , Hyper-K , <u>LBNE</u> , SNO+, LAGUNA , <u>WATCHMAN</u>					Super-K, Borexino, KamLAND, LVD <u>IceCube/PINGU</u> , Hyper-K , <u>LBNE</u> , SNO+, LAGUNA , <u>WATCHMAN</u>
Atmospheric	Super-K, <u>MINOS+</u> , <u>IceCube/PINGU</u> , <u>LBNE</u> , ICAL , Hyper-K , LAGUNA					
Pion DAR	<u>DAEδALUS</u>			<u>OscSNS</u> , <u>CSI</u> CENNS , CAPTAIN	<u>OscSNS</u>	
Pion DIF	<u>MINOS+</u> , T2K, <u>NOνA</u> , Hyper-K , LAGUNA-LBNO , <u>RADAR</u> , <u>CHIPS</u> , <u>LBNE</u> , <u>ESSνSB</u>			<u>MicroBooNE</u> , <u>MINERνA</u> <u>NOνA</u> <u>SciNOνA</u>	<u>MicroBooNE</u> , <u>MiniBooNE+ /II</u> , Icarus/NESSiE , <u>LAr1</u> , <u>LAr1-ND</u> <u>MINOS+</u>	
μDIF	<u>NuMAX</u>			<u>nuSTORM</u>	<u>nuSTORM</u>	
Radioactive Isotopes		Many: see Nu2 report for table	KATRIN, <u>Project 8</u> , ECHO , <u>PTOLEMY</u>		SOX , CeLAND , Daya Bay Source, BEST <u>IsoDAR</u>	
Cosmic neutrinos						<u>IceCube/PINGU</u> , ANTARES/ <u>ORCA</u> , <u>ARA</u> , <u>ARIANNA</u> , <u>ANITA</u> , <u>EVA</u> , KM3NET

¹Included are only kton-class underground detectors; many others would also record events. ²We note that nearly all experiments can address anomalies at some level; we include in this column only those with this as a primary physics goal.

of the SU(5) GUT, the first GUT model, predicted proton decay within reach of the first-generation proton decay experiments. This model has already been excluded by their experimental limits on $p \rightarrow e^+ \pi^0$, as well as the mismatch of the three gauge coupling unification when extrapolated to scales of 10^{14} to 10^{15} GeV. GUTs based on larger symmetries such as SO(10) are consistent with both gauge coupling unification and experimental constraints, particularly if they include supersymmetry, as discussed below. Other possible theories include flipped SU(5), which favors the second generation and predicts the decay $p \rightarrow \mu^+ \pi^0$, and higher-dimensional GUTs, including those where quarks and leptons live on separate branes. Even negative experimental results in the search for proton decay provide valuable information to the pursuit of realistic grand unified theories.

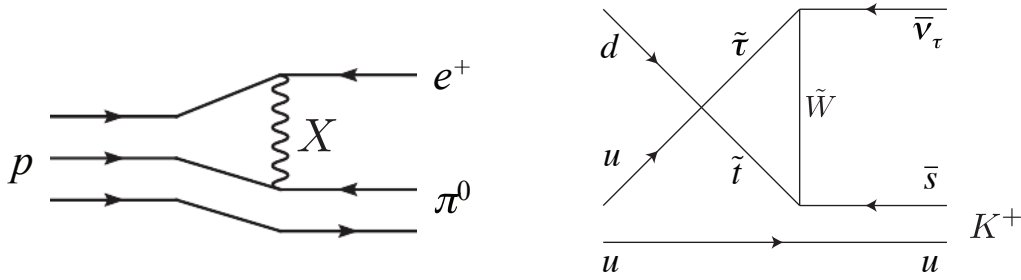


Figure 2-3. Diagrams inducing proton decay in SUSY GUTs. $p \rightarrow e^+ \pi^0$ mediated by X gauge boson (left), and $p \rightarrow \bar{\nu} K^+$ generated by a $d = 5$ operator. (right).

Supersymmetric grand unified theories (SUSY GUTs) are natural extensions of the SM that preserve the attractive features of GUTs, and predict a more precise unification of the three gauge couplings. The unification occurs at the higher energy of 2×10^{16} GeV, suppressing the dimension-6 gauge mediation responsible for $p \rightarrow e^+ \pi^0$ to a lifetime of at least a few $\times 10^{34}$ years, compatible with current experimental limits. Lifetimes of this magnitude are now being probed by Super-Kamiokande. However, the predictions are uncertain by an order of magnitude or so, due to the unknown masses of supersymmetric particles and GUT scale particles.

Supersymmetric GUTs (SUSY GUTs) bring a new twist to proton decay, by predicting the decay mode $p \rightarrow \bar{\nu} K^+$ which is mediated by a colored Higgsino generating a dimension-5 operator, as shown in the right panel of Fig. 2-3. Here, the $d = 5$ operator involves the electroweak wino and supersymmetric partners of the quarks and leptons. The predicted lifetime for this mode in minimal supersymmetric SU(5) theories is typically less than 10^{32} years, much shorter than the current experimental lower limit of 6×10^{33} years, provided that the supersymmetric particle masses are less than about 3 TeV. This limit is difficult to avoid in minimal SUSY SU(5) theories unless the supersymmetric particle masses are much above 3 TeV. However, there are non-minimal supersymmetric SU(5) models, as well as SUSY GUTs based on the larger symmetry SO(10), that accommodate the current experimental bounds and predict proton decay within reach of current and next generation experiments. SUSY GUTs generally prefer decays into strange mesons, and although $p \rightarrow \bar{\nu} K^+$ is predominant, modes such as $p \rightarrow \mu^+ K^0$ are sometimes favored.

One class of minimal SO(10) models which employs a single representation that contains the Higgs boson has been developed. Owing to their minimality, these models are quite predictive with regard to the neutrino mass spectrum and oscillation angles. Small quark mixing angles and large neutrino oscillation angles emerge simultaneously in these models at the weak scale, despite their parity at the fundamental level. The neutrino oscillation angle θ_{13} is predicted to be large in these models. In fact, this mixing angle was predicted to have the value $\sin^2 2\theta_{13} \simeq 0.09$, well before it was measured to have this central value. Proton decay studies of

these models in the supersymmetric context show that at least some of the modes among $p \rightarrow \bar{\nu}\pi^+$, $n \rightarrow \bar{\nu}\pi^0$, $p \rightarrow \mu^+\pi^0$ and $p \rightarrow \mu^+K^0$ have inverse decay rates of order 10^{34} years, while for $p \rightarrow e^+\pi^0$ the rate is of order 10^{35} years.

A common prediction of many GUTs is that proton decay modes obey the selection rule $\Delta(B - L) = 0$, where L denotes lepton number. For example, the proton decay channels $p \rightarrow e^+\pi^0$ and $p \rightarrow \bar{\nu}K^+$ follow this rule. However, theories with additional spatial dimensions predict that $|\Delta(B - L)| = 2$, which leads to decays such as $n \rightarrow e^- + \pi^+$ and $n \rightarrow e^- K^+$. Other possible operators may result in three-body proton decay modes such as $p \rightarrow l^+\bar{\nu}\nu$ or $p \rightarrow l^+\pi^0\pi^0$. A thorough search for nucleon decay should consider a multitude of possible final states. However, $p \rightarrow e^+\pi^0$ and $p \rightarrow \bar{\nu}K^+$ serve as adequate benchmarks to evaluate future experiments, since they are routinely predicted to be the dominant decay modes, and the detector features that efficiently find these modes will usually perform well when searching over a wide array of modes.

2.3.3 Experimental programs

The Super-Kamiokande (Super-K) water Cherenkov experiment dominates the current limits set on the lifetime of the proton and bound neutron. No signs of nucleon decay have been seen yet in any Super-K analysis of any nucleon decay mode. The 22,500-ton fiducial mass of the detector contains 7.5×10^{33} protons and 6.0×10^{33} neutrons. Fully contained atmospheric neutrino interactions in the GeV range constitute the background. The background is estimated using the detailed model that is standard for Super-K, one that takes into account the atmospheric neutrino flux, neutrino-nucleus cross sections, and interactions within the nucleus. The proton decay signal is simulated using a Monte Carlo program that includes effects due to Fermi motion, nuclear binding potential, intranuclear reactions, and correlated nucleon effects (all of which are absent for the free proton that is the hydrogen nucleus). For the two benchmark modes mentioned above, Super-Kamiokande has observed no candidates for proton decay and reported the following 90% C.L. on the partial lifetimes:

$$\tau(p \rightarrow e^+\pi^0) > 1.4 \times 10^{34} \text{ yrs}, \quad \tau(p \rightarrow \bar{\nu}K^+) > 5.9 \times 10^{33} \text{ yrs}. \quad (2.1)$$

The Super-K collaboration expects to operate the detector for many more years, as the far detector for T2K and as a supernova detector, but also to continue the search for proton decay.

A next-generation underground water Cherenkov detector, Hyper-Kamiokande (Hyper-K), is proposed in the Kamioka mine in Japan. It will serve as the far detector of a long baseline neutrino oscillation experiment using the off-axis J-PARC neutrino beam. It will also be capable of observing nucleon decays, atmospheric neutrinos, and neutrinos of astronomical origin. The fiducial mass of the detector is planned for 0.56 million metric tons, and 99,000 photomultipliers would instrument the detector with photon coverage of 20% (but probably with enhanced quantum efficiency compared to the PMTs used in Super-K).

The sensitivity of Hyper-K for nucleon decays has been studied based on scaling of the Super-K analysis. Due to the very large mass, Cherenkov ring reconstruction in the GeV range, and demonstrated low background rate, water Cherenkov is the best detector technology for the proton decay signatures $e^+\pi^0$ and $\mu^+\pi^0$. It is safe to assume an overall signal efficiency of 40%, where the inefficiency is dominated by the nuclear interaction of the pion. For comparison, the detection efficiency for the decay of a free proton in water is 87%. The background rate is well-established by Monte Carlo studies, plus measurements using the T2K near detector; one can conservatively assume two events per megaton-years. Based on these numbers, the 90% C.L. sensitivity of Hyper-K for a 10-year exposure is greater than 10^{35} years.

For $p \rightarrow \nu K^+$, most sensitivity comes from the two relatively background-free decay modes: $K^+ \rightarrow \pi^+\pi^0$ and $K \rightarrow \mu^+\nu$ with nuclear- γ tag. Based on the estimated background rate for Super-K, a 10-year exposure

of Hyper-K would have an expected background between 20 and 35 events with an efficiency in the range 13% to 19%. If the detected number of events is equal to the background expectation, the 90% CL limit would be roughly 3×10^{34} years for this mode. This estimation assumes that a reoptimization of the analysis (*i.e.*, tighter cuts) to accommodate the higher background rate will not be performed.

The ability to fully reconstruct particle interactions in high resolution has long been recognized as the selling point for the liquid argon time projection chamber (LArTPC) technology. Resolving neutrino interactions was the prime motivation for selecting the LArTPC technology for the far detector of the LBNE long-baseline neutrino oscillation experiment. Using this detector to search for proton decay (and other non-accelerator physics) is the primary motivation for locating LBNE deep underground. The fine spatial resolution of the LArTPC coupled with a dE/dx measurement allows the reconstruction of all final-state charged particles, including an accurate assessment of particle type, by distinguishing between muons, pions, kaons, and protons. Electromagnetic showers are also readily measured with a significant ability to distinguish between those that originate from π^0 decay into photons and those that originate from charged-current electron neutrino interactions.

Per kiloton, LArTPC technology will outperform water Cherenkov in both detection efficiency and atmospheric neutrino background rejection for most nucleon decay modes, although intranuclear effects are smaller for oxygen and non-existent for hydrogen. Taking mass and cost into account, however, water Cherenkov technology still performs the best for the $p \rightarrow e^+ \pi^0$ final state topology, where the signal efficiency is roughly 40% and the background rate is two events per megaton-year. The corresponding estimate for a LArTPC is 45% efficiency and one event per megaton-year, which is not enough of an improvement to overcome the penalty of lower mass.

For the $p \rightarrow \nu K^+$ channel, the final-state kaon is below Cherenkov threshold, the signal efficiency is less than 20%, and the background rate is substantial. This mode is thus well-suited to a LArTPC, where the K^+ track is reconstructed with the correct specific ionization and range. The decay products of the kaon are also identified with high efficiency for all kaon decay channels. The total efficiency for the νK^+ mode is estimated to be as high as 96.8%, with a background rate of one event per megaton-year. Based on these numbers and a ten-year exposure, the 34 kiloton LBNE and 560 kiloton Hyper-K detectors have comparable sensitivity (at 90% C.L.), but the LArTPC would have an estimated background of 0.3 events compared to tens of events for Hyper-K. Experimental searches for rare events in the presence of significant backgrounds are notoriously more problematic than background-free searches. A 10-year exposure of the 34-kiloton LBNE LArTPC detector would set a partial lifetime limit of roughly 3×10^{34} years, nearly the same as the sensitivity of Hyper-K, despite 20 times fewer proton-years. There are a number of other nucleon decay modes for which a LArTPC competes well with water Cherenkov, mainly because of high background rates in the water Cherenkov analysis.

Figure 2-4 shows past experimental limits on various proton decay modes from first generation experiments, the current lifetime limits from Super-K, and projected lifetime limits for ten years of running of Hyper-K and a 34-kiloton LBNE LArTPC. These are compared with theoretical predictions from the various GUTs mentioned above, where the depicted bands roughly capture the expected ranges. The next generation experiments Hyper-K and LBNE will make substantial progress beyond the Super-K search and are sensitive to lifetimes predicted by numerous well-motivated GUT models.

2.3.4 Neutron-antineutron oscillation

Grand unified theories and the search for nucleon decay, as described above, are the principal tools for understanding baryon number violation by one unit. However, baryon number may be violated by two units

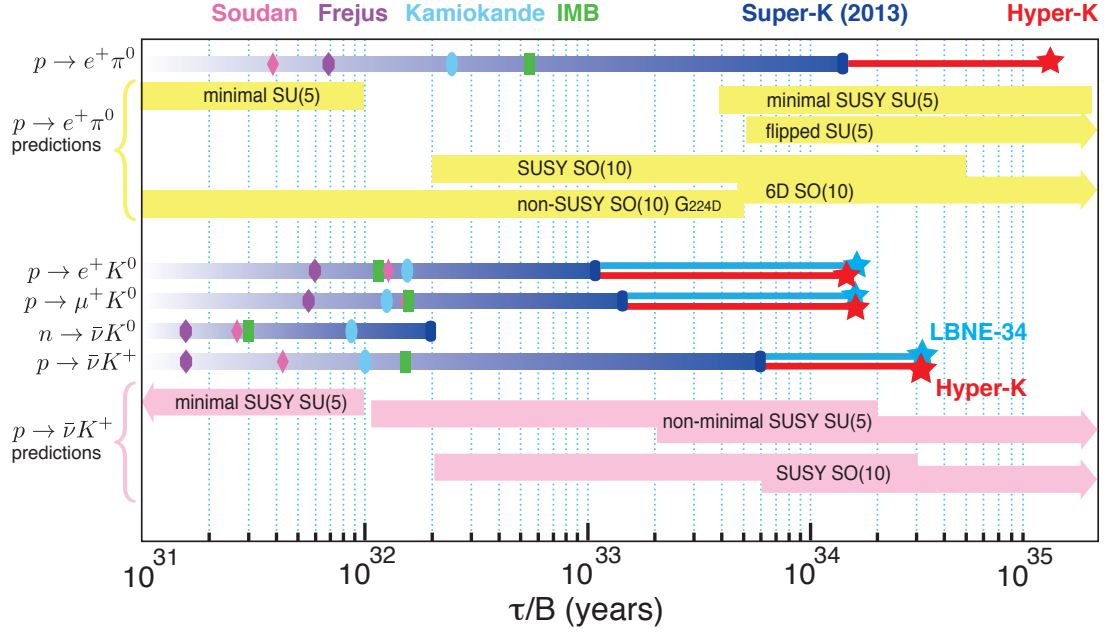


Figure 2-4. The experimental reach of Hyper-Kamiokande and a 34-kton LBNE LArTPC are compared to prior experiments and the rough lifetime predictions from a wide range of GUT models. The projected limits are for 10 live years of running, at 90% C.L., calculated for a Poisson process including background assuming the detected events equal the expected background.

through processes such as dinucleon decay and neutron-antineutron oscillation and is predicted at observable rates in other classes of models. These processes afford violation of baryon number without violation of lepton number. They arise from operators with new physics contributions at mass scales of 100 – 1000 TeV, well below the GUT scale. Interesting theoretical models, especially those based on quark-lepton symmetry realized at the multi-TeV scale, predict a $n - \bar{n}$ oscillation time of order 10^{10} sec. Observation of $n - \bar{n}$ oscillations with probability not far from the current limit could refute the paradigm of leptogenesis, and in itself would suggest an alternate source for generating the baryon asymmetry of the universe.

One search technique for neutron-antineutron oscillation involves a beam of free neutrons where one neutron could transform into an antineutron and annihilate in a distant detector. The probability of oscillation is given by $P(n \rightarrow \bar{n}, t) \simeq [\delta m t]^2$, where δm is the baryon-violating interaction strength. The same process may also occur for nuclei, where bound neutrons may transform into antineutrons and annihilate within the nucleus, producing an isotropic final state of pions. In the case of bound neutrons, a nuclear suppression factor must be applied to calculate a free oscillation probability. Currently, the limits from bound $n - \bar{n}$ transformation in oxygen nuclei in Super-Kamiokande are slightly more restrictive than the result from the best free neutron oscillation search. However, the sensitivity in water is hindered by a large background rate of roughly 25 events per 100 kiloton years with 12% signal efficiency. It is hoped that a LArTPC will provide better performance, but this is still under study.

The previous experimental search for free $n - \bar{n}$ transformations employed a cold neutron beam from the research reactor at Institute Laue-Langevin (ILL) in Grenoble, and reported a limit on the transformation of $\tau > 8.6 \times 10^7$ seconds. The average velocity of the cold neutrons was roughly 600 m/s and the neutron observation time was approximately 0.1 seconds. A net magnetic field of less than 10 nT was maintained

over the evacuated flight volume in order to satisfy the quasi-free conditions required for oscillations to occur. Antineutron appearance was sought through annihilation in a $130\mu\text{m}$ carbon film target, generating a star pattern of several secondary pions, viewed by a tracking detector, and an energy deposition of 1-2 GeV in the surrounding calorimeter. In one year of operation this ILL experiment saw zero candidate events.

A new experimental search for oscillations using free neutrons from a 1 MW spallation target, NNbarX at Project X, has the potential to improve existing limits on the free oscillation probability by at least 2 orders of magnitude (and potentially up to 4 orders of magnitude with further R&D) by exploiting a new slow neutron source together with an optics technology developed for materials science experiments. The possibility to create a dedicated neutron source for particle physics experiments offers a unique opportunity to design a neutron optical system that accepts a large fraction of the neutron flux. The initial goal of NNbarX will be to improve on the probability of an $n - \bar{n}$ transition by at least a factor 30 compared to the previous limit set in the ILL-based experiment. This is achievable with existing technology. A substantial feature of the experiment is the opportunity to validate a discovery by removing the required magnetic field shielding, destroying the quasi-free condition, and eliminating the signal events. The R&D phase of the experiment, including development of the conceptual design of the cold neutron spallation target, and conceptual design and optimization of the performance of the first-stage of NNbarX, is expected to take two to three years. Preliminary results from this effort suggest that an improvement over the ILL experiment by a factor of more than 100 may be realized in the horizontal mode. A vertical flight path currently under study could achieve further increases in sensitivity. The running time of the first stage of NNbarX experiment is anticipated to be 3 years. Future stages of NNbarX will depend upon the demonstration of the technological principles and techniques of the first stage.

2.4 Charged leptons

The charged lepton experimental program has enormous physics potential. There are discovery opportunities in experiments that will be conducted over the coming decade using existing facilities, and in more sensitive experiments possible with future facilities such as Project X. Exquisitely sensitive searches for rare decays of muons and tau leptons, together with precision measurements of their properties, will either elucidate the scale and dynamics of flavor generation, or limit the scale of flavor generation to well above 10^4 TeV.

2.4.1 Flavor-violating processes

The crown jewel of the program is the discovery potential of muon and tau decay experiments searching for charged lepton flavor violation (CLFV) with several orders-of-magnitude improvement in sensitivity in multiple processes. An international program of CLFV searches exists, with experiments recently completed, currently running, and soon to be constructed in the United States, Japan, and Europe. These include the completion of the MEG experiment at PSI, an upgrade of MEG, the proposed $\mu 3e$ search at PSI, new searches of muon to electron conversion ($\text{Mu}2e$ at Fermilab, COMET at J-PARC), studies of τ decay at SuperKEKB, and over the longer term, experiments exploiting megawatt proton sources such as Project X.

Over the next decade, gains of up to five orders of magnitude are feasible in muon-to-electron conversion and in the $\mu \rightarrow 3e$ searches, while gains of at least two orders of magnitude are possible in $\mu \rightarrow e\gamma$ and $\tau \rightarrow 3\ell$ decay and more than one order of magnitude in $\tau \rightarrow \ell\gamma$ CLFV searches. The relative sensitivity to new physics between these processes depends on the type of physics amplitude responsible for lepton flavor violation. The four-fermion operators that mediate these decays or conversions can be characterized by two

parameters: Λ , which sets the mass scale of the four fermion amplitude, and κ , which governs the ratio of the four fermion amplitude and the dipole amplitude. Λ depends on both the mass and coupling strength of new particles that may mediate μu to e transitions. For $\kappa \ll 1$, the dipole-type operator dominates CLFV phenomena, while for $\kappa \gg 1$, the four-fermion operators are dominant. Figures 2-5 and 2-6 show these relationships and the capability of new experimental searches which can extend our knowledge quite dramatically in the next decade.

The pattern of violation that emerges yields quite specific information about new physics in the lepton sector. Existing searches already place strong constraints on many models of physics beyond the Standard Model; the contemplated improvements increase these constraints significantly, covering substantial regions of the parameter space of many new physics models. These improvements are important regardless of the outcome of new particle searches of the LHC. The next generation of CLFV searches is an essential component of the particle physics road map going forward. If the LHC finds new physics, then CLFV searches will confront the lepton sector in ways that are not possible at the LHC, while if the LHC uncovers no sign of new physics, CLFV may provide the path to discovery.

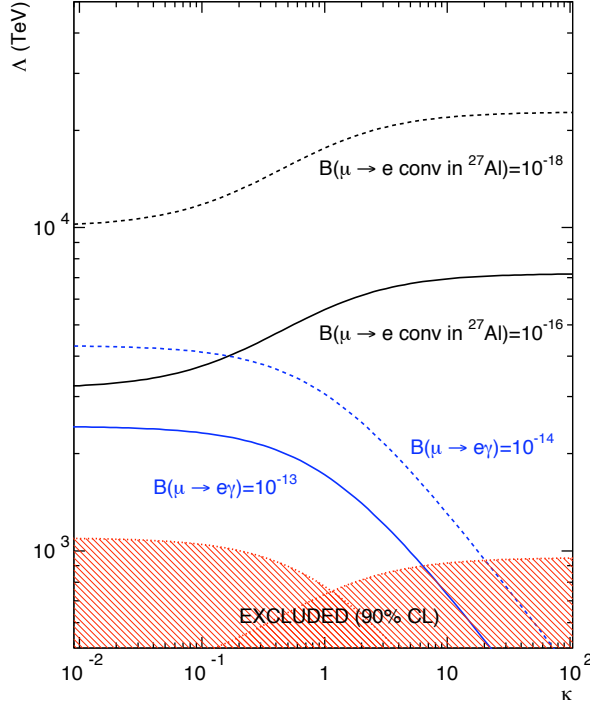


Figure 2-5. Sensitivity of a $\mu \rightarrow e$ conversion in ^{27}Al experiment that can probe a normalized capture rate of 10^{-16} and 10^{-18} , and of a $\mu \rightarrow e\gamma$ search that is sensitive to a branching ratio of 10^{-13} and 10^{-14} , to the new physics scale Λ as a function of κ , as defined in the text. Also depicted is the currently excluded region of this parameter space. From [10].

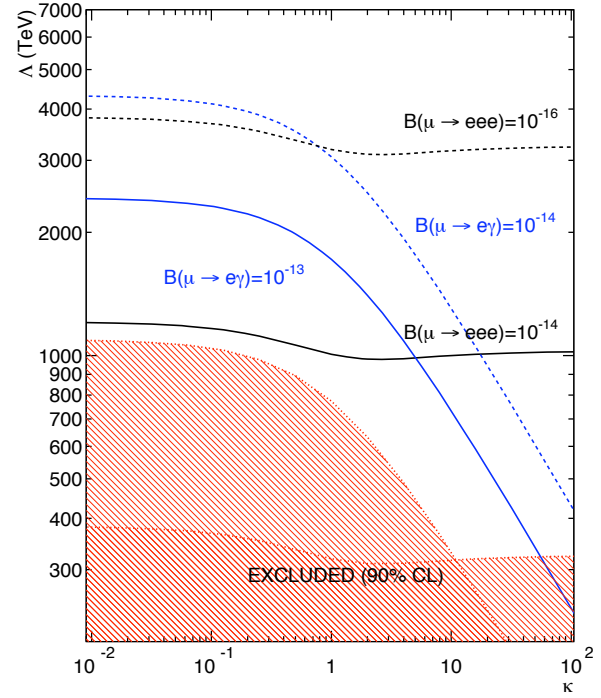


Figure 2-6. Sensitivity of a $\mu \rightarrow eee$ experiment that is sensitive to branching ratios 10^{-14} and 10^{-16} , and of a $\mu \rightarrow e\gamma$ search that is sensitive to a branching ratio of 10^{-13} and 10^{-14} , to the new physics scale Λ as a function of κ , as defined in the text. Also depicted is the currently excluded region of this parameter space. From [10].

In general, muon measurements have the best sensitivity over the largest range of the parameter space of many new physics models. There are, however, models in which rare tau decays could provide the discovery channel. Tau flavor violation searches will have their sensitivity extended by around an order of magnitude at

new e^+e^- flavor factories. Polarized electron beams can provide an additional gain in sensitivity. All feasible CLFV searches should be conducted, since the best discovery channel is model-dependent and the model of new physics is not yet known. Should a signal be observed in any channel, searches and measurements in as many CLFV channels as possible will be crucial in determining the nature of the underlying physics, since correlations between the rates expected in different channels provide a powerful discriminator between physics models.

2.4.2 Flavor-conserving processes

Flavor-conserving processes have played an important role in defining and understanding the boundaries of the SM. The measurement of the muon $g-2$ value confirmed the muon was a Dirac particle. The muon lifetime defines the strength of the weak force, G_F . Low-energy parity-violating scattering experiments confirm the running of the weak mixing angle and provide the best measurements of its value away from the Z pole. Limits on the electron electric dipole moment (EDM) provide some of the strongest constraints on new sources of CP violation. The experimental uncertainties on many of these values are at the point where they are sensitive to radiative corrections from TeV scale particles, making them an important complementary probe to new physics searches at the LHC. The lack of significant discrepancies from SM expectations in flavor-changing processes may indicate a flavor-diagonal or minimal-flavor-violating structure for any new physics at the TeV scale. This makes the push for improved precision in these flavor-diagonal observables all the more important.

A compelling program of experiments that can be carried out this decade using facilities currently under construction has been assembled. These include electron scattering experiments at the JLab 12 GeV upgrade, muon $g-2$ and EDM experiments at the Fermilab muon campus and at J-PARC, and tau $g-2$ and EDM measurements at Belle II. These have been described in detail in the reports associated with the Fundamental Physics at the Intensity Frontier report [7].

As part of the Snowmass process, several initiatives have come to light that have the ability to enhance the currently planned program and also take steps towards the next generation of experiments. These include major advances in lattice gauge theory that should aid in the interpretation of the muon $g-2$ value. New initiatives include next-generation measurements of Michel parameters, precise measurements of the weak mixing angle using deep inelastic scattering and an electron-ion collider, and a program for directly measuring fundamental particle EDMs using storage rings.

The muon anomalous magnetic moment a_μ provides one of the most precise tests of the SM and places important constraints on its extension. With new experiments planned at Fermilab (E989, also known as Muon $g-2$) and J-PARC (E34) that aim to improve on the current 0.54 ppm measurement at BNL by at least a factor of four, a_μ will continue to play a central role in particle physics for the foreseeable future. In order to leverage the improved precision on $g-2$ from the new experiments, the theoretical uncertainty on the SM prediction must be improved and eventually brought to a comparable level of precision.

The largest sources of uncertainty in the SM calculation are from the non-perturbative hadronic contributions. The hadronic vacuum polarization (HVP) contribution to the muon anomaly, $a_\mu(\text{HVP})$, has been obtained to a precision of 0.6% using experimental measurements of $e^+e^- \rightarrow \text{hadrons}$ and $\tau \rightarrow \text{hadrons}$. There is tension between the e^+e^- data from different experiments and between the values of $a_\mu(\text{HVP})$ extracted from e^+e^- and τ data. A direct lattice-QCD calculation of the hadronic vacuum polarization with $\sim 1\%$ precision may help shed light on the apparent discrepancy between e^+e^- and τ data; ultimately a lattice-QCD calculation of $a_\mu(\text{HVP})$ with sub-percent precision can circumvent these concerns. The HVP contribution to the muon anomalous magnetic moment has been computed in lattice QCD by several groups,

as discussed in the Computing Frontier Snowmass report. Statistical errors on lattice calculations of $a_\mu(\text{HVP})$ are currently at about the 3–5% level, but important systematic errors remain. Anticipated increases in computing resources will enable simulations directly at the physical quark masses with large volumes, and brute-force calculations of quark-disconnected diagrams, thereby eliminating important systematic errors.

Unlike the case for the HVP, the hadronic light-by-light (HLbL) contribution to the muon anomaly cannot be extracted from experiment. Present estimates of this contribution rely on models, and report errors estimated to be in the 25–40% range. Therefore an *ab initio* calculation of $a_\mu(\text{HLbL})$ is the highest theoretical priority for $(g - 2)$. A promising strategy to calculate $a_\mu(\text{HLbL})$ uses lattice QCD plus lattice QED, where the muon and photons are treated nonperturbatively along with the quarks and gluons. First results using this approach for the single quark-loop part of the HLbL contribution have been reported recently. Much effort is still needed to reduce statistical errors which remain mostly uncontrolled. In order to bring the error on the HLbL contribution to the level where it is consistent with the projected experimental uncertainty on the time scale of the Muon $g - 2$ experiment, one must reduce the error on $a_\mu(\text{HLbL})$ to approximately 15% or better. Assuming this accuracy, a reduction of the HVP error by a factor of 2, and the expected reduction in experimental errors, then the present central value would lie 7–8 σ from the SM prediction.

Improved measurements of muon decay parameters have the potential to probe new physics. While some terms of the effective Lagrangian of muon decay are tightly bound by the neutrino mass scale if naturalness is assumed for both Dirac and Majorana neutrinos, most of the allowed effective couplings are not constrained by these considerations, and their best limits come from experiments.

Moreover, g_{RL}^S and g_{RL}^V , the scalar and vector couplings of left-handed to right-handed particles, are only constrained by limits on the Majorana nature of neutrinos. If a deviation from the SM is found above these constraints that assume a Majorana neutrino, and if it is possible to establish that the deviation is due to one of these couplings, it would suggest that neutrinos are Dirac particles. This is a unique probe for the existence of Dirac neutrinos, as other known experiments positively test for the Majorana nature of neutrinos. The large and versatile muon fluxes available in the next generation sources such as Project X make interesting searches possible. Proponents are considering a TWIST-like experiment at Project X to perform these searches.

Plans for an electron-ion collider also open several new avenues for the charged lepton program. In particular, the beam polarization makes it possible to construct parity-violating observables in deep inelastic scattering. This enables a measurement of the weak mixing angle over a broad range of momentum transfer. The 4π nature of the detector also allows it to cover a large region of phase space, removing limitations associated with fixed target configurations. It is projected that with 200 fb^{-1} at center-of-mass energy of 145 GeV, precise measurements of the weak mixing angle can be made between 10 GeV and the Z -boson pole, a region where the value of the mixing angle is changing rapidly and has yet to be probed experimentally.

EDMs also play a quintessential role in new physics searches, particularly in trying to find new sources of CP violation required for electroweak baryogenesis. Future experiments may bring us to the point where the lack of evidence for CP violation in EDMs and in quark flavor experiments, plus the lack of evidence for new scalar particles at the LHC, may allow us to rule out the electroweak phase transition as the source of the matter-antimatter asymmetry in the universe. This would be a major discovery.

Through the Snowmass process, the group has investigated EDM measurements of charged leptons. The tau EDM can be probed through triple-vector product correlations in e^+e^- collisions at Belle II. The muon EDM can be probed in $g - 2$ experiments since the relativistic muons see a motional electric field in the rest frame that tilts the precession plane and leads to an asymmetry in the positron decay angle. The electron EDM is extracted from limits on EDMs in atoms or molecules. In these systems, several sources of EDMs are

possible including the electron, the nucleons, nucleon-nucleon interactions, or electron-nucleon interactions. Model-independent extractions of the electron EDM are complicated and require multiple systems.

It is possible to make a direct measurement of the electron EDM using a storage ring, analogous to the way $g-2$ is extracted for the muon. The key is to use an electrostatic storage ring so that any spin precession can be attributed to an EDM. Stray radial magnetic fields would lead to a false signal. The effects of such fields can be measured and controlled by using counter-rotating beams. Several years of R&D have already been invested into this technique for searching for a proton, deuteron, or muon EDM. Fortuitously, the ratio of the $g-2$ value to the mass of the electron is very similar to this ratio for the proton. Many systematic effects scale with this ratio so that many studies already performed for the proton can be used for the electron. Conversely, performing a storage ring EDM experiment on the electron would be an excellent test bed for a (more expensive) proton EDM experiment. Studies of the sensitivities of such experiments are underway.

Also fortuitous for the electron is that its magic momentum, the momentum at which the motional magnetic precession cancels, is 15 MeV, requiring a relatively small and inexpensive storage ring. The technology for the polarized source, electrostatic magnets, and beam position monitors all seem to be available. Concepts for the polarimeter are still being developed and are expected to be the limiting factor in the ultimate sensitivity of the experiment. A polarimeter with high analyzing power would most likely lead to a sensitivity comparable with model-independent extractions of the electron EDM from atoms and molecules.

2.5 Quark flavor physics

2.5.1 Overview and summary

Flavor-physics experiments probe very high mass scales, beyond those directly accessible in collider experiments, because quantum effects allow heavy virtual particles to modify the results of precision measurements. Even as the LHC embarks on probing the TeV scale, the ongoing and planned precision flavor-physics experiments are sensitive to interactions beyond the SM at mass scales which are higher by several orders of magnitude. These experiments will provide essential constraints and complementary information on the structure of models put forth to explain any discoveries at the LHC, and they have the potential to reveal new physics that is inaccessible to the LHC.

Throughout the history of particle physics, discoveries made in studies of rare processes have led to a new and deeper understanding of nature. A classic example is beta decay, which foretold the electroweak mass scale and the ultimate observation of the W boson. Results from kaon decay experiments were crucial for the development of the Standard Model; e.g., the discovery of CP violation in $K_L^0 \rightarrow \pi^+\pi^-$ decay ultimately pointed toward the three-generation CKM model. Precision measurements of time-dependent CP-violating asymmetries in B -meson decays in the *BABAR* and *Belle* experiments established the CKM phase as the dominant source of CP violation observed to-date in flavor-changing processes — leading to the 2008 Nobel Prize for Kobayashi and Maskawa.

In the past decade our understanding of flavor physics has improved significantly due to *BABAR*, *Belle*, *CLEO*, the Tevatron experiments, and most recently LHCb. They provided many stringent tests by precisely measuring numerous CP-violating and CP-conserving quantities. The consistency of the measurements with the SM predictions and their agreement with CP violation in $K^0-\bar{K}^0$ mixing, ϵ_K , strengthened the “new physics flavor problem.” The new physics flavor puzzle is the question of why, and in what way, the flavor structure of the new physics is non-generic. Flavor physics, in particular measurements of meson mixing and CP violation, puts severe lower bounds on the scale of new physics, Λ . For some of the most important

four-quark operators contributing to the mixing of the neutral K , D , B , and B_s mesons, the bounds on the coefficients, C/Λ^2 , are at the scale $\Lambda \sim (10^2 - 10^5)$ TeV if $C \sim 1$. Conversely, for $\Lambda \sim 1$ TeV, the coefficients must be extremely small. Therefore, there is a tension between the relatively low (TeV) scale required to stabilize the electroweak scale and the high scale that is seemingly required to suppress contributions of physics beyond the SM to flavor-changing processes. This problem arises because the SM flavor structure is very special, containing small mixing angles, and because of additional strong suppressions of flavor-changing neutral-current (FCNC) processes. Any TeV-scale new physics must preserve these features.

The motivation for a broad program of precision flavor physics measurements has gotten even stronger in light of the first LHC run. With the discovery of a new particle whose properties are similar to the SM Higgs boson, but no sign of other high-mass states, the LHC has begun to test naturalness as a guiding principle. If the electroweak scale is unnatural, we have little information on the next energy scale to explore. If the electroweak symmetry breaking scale is stabilized by a natural mechanism, new particles should be found at the LHC. They would provide a novel probe of the flavor sector, and flavor physics and the LHC data would provide complementary information. Their combined study is our best chance to learn more about the origin of both electroweak and flavor symmetry breaking.

2.5.2 Kaons

Kaon decays have played a pivotal role in shaping the SM, and they continue to have high impact in constraining the flavor sector of possible extensions of the SM. In the area of kaon decays, the FCNC modes mediated by the quark-level processes $s \rightarrow d(\gamma, \ell^+ \ell^-, \nu \bar{\nu})$ play a pivotal role. These include the four theoretically cleanest modes $K^+ \rightarrow \pi^+ \nu \bar{\nu}$, $K_L \rightarrow \pi^0 \nu \bar{\nu}$, $K_L \rightarrow \pi^0 e^+ e^-$, and $K_L \rightarrow \pi^0 \mu^+ \mu^-$. Because of the peculiar suppression of the SM amplitude (where the top-quark loop is CKM-suppressed by $|V_{td} V_{ts}| \sim \lambda^5$, where λ is the Wolfenstein parameter $\lambda \sim 0.2$) which is not present in SM extensions, kaon FCNC modes offer a unique window into the flavor structure of new physics. This argument by itself provides a strong and model-independent motivation to study these modes in the LHC era. Rare kaon decays can elucidate the flavor structure of SM extensions, information that is in general not accessible from high-energy colliders.

Discovery potential depends on the precision of the SM prediction for these kaon decays, the level of constraints from other observables, and how well we can measure their branching fractions. In the modes $K^+ \rightarrow \pi^+ \nu \bar{\nu}$ and $K_L \rightarrow \pi^0 \nu \bar{\nu}$, the intrinsic theoretical uncertainty is a small fraction of the total, which is currently dominated by the uncertainty in CKM parameters. It is expected that in the next decade progress in lattice QCD and in B meson measurements from LHCb and Belle II will reduce the theory uncertainty on both $K \rightarrow \pi \nu \bar{\nu}$ modes to the 5% level.

Rare kaon decays have been extensively studied within well-motivated extensions of the SM (see Fig. 2-7), such as models with supersymmetry and warped extra dimensions. In all cases, deviations from the SM can be sizable, and the correlations between various rare K decays (as well as with B decays) are essential in discriminating among models. The $K \rightarrow \pi \nu \bar{\nu}$ modes also probe the existence of light states that are very weakly coupled to the SM, such as those that appear in various dark sector models, through the experimental signature $K \rightarrow \pi +$ (missing energy) and distortions to the pion spectrum.

Besides the FCNC modes, kaon decays also provide exquisite probes of the charged-current sector of SM extensions, probing the TeV or higher scales. Theoretically, the cleanest probes are (1) the ratio $R_K \equiv \Gamma(K \rightarrow e \nu)/\Gamma(K \rightarrow \mu \nu)$, which tests lepton universality, scalar, and tensor charged-current interactions and (2) the transverse muon polarization P_μ^T in the semi-leptonic decay $K^+ \rightarrow \pi^0 \mu^+ \nu_\mu$, which is sensitive to new sources of CP violation in scalar charged-current operators. In both cases there is a clean discovery window provided by the precise SM theoretical prediction of R_K and by the fact that P_μ^T is generated in the SM only

Observable	SM Theory	Current Expt.	Future Experiments
$\mathcal{B}(K^+ \rightarrow \pi^+ \nu \bar{\nu})$	$7.81(75)(29) \times 10^{-11}$	$1.73^{+1.15}_{-1.05} \times 10^{-10}$ E787/E949	$\sim 10\%$ at NA62 $\sim 5\%$ at ORKA $\sim 2\%$ at ProjectX
$\mathcal{B}(K_L^0 \rightarrow \pi^0 \nu \bar{\nu})$	$2.43(39)(6) \times 10^{-11}$	$< 2.6 \times 10^{-8}$ E391a	1 st observation at KOTO $\sim 5\%$ at ProjectX
$\mathcal{B}(K_L^0 \rightarrow \pi^0 e^+ e^-)$	$(3.23^{+0.91}_{-0.79}) \times 10^{-11}$	$< 2.8 \times 10^{-10}$ KTeV	$\sim 10\%$ at ProjectX
$\mathcal{B}(K_L^0 \rightarrow \pi^0 \mu^+ \mu^-)$	$(1.29^{+0.24}_{-0.23}) \times 10^{-11}$	$< 3.8 \times 10^{-10}$ KTeV	$\sim 10\%$ at ProjectX
$ P_T $ in $K^+ \rightarrow \pi^0 \mu^+ \nu$	$\sim 10^{-7}$	< 0.0050	< 0.0003 at TREK < 0.0001 at ProjectX
$\Gamma(K_{e2})/\Gamma(K_{\mu2})$	$2.477(1) \times 10^{-5}$	$2.488(10) \times 10^{-5}$ (NA62, KLOE)	$\pm 0.0054 \times 10^{-5}$ at TREK $\pm 0.0025 \times 10^{-5}$ at ProjectX
$\mathcal{B}(K_L^0 \rightarrow \mu^\pm e^\mp)$	$< 10^{-25}$	$< 4.7 \times 10^{-12}$	$< 2 \times 10^{-13}$ at ProjectX

Table 2-2. The reach of current and proposed experiments for some key rare kaon decay measurements, compared to SM theory and the current best experimental results. In the SM predictions for $K \rightarrow \pi \nu \bar{\nu}$ and $K \rightarrow \pi \ell^+ \ell^-$ the first error is parametric, and the second is the intrinsic theoretical uncertainty.

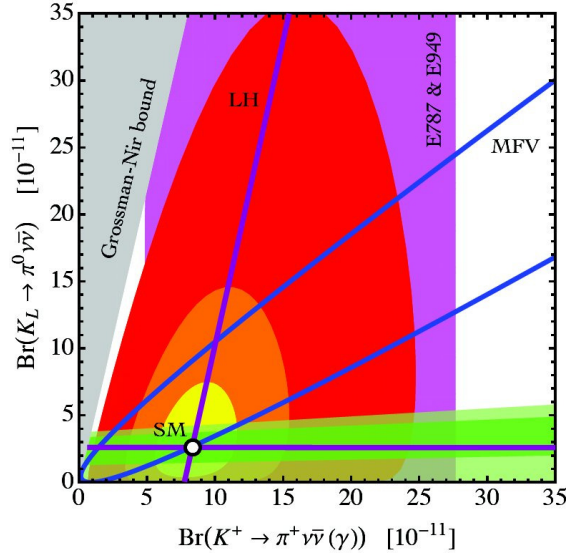


Figure 2-7. Predictions for the $K \rightarrow \pi \nu \bar{\nu}$ branching ratios assuming dominance of Z-penguin operators, for different choices of the effective new physics couplings C_{NP} . The SM point is indicated by a white dot with black border. The yellow, orange, and red shaded contours correspond to $|C_{\text{NP}}| \leq \{0.5, 1, 2\} |\lambda_t C_{\text{SM}}|$, the magenta band indicates the 68% confidence level (CL) constraint on $\mathcal{B}(K^+ \rightarrow \pi^+ \nu \bar{\nu} (\gamma))$ from experiment, and the gray area is theoretically inaccessible. The blue parabola represents the subspace accessible to MFV models. The purple straight lines represent the subspace accessible in models that have only left-handed currents, due to the constraint from ϵ_K . The green band shows the region accessible taking into account the constraint from ϵ'_K/ϵ_K ; more general models do not have this constraint.

by very small and known final state interactions. Table 2-2 provides a summary of SM predictions for these processes, along with current and projected experimental sensitivities at ongoing or planned experiments.

A number of rare kaon decay experiments are in progress in Japan and in Europe. These include: the NA62 experiment at CERN to measure the $K^+ \rightarrow \pi^+ \nu \bar{\nu}$ branching fraction; KOTO at J-PARC, which expects to reach below the SM level for $K_L \rightarrow \pi^0 \nu \bar{\nu}$; TREK at J-PARC, which will search for T-violation in $K^+ \rightarrow \pi^0 \mu^+ \bar{\nu}$ decays but also has a broader program of measurements; and KLOE-2 at the Frascati laboratory, which will improve measurements of neutral kaon interference, tests of CPT and quantum mechanics, and non-leptonic and radiative K decays.

No kaon experiments are currently underway in the U.S. The proposed ORKA experiment at Fermilab has the potential to utilize the Main Injector and other existing infrastructure, along with a well-tested experimental technique, to make a precise measurement of the $K^+ \rightarrow \pi^+ \nu \bar{\nu}$ branching fraction based on 1000 or more events. ORKA will build on the proven background rejection of BNL E787/E949 (where seven $K^+ \rightarrow \pi^+ \nu \bar{\nu}$ events were observed). The new detector will take advantage of technology improvements, but to achieve the goals of ORKA, the background rejection achieved at BNL is adequate. Since the Main Injector is already scheduled to run to support NO ν A, ORKA provides the opportunity to mount a world-leading rare kaon decay experiment in this decade.

In the longer term, Project X has the potential to provide unprecedented beam power for producing kaons, leading to at least an order of magnitude higher kaon fluxes. Also, the CW-linac of Project X has the ability to provide a well-controlled bunch structure that can be exploited in rare K_L -decay experiments by use of time-of-flight to measure the decaying K_L momenta. This provides valuable kinematic information that will reduce background in a high-statistics $K_L \rightarrow \pi^0 \nu \bar{\nu}$ measurement.

2.5.3 B and charm physics

The list of interesting observables in B physics is very long. Two experiments are poised to exploit this rich promise over the next decade. The LHCb experiment has already delivered striking results, for example the first measurement of $B_s \rightarrow \mu^+ \mu^-$. This experiment will continue running in its current configuration until 2018, when a major upgrade will be implemented during the LHC shutdown. Subsequently, LHCb will be able to collect 5 fb^{-1} per year and should reach 50 fb^{-1} around 2030. In Japan the KEK B factory is being upgraded to SuperKEKB, and the Belle II detector is being built to run there. SuperKEKB running is planned to begin in 2017, and an integrated luminosity of 50 ab^{-1} is projected by 2023. In both cases these large data sets will make dramatic improvements in sensitivity to new physics across a broad program of measurements. Examples include precision measurements of phases and magnitudes of CKM angles (β_s , γ , $|V_{ub}|$, etc.), CP violation in decays dominated by loop diagrams, leptonic B decays, and properties of flavor-changing neutral current decays. The ATLAS and CMS experiments at LHC may be competitive on B -decay modes with dimuon final states, such as $B_{s,d} \rightarrow \mu^+ \mu^-$. Belle II and LHCb will also have broad programs of charm studies, as well as bottomonium spectroscopy.

Tables 2-3 and 2-4 show the current and projected experimental precision for a number of important measurements for Belle II and LHCb, respectively, along with the SM theory uncertainties.

Searches for new physics in charm decays are complementary to K and B physics, since in FCNC charm decays and in D^0 – $\bar{D}^0$ oscillations intermediate down-type quarks contribute in the loops. CP violation in D^0 – $\bar{D}^0$ mixing is especially interesting, and Table 2-5 summarizes the future prospects, using the usual convention for D^0 – $\bar{D}^0$ mixing, where the mixing parameters satisfy $|q/p| = 1$ and $\arg(q/p) = 0$ in the absence of CP violation. The study of many D decay rates and strong phases between amplitudes is also crucial for the B physics program, for example, the extraction of the CKM phase γ . In addition, Belle II and LHCb, along with the dedicated charm experiments BESIII in China and PANDA in Germany, will study charmonium spectroscopy, exploring, for example, the so-called XYZ states.

Observable	SM theory	Current measurement (early 2013)	Belle II (50 ab ⁻¹)
$S(B \rightarrow \phi K^0)$	0.68	0.56 ± 0.17	± 0.03
$S(B \rightarrow \eta' K^0)$	0.68	0.59 ± 0.07	± 0.02
α from $B \rightarrow \pi\pi, \rho\rho$		$\pm 5.4^\circ$	$\pm 1.5^\circ$
γ from $B \rightarrow DK$		$\pm 11^\circ$	$\pm 1.5^\circ$
$S(B \rightarrow K_S \pi^0 \gamma)$	< 0.05	-0.15 ± 0.20	± 0.03
$S(B \rightarrow \rho \gamma)$	< 0.05	-0.83 ± 0.65	± 0.15
$A_{\text{CP}}(B \rightarrow X_{s+d} \gamma)$	< 0.005	0.06 ± 0.06	± 0.02
A_{SL}^d	-5×10^{-4}	-0.0049 ± 0.0038	± 0.001
$\mathcal{B}(B \rightarrow \tau \nu)$	1.1×10^{-4}	$(1.64 \pm 0.34) \times 10^{-4}$	$\pm 0.05 \times 10^{-4}$
$\mathcal{B}(B \rightarrow \mu \nu)$	4.7×10^{-7}	$< 1.0 \times 10^{-6}$	$\pm 0.2 \times 10^{-7}$
$\mathcal{B}(B \rightarrow X_s \gamma)$	3.15×10^{-4}	$(3.55 \pm 0.26) \times 10^{-4}$	$\pm 0.13 \times 10^{-4}$
$\mathcal{B}(B \rightarrow K \nu \bar{\nu})$	3.6×10^{-6}	$< 1.3 \times 10^{-5}$	$\pm 1.0 \times 10^{-6}$
$\mathcal{B}(B \rightarrow X_s \ell^+ \ell^-) (1 < q^2 < 6 \text{ GeV}^2)$	1.6×10^{-6}	$(4.5 \pm 1.0) \times 10^{-6}$	$\pm 0.10 \times 10^{-6}$
$A_{\text{FB}}(B^0 \rightarrow K^{*0} \ell^+ \ell^-)$ zero crossing	7%	18%	5%
$ V_{ub} $ from $B \rightarrow \pi \ell^+ \nu$ ($q^2 > 16 \text{ GeV}^2$)	9% $\rightarrow$ 2%	11%	2.1%

Table 2-3. The expected reach of Belle II with 50 ab⁻¹ of data for various topical B decay measurements. Also listed are the SM expectations and the current experimental results. For $\mathcal{B}(B \rightarrow X_s \ell^+ \ell^-)$, the quoted measurement [11] covers the full q^2 range. For $|V_{ub}|$ and the A_{FB} zero crossing, we list the fractional errors.

2.5.4 Theory

To find a convincing deviation from the SM, a new physics effect has to be several times larger than the experimental uncertainty of the measurement and the theoretical uncertainty of the SM prediction. Two kinds of theoretical uncertainties can be distinguished: perturbative and nonperturbative. Perturbative uncertainties come from the truncation of expansions in small (or not-so-small) coupling constants, such as α_s at a few GeV scale. There are always higher-order terms that have not been computed. Nonperturbative effects arise because QCD becomes strongly interacting at low energies, and these are often the limiting uncertainties. There are, nevertheless, several possibilities to get at the fundamental physics in certain cases.

- For some observables the hadronic parameters (mostly) cancel, or can be extracted from data. For example, it is possible to use the measured $K \rightarrow \pi \ell \nu$ form factor to predict $K \rightarrow \pi \nu \bar{\nu}$.
- In many cases, CP invariance of the strong interaction implies that the dominant hadronic physics cancels, or is CKM suppressed. For example, the unitarity triangle angle β can be measured from $B \rightarrow \psi K_S$, and some other CP asymmetries.
- In some cases symmetries of the strong interaction that arise in certain limits, such as the chiral or the heavy quark limit, can establish that nonperturbative effects are suppressed by small parameters. It is sometimes possible to estimate or extract them from data (e.g., measuring $|V_{us}|$ and $|V_{cb}|$, inclusive rates).
- Lattice QCD is a model-independent method to address nonperturbative phenomena. The most precise results to date are for matrix elements involving at most one hadron in the initial and the final state (allowing, e.g., extractions of magnitudes of CKM elements).

Observable	Current SM theory uncertainty	Precision as of 2013	LHCb Upgrade (50 fb ⁻¹)
$2\beta_s(B_s \rightarrow J/\psi\phi)$	~ 0.003	0.09	0.008
$\gamma(B \rightarrow D^{(*)}K^{(*)})$	$< 1^\circ$	8°	0.9°
$\gamma(B_s \rightarrow D_s K)$	$< 1^\circ$	—	2°
$\beta(B^0 \rightarrow J/\psi K_S^0)$	small	0.8°	0.2°
$2\beta_s^{\text{eff}}(B_s \rightarrow \phi\phi)$	0.02	1.6	0.03
$2\beta_s^{\text{eff}}(B_s \rightarrow K^{*0}\bar{K}^{*0})$	< 0.02	—	0.02
$2\beta_s^{\text{eff}}(B_s \rightarrow \phi\gamma)$	0.2%	—	0.02
$2\beta^{\text{eff}}(B^0 \rightarrow \phi K_S^0)$	0.02	0.17	0.05
A_{SL}^s	0.03×10^{-3}	6×10^{-3}	0.25×10^{-3}
$\mathcal{B}(B_s \rightarrow \mu^+\mu^-)$	8%	36%	5%
$\mathcal{B}(B^0 \rightarrow \mu^+\mu^-)/\mathcal{B}(B_s \rightarrow \mu^+\mu^-)$	5%	—	$\sim 35\%$
$A_{\text{FB}}(B^0 \rightarrow K^{*0}\mu^+\mu^-)$ zero crossing	7%	18%	2%

Table 2-4. Sensitivity of LHCb to key observables. The current sensitivity (based on 1–3 fb⁻¹, depending on the measurement) is compared to that achievable with 50 fb⁻¹ by the upgraded experiment.

Observable	Current status	Belle II (50 ab ⁻¹)	LHCb upgrade (50 fb ⁻¹)
$ q/p $	0.91 ± 0.17	± 0.03	± 0.03
$\arg(q/p)$	$(-10.2 \pm 9.2)^\circ$	$\pm 1.4^\circ$	$\pm 2.0^\circ$

Table 2-5. Sensitivities of Belle II and LHCb to CP violation in D^0 mixing.

Quark flavor physics also provided crucial motivations for developing and testing numerous effective theories, which have had a big impact for understanding strong interaction processes at the LHC.

In the last five years lattice QCD has matured into a precision tool. A sample of present errors is collected in Table 2-6. The lattice community is embarking on a three-pronged program of future calculations: (1) make significant improvements in “standard” matrix elements of the type just described in the last bullet above, leading to better precision for CKM parameters; (2) calculate results for many additional matrix elements relevant for searches for new physics; and (3) extend lattice methods to more challenging matrix elements, which can both make use of old results and provide important information for upcoming experiments. These plans rely crucially on access to high-performance computing, as well as support for algorithm and software development. The ultimate aim of lattice-QCD calculations is to reduce errors in hadronic quantities so that other errors dominate. As can be seen from Table 2-6, several kaon matrix elements are approaching this level, while lattice errors remain dominant in most quantities involving heavy quarks. Forecasts for the expected reductions by 2018 are also shown, based on a Moore’s law increase in computing power, and extrapolations using existing algorithms. Conservative assumptions project that exascale performance (10¹⁸ floating point operations/second) will be achieved by 2022, and that a further factor of 100 will be available by 2032. These represent factors of 10² and 10⁴ over currently available capability. While difficult to forecast, this 10⁴ increase in capability can be expected to significantly expand the range of quantities that can be computed using lattice methods.

Thus, the full exploitation of the experimental program requires continued support of theoretical developments, including lattice QCD.

Quantity	CKM element	Present exp. error	2007 forecast lattice error	Present lattice error	2018 lattice error
f_K/f_π	$ V_{us} $	0.2%	0.5%	0.4%	0.15%
$f_+^{K\pi}(0)$	$ V_{us} $	0.2%	–	0.4%	0.2%
f_D	$ V_{cd} $	4.3%	5%	2%	< 1%
f_{D_s}	$ V_{cs} $	2.1%	5%	2%	< 1%
$D \rightarrow \pi \ell \nu$	$ V_{cd} $	2.6%	–	4.4%	2%
$D \rightarrow K \ell \nu$	$ V_{cs} $	1.1%	–	2.5%	1%
$B \rightarrow D^* \ell \nu$	$ V_{cb} $	1.3%	–	1.8%	< 1%
$B \rightarrow \pi \ell \nu$	$ V_{ub} $	4.1%	–	8.7%	2%
f_B	$ V_{ub} $	9%	–	2.5%	< 1%
$(f_{B_s}/f_B)\sqrt{B_{B_s}/B_B}$	$ V_{ts}/V_{td} $	0.4%	2–4%	4%	< 1%
Δm_s	$ V_{ts}V_{tb} ^2$	0.24%	7–12%	11%	5%
B_K	$\text{Im}(V_{td}^2)$	0.5%	3.5–6%	1.3%	< 1%

Table 2-6. History, status and future of selected lattice-QCD calculations needed for the determination of CKM matrix elements.

2.5.5 Conclusions

Because the sensitivities of currently running and proposed flavor physics experiments are complementary to other searches, phenomena beyond the SM may be discovered any time the measurements improve. This gives a strong reason to pursue flavor-physics measurements whose experimental sensitivity can improve substantially, and for which the interpretation of results is not limited by theoretical uncertainties.

The plans in Europe and Asia appear to be set for the remainder of this decade, and the experiments there (those already running or under construction) will define the frontier of quark-flavor physics. These are LHCb and NA62 at CERN, KLOE2 in Italy, Panda in Germany, BESIII in China, and Belle II, KOTO, and TREK in Japan. This is a rich program, and fortunately U.S. physicists have some involvement in most of these experiments. While they all have important physics goals and capabilities, the scale of LHCb and Belle II, and their broad physics menus including both bottom and charm, means that they will be the flagship experiments in quark-flavor physics. In view of that, the U.S. should try to play a significant role in these experiments.

An outstanding question is whether the ORKA experiment will go forward at Fermilab. It received “Stage 1” approval from Fermilab in the fall of 2011, but has not been integrated into DOE’s planned program thus far. ORKA presents an extraordinary opportunity, and if carried out in a timely way, it can help the U.S. HEP program establish a leading role at the Intensity Frontier.

In the decade beginning around 2020, LHCb will be well on its path toward collecting 50 fb^{-1} and Belle II will be well on its path toward 50 ab^{-1} . If the U.S. HEP program makes a commitment to providing a high-intensity proton source for the production of neutrino beams, it will also have the ability to support the next generation of rare kaon decay experiments. In particular, Project X can deliver more than an order of magnitude increase in the beam power available for producing kaons compared to any other laboratory in the world. Project X can be the leading facility in the world for rare kaon decay experiments.

2.6 Nucleons, nuclei and atoms

2.6.1 Overview

Tests of fundamental symmetries with nucleons, nuclei, and atoms have played a vital role in developing and probing the SM. The observation of parity violation in the radioactive decay of ^{60}Co , shortly preceding the observation of parity violation in muon decay, provided the first experimental evidence that the weak interaction does not respect this symmetry, ultimately leading to the SM description of charged weak currents as being purely left-handed. Similarly, the measurements of the parity-violating asymmetry in polarized deep-inelastic electron-deuteron scattering in the 1970's singled out the SM structure for the neutral weak current from among competing alternatives, well in advance of the discovery of the electroweak gauge bosons at CERN. Searches for a permanent electric dipole moment (EDM) of the neutron and ^{199}Hg atom have placed stringent bounds on CP violation in the strong interaction, motivating the idea of the spontaneously-broken Peccei-Quinn symmetry, and the associated axion that remains a viable candidate for the cosmic dark matter.

Today, this use of nucleons, nuclei, and atoms as “laboratories” for the study of fundamental interactions is entering a qualitatively new era. The sensitivity of EDM searches in a variety of systems is poised to improve by two orders of magnitude. Measurements of parity-violating (PV) asymmetries in electron scattering and β -decay are aiming for improvements in precision by an order of magnitude. Advances in theory at the elementary particle, hadronic, nuclear, and cosmological scales will enable us to exploit the results of these experiments in order to shed new light on outstanding questions in fundamental physics.

At Snowmass, this working group concentrated on EDM searches as well as precise measurements of PV in electron scattering and weak decays of light hadrons, as these topics represent a valuable window to physics beyond the SM. If the cosmic matter-antimatter asymmetry was produced during the era of electroweak symmetry-breaking (EWSB) – roughly 10 picoseconds after the Big Bang – then the CP-violating interactions needed at that time would leave their footprints in non-vanishing EDMs at a level which could be observed with the next generation of experiments. Conversely, the absence of an EDM observation would imply that the matter-antimatter asymmetry was generated at much earlier times, possibly associated with the same dynamics responsible for the tiny but non-vanishing neutrino masses. CP-conserving interactions associated with the EWSB era that may hold keys to grand unification or weak scale stability could engender tiny deviations of the PV asymmetries from SM expectations. The pattern of such deviations (or their absence) would provide a unique window into an era in cosmic history.

2.6.2 Electric dipole moments

At the classical level, the EDM of a particle arises from the spatial separation of opposite charges along the axis of the particle's angular momentum. The existence of an EDM would be a direct signature of the violation of both parity (P) and time-reversal symmetry (T). It also probes the physics of CP violation which necessarily accompanies T violation under the principle of CPT conservation. EDM measurements conducted in many laboratories around the world, employing a variety of techniques, have made tremendous progress, and all have so far obtained results consistent with vanishing EDMs. For example, in the past six decades, the search sensitivity of the neutron EDM has improved by six orders of magnitude to reach the current upper limit of $2.9 \times 10^{-26} e \cdot \text{cm}$.

Table 2-7. *Upper limits on EDMs in three different categories.*

Category	EDM Limit ($e\cdot\text{cm}$)	Experiment	Standard Model value ($e\cdot\text{cm}$)
Electron	1.0×10^{-27}	YbF molecules in a beam	10^{-38}
Neutron	2.9×10^{-26}	Ultracold neutrons in a bottle	10^{-31}
Nucleus	3.1×10^{-29}	^{199}Hg atoms in a vapor cell	10^{-33}

As discussed above, CP violation in flavor-changing decays of K and B mesons has been observed. The results so far can be explained by the CKM mechanism within the framework of the SM, in which all observed weak interaction CP violation phenomena originates from a single complex phase. The SM also contains one as-yet unobserved source of CP-violation associated with the strong interaction. The corresponding strength is governed by a dimensionless parameter called “ $\bar{\theta}$ ”. While one naively expects its value to be of order one, the neutron and ^{199}Hg atom EDM bounds imply that it is at least ten orders of magnitude smaller, hinting at the existence of the spontaneously broken Peccei-Quinn symmetry.

Additional sources of CP violation are generally anticipated in extensions of the SM. For example, in supersymmetry, the supersymmetric partners of the quarks and gauge bosons naturally allow additional complex phases and induce new CP-violating phenomena. CP-violating mechanisms beyond the SM are also called for by the observation that the baryon-to-photon ratio in the Universe is as much as nine orders of magnitude higher than the level that can be accommodated by the SM. A much more significant matter-antimatter asymmetry is likely to have been present in the early Universe and provided the favorable condition for the survival of matter that we observe today.

EDMs provide a powerfully sensitive probe for new CP-violating mechanisms, and are a promising path towards discovering new physics beyond the SM. The CKM mechanism in the SM can only generate EDMs of elementary fermions at the three- and four-loop level, leading to EDM values many orders of magnitude lower than the current experimental limits. For example, the SM prediction for the neutron EDM is expected to be approximately $10^{-31} e\cdot\text{cm}$, or five orders of magnitude below the current upper limit. Any EDM observed in the foreseeable future would require either CP violation in the strong interaction or physics beyond the SM. Perhaps unsurprisingly, extensions of the SM generally allow a range of EDM values that are within the reach of the on-going experiments. The negative findings so far provide invaluable constraints on new theories and the upper limits yield insight on the scale of the next Energy Frontier. The scientific importance and discovery potential of EDM searches are strongly endorsed by both the particle and nuclear physics communities[12, 13].

The most sensitive EDM searches have so far been conducted on the neutron, nuclei (^{199}Hg), and the electron (Table 2-7). Experiments in these three categories all compete for the prize of being the first to observe a non-zero EDM. However, they complement each other, as each category is most sensitive to different sources of CP violation. For example, the neutron is more sensitive to the EDMs of its constituent quarks; heavy nuclei are more sensitive to the quark chromo-EDM and other CP-violation mechanisms in the nuclear force. The recently proposed storage ring EDM experiments of the proton and deuteron aim to probe combinations of CP-violating contributions that differ from the neutron EDM. In the future, if a non-zero EDM is discovered in one particular system, it would still be necessary to measure EDMs in other categories to help resolve the underlying CP-violation mechanisms.

At the Intensity Frontier, new sources for cold neutrons and ultracold neutrons (UCNs) are becoming available. The higher output in neutron flux will enable searches for the neutron EDM at a sensitivity level of $10^{-28} e\cdot\text{cm}$, or two orders of magnitude below the current bound. These experiments could provide a conclusive test of many baryogenesis scenarios, including those that arise in supersymmetry.

Neutron EDM searches are currently underway at a variety of laboratories around the world, including the Fundamental Neutron Physics Beamline at the Spallation Neutron Source (U.S.), the Institut Laue-Langevin (France), the Paul Scherrer Institute (Switzerland), the FRM-II reactor at the Technical University of Munich (Germany), and at TRIUMF (Canada). Further improvements in neutral atom EDM searches are in progress at the University of Washington (^{199}Hg), Argonne National Laboratory and Kernfysisch Versneller Instituut (KVI) in the Netherlands (^{225}Ra), TRIUMF (^{221}Rn), and the Technical University of Munich (^{129}Xe). More sensitive probes of the electron EDM may be achieved with polar molecules, including YbF (Sussex) and ThO (Harvard-Yale).

The long-term prospects for EDM searches are equally compelling. Future isotope production facilities such as FRIB at Michigan State or Project X will produce prolific amounts of selected isotopes that possess enhanced sensitivities to the EDMs of nuclei or electrons. Neutral atoms such as ^{225}Ra are particularly attractive, as unique features of nuclear structure associated with “octupole deformation” can enhance the size of the effect at the elementary particle level. An additional promising direction entails the development of a storage ring EDM search, which could enable the direct measurement of the EDM of a charged particle, such as the proton or electron. At present, we may only infer limits on the EDMs of these fermions from neutral atom or molecular EDM searches. In these cases, there exist other possible dynamics, such as T- and P-violating electron-nucleus interactions or nucleon-nucleon interactions, that could be responsible for any observation of an EDM. A direct measurement of these charged particle EDMs would open a new era in the tests of CP violation.

2.6.3 Neutral currents

Experiments employing intense beams of polarized electrons scattering from fixed targets, as well as those exploiting parity-forbidden atomic transitions, have played a vital role in developing and testing the electroweak sector of the SM. Most recently, a program of parity-violating electron scattering experiments performed at MIT-Bates, Mainz, SLAC and Jefferson Laboratory (JLab) have provided the most stringent test to date of the energy dependence of the weak mixing angle θ_W below the weak scale. Similarly powerful measurements have been carried out with beams of atomic cesium, yielding the most precise determination of the nuclear weak charge. These achievements have built on the steady improvements in experimental sensitivity since the pioneering measurements in the 1970’s, together with refinements of the theoretical interpretation. The frontier of this field now promises a compelling capability to probe possible new physics at the TeV scale.

The Qweak collaboration is conducting the first precision measurement at JLab of the weak charge of the proton, Q_W^p . At leading order in the SM, the weak charge depends on the weak mixing angle via the relation $Q_W^p = 1 - 4\sin^2\theta_W$. Because $\sin^2\theta_W \approx 1/4$, Q_W^p is unusually sensitive to the value of the weak mixing angle as well as to possible deviations from new physics. The measurement will determine the weak charge of the proton to the level of 4.1%, which corresponds to constraints on parity-violating new physics at a mass scale of 2.3 TeV at 95% C.L. This also allows the determination of $\sin^2\theta_W$ to 0.3% accuracy, providing a sensitive measurement of the energy-dependence of the weak mixing angle.

Looking to the future, initial simulations indicate that it is technically feasible to reach a $\sim 2\%$ sensitivity on Q_W^p at ultra-low energies at the JLab Free Electron Laser (FEL) accelerator complex. Lower-energy measurements have a cleaner theoretical interpretation than the Qweak measurement. An experiment has been proposed for the newly funded MESA facility at Mainz to pursue a similar measurement with 2% sensitivity to Q_W^p . The design and required R&D will be carried out in the next few years so that the experiment would be ready to start commissioning when MESA first produces external beams, anticipated for 2017.

Parity violation in electron-electron (Møller) scattering has the potential to search for new flavor-conserving amplitudes as small as $10^{-3} \times G_F$. The leading term in the PV Møller asymmetry is also suppressed by $1 - 4\sin^2\theta_W$ at tree level. The MOLLER experiment has been proposed at JLab to improve on previous measurement by more than a factor of 5. The goal is a measurement of the weak charge of the electron Q_W^e to a fractional accuracy of 2.3%. This would lead to a determination of the weak mixing angle at the level of $\sim 0.1\%$, which is comparable to the two best such determinations from measurements in Z -boson decays at LEP at CERN and SLC at SLAC.

The most precise measurements of the weak mixing angle have been made from studies of Z^0 decays at LEP and SLC. With the discovery of the Higgs boson at the LHC, the weak mixing angle is now predicted (within the context of the SM) to better precision than all of these measurements individually as well as their combined average. While the world average is consistent with the measured mass of the Higgs boson, the scatter in the measurements is disconcerting; it would be very useful to have new measurements such as MOLLER with comparable precision.

Atomic physics provides a complementary and powerful probe of the PV neutral current interaction. Atomic PV scales with the nuclear charge roughly as Z^3 , favoring experiments with heavy atoms. The current most precise measurement of the weak charge of any system has been obtained with atomic cesium, with a relative precision of better than 0.5% (combined experimental and atomic theory uncertainties). Unlike the proton or electron weak charge, the heavy nucleus weak charge is given by $Z(1 - 4\sin^2\theta_W) - N$. This requires roughly ten times better precision to probe the same level of new physics in these systems. Ongoing efforts in atomic PV include, but are not limited to: Yb (Berkeley), Fr (FrPNC Collaboration at TRIUMF, and the Ferrara, Legnaro, Pisa, Siena collaboration), Ra^+ (KVI), and Dy (Berkeley).

Further improvements in sensitivity require experiments utilizing as many atoms as possible. The planned energy and current for Project X could produce many orders of magnitude more rare isotopes than in any other facility in the world. Given the prospective flux, the possibility of multiuser parallel operation mode would give the Project X program a unique capability for atomic PV studies in the future.

2.6.4 Weak decays

Weak decays of the pion, neutron, and nuclei provide sensitive tests of the universality of the SM charged current (CC) weak interaction. Electron-muon universality is the hypothesis that these charged leptons have identical electroweak interactions, differing only by their masses and coupling to the Higgs boson. However, there could be additional effects, such as non-universal gauge interactions, or scalar or pseudoscalar bosons with couplings not simply proportional to the lepton masses, that would violate universality. A sensitive approach to seeking such new interactions is the study of the ratio $R_{e/\mu}^\pi \equiv \Gamma(\pi \rightarrow e\nu(\gamma))/\Gamma(\pi \rightarrow \mu\nu(\gamma))$ which in the SM is predicted to be $1.2351(2) \times 10^{-4}$ with a relative uncertainty of $\pm 0.02\%$. New physics at scales as high as 1000 TeV can be constrained or conceivably unveiled by improved measurements of this ratio. Two experiments are currently underway that aim to determine $R_{e/\mu}^\pi$ to 0.05%: The PIENU experiment at TRIUMF and PEN at PSI.

In the case of nucleons and nuclei, tests of the unitarity of the first row of the CKM matrix provide a powerful probe of physics that may violate lepton-quark universality. The CKM unitarity test requires using input from nuclear and/or neutron β -decays and leptonic decays of kaons. Currently, the best-known superallowed decays are from nuclei rather close to stability, which are easily produced. However, future improvements in precision will need comparable measurements on transitions from nuclei much farther from stability. Higher-intensity beams will be required to produce them. Ongoing neutron decay measurements have set the stage

for a number of ambitious experiments under development or construction which target precisions in the 10^{-4} range.

2.7 New light, weakly-coupled particles

2.7.1 Overview

The Standard Model of particle physics has achieved remarkable success as a result of several decades of exploration, of constantly pushing the boundaries of our knowledge of theory, experiment, and technology. However, while the SM provides a theoretically consistent description of all known particles and their interactions (ignoring gravity) up to the Planck scale, it is clearly incomplete as it does not address several pieces of evidence for new physics beyond the SM.

One particularly powerful piece of evidence for new physics comes from the existence of dark matter (DM). DM dominates the matter density in our Universe, but very little is known about it. Its existence provides a strong hint that there may be a dark sector, consisting of particles that do not interact with the known strong, weak, or electromagnetic forces. Given the intricate structure of the SM, which describes only a subdominant component of the Universe, it would not be too surprising if the dark sector contains a rich structure itself, with dark matter making up only a part of it. Indeed, many dark sectors could exist, each with its own beautiful structure, distinct particles, and forces. These dark sectors may contain new light weakly-coupled particles (NLWCPs), particles well below the weak scale that interact only feebly with ordinary matter. Such particles could easily have escaped past experimental searches, but a rich experimental program has now been devised to look for several well-motivated possibilities.

The existence of dark sectors is motivated by bottom-up and top-down theoretical considerations. These sectors arise in many theoretical extensions to the SM, such as moduli that are present in string theory or new (pseudo-)scalars that appear naturally when symmetries are broken at high energy scales. Other powerful motivations include the strong CP problem, and various experimental findings, including the discrepancy between the calculated and measured anomalous magnetic moment of the muon and puzzling results from astrophysics. Besides gravity, there are a few interactions allowed by SM symmetries (given below) that provide a “portal” from the SM sector into the dark sector. These portals include

“Vector” portal:	Dark photons	$-\frac{\epsilon}{2\cos\theta_W}B_{\mu\nu}F'^{\mu\nu}$
“Axion” portal:	Pseudoscalars	$\frac{\partial_\mu a}{f_a}\bar{\psi}\gamma^\mu\gamma^5\psi$
“Higgs” portal:	Dark scalars	$(\mu S + \lambda S^2)H^\dagger H$
“Neutrino” portal:	Sterile neutrinos	$y_N L H N$

The Higgs and neutrino portals are best explored at high-energy colliders and neutrino facilities, respectively. Our focus here will be on the vector and axion portals, which are particularly compelling possibilities and can be explored with low-cost, high-impact experiments.

2.7.2 Axions and axion-like particles

The QCD axion has an origin that lies as the solution to the strong-CP problem of Quantum Chromodynamics (QCD). As discussed in the previous section, limits on the QCD vacuum angle parameter from the non-observation of a neutron electric dipole moment give $\bar{\theta} < 10^{-10}$, when any angle between 0 and 2π would have been equally likely. The axion was introduced as the Goldstone boson associated with the breaking of the Peccei-Quinn symmetry that makes the value of $\bar{\theta}$ near zero. Interestingly, the axion is a natural candidate to contribute to the DM of the universe, with thermal production, a process known as the “misalignment mechanism,” and radiation from cosmic strings or domain walls all possibly contributing to the axion matter density. The QCD axion theory has interrelated parameters describing its mass and couplings to other SM particles. For instance, the axion mass is given approximately by the following:

$$m_a \sim \frac{\Lambda_{\text{QCD}}}{f_a} m_\pi \sim \frac{0.6 \text{ meV}}{f_a/10^{10} \text{ GeV}} \quad (2.2)$$

where f_a is the Peccei-Quinn symmetry breaking scale. The coupling to two photons is often used in experimental searches and is also suppressed by the high symmetry-breaking scale. The pseudo-scalar nature of the axion gives rise to a coupling dependent on $g_{a\gamma\gamma} \vec{E} \cdot \vec{B}$, where

$$g_{a\gamma\gamma} \sim \frac{\kappa}{f_a} \quad (2.3)$$

and κ represents a range of model parameters and includes a factor of $\alpha/4\pi$. Fig. 2-8 shows current constraints on axions, where the diagonal band of coupling versus mass represents what is expected for the QCD axion from the cosmological abundance. An axion-like particle (ALP), in principle, can have mass and couplings anywhere in the allowed region since its properties are not necessarily prescribed within the framework of QCD but are still motivated as one of the portals to the dark sector. Fig. 2-8 shows a number of experimental constraints on the parameter space for axions and axion-like particles that highlight the current and near future plans. Also shown are constraints on the coupling that require $g_{a\gamma\gamma} \lesssim 10^{-10} \text{ GeV}^{-1}$ due to the observation that star cooling is well-described without an axion-like particle cooling mechanism. We now discuss the various search techniques.

Microwave cavities: The Axion Dark Matter Experiment, ADMX, is an ongoing program to detect possible dark matter axions or ALPs by using a high-Q tuned microwave cavity immersed in an 8T magnetic field. If a dark matter axion enters the cavity and if the energy of the axion matches the cavity frequency, then a possible signal can be detected above the dominant thermal noise. By adjusting tuning rods within the cavity, scans in frequency provide sensitivity over a range of axion masses. The experiment has been approved to continue running with very-low-noise SQUID detectors and with an upgrade to a dilution refrigerator to improve sensitivity. The current plans are to scan over a mass range in the region between approximately 10^{-6} and 10^{-5} eV and to cover the QCD axion band. Experiments using smaller cavities may be able to probe the next decade of masses up to 10^{-4} eV in the QCD axion band. The ADMX program is complementary to the WIMP dark matter direct detection experiments in the overall portfolio of searching for likely dark matter candidates.

Light shining through the wall and solar helioscopes: One search strategy for axion-like particles is a laser light-shining-through-a-wall (LSW) experiment, where a laser beam is directed into the bore of a dipole magnet. Through the axion-photon-photon coupling, some of the laser photons, in the presence of the magnetic field, may convert into ALPs, which, in turn, are weakly interacting and may propagate through an opaque barrier, the “wall.” The ALPs may be detected if they reconvert into detectable photons in the magnetic field region on the other side of the wall. A world-wide effort excluded a claim that an ALP had been detected within the last decade. Experiments at Fermilab, JLab, CERN, and DESY set the limit

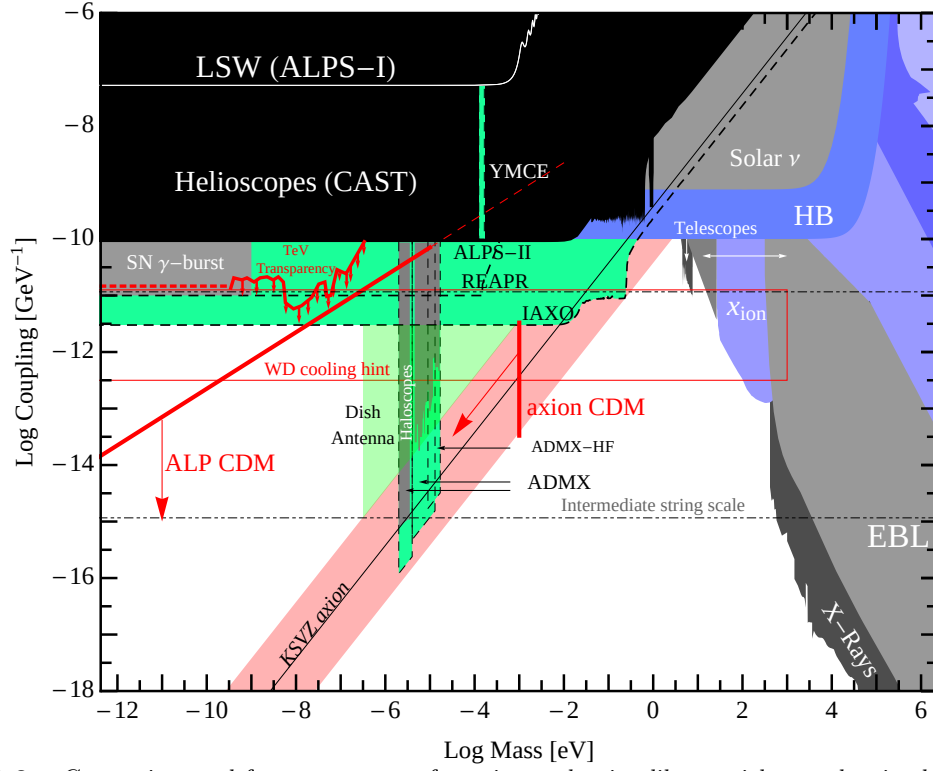


Figure 2-8. Constraints and future prospects for axion and axion-like particle searches in the coupling-mass plane.

$g_{a\gamma\gamma} \lesssim 10^{-7} \text{ GeV}^{-1}$. Future efforts include developing a resonantly enhanced axion-photon regeneration LSW experiment where phased-locked cavities on both the generation and regeneration sides of the wall enhance the sensitivity to the coupling to probe the region around $g_{a\gamma\gamma} \lesssim 10^{-11} \text{ GeV}^{-1}$, where there are astrophysical hints of its relevance. Some of the increased sensitivity arises from utilizing a much longer string of either Tevatron (REAPR) or HERA (ALPS-II) dipole magnets.

If ALPs are produced in the Sun, then an analogous LSW process might occur that would allow for the regeneration of solar axions back into X-ray photons that could be detected by observing the sun opaquely through an accelerator magnet. The CAST helioscope project has used this strategy to probe the $g_{a\gamma\gamma} \lesssim 10^{-10} \text{ GeV}^{-1}$ region and has extended that search over a broad range of masses including the QCD axion band around 1 eV where astrophysical constraints from hot dark matter (HDM) also apply. The follow-up concept is the 4th-generation helioscope international axion X-ray observatory (IAXO) that would require a new custom large toroidal magnet capable of tracking the Sun. Besides X-ray detectors, IAXO would make available ports for other technologies that might have sensitivity to more exotic NLWCPs.

2.7.3 Dark photons and other dark-sector states

Extensions of the SM can give rise to extra U(1) gauge symmetries, such as dark-sector analogues of the SM hypercharge U(1)_Y. Such a U(1) can give rise to a massive vector boson, often called a dark photon and abbreviated as A' , which may mediate interactions among dark-sector particles (including DM) charged

under the $U(1)$. A generic mechanism known as kinetic mixing generates a mixing between the A' and the SM photon at energies below the weak scale. This in turn induces a weak coupling between the A' and electrically charged SM particles, providing a portal into the dark sector. This coupling allows A' 's to be produced in charged particle interactions and, if sufficiently massive, decay into pairs of charged particles like e^+e^- . DM interactions with an A' can also produce signals in DM direct and indirect detection, as well as other types of experiments. For very light A' , kinetic mixing allows for $A' \leftrightarrow \gamma$ oscillations, analogous to neutrino oscillations.

In contrast to the other portals, kinetic mixing and the vector portal generates an interaction that is unsuppressed by a high mass scale. Simple mechanisms for kinetic mixing lead us to expect A' couplings to electrically charged particles of ϵe , where $\epsilon \sim 10^{-2}$ to 10^{-5} and $m_{A'} \sim \sqrt{\epsilon} m_Z \sim \text{MeV to GeV}$, making production and detection of A' 's possible at existing collider and fixed-target facilities. If A' decays into dark-sector particles are kinematically allowed, these “invisible” decays may dominate and produce light DM particles, which could in turn interact through A' exchange and be detected. While the MeV to GeV mass range is a particularly motivated region to explore, other regions with lower A' masses and lower values of ϵ are also natural in simple extensions of the SM, and should thus be rigorously pursued as well.

The existence of dark photons is also motivated by anomalies in particle physics and astrophysics, and from recent hints of DM detection in direct detection experiments. Observation of an excess of high-energy electrons and positrons in the cosmic-rays, most recently confirmed by AMS-02, re-invigorated the search for an A' . The persistent discrepancy between the experimental and SM-predicted values of $g - 2$ of the muon could be evidence for a ~ 10 MeV A' with $\epsilon \sim 10^{-3}$. Recent candidate events of ~ 10 GeV DM in direct detection experiments are hard to accommodate if the DM has only SM interactions, but are natural in models with a new light mediator. An A' with mass well below the MeV-scale can itself constitute all the DM in a large region of parameter space.

It is useful to consider three cases in detail:

- (1) $m_{A'} \sim \text{MeV to GeV}$ with *visible* A' decays: A series of re-interpreted beam dump experiments, measurements of $g - 2$ of the electron and muon, and astrophysical limits complement recent direct searches at e^+e^- colliders and fixed target electro-production experiments, to constrain the region shown in Fig. 2-9 (left). The region for which an A' can explain the muon $g - 2$ discrepancy is shown in a green band. The reach of several fixed-target experiments (APEX, HPS, and DarkLight at JLab and experiments at MAMI and VEPP-3) are delineated with lines. These are now in the planning phase or under construction and will explore much of the motivated parameter space. Future searches (not shown) at the very high luminosity e^+e^- colliders Belle II and KLOE 2 complement these searches, but leave unexplored higher masses and lower couplings. New ideas for future experiments (not shown) suggest much broader coverage is possible.
- (2) $m_{A'} \sim \text{MeV to GeV}$ with *invisible* A' decays: Constraints and opportunities to look for invisible decays of the A' are still being actively worked out. Fig. 2-9 (right) shows the constraints from the electron and muon $g - 2$ (along with the muon $g - 2$ preferred region), rare kaon decays, and a reinterpreted BaBar analysis. Also shown are future opportunities for ORKA, Belle II, DarkLight, and VEPP-3, which use kinematic constraints to identify invisible A' decays. Invisible decays can also be detected by observing the interactions of the hidden sector particles (for example, sub-GeV dark matter) produced in A' decays. Both electron and proton beam dump experiments could produce an A' , which could decay directly to these particles. They in turn would escape the dump and could interact downstream in detectors. Also shown are the proposed reach for MiniBooNE to search for sub-GeV dark matter and future opportunities for electron beam-dumps at JLab, SLAC, KEK, MAMI, ILC etc. These experiments will complement searches for sub-GeV DM in direct detection experiments.

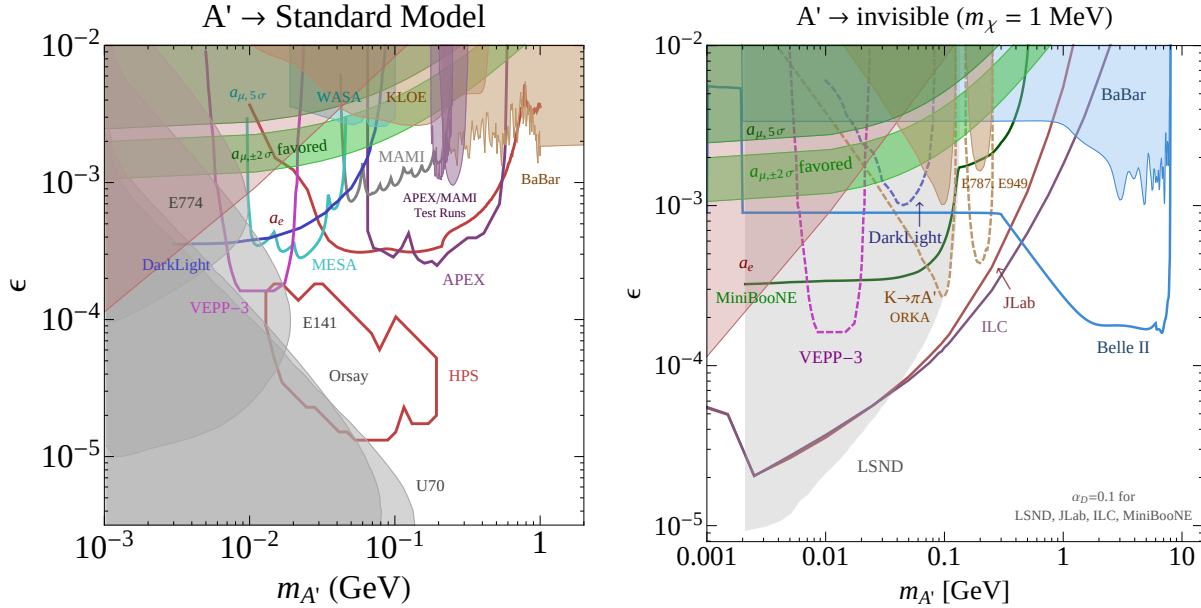


Figure 2-9. Left: Existing constraints and future opportunities to search for dark photons with masses above 1 MeV decaying visibly to Standard Model states (e.g. e^+e^-). Opportunities over the next decade include experiments at JLab, Mainz, and VEPP-3, as well as Belle II and others. **Right:** Existing constraints and future opportunities to search for dark photons with masses above 1 MeV decaying invisibly to light dark-sector states, including sub-GeV DM, assumed to have a mass of 1 MeV. Opportunities over the next decade include proton dumps (e.g. with MiniBooNE, LSND, NOvA, Project X), electron dumps (e.g. at SLAC, JLAB, SuperKEKB, ILC), Belle II, ORKA, and electron fixed-target experiments (e.g. DarkLight and VEPP-3). Note that existing constraints and prospects of different experiments can change drastically for different masses of the dark-sector states, for different A' -to-dark-sector coupling, and for minor changes in the model; this plot is thus not representative of the full parameter space that needs to be explored. See text and [6] for more details.

- (3) $m_{A'} < \text{MeV}$: Below an MeV, the A' is unable to decay to e^+e^- and is thus long-lived. Existing constraints and opportunities for future searches are shown in Fig. 2-10. Constraints arise from stellar cooling, precision measurements of Coulomb's law, and past light-shining-through-walls (LSW) experiments. A large parameter space (shown in light green) remains accessible to future experimentation, including regions in which the A' itself could constitute all of the DM. Experiments searching for axions and ALPs can take advantage of $A' \leftrightarrow \gamma$ oscillations and probe low-mass dark photons.

2.7.4 Chameleons

Chameleon particles are new particles predicted in some theories that have properties that depend on their environment. In particular, the chameleon is nearly massless in regions with low matter density and becomes massive in regions of high matter density. Such particles have properties that could explain a possible scalar field responsible for the accelerated expansion of the universe (dark energy) while avoiding constraints from terrestrial laboratory and solar system fifth force experiments. The chameleon mechanism ($m_{\text{eff}} \propto \rho$) arises naturally with a scalar coupling to the stress energy tensor within a wide range of possible potentials. An afterglow experiment, GammeV-CHASE, set limits on the chameleon coupling to photons by using a reconfigured LSW apparatus. Other experiments using neutrons and a torsion pendulum also constrain the

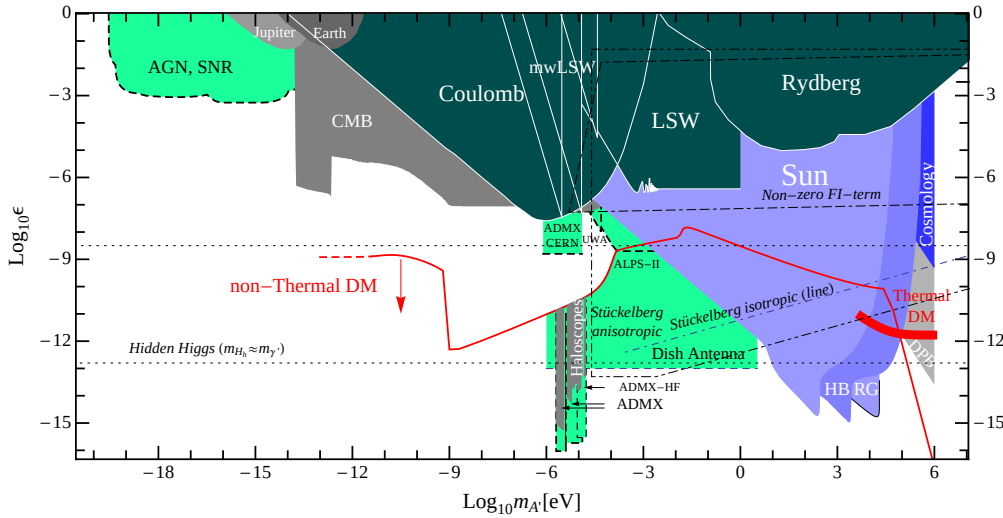


Figure 2-10. Parameter space, constraints, and opportunities for dark photons with masses below 1 MeV. The region shown in light green remains accessible to future experimentation.

phase space of allowed parameters. These limits include those from colliders sensitive to this physics at the TeV scale, as shown in Fig. 2-11.

2.8 Benchmark scenarios

As part of the Snowmass study, an investigation of a well-chosen set of benchmark scenarios was performed. These studies provide a quantitative comparison of experimental capabilities across the various programs of the Intensity Frontier. They also provide the opportunity to demonstrate connections across the Intensity, Energy, and Cosmic Frontiers. Here, we report on flavor studies in supersymmetry and warped extra dimensions as well as signatures of neutrino models that span the frontiers.

The Supersymmetry study was based on the phenomenological MSSM (pMSSM)[14]. The pMSSM assumes the most general CP-conserving model with R-parity, minimal flavor violation (MFV), and does not employ any GUT, supersymmetry-breaking, or any other assumptions about the nature of physics at high scales. This set of guiding principles results in a 19-dimensional supersymmetric parameter space. This study took the lightest supersymmetry particle to be the lightest neutralino. A sample of $\sim 225,000$ such models was generated by scanning over the parameter space – where all supersymmetric particle masses were set to be < 4 TeV – and are consistent with the global data set on collider, astrophysical, precision electroweak, and heavy flavor measurements. This set of pMSSM models was employed for Snowmass studies in all three physics frontiers. For Intensity Frontier studies, the dimensionality of the parameter space was enlarged to include an additional scan over CP-violating phases, and a scan over the parameters representing an expansion in MFV. Several observables from Intensity Frontier measurements, including EDMs and quark flavor and charged lepton processes, provide a large discovery window which is complementary to that of experiments at the other Frontiers. In particular, pMSSM models that are not expected to be detected at the 14 TeV, 3 ab^{-1} LHC were found to have observable signatures in several proposed Intensity Frontier experiments; an example employing electric dipole moments is shown in Fig. 2-12.

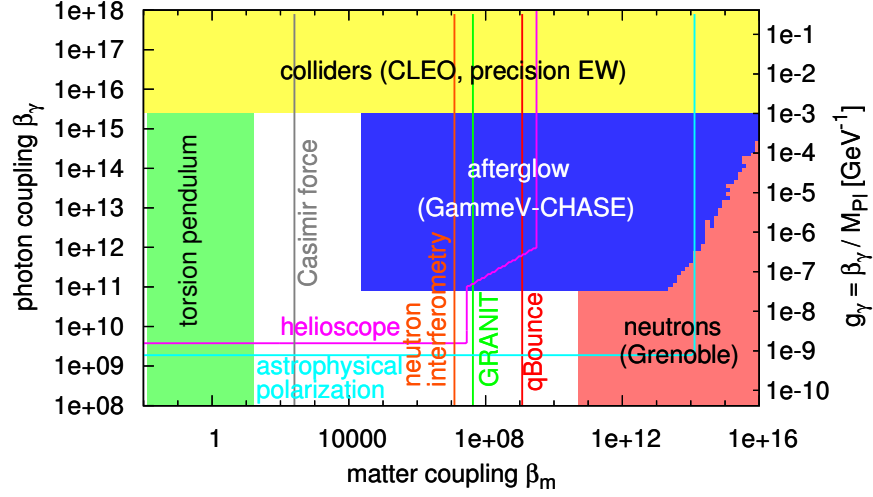


Figure 2-11. Constraints on the matter and photon couplings for a chameleon dark energy model with $V(\phi) = M_\Lambda^4(1 + M_\Lambda/\phi)$. Current constraints are shown as shaded regions, while forecasts are shown as solid lines.

Intensity Frontier measurements provide an excellent probe of theories with warped extra dimensions. A Snowmass study[15] focused on Randall-Sundrum models of quark flavor, which explain the quark mass and mixing hierarchies in terms of spatial geometries, *i.e.*, the localization parameters of the fermion's extra-dimensional wavefunction. In this scenario, all SM fermions and gauge bosons reside in the additional dimension, resulting in a Kaluza-Klein tower of particles in our four-dimensional spacetime. The properties of these Kaluza-Klein particles are governed by the geometry of the extra dimension, and measurement of these properties would yield information about the structure of space-time in the universe. The parameter points in this work, which determine the Kaluza-Klein particle properties, were chosen to be consistent with electroweak precision data, the Z -boson couplings to b -quarks, and flavor parameters associated with kaon meson mixing. The resulting signatures in several quark and lepton flavor observables were examined, where it was found that such measurements would be powerful in distinguishing models of warped extra dimensions; an example employing rare kaon decays and CLFV in muon transitions is presented in Fig. 2-13. The LHC signatures for these benchmark models were also examined and correlated with the flavor observables.

The observation of non-zero neutrino masses points to the possibility of a new mechanism beyond the SM which gives mass to the neutrinos, adding further structure over the SM Higgs mechanism. A leading paradigm for generating small neutrino masses is the so-called seesaw mechanism, which posits the existence of a heavy right-handed Majorana neutrino and leads to tiny masses through mixing. The classic signal for both light and heavy Majorana neutrinos is the presence of neutrinoless double-beta decay. However this reaction may not be able to discern the full heavy-light neutrino mixing effects, depending on the exact value of the parameters. It is possible to measure such mixing effects at the LHC, if the seesaw scale is near a TeV. This synergy between the Intensity and Energy Frontiers shows that direct production of the right-handed neutrino and gauge boson at the LHC was compared to the sensitivity in flavor-violating muon and B -meson decays, as well in $0\nu\beta\beta$ [16]. Probing this mixing at the LHC and ILC [17] can provide crucial information about the detailed nature of the seesaw mechanism, adding to the knowledge gained from Intensity Frontier experiments.

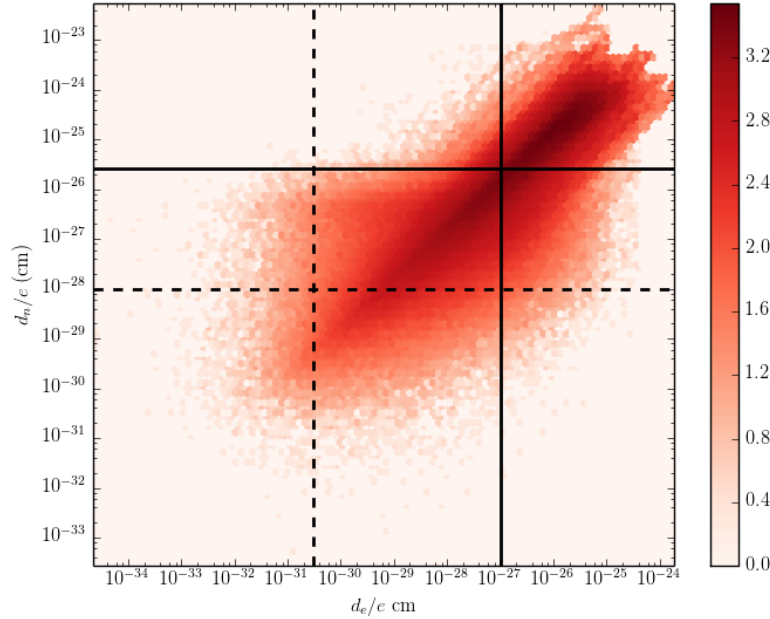


Figure 2-12. Values of the neutron EDM versus the electron EDM in the pMSSM including a scan over allowed phases. The solid (dashed) lines indicate present (future) experimental reach. This set of pMSSM models is not expected to be observed at the high-luminosity 14 TeV LHC. From [14].

2.9 Conclusions

Here, we briefly summarize the main message from each working group.

The neutrino message: The past two decades have brought tremendous new information about neutrinos, and we are now poised to build on our success. The experimental path forward is clear. Long-baseline beam experiments will be major players in fully testing the three-flavor paradigm. There are multiple approaches for determining the neutrino mass hierarchy; for leptonic CP violation, intense beams will be required. While the three-flavor paradigm describes most of the data, wide gaps that could accommodate new phenomena remain due to the absence of precise and detailed information. Such new physics may be revealed by precision measurements, or by experiments that pursue some of the existing intriguing anomalies. An array of neutrinoless double-beta decay search experiments with different isotopes will address the Majorana-vs-Dirac nature of the neutrino, and the question of the absolute neutrino mass scale will be answered by kinematic experiments and precision cosmology.

There is a large diversity of physics topics within the neutrino sector, and the interplay between neutrino physics and other fields is rich. Neutrinos have provided and will continue to provide important information on structure formation in the early universe, Earth, solar and supernova physics, nuclear properties, and rare decays of charged leptons and hadrons. Conversely, information regarding neutrino properties and the origin of neutrino masses is expected from the Energy and Cosmic Frontiers, and from other areas of Intensity Frontier research, as well as nuclear physics. In other words, neutrino physics sits at the nexus of the worldwide effort across the frontiers of particle physics.

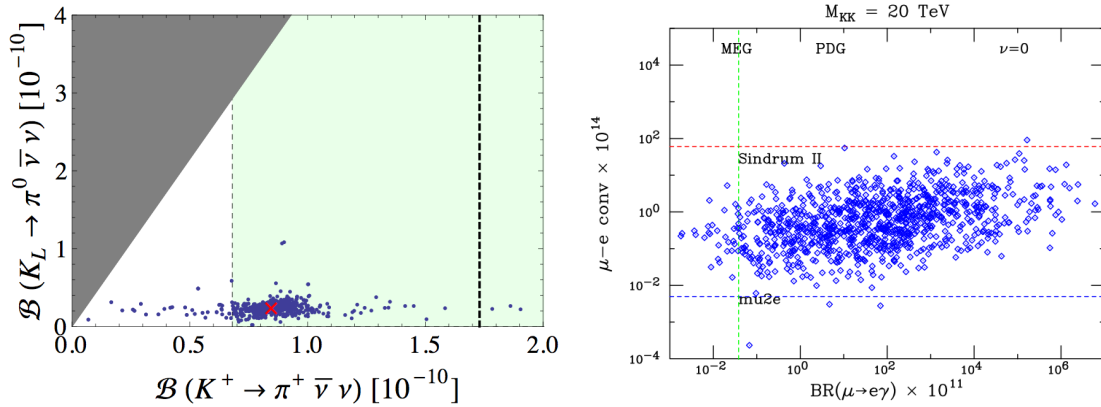


Figure 2-13. Branching fractions of charged versus neutral kaon decay modes (the red ‘x’ indicates the SM value) (left) and rates for $\mu N \rightarrow e N$ versus $\mu \rightarrow e \gamma$ (right) in benchmark models of warped extra dimensions. Present and proposed future experimental reaches are as indicated. From [15].

The baryon number violation message: Baryon number violation probes fundamental symmetries at very high energy scales. Estimates predict fewer background events for a LArTPC, and the 34 kiloton LBNE and 560 kiloton Hyper-Kamiokande, placed underground, have comparable sensitivity to proton decay modes. A factor of 5 to 10 increase in reach for the proton lifetime is expected, resulting in bounds on the lifetime of order 10^{34} to 10^{35} years. This level matches that predicted by favored supersymmetric grand unified theories. In addition, a new experimental search for neutron-antineutron oscillations at Project X could improve existing limits by up to 4 orders of magnitude. The observation of baryon number violation would have a profound impact on society as a whole due to the knowledge that the universe would have a finite lifetime.

The charged leptons message: The charged lepton experimental program offers significant discovery opportunities in this decade’s experiments and in even more sensitive experiments possible with future facilities such as Project X. The experimental program consists of a large and diverse set of opportunities and includes multi-purpose experiments that utilize the large tau production rates at high-luminosity B factories, as well as highly optimized single-purpose experiments that explore muon transitions. In particular E989 is under construction and is expected to settle the issue of whether the value for the anomalous magnetic moment of the muon agrees with the SM prediction, or provides a clear signal for the existence of new physics. Mu2e at Fermilab is expected to also come on-line during this decade and provide an increase in sensitivity of several orders of magnitude to the direct conversion of a muon to an electron in the field of a nucleus.

Extremely sensitive searches for rare transitions of muons and tau leptons, together with precision measurements of their properties, will either elucidate the scale and dynamics of flavor generation, or could limit the scale of new physics contributions to flavor generation to well above 10^4 TeV. Any indication of charged lepton number violation would be an indisputable discovery of new physics. Precision measurements of lepton flavor-conserving processes can be used to verify predictions of the SM and look for signs of new physics.

The quark flavor message: Quark flavor physics is an essential element in the international particle physics program. Experiments that study the properties of highly suppressed decays of strange, charm, and bottom quarks have the potential to observe signatures of new physics at mass scales well beyond those directly accessible by current or foreseeable accelerators. The importance of quark flavor physics is recognized in

Europe and Asia, as demonstrated by the commitments to LHCb, NA62, KLOE-2, and PANDA in Europe, and to Belle II, BESIII, KOTO, and TREK in Asia.

In order for the U.S. particle physics program to have the breadth to assure meaningful participation in future discoveries, significant U.S. contributions to offshore quark-flavor experiments is important, and continued support for U.S. groups in these efforts is a sound investment. In particular, U.S. contributions to LHCb and Belle II should be encouraged because of the richness of the physics menus of these experiments and their reach for new physics.

Existing facilities at Fermilab are capable of mounting world-leading rare kaon decay experiments in this decade at modest incremental cost to running the Fermilab neutrino program. The proposed ORKA experiment, to measure the rare decay $K^+ \rightarrow \pi^+ \nu \bar{\nu}$, with high precision, provides such an opportunity to probe new physics with unmatched sensitivity. This is a compelling opportunity that should be exploited. In the longer term, Project X can become the dominant facility in the world for rare kaon decay experiments. Its potential to provide ultra-high intensity kaon beams with tunable time structure is unprecedented. While the physics case for Project X is much broader than its capabilities for kaon experiments, the power of a Project X kaon program is a strong argument in its favor.

Collaboration between theory and experiment has always led to unexpected progress in quark flavor physics, and this is expected to continue. Therefore, stable support for theorists working in this area is essential. Lattice QCD plays a crucial role, and support for the computing facilities needed for LQCD progress should be maintained.

The nucleons, nuclei, and atoms message: Electric dipole moments of the electron, neutron and nuclei provide stringent tests for new sources of CP violation. Present limits already place tight constraints on physics beyond the SM such as Supersymmetry. A worldwide effort is underway with several new experiments aiming to improve the EDM search reach by several orders of magnitude. This would reach the level of sensitivity of predictions from theories that attempt to explain the matter-antimatter asymmetry of the universe.

The new light, weakly-coupled particles message: Dark sectors, and the new light weakly-coupled particles they may contain, are a well-motivated possibility for new physics. Axions, invented to solve the strong CP problem, are a perfect dark matter candidate. Dark photons and other hidden sector particles are equally compelling dark matter candidates. They could resolve outstanding puzzles in particle and astro-particle physics, and could explain dark matter interactions with the SM sector as well. Exploring dark sectors can proceed with existing facilities and comparatively modest experiments. A rich, diverse, and low-cost experimental program has been identified that has the potential for one or more game-changing discoveries. The U.S. high energy physics community should vigorously pursue these experiments.

The Intensity Frontier message: The working group reports and this summary show very clearly that the Intensity Frontier physics program is diverse and rich in opportunities for paradigm-changing discovery. Such an extensive and multi-pronged program is necessary to address the unresolved fundamental questions about nature. We are at a stage in our understanding where the SM provides sharp predictive capabilities; however, we know it is not the final theory and there is more to learn about the universe. The knowledge we seek cannot be gained by a single experiment, or on a single frontier, but rather from the combination of results from many distinct approaches working together in concert.

We hope this report provides valuable input into strategic decision-making processes in the U.S. and elsewhere, and contributes to establishing a worldwide Intensity Frontier program in which the U.S. plays a leading role. This study confirmed that the proposed experiments will position the U.S. as a global center in Intensity Frontier science.

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Energy Frontier

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3.1 Introduction

The original goal of elementary particle physics was to understand the nature of the subnuclear strong, electromagnetic, and weak forces. In the late 1960's and early 1970's, specific models for these forces were proposed in the form of Yang-Mills gauge theories, giving a beautiful explanation of all three interactions from a unified point of view. Together, these theories became known as the “Standard Model.” Today, we have a great deal of confidence that describing fundamental forces using the gauge principle is correct. Through precision experiments involving W and Z bosons carried out over the past twenty-five years, we have tested the Standard Model stringently, and the theory has passed every test. The most recent such experiments included the search for the Higgs boson, required in the Standard Model to generate quark, lepton, and vector boson masses. A year ago, the ATLAS and CMS experiments at the Large Hadron Collider discovered a candidate for this particle which, at the current level of our measurements, has all of the properties predicted in the Standard Model.

This is an historic level of success for theory and experiment: This economical model predicted the existence of fundamental fields, their dynamics, a scalar field responsible for the breaking of a gauge symmetry, and interactions among the particles with precision unmatched in the history of science. It all seems to have come true with remarkable accuracy. And yet, we find the result still unsatisfying. It is typically true in science that revolutionary changes in our understanding lead to a new set of vexing questions. The success of the Standard Model is no different. Though we have answered many questions about the structure of elementary particles, we have a new set of questions about the structure of the Standard Model itself. The discovery of the Higgs boson sharpens these issues and makes them even more mysterious.

There are many phenomena in nature that obviously lie outside of the Standard Model.

- We now know that 85% of the matter in the universe is *dark matter* — neutral, weakly interacting matter composed of one or more species not contained in the Standard Model.
- The excess of baryons over antibaryons in the universe is not explained by the Standard Model. Even though the Standard Model contains all of the necessary ingredients to generate baryon number in the early universe, including baryon number violation, CP violation, and a phase transition in cosmic history, it predicts a baryon-antibaryon asymmetry that is too small by ten orders of magnitude.
- The quantum numbers of the quarks and leptons under the Standard Model gauge symmetry $SU(3) \times SU(2) \times U(1)$ strongly suggests that these symmetry groups are unified into a larger *grand unification*

group $SU(5)$ or $SO(10)$; however, the results of precision measurements of the strengths of the gauge couplings is inconsistent with this hypothesis.

- The Standard Model cannot account for neutrino masses without the addition of some new particles.
- Further, the pattern of weak interaction mixing among neutrinos is completely different from that observed for quarks.
- The Standard Model does not include the force of gravity or the small but nonzero energy in empty space that gives rise to *dark energy*.

In addition, there is a major theoretical puzzle with the Standard Model. If the Higgs boson is an elementary scalar particle, its mass is sensitive to the masses of any heavier particle to which it couples. It appears to require a cancellation of one part in 10^{32} to explain why the Higgs boson mass is smaller than the Planck mass.

The discovery of the Higgs boson has changed our viewpoint in how we address these unexplained phenomena and theoretical questions. This is true for three reasons.

First, the Higgs boson completes the particle spectrum of the Standard Model. We have now discovered all of the Standard Model particles and have measured many of their properties. It is clear now exactly what the model does *not* explain. We have entered a new era in which the verification of the Standard Model takes second place to a search for new, unknown forces and interactions.

Second, one of the key mysteries concerns the Higgs boson itself. The Higgs boson was predicted as a direct consequence of the simplest model of the generation of mass for quarks, leptons, and gauge bosons. For a long time, many particle physicists have expressed discomfort with this model. Now the prediction has become a reality. We have to grapple with it and understand why nature chooses a particle with these properties to do its work.

Third, the Higgs boson itself gives us a new experimental approach. Within the Standard Model, all properties of the Higgs boson are precisely predicted from the value of the Higgs mass. But, as soon as we step outside the Standard Model, the properties of the Higgs boson are hardly constrained by theory. It is compelling to tug on this particle until the Standard Model breaks. We need to apply to the Higgs boson the same scrutiny that we have applied in previous decades to hadron structure, heavy quark system, the W and Z bosons, and top quark. Each study was done at the Energy Frontier machines of its day. This fruitful experimental approach has acquired a new, promising target.

For exploration of the unknown regions outside the Standard Model, we are encouraged that very powerful experimental tools will be put into play. In the next ten years, the Large Hadron Collider (LHC) at CERN is expected to almost double its energy and to increase its total event sample, or “integrated luminosity,” from the current 25 fb^{-1} to 400 fb^{-1} . This new capability will put to the test many models that predict new physics beyond the Standard Model and address the unexplained phenomena listed earlier in this section. In the decade after that, the LHC should increase its data set by a further factor, up to 3000 fb^{-1} . Particle physicists have proposed lepton colliders and higher energy hadron colliders with capabilities beyond those of the LHC. The mysteries associated with the Higgs boson call for new particles and forces at the TeV energy scale or the attometer distance scale. We now have before us tools for a thorough exploration of this region of masses and distances. This is a compelling program. The purpose of this report is to describe it in detail.

The structure of this summary report is as follows: In Section 3.2, we present the arguments for new fundamental interactions at the TeV energy scale and the experimental program at colliders that these

arguments motivate. Section 3.3 describes the organization of the Energy Frontier study. In Sections 3.4–3.9, we review in a more specific way the physics issues of collider experiments at the TeV energy scale. We consider in turn the prospects for exploration of new physics through studies of the Higgs boson, the W and Z bosons, quantum chromodynamics (QCD), the top quark, and searches for and study of new particles. We present the questions that need to be answered and the methodologies to attack these questions. In Section 3.10, we present the capabilities of current and proposed colliders in relation to these physics goals. Finally, in Section 3.11, we trace out the implications of two possible scenarios for the discovery of new physics at the LHC. This gives an orthogonal way of appreciating the contributions that might be made by proposed accelerators. Section 3.12 gives the summary statements from each of the working groups, and Section 3.13 gives our overall conclusions.

3.2 Importance of the TeV Scale

We have listed a number of motivations for new fundamental interactions beyond the Standard Model (SM). Where will we find them?

Explanations for baryogenesis, higher unification, and dark energy span a bewildering range of mass and distance scales. However, many of the questions we have listed in the previous section relate specifically to the energy scale of hundreds to thousands of GeV that we are exploring today at the LHC. We consider it imperative to understand particles and forces at this “TeV scale” thoroughly, using all of the tools at our disposal. In this section, we will discuss the importance of this regime of energies and short distances.

There is a sharp boundary at which our well-founded knowledge of the fundamental elementary particle interactions runs out. This is related to two different faces that the SM presents, which stand on very different theoretical foundations. On one side are the Yang-Mills gauge interactions. On the other side are the interactions of the Higgs field. The Yang-Mills interactions of quarks, leptons, and vector bosons are tightly determined by their quantum numbers and the strength of the coupling constants of the $SU(3) \times SU(2) \times U(1)$ vector bosons. Precision tests of the SM confirm the structure of these interactions to impressive accuracy [1]. There is little doubt that the SM is a correct representation of nature at the energies we have currently explored.

On the other hand, the interactions of the SM fermions with the Higgs field, and the dynamics of the Higgs field itself, are essentially unconstrained and conceptually cumbersome. The SM Lagrangian is constructed by writing down the most general terms allowed by gauge symmetry and renormalizability. The resulting potential term contains much of what is perplexing about the SM:

$$V = \mu^2 \Phi^\dagger \Phi + \lambda (\Phi^\dagger \Phi)^2 + \sum_{f,f'} [g_{\Phi f f'} \bar{f}_L f'_R \Phi + h.c.] . \quad (3.1)$$

Here Φ is a spin 0 field and λ , $g_{\Phi f f'}$, and μ are parameters. The first two terms give the potential energy of the Φ field. When $\mu^2 < 0$, the state of lowest energy or vacuum state is obtained by shifting the Φ field to a nonzero overall value. The shift is written $\Phi^0 \rightarrow (v + h)/\sqrt{2}$, where $v = \sqrt{-\mu^2/\lambda}$. The quantity v is the Higgs field vacuum value, which gives mass to all quarks, leptons, and gauge bosons. The field $h(x)$ is the quantum Higgs field, whose quanta are the Higgs bosons. In this report, we will refer to the process in which the Φ field takes on a nonzero value at every point in space as “Higgs condensation”.

At the same time, we diagonalize the matrix $g_{\Phi f f'}$. The last term in brackets then becomes

$$V(ff) = \sum_f \left[\frac{g_{Hff} v}{\sqrt{2}} \bar{f}_L f_R + \frac{g_{Hff}}{\sqrt{2}} \bar{f}_L f_R h + h.c. \right] . \quad (3.2)$$

The first term in the brackets is a mass term for each type of fermion, quark or lepton. The second term gives the corresponding interaction, or Yukawa coupling, of each fermion with the Higgs boson.

Every term in (3.1) and (3.2) is of critical importance and each presents special challenges to interpretation and measurement. The fermion masses are given by $m_f = g_{Hff}v/\sqrt{2}$, proportional to the couplings to the Higgs particle. The pattern of the fermion masses is totally unconstrained. The eigenvalues of the matrix g_{Hff} contains the origin of mass and mixings among quarks and leptons and of CP violation in the weak interactions. Many of these masses and mixing angles are well measured, but there is no theory that explains their origin and structure.

As to the couplings of the Higgs boson itself (the first two terms of (3.1)), the picture given in the Standard Model is just one choice among many possibilities. There may be additional Higgs bosons and additional particles of other types forming a larger “Higgs sector.” We have almost no information about these particles except that their masses are probably larger than the mass of the known Higgs boson at 125 GeV,

3.2.1 The mystery of Higgs field symmetry breaking

The issues raised in Section 3.1 are brought to a focus by a single underlying question: In order for any SM quark, lepton, or vector boson to obtain mass, the Higgs field must condense and acquire a nonzero value throughout the universe. Why does this happen?

The SM itself provides no help with this question. It states only that symmetry breaking occurs if $\mu^2 < 0$, which, as a physics explanation, is completely empty. Potentials of the form of (3.1) appear in many condensed matter systems, including superconductors, magnets, and binary alloys. In those systems, it is possible to compute the parameters of the potential from the underlying details of atomic structure and explain why $\mu^2 < 0$. For the SM, if there is an underlying dynamics, its form is unknown. Attempts to compute μ^2 within the SM, even to determine its sign, give disastrous results. The answers for μ^2 depend quadratically on the values of large, unknown mass scales, with competing contributions of opposite sign.

There are models, generalizing the SM, in which μ^2 can be computed. However, these are not simple extensions of the SM. The barrier to creating such a model is that the quadratic dependence on unknown scale parameters at very high energy must be removed. However, this dependence is a generic property of models with fundamental scalar fields, associated with the fact that the radiative corrections to the scalar field mass are quadratically divergent. Cures for this problem require that the Higgs particle be non-generic in some important way: Either it is a composite particle or it is related by a symmetry to a fermion or a vector boson. Symmetries of these types can be included consistently only by profound extension of the structure of space-time, to supersymmetry in the fermion case or higher dimensions of space in the vector boson case.

It is remarkable that, in each of these classes of models, easily identified radiative corrections give contributions to μ^2 with a negative sign, predicting the instability of the Higgs field to condensation [2]. In all of the models mentioned in the previous paragraph, these contributions come from quantum corrections due to partners of the top quark that are required by the postulated new interactions.

The idea that the condensation of the Higgs field has a definite mechanical explanation from quantum physics thus has major implications. It requires a new set of particles at the TeV mass scale. The examples above include exotic partners of the top quark that are likely to be produced at the LHC. A TeV particle spectrum can also supply explanations for other issues that require physics beyond the SM. TeV particle spectra typically contain a massive neutral particle that can be absolutely stable and thus a candidate for the

particle of dark matter [3]. New couplings among the TeV particles potentially provide new sources of CP violation, offering mechanisms for creating the matter-antimatter asymmetry of the universe. Corrections to the SM coupling constants from the new particles can correct the evolution of the SM couplings, allowing the three SM gauge interactions to unify at very short distances [4].

Most importantly, if the explanation for Higgs condensation changes our view of the SM itself — by making SM particles composite or by enlarging the structure of space-time — these changes must be taken into account in any explanation of phenomena that occur at still smaller distances scales, including the generation of neutrino masses, the generation of flavor mixing among quarks and leptons, and the unification of the particle physics interactions with gravity.

In short: mechanisms that shed light on the physics behind the otherwise mysterious potential in (3.1) are needed to directly address the major experimental anomalies that require physics beyond the SM.

3.2.2 Naturalness

To test models that postulate new particles, we must find and characterize those particles. To do this, it is helpful to know to what high energy we must probe. Unfortunately, there is no crisp answer to this question. We have only a hint from the principle of “naturalness.”

Naturalness is the statement that new particles that generate the μ^2 term in the Higgs potential (3.1) must have masses at the scale of μ^2 itself,

$$\mu^2 \sim (100 \text{ GeV})^2 . \quad (3.3)$$

Taken most naively, naturalness implies that new particles associated with the Higgs potential should have been found in the 1990’s at the experiments at LEP and the Tevatron. Today, the LHC experiments have carried out much deeper searches for these particles. How much further must we go?

One approach to naturalness looks more critically at the radiative corrections to the μ^2 parameter in the SM. The first-order corrections due to the top quark, the W and Z bosons, and the Higgs boson itself are

$$\delta\mu^2 = -\frac{3g_{Htt}^2}{8\pi^2}\Lambda^2 + \frac{3\alpha_w(3 + \tan^2\theta_W)}{4\pi}\Lambda^2 + \frac{\lambda}{8\pi^2}\Lambda^2 , \quad (3.4)$$

where g_{Htt} is the same Yukawa coupling as in (3.2), α_w and λ are the couplings of the W , Z , and Higgs bosons, and θ_W is the weak mixing angle. All three terms are divergent and proportional to Λ^2 , where Λ is a mass scale at which the SM is replaced by a more complete underlying theory. Contributions from new particles add to (or subtract from) this expression. If the sum of SM and new particle contributions is to give a well-defined result for μ^2 , the new terms must cancel the dependence on Λ in (3.4). If we allow the new contributions to cancel the SM ones over many decimal places, Λ can be arbitrarily high. However, this might be considered “unnatural.” If we assume that at most one significant figure is cancelled, we obtain interesting limits on top quark, W boson, and Higgs boson partners at roughly 2 TeV.

Another approach looks into the computation of μ^2 in specific models [5]. In most supersymmetry (SUSY) models, the parameter called μ_{SUSY} — the Higgsino mass term — contributes to the Higgs parameter μ^2 at the tree level. Forbidding cancellations beyond one significant figure gives limit on the SUSY parameter, $\mu_{SUSY} < 200 \text{ GeV}$. This is a strong upper bound on the mass of the supersymmetric partner of the Higgs boson, a particle that will be difficult to discover at the LHC. The supersymmetric partners of the top quark ($\tilde{t}$) and the gluon ($\tilde{g}$) contribute to the Higgs potential in one-loop and two-loop order, respectively. The corresponding naturalness bounds for one-significant-figure cancellations are

$$m(\tilde{t}) < 1 \text{ TeV} \quad \text{and} \quad m(\tilde{g}) < 2 \text{ TeV} . \quad (3.5)$$

In models in which the Higgs boson is a composite Nambu-Goldstone boson, the formula for the radiative correction to μ^2 from a new fermionic partner T of the top quark has the form

$$\delta\mu^2 = C \frac{3\lambda_t^2}{8\pi^2} m(T)^2, \quad (3.6)$$

where C is a model-dependent constant of order 1. This gives a bound for one-significant-figure cancellation

$$m(T) < 2 \text{ TeV}. \quad (3.7)$$

In all cases, we might have stronger cancellations in the expressions for μ^2 . These cancellations might eventually find some physics explanation. However, each factor of 10 in mass above the bounds quoted requires cancellations of another *two* significant figures. Even such an imprecise criterion as naturalness probably limits top quark partners to lie below about 10 TeV.

However unsatisfactory these naturalness estimates might be, our interest in these estimates remains very strong. Higgs condensation is the mechanism that generates the whole spectrum of masses of the SM quarks, leptons, and vector bosons. Can it be just an accident? If not, there *must* be a spectrum of new particles at the TeV scale. Even if we cannot predict the value of this scale incisively, the importance of mass scale is clear. We must find these new states.

3.2.3 The mystery of dark matter

Independent of the naturalness argument, there is another argument for new particles at the TeV mass scale. The Standard Model does not account for the dark matter which makes up 85% of the total matter content of the universe. Among the many explanations for dark matter, there is one that is particularly compelling. This is the model of dark matter as composed of a neutral, weakly-interacting, massive particle (WIMP) that was produced in the hot conditions of the early universe. The WIMP model of dark matter is discussed in full detail in the Cosmic Frontier report [6]. What is interesting here is one implication of the model: In order to obtain the observed density of dark matter, the energy scale of the interactions of dark matter must be close to 1 TeV. The TeV mass scale arises as a combination of astrophysical parameters with no obvious relation to the Higgs potential. This could be a coincidence, but it might be a suggestion that models for the Higgs potential also solve the dark matter problem.

Theoretical models with WIMPs typically contain many particles with TeV scale masses. These particles share a common quantum number. They decay to the lightest particle carrying this quantum number, which is then stable for the lifetime of the universe. Whether or not this new spectrum of particles is connected to the Higgs problem, it is important use colliders to search for the new particles required by these models.

3.2.4 Summary

The ideas reviewed in this section predict a spectrum of new particles at the TeV mass scale. Those particles should be discoverable in experiments at the LHC and planned future accelerators. These experiments will provide the crucial tests of those ideas. Furthermore, if such particles are discovered, they can be studied in detail in collider experiments to determine their properties and to establish new fundamental laws of nature.

A research program in pursuit of new particles with TeV masses consists of three threads:

1. We must study the Higgs boson itself in as much detail as possible, searching for signs of a larger Higgs sector and the effects of new heavy particles.
2. We must search for the imprint of the Higgs boson and its possible partners on the couplings of the W and Z bosons and the top quark.
3. We must search directly for new particles with TeV masses that can address important problems in fundamental physics.

To the extent that the naturalness or dark matter arguments above are a guide, all three approaches will be accessible at high-energy collider experiments in the near future. In the next section, we will describe the tools that we have available for that search.

3.3 Organization of the Energy Frontier study

We divided the study of the TeV energy scale thematically, in terms of probes of this scale using different particles and interactions. The participants in the study were asked to enunciate and compare the capabilities of a variety of operating and planned accelerators for each experimental approach. The results summarized here constitute the efforts of hundreds of physicists who worked through the winter and spring of 2013 within six working groups. The leaders of these groups are co-authors of this report.

3.3.1 Working groups for the study of the Energy Frontier

The Energy Frontier working groups covered the following topics:

1. The Higgs Boson
2. Electroweak Interactions
3. Quantum Chromodynamics and the Strong Force
4. Understanding the Top Quark
5. The Path Beyond the Standard Model - New Particles, Forces, and Dimensions
6. Flavor Mixing and CP Violation at High Energy

Highlights of each group's work are presented in this order in the following six sections. We follow with the scientific cases to be made for each possible accelerator organized around each physics group's conclusions for that facility. We summarize the most important conclusions of each group in Section 3.12.

3.3.2 Accelerators for the Study of the Energy Frontier

Specific estimates of the capabilities of the methods that we discussed are made in the context of proposed accelerator programs discussed at Snowmass. We provide here a brief orientation to these programs. Energies

refer to the center-of-mass energy of colliding beam experiments. For details on the design and current status of these proposals, see the Capabilities Frontier working group report [7].

The baseline for our study is the Large Hadron Collider (LHC), the pp collider now operating at CERN. The most recent LHC schedule calls for $75\text{--}100\text{ fb}^{-1}$ to be collected in a run starting in 2015 using the current detectors with minor upgrades. Following a shutdown in approximately 2018, the Phase 1 detector upgrades will be installed and running will resume at a projected instantaneous luminosity of $2 \times 10^{34}\text{cm}^{-2}\text{s}^{-1}$. This stage would produce another 300 fb^{-1} of data. In approximately 2023, after another shutdown, the luminosity is expected to increase to $5 \times 10^{34}\text{cm}^{-2}\text{s}^{-1}$. In the Snowmass study, we compared the current results from the LHC, at 7-8 TeV with an integrated luminosity of 20 fb^{-1} , to future data samples at 14 TeV with 300 fb^{-1} and with 3000 fb^{-1} . We refer to the latter program as the high-luminosity LHC or HL-LHC. The projected evolution of the LHC program is described in [8].

Our study considered higher energy pp colliders, with energy 33 TeV and 100 TeV. Unless it is indicated otherwise, the event sample assumed is 3000 fb^{-1} . A high-energy upgrade of the LHC to 33 TeV (HE-LHC) is discussed in [9]. Colliders of 100 TeV energy are described in [10, 11]. In the following we will refer to such a collider generically as VLHC.

Our study considered e^+e^- linear colliders, both the International Linear Collider (ILC), covering the energy range 90 GeV–1000 GeV and the Compact Linear Collider (CLIC), covering the energy range 350 GeV–3000 GeV. The ILC is described in [12] and in its technical design report [13]. The TDR/CDR luminosity samples are 1000 fb^{-1} at 1 TeV and scale linearly with energy. Luminosity upgrades of the baseline ILC using strategies outlined in the TDR, to 2500 fb^{-1} at 1 TeV and similar enhancements at other energies, with long running periods, are described in [14]. CLIC is described in [15] and in its Conceptual Design Report [16].

Our study considered $\mu^+\mu^-$ colliders operating over a range from 125 GeV to 3000 GeV. The luminosity samples assumed were similar to those for linear e^+e^- colliders. The technology of the muon collider is described in [17, 18].

Our study considered a circular e^+e^- collider in a large (80-100 km) tunnel. Accelerator parameters for such a machine are described in [19] in the context of one such proposal, TLEP, for a large tunnel near CERN. In principle, accelerator techniques invented for super- B -factories can produce very high luminosities, in excess of $10^{36}\text{cm}^{-2}\text{s}^{-1}$ at 90 GeV and $10^{35}\text{cm}^{-2}\text{s}^{-1}$ at 250 GeV, summed over 4 detectors. However, there is as yet no complete accelerator design. The luminosities are lower, and/or the power consumption rises dramatically, at 350 GeV and above. In the following, we will refer to such a collider as TLEP (wherever it might be built). We will assume the above luminosities and operation with four detectors.

Two more types of accelerators received more limited attention from our study. Linear e^+e^- colliders can be converted to photon-photon colliders, with roughly 80% of the energy and similar luminosity, by backscattering laser light from the electron beams. Proposals for photon-photon colliders are described in [20, 21]. Colliding the LHC beam with an e^- or e^+ beam from a linear accelerator offers the opportunity of high energy ep collisions. This has been studied for a facility at CERN called LHeC, described in [22].

3.4 The Higgs Boson

We begin with the study of the Higgs boson itself. In this section, we will refer to the new boson with mass 125 GeV as “the Higgs boson,” while recognizing that its properties could well be very different from the simplest expectations.

We have already emphasized that the study of the Higgs boson gives a completely new avenue along which to probe the physics of the TeV scale. The picture of the Higgs boson given by the SM is precise. All properties of the Higgs boson can be computed now that the mass of the Higgs boson is known. And yet, this precise theory has no conceptual foundation. Current measurements are consistent with the predictions of the SM at their current level of accuracy, but deviations from the SM predictions at the 30%, 10%, or 3% level are all possible in different highly plausible models. The nature of the Higgs boson is a central part of the mystery of TeV physics. New physics responsible for Higgs condensation must couple to the Higgs boson and affect its properties at some level.

Full details of the future program on the Higgs boson, and more precise statements of the uncertainty estimates given below, can be found in the Higgs Boson working group report [23].

3.4.1 Higgs boson couplings

The most direct question to ask about the new boson is whether it is in fact the sole source of mass for all quarks, leptons, and gauge bosons. For this to be true, the couplings of the Higgs boson to the various species of SM particles must follow a definite pattern. The couplings of the particle to fermions and vector bosons must be, from Eq. 3.2,

$$g_{Hf\bar{f},SM} = \frac{m_f}{v} , \quad g_{HVV,SM} = 2 \frac{m_V^2}{v} , \quad (3.8)$$

where v is the value of the SM Higgs condensate, equal to 246 GeV. (More properly, this is the leading-order prediction for a coupling defined to all orders with all three particles on shell.) These couplings have a simple pattern that should be tested for as many SM species as possible. In the following discussion, we define the scale factors

$$\kappa_A = g_{HA\bar{A}}/(SM) , \quad (3.9)$$

where (SM) denotes the SM prediction.

The Higgs boson also couples to pairs of vector bosons gg , $\gamma\gamma$, and γZ through loop diagrams. In the SM, these couplings are dominated by contributions from W boson and top quark loops. In more general theories, these couplings can also receive contributions from radiative corrections with new particles in loops. We will denote ratios of the on-shell couplings to the SM predictions by κ_g , κ_γ , and $\kappa_{\gamma Z}$.

Corrections to the predictions (3.8) can appear at many levels. If there are multiple Higgs fields that mix into the observed boson, the κ_A will contain cosines of the mixing angles. These can be as large as the data permit. Radiative corrections due to loop effects of new particles are expected to be below the 10% level.

Corrections to the Higgs couplings are also affected by the decoupling theorem [24]: If all new particles have masses greater than M , we can integrate out these particles. This leaves the SM, in which the properties of the Higgs boson are predicted precisely in terms of its mass. The corrections to the SM values are generated by effective higher-dimension operators added to the SM Lagrangian. These corrections will then be at most of the order of m_h^2/M^2 . The decoupling theorem implies an apparently paradoxical but nevertheless important conclusion: In a model in which the Higgs sector is very complex but all new particles in it are heavier than 500 GeV, corrections to the Higgs boson properties are at most at the 5-10% level. We are likely to be in this situation, in which the picture of the Higgs boson may be very different from that in the SM but, since the other particles in the sector are heavy, it is difficult to conclude this except by precision measurement.

Typical sizes of Higgs boson coupling modifications are shown in Table 3-1. More details of these estimates are given in [23].

Model	κ_V	κ_b	κ_γ
Singlet Mixing	$\sim 6\%$	$\sim 6\%$	$\sim 6\%$
2HDM	$\sim 1\%$	$\sim 10\%$	$\sim 1\%$
Decoupling MSSM	$\sim -0.0013\%$	$\sim 1.6\%$	$< 1.5\%$
Composite	$\sim -3\%$	$\sim -(3-9)\%$	$\sim -9\%$
Top Partner	$\sim -2\%$	$\sim -2\%$	$\sim +1\%$

Table 3-1. Generic size of Higgs coupling modifications from the Standard Model values in classes of new physics models: mixing of the Higgs boson with a singlet boson, the two-Higgs doublet model, the Minimal Supersymmetric Standard Model, models with a composite Higgs boson, and models with a heavy vectorlike top quark partner. For these estimates, all new particles are taken to have $M \sim 1$ TeV and mixing angles are constrained to satisfy precision electroweak fits.

Tests of the values of the Higgs couplings relative to the SM must take account of the theoretical uncertainty in the comparison to the SM predictions. A potentially observable quantity is the partial decay width $\Gamma(h \rightarrow A\bar{A})$, related to κ_A by

$$\kappa_A^2 = \Gamma(h \rightarrow A\bar{A}) / (SM) . \quad (3.10)$$

Currently, the SM predictions for the values of some Higgs partial widths have large uncertainties. The uncertainty in the partial width $\Gamma(h \rightarrow b\bar{b})$, which accounts for more than half of the SM Higgs total width, is quoted as 6% [25]. A concerted program is required to bring the uncertainties in the SM predictions below 1%. This requires complete evaluation of the 2-loop electroweak corrections to the partial widths. It also requires improvement of the uncertainty in the crucial input parameters α_s , m_b , and m_c . Lattice gauge theory promises to reduce the errors on all three quantities to the required levels [26]. Further methods for improvement in our knowledge of α_s are discussed in Section 3.6.

There are only a few cases in which the partial widths $\Gamma(h \rightarrow A\bar{A})$ can be measured directly. More often, the Higgs decay partial widths are measured from the rates of reactions that involve the Higgs boson in an intermediate state. An example is the rate of $\gamma\gamma$ production through gg fusion at the LHC. The rate of this process is proportional to the Higgs boson production cross sections times the branching ratio of the Higgs boson to $\gamma\gamma$,

$$\sigma(gg \rightarrow h) \cdot BR(h \rightarrow \gamma\gamma) \sim \frac{\Gamma(h \rightarrow gg)\Gamma(h \rightarrow \gamma\gamma)}{\Gamma_T(h)} , \quad (3.11)$$

where $\Gamma_T(h)$ is the total Higgs boson width. In terms of the κ_A quantities, the measured rates are proportional to

$$\sigma(A\bar{A} \rightarrow h)BR(h \rightarrow B\bar{B})/(SM) = \frac{\kappa_A^2 \kappa_B^2}{\sum_C \kappa_C^2 BR_{SM}(h \rightarrow C\bar{C})} . \quad (3.12)$$

The SM prediction for the total width of the Higgs boson is 4 MeV, a value too small to be measured directly except at a muon collider where the Higgs boson can be produced as a resonance. At all other cases of hadron and lepton colliders, the total width must be determined by a fit to the collection of measured rates. Such fits entail some model-dependence to control the size of modes of Higgs decay that are not directly observed.

The report [23] compares the abilities of experiments at the LHC and at a variety of lepton colliders to extract the values of the Higgs boson couplings. At the LHC, the total number of Higgs bosons produced is very high, over 170 million per experiment for integrated luminosity of 3000 fb⁻¹. However, Higgs boson production at the LHC is accompanied by very high backgrounds. The discrimination of signal from background brings

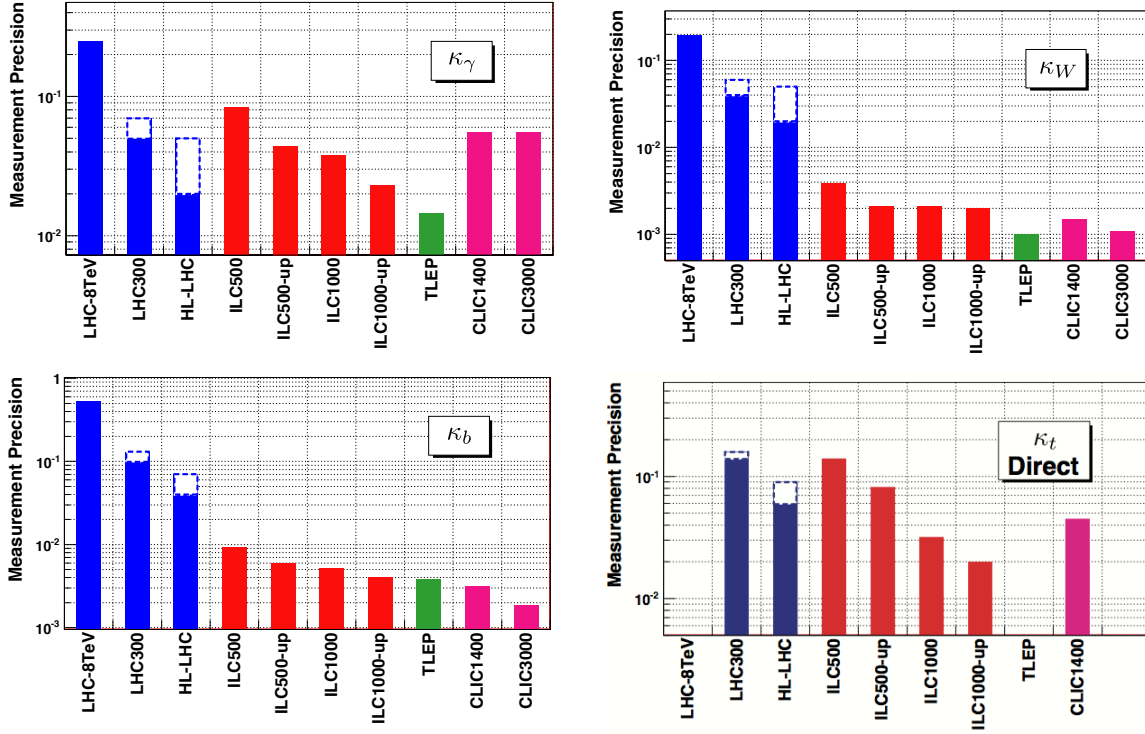


Figure 3-1. Expected measurement precision on Higgs boson couplings at different colliders. The couplings considered are: (top row) $\kappa_{\gamma\gamma}$, κ_W , (bottom row) κ_b , κ_t . Blue bars correspond to stages of LHC, with the white band showing a range between pessimistic and optimistic scenarios. Red bars correspond to stages of ILC, including the projections for TDR luminosity samples and proposed luminosity upgrades. Green and purple bars corresponds to TLEP and CLIC. There is no TLEP entry in the κ_t plot.

in substantial systematic uncertainties. The extraction of couplings from cross sections is complicated by significant QCD uncertainties in the calculation of cross sections, currently about 12% for gluon fusion and 3% for vector boson fusion.

At electron colliders, the Higgs boson is produced in the relatively background-free processes $\ell^+\ell^- \rightarrow Zh$ and $\ell^+\ell^- \rightarrow \nu\bar{\nu}h$ (vector boson fusion). The systematic errors in the extraction of Higgs couplings are small. The main uncertainties come from limited statistics. The Zh reaction offers tagged Higgs bosons, giving the possibility of observing decay modes not accessible at the LHC (such as decay to $c\bar{c}$), and invisible and exotic modes of Higgs decay. The total cross sections for the two e^+e^- reactions are directly proportional to $\Gamma(h \rightarrow ZZ^*)$ and $\Gamma(h \rightarrow WW^*)$, respectively, without dependence on $\Gamma_T(h)$. This allows lepton collider measurements to determine $\Gamma_T(h)$ and all individual partial decay widths by fitting of Higgs boson rates without any model assumptions.

Figure 3-1 compares the projected uncertainties in the measurement of Higgs boson couplings for a variety of pp and lepton collider programs. The first three figures show the uncertainties in the couplings to $\gamma\gamma$, WW , and $b\bar{b}$ from a 6-parameter fit appropriate to the analysis of LHC results, described in [23]. Although the model-independent results from lepton colliders do not require such a fit, the same fit is used for those cases to facilitate comparisons to the hadron colliders. The fourth figure shows the projected error on the Higgs coupling to $t\bar{t}$ from experiments directly sensitive to this quantity. The facilities considered are the LHC at its current stage, the LHC after 300 fb^{-1} and after 3000 fb^{-1} , the ILC up to 500 GeV and up to 1000 GeV,

the TLEP circular e^+e^- collider, and the CLIC linear collider operating at 1400 and 3000 GeV. For LHC, the upper and lower estimates reflect a pessimistic scenario, in which systematic errors not evaluated from data do not improve, and an optimistic scenario, in which theory errors are halved and all experimental systematic errors decrease as the square root of the integrated luminosity. For the ILC stages, the first error bar corresponds to the baseline event samples considered in the ILC TDR, while the second includes, more optimistically, a set of luminosity upgrades described in the TDR. Full details, and tables of the numerical results of the fits, can be found in [23]. The figures show that the LHC, especially in its high luminosity phase, will measure Higgs couplings with an impressively high precision of several percent. However, the discovery of perturbations of the Higgs boson couplings at the level shown in Table 3-1, at $3\text{--}5\sigma$ significance, will need even higher precision. This will require both the much lower level of systematic errors available at a lepton collider and very large event samples to reduce the statistical errors.

3.4.2 Higgs boson self-coupling

A particularly important coupling of the Higgs boson is the Higgs self-coupling, λ in (3.1), which determines the shape of the Higgs potential. In the SM, after Higgs condensation, there is a triple Higgs boson coupling proportional to $\sqrt{\lambda}$, given alternatively by

$$\lambda_{hhh} = \frac{3m_h^2}{v} . \quad (3.13)$$

This coupling can be extracted from the rate for double Higgs production, for example $pp \rightarrow hh + X$ or $e^+e^- \rightarrow \nu\bar{\nu}hh$.

Theoretical models with extended Higgs sectors or composite Higgs bosons can predict that the triple Higgs coupling will deviate from the SM expectation by 20%. This is predicted to be a larger effect than those expected in the Higgs couplings to fermions and vector bosons. However, the measurement is also much more difficult. The cross sections at lepton colliders are at the fb level. At the LHC, the cross sections are larger, but it may be necessary to detect the Higgs boson in a rare decay mode such as $\gamma\gamma$ in order to reduce the background. Studies using only a few decay modes indicate a precision of 50% in the Higgs self-coupling measurement per detector. By combining the results of the detectors, the HL-LHC program would likely provide the first evidence of the Higgs self-coupling. The projected uncertainties on the Higgs self-coupling are 13% in a long-term program at the ILC at 1 TeV or 10% for CLIC at 3 TeV. The double Higgs production cross section increases rapidly with energy. Measurements at a 100 TeV pp collider are estimated to reach an uncertainty of 8%.

3.4.3 Higgs boson spin and CP

A crucial test of the identification of the 125 GeV resonance with the Higgs boson is the measurement of its spin and parity. This issue is almost settled with the current data from the LHC. The fact that the resonance decays to $\gamma\gamma$ implies that it has integer spin and cannot have spin 1. The distribution of the four leptons in $h \rightarrow ZZ^*$ decays already strongly favors the 0^+ over the 0^- spin-parity hypothesis and excludes the simplest forms of spin 2 coupling [27, 28]. This issue should be decided with the next LHC data set.

However, there is a more subtle issue associated with the Higgs boson CP. If there are multiple Higgs bosons and CP violation in the Higgs sector, the Higgs boson at 125 GeV can contain an admixture of CP scalar and pseudoscalar states. CP violation in the Higgs sector has major implications. Most importantly, it can

provide the new source of CP violation outside the SM that allows the matter-antimatter asymmetry of the universe to be generated at the electroweak phase transition.

CP violation in the Higgs sector can be reflected both in production and decay of the Higgs boson. The most accurate tests are available in the study of the 4-lepton final state in $h \rightarrow ZZ^*$. The scalar Higgs couplings to massive vector bosons appear at tree level, while pseudoscalar couplings are expected to appear only in loop corrections. CP-violating terms in this vertex are therefore masked by the large CP-conserving tree-level amplitude. However, measurements in vector boson fusion and in associated production are potentially accurate enough to overcome the loop suppression. Lepton colliders can search for CP violation in the decay $h \rightarrow \tau^+\tau^-$ and in the production process $\ell^+\ell^- \rightarrow t\bar{t}h$. The first process can reach 1% precision in the measurement of a cross-section fraction corresponding to the CP-odd decay amplitude [29].

Photon-photon colliders, which produce the Higgs boson as a resonance, can use initial-state polarization to search for CP-violating terms in the Higgs boson coupling to $\gamma\gamma$, which has no tree-level contributions. Similarly, a muon collider can probe for CP-violating contributions to the Higgs boson coupling to $\mu^+\mu^-$ if the accelerator provides transverse beam polarization.

3.4.4 Higgs boson mass and width

The Higgs boson mass is currently known from the LHC experiments to better than 600 MeV. This accuracy is already sufficient for the uncertainty in the Higgs mass not to be significant in precision electroweak tests. The most important influence of a highly accurate Higgs mass within the SM comes in the evaluation of the predictions for the Higgs couplings to WW and ZZ , for which one boson must be off the mass shell. A 100 MeV error in the Higgs mass corresponds to a 0.5% uncertainty in κ_W . We expect that the error in the Higgs mass can be decreased to 100 MeV and to 50 MeV, respectively, for the LHC programs with 300 fb^{-1} and 3000 fb^{-1} by using the $\gamma\gamma$, ZZ^* , and $\mu^+\mu^-$ modes, in which the Higgs boson can be fully reconstructed. A lepton collider studying the Higgs boson in the Zh production mode would push this uncertainty down further, to about 35 MeV for linear colliders and 7 MeV for a very high luminosity program at a circular collider.

Predictions of the Higgs mass in models of new physics might provide further motivation for measuring the Higgs mass accurately. An example of such a model is the Minimal Supersymmetric Standard Model (MSSM). To evaluate the prediction to an accuracy of 100 MeV, however, the masses of the top squarks must be known, and the top quark mass must be known to 100 MeV.

We have noted already that lepton colliders offer the possibility of a model-independent determination of the Higgs boson total width. Because the couplings of the Higgs boson to ZZ and WW appear in both the expressions for measurable total cross sections and those for branching ratios, these couplings can be eliminated to evaluate the total width through the relations

$$\Gamma_T(h) \sim \sigma(\ell^+\ell^- \rightarrow Zh)/BR(h \rightarrow ZZ^*) \sim \sigma(\ell^+\ell^- \rightarrow \nu\bar{\nu}h, h \rightarrow b\bar{b})/BR(h \rightarrow WW^*)BR(h \rightarrow b\bar{b}) \quad (3.14)$$

This gives the Higgs boson width to 3% for a long-term program at the ILC and to 0.6% for a high luminosity program at a circular collider with multiple detectors. These uncertainties are reflected in the coupling uncertainties quoted in Section 3.4.1.

A muon collider would have the capability of observing the Higgs boson as a narrow resonance. For the projected beam energy resolution of 4×10^{-5} , the mass of the Higgs boson would be measured to 0.06 MeV and the width would be measured directly in the s -channel to a precision of 4% [18]

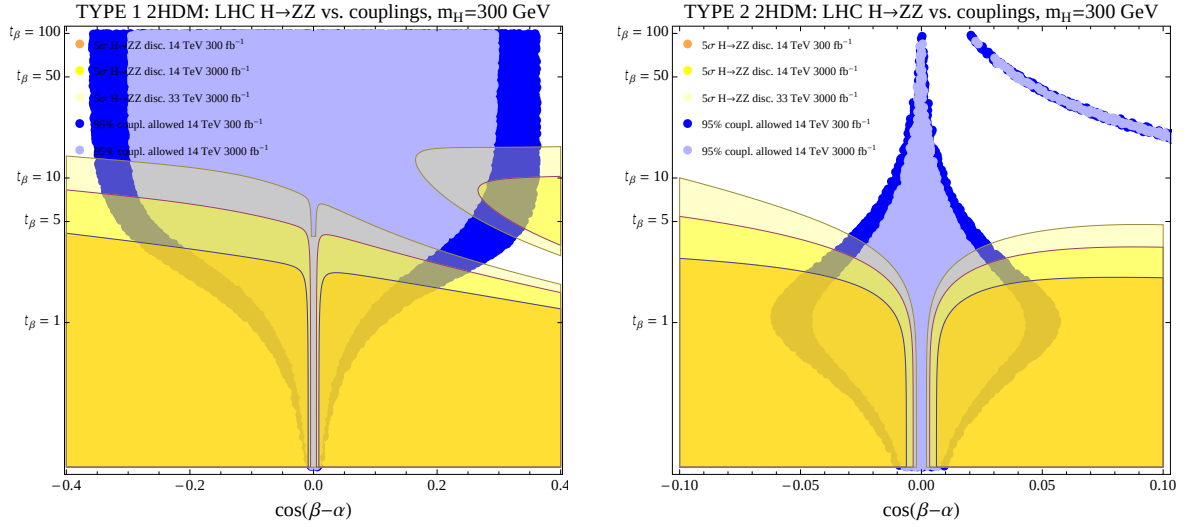


Figure 3-2. 5σ discovery reach for a 300 GeV H decaying via $H \rightarrow ZZ \rightarrow 4\ell$, for the Type I and Type II two-Higgs-doublet models, shown in the left and right panels, respectively. The coordinates are the combinations of Higgs boson mixing angles $\cos(\beta - \alpha)$ and $\tan \beta$. The yellow regions show the 5σ reach in direct searches at the LHC. The successive larger regions correspond to the LHC at 300 fb^{-1} and 3000 fb^{-1} and a 33 TeV pp collider with 3000 fb^{-1} . The blue regions show the regions allowed at 95% CL by precision Higgs coupling measurements; the darker and lighter regions correspond to the LHC with 300 fb^{-1} and 3000 fb^{-1} [30].

3.4.5 Searches for additional Higgs bosons

There are strong motivations for expecting the existence of additional Higgs particles. These motivations begin with the overall mysteries of the physics of Higgs condensation and the question of whether the Higgs boson is the only particle of the SM whose quantum numbers do not come in multiples. Beyond this, virtually all models of new physics to explain the Higgs potential contain additional Higgs doublet fields. These fields are required in supersymmetric models in order for Higgs fields give mass to both the up-type and the down-type quarks. In models with new space dimensions, additional Higgs fields arise as the Kaluza-Klein excitations of the fundamental Higgs doublet. Each additional Higgs doublet gives rise to four new particles, CP-even and CP-odd neutral scalars H and A , and a charged pair $H^\pm$.

Often, extended Higgs bosons have enhanced couplings to heavy flavors, either to b quarks and τ leptons or to t quarks, depending on whether the extended Higgs parameter $\tan \beta$ is greater than or less than 1. This emphasizes searches in which the extended Higgs bosons are produced in $b\bar{b}$ annihilation.

Currently, the LHC experiments based on heavy flavor signatures exclude additional Higgs bosons for masses as high as 1 TeV in restricted ranges of $\tan \beta$. The region of large $\tan \beta$ is surveyed by reactions such as $b\bar{b} \rightarrow H, A \rightarrow \tau^+\tau^-$, while the region of low $\tan \beta$ is surveyed by reactions such as $gg \rightarrow H, A \rightarrow t\bar{t}$, $gg \rightarrow A \rightarrow Zh$. A gap remains for intermediate values, roughly $2 < \tan \beta < 20$, which is closed only if the extended Higgs bosons have masses below 200 GeV. Future runs of the LHC, up to 3000 fb^{-1} , are expected to close this window up to masses of about 500 GeV.

Additional Higgs particles are typically predicted to have smaller couplings to WW and ZZ than the lightest Higgs boson. These couplings are proportional to the combination of mixing angles $\cos(\beta - \alpha)$, which are also constrained by measurements of the vector boson couplings to the known Higgs boson. The

complementarity of these search strategies is illustrated with a search for additional Higgs bosons in a model with two Higgs doublets [30, 31, 32]. Fig. 3-2 shows the discovery reach for a 300 GeV Higgs boson H decaying via $H \rightarrow ZZ \rightarrow 4\ell$ compared with the allowed region from Higgs coupling measurements at various experimental facilities. The reach for a pseudoscalar A decaying via $A \rightarrow Zh \rightarrow \ell\ell b\bar{b}, \ell\ell\tau\tau$ is comparable [32]. The important parameters for this search are the Higgs mixing angle α and the ratio of the Higgs vacuum expectation values $\tan\beta$. The efficacy of the search also depends on which Higgs bosons couple to which quarks and leptons. (This distinguishes the Type I and Type II models in the figure.) The line $\cos(\beta - \alpha) = 0$ is the limit where the additional Higgs particles decouple from gauge bosons and the couplings of the 125 GeV Higgs boson are very close to the predictions of the SM. Even for Type II models (which include supersymmetry), where the Higgs coupling measurements are already very constraining, the direct Higgs search probes significant additional parameter space. There is roughly a factor of 2 increase in the reach for large $\tan\beta$ in each step in going from LHC at 300 fb⁻¹ to HL-LHC to a 33 TeV collider with 3000 fb⁻¹.

Lepton collider experiments can search for extended Higgs boson states through the reaction $\ell^+\ell^- \rightarrow HA$ up to the kinematic limit, independently of the value of $\tan\beta$. The cross section depends only on the electroweak quantum numbers of the extended Higgs particles. This covers the parameter space up to 500 GeV for ILC at 1 TeV and up to 1500 GeV for CLIC running at 3 TeV. Photon and muon colliders have the opportunity to discover additional Higgs bosons as resonances up to the full center of mass energy of the machine.

3.5 Electroweak Interactions

All particle species with couplings to the electroweak interactions eventually influence the properties of the weak interaction bosons W and Z . Very precise measurements of the properties of these bosons then have the potential to reveal new, undiscovered particles. The precision electroweak experiments of the 1990's established the $SU(2) \times U(1)$ theory of electroweak interactions at the sub-percent level of accuracy. But, also, they indicated the presence of a heavy top quark and a light Higgs boson and estimated the masses at which these particles were eventually discovered. They disfavored a fourth generation of quarks and leptons, now excluded by direct search at the LHC.

Increased precision in the properties of the weak interaction bosons could well turn up the first evidence of the TeV spectrum of particles discussed in Section 3.2.1. Some observed deviations from the predictions of the SM have remained intriguing as the measurements have improved. For example, the measured value of M_W has persistently remained 1–2 σ higher than the SM expectation. Experiments over the next decade will explore whether these deviations could become significant effects requiring radiative corrections due to new particles.

In a description of possible new interactions in terms of effective operators, the electroweak precision observables probe only the first few terms. Experiments at higher energy probe additional operators by observing and constraining the nonlinear interactions of the W and Z bosons. These operators can receive corrections from loop diagrams involving new TeV mass particles but, more strikingly, they can receive leading-order corrections if there is new strong dynamics or resonances in the Higgs sector.

We will review these topics in this section. Full details of the program, and more precise statements of the projected uncertainties described below, can be found in the Electroweak Interactions working group report [33].

ΔM_W [MeV]	LHC		
$\sqrt{s}$ [TeV]	8	14	14
$\mathcal{L}$ [fb ⁻¹]	20	300	3000
PDF	10	5	3
QED rad.	4	3	2
$p_T(W)$ model	2	1	1
other systematics	10	5	3
W statistics	1	0.2	0
Total	15	8	5

Table 3-2. Current and target uncertainties (in MeV) for individual components of the precision measurement of M_W at the LHC.

3.5.1 Precision observables M_W and $\sin^2 \theta_{eff}$

The current uncertainty in the W boson mass is 15 MeV, corresponding to a relative precision of 2×10^{-4} . Currently, the most accurate determinations of M_W come from the hadron collider experiments CDF and DØ. Precision measurement of M_W at hadron colliders is very challenging, but certain features of W boson production make it feasible to reach high accuracies. The directly measured transverse mass distribution is very sensitive to M_W , having a relatively sharp endpoint at the W boson mass. Likewise the transverse momentum distributions of the leptons are also sensitive to the W boson mass with different, but manageable, systematic uncertainties. The W boson production cross section is large, and the event samples generally have very small contamination by background. Currently, the largest source of systematic error is the dependence of the acceptance on the rapidity of the produced W boson, requiring a correction that depends on quark and antiquark parton distribution functions (PDFs). Experimental uncertainties are at the same level as those due to PDFs and are expected to continue to decrease accordingly.

We see good prospects for improving the measurement of M_W at the LHC. The statistical component of the error will be negligible already with the current LHC data set. The error from PDFs doubles in going from the Tevatron to the LHC because proton-proton collisions involve no valence antiquarks. However, we anticipate that this error will be decreased using new data on the vector boson rapidity and charge asymmetries. The issue of PDF improvement is discussed further in Section 3.6.1. The huge statistical precision available at the LHC will allow for control of calorimetric and tracking systematic uncertainties. In Table 3-2 we see that the PDF error in M_W is expected to be brought down to ± 5 MeV with 300 fb⁻¹ and to ± 3 MeV with 3000 fb⁻¹. For this last estimate, the uncertainties in the PDFs must be pushed to a factor of 7 smaller than those today. At each stage, the experimental systematics are expected to keep pace with the improvements in the PDFs, leading to a final uncertainty in M_W of ± 5 MeV.

Lepton colliders offer an opportunity to push the uncertainty in M_W down even further. The W mass was measured at LEP to ± 36 MeV from the kinematics of W^+W^- production. The uncertainty was dominated by statistical errors, with a substantial additional contribution from the modeling of hadronization. Both sources will benefit from the data set on W^+W^- , about 1000 times larger, that will be available at next-generation e^+e^- colliders such as ILC and TLEP. We estimate an error below ± 4 MeV from this method, and a similar error from independent measurements on single W boson production.

The ultimate W mass measurement would come from a dedicated energy scan of the W^+W^- threshold at 160 GeV. Such a measurement could reach ± 2.5 MeV with the statistics available from the ILC and $\pm$

1 MeV with the statistics available from TLEP. At this level, systematic errors become dominant. The program also requires a detailed precision theory of the W^+W^- threshold, using methods now applied to the $t\bar{t}$ threshold.

The measurement of the value of the weak mixing angle $\sin^2 \theta_{eff}$ associated with quark and lepton couplings to the Z boson resonance offers an orthogonal probe of the electroweak interactions. The current accuracy in $\sin^2 \theta_{eff}$ is at the 16×10^{-5} level of precision and is dominated by measurements from LEP and SLC. This level might be reached but probably will not be surpassed at the LHC. Again, uncertainties in PDFs give the limiting systematic error. Measurements from the polarization-dependence of the Z cross section and from the b quark forward-backward asymmetry are discrepant by about 3σ , indicating an experimental question that should be resolved.

Future lepton colliders give an opportunity to improve the precision in $\sin^2 \theta_{eff}$. The ILC program includes a few months of running at the Z resonance to produce a data set of 10^9 Z 's, improving the statistics from LEP by a factor of 100 with highly polarized beams. The ILC detectors should also dramatically improve the capability for heavy flavor tagging. This “Giga- Z ” program should improve the uncertainty in $\sin^2 \theta_{eff}$ by a factor of 10. The program also would give new measurements of other Z pole observables sensitive to new TeV mass particles, most importantly, the fraction R_b of Z decays to $b\bar{b}$.

TLEP envisions a multi-year program at higher luminosity to collect 10^{12} events on the Z resonance. This potentially pushes the precision of electroweak measurements by another order of magnitude, though systematic contributions to the errors must still be understood. Among other factors, the Z mass must be measured more accurately than the current 2.5 MeV. This is possible at TLEP if transverse polarization can be achieved in single-beam operation. A direct measurement of $\sin^2 \theta_{eff}$ is most effectively done using longitudinal polarization in colliding beam mode. The feasibility of achieving this at TLEP needs to be understood.

Loop effects from TeV mass particles can produce shifts from the SM expectations at the 10^{-4} level in both M_W and $\sin^2 \theta_{eff}$, so the improved capabilities for precision electroweak measurements may give significant evidence for new particles. Quantitative estimates of these shifts for a number of new physics models are given in [33].

3.5.2 Interactions of W and Z bosons

The interactions of W and Z bosons can be studied through the measurement of vector boson pair production and multi-vector boson production. This study has already begun at LEP and the Tevatron, where parameters of the triple gauge boson interactions were bounded within a few percent of their SM values.

Vector boson interactions are described in a unified way through the formalism of effective Lagrangians. In this formalism, we parametrize departures from the SM Lagrangian, in which the Yang-Mills vertices for γ , W , and Z appear as terms of dimension 4, by adding terms with operators of higher dimension that are invariant under the $SU(2) \times U(1)$ gauge symmetry. A typical term involving an operator of dimension 6 is

$$\delta\mathcal{L} = \frac{c_W}{\Lambda^2} (D_\mu \Phi)^\dagger W_{\mu\nu} (D_\nu \Phi) , \quad (3.15)$$

where Φ is the Higgs doublet field and $W_{\mu\nu}$ is the W boson field strength. The overall size of the interaction is controlled by the parameter Λ , which has the dimensions of GeV and gives the energy scale of the new physics that is added to the SM. The coefficient c_W is a number of order 1 giving the size of this interaction relative to those of other dimension 6 operators. The term (3.15) and other similar, terms contribute to

the triple and quartic gauge boson vertices. Additional operators of dimension 8 can modify the quartic gauge boson interactions independently of the triple-boson interactions. A typical term with a dimension 8 operator is

$$\delta\mathcal{L} = \frac{f_{T,0}}{\Lambda^4} \text{tr}(W_{\mu\nu})^2 \text{tr}(W_{\lambda\sigma})^2. \quad (3.16)$$

In a weak-coupling theory such as the SM, the coefficients c_i and f_j are induced by loop diagrams and should be highly suppressed by powers of $\alpha_w/4\pi \sim 10^{-3}$. However, in theories with strong interactions in the Higgs sector, the c_i and f_j coefficients could be of order 1. In this context, the Λ parameters would be interpreted as the masses of Higgs sector resonances. For example, the operator (3.16) would be induced by a scalar resonance in the Higgs sector.

The current bounds on triple gauge boson couplings imply that the Λ parameters associated with dimension 6 operators are higher than about 600 GeV. High-statistics measurements of the triple gauge bosons by observation of W^+W^- and ZZ production in e^+e^- reactions at 500 GeV are expected to be sensitive to deviations from the SM that are 10 times smaller, pushing the sensitivity to Λ almost to 2 TeV.

It is difficult for hadron colliders to have similar sensitivity to triple gauge couplings. One source of this difficulty is that the LHC experiments study diboson reactions at higher energies, where additional terms from higher-dimension operators are important and so the extraction of the coefficients of dimension 6 operators is model-dependent. But there is a compensatory advantage. Working at higher energy, the LHC will study W and Z bosons at energies where Higgs sector strong interactions can dramatically alter the amplitudes for vector boson scattering and triboson production.

Some quantitative examples are presented in [33]. In examples studied there, the sensitivity to the coefficients f/Λ^4 of dimension 8 operators implies that, assuming $f = 1$, the LHC at 300 fb $^{-1}$ would achieve exclusion of a Λ value greater than 1.5 TeV. The HL-LHC, with 3000 fb $^{-1}$, would roughly triple the significance of the effect, allowing 5 sigma discovery at 1.5 TeV or exclusion up to 1.8 TeV. If the origin of this new physics lies in strongly-coupled dynamics, the HL-LHC provides discovery-level sensitivity to virtual effects of new resonances with masses ($4\pi\Lambda$) up to 19 TeV. If interpreted as sensitivity to f , the HL-LHC is more sensitive by a factor of 2-3 compared to the LHC at 300 fb $^{-1}$.

Increasing the energy allowed for the diboson system dramatically increases the physics reach. For discovery of anomalous couplings, a 33 TeV pp collider would reach Λ values above 1.8 TeV, while a VLHC at 100 TeV would reach above 4.6 TeV. These values extend well into the region in which new Higgs sector dynamics would be expected in models of this type.

3.6 Quantum Chromodynamics and the Strong Interaction

Every probe of particle physics at high energies eventually requires detailed knowledge of the strong interactions. Even in pure electroweak processes, the strong interactions affect the values of the electroweak coupling constants α and α_w through coupling constant renormalization. In the next decade, when most of our new knowledge about particle physics will come from hadron collider experiments, our understanding of the strong interactions will influence every aspect of the data. Observables measured at the LHC are affected by the structure of the proton, by radiation from initial- and final-state quarks and gluons, by the transition from quarks to hadrons, and by the detailed physics that produces multi-jet events.

The strong interactions are known to be described by the Yang-Mills theory Quantum Chromodynamics (QCD). This theory has a coupling constant that is weak at short distances and strong at large distances. Our understanding of QCD is imperfect. We have limited tools for the strongly coupled regime, and precision

calculation in the weakly coupled regime is technically complex. Nevertheless, our knowledge of QCD has taken enormous strides since the previous Snowmass workshop a decade ago. In this section, we review the current state of our tools for QCD and indicate the opportunities for further progress. More details on all of the topics discussed here can be found in the working group report [34].

In our discussion of precision quantum field theory calculation, we will describe one-loop radiative corrections as next-to-leading order (NLO), and higher corrections as NNLO, *etc.*

3.6.1 Parton distribution functions

Our knowledge of the initial state in hadron-hadron collisions is encoded in the representation of the proton structure given by the parton distribution functions (PDFs). The provision of PDF distributions with uncertainties is an innovation of the past decade [35]. The uncertainties quoted have been continually improved through the addition of new data sets, especially from the Tevatron and HERA experiments.

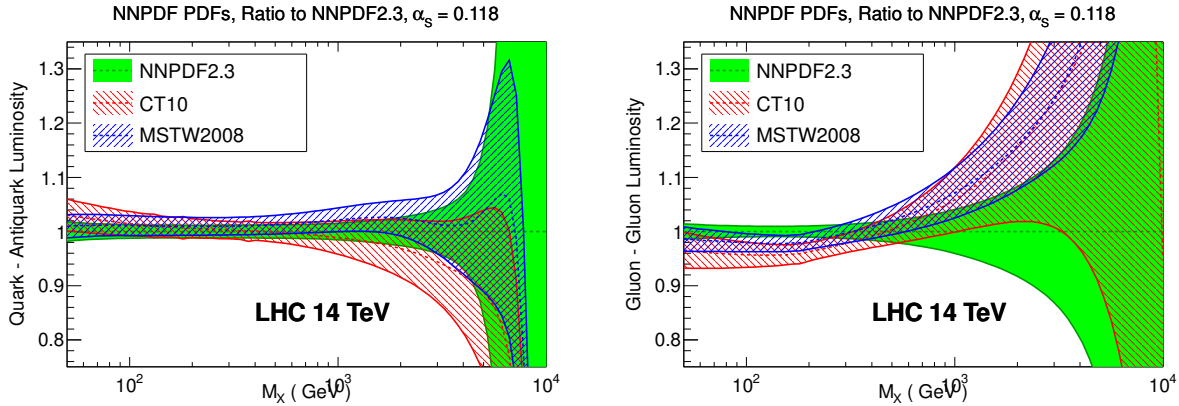


Figure 3-3. Comparison of the partonic luminosities at the 14 TeV LHC, as a function of the parton center of mass energy, from the CT10, MSTW, and NNPDF2.3 NNLO PDF sets: Left: $q\bar{q}$ luminosity; Right: gg luminosity, from [34]

Still, there are gaps in our knowledge, especially in the components relevant to the study of physics beyond the SM. The leading PDF distributions disagree in their estimates of the gluon-gluon luminosity function at the mass of the Higgs boson; this accounts for an 8% systematic uncertainty in the extraction of the cross section for Higgs production. We have noted already in Section 3.5.1 that, at the LHC as opposed to the Tevatron, the less-constrained antiquark distributions in the proton play a more important role. Finally, all parton luminosities are poorly constrained by data for parton-parton invariant masses greater than about 500 GeV. This is clearly shown in Fig. 3-3.

We expect that these difficulties can be addressed using future data from the LHC. PDFs at the few-percent level of accuracy require theoretical calculations at the NNLO level. These are already available for the Drell-Yan process [36]. The NNLO computation of the total cross section for top quark pair production has recently been completed [37] and is now being extended to the rapidity distribution. NNLO calculations for 2-jet production are in progress. Over the next few years, these calculations will be used in conjunction with a very high-statistics data set on jet, top quark, and lepton pair production from the LHC. The LHCb experiment has an important role to play in the measurement of rapidity distributions at high values of rapidity greater than 2.5 [38].

Further improvements in PDFs can result from the program of the LHeC. The data expected will reduce the error in the gluon luminosity to a few percent at the Higgs boson mass and to 5-10% in the multi-TeV region.

3.6.2 QCD coupling constant α_s

The strength of the QCD coupling is determined by the value of the coupling constant α_s , usually expressed as its $\overline{MS}$ value at the Z mass. The value of this quantity currently quoted by the Particle Data Group [39] has 0.6% uncertainty. We have pointed out in Section 3.4.1 that this is a limiting systematic error in the evaluation of the SM predictions for Higgs boson couplings.

There are three strategies to improve the precision of α_s . First, α_s can be obtained from the Z boson width or the ratio of hadronic to leptonic Z decays. This technique is theoretically unambiguous but currently is limited by statistics. The present value of α_s from Z decays has a 4% uncertainty. The Giga- Z program at the ILC discussed in Section 3.5.1 should collect 100 times more data and so improve this uncertainty to 0.4%. The very-high-luminosity Z program envisioned for TLEP could decrease this uncertainty further to 0.1%. We judge that higher-statistics measurements of e^+e^- event shapes are not competitive with these improvements.

Proposed improvements of PDFs from LHeC will lead to an improved value of α_s from the measurement of PDF evolution. The expected statistical error would be $\pm 0.2\%$ from LHeC alone and $\pm 0.1\%$ from the combination of LHeC and HERA. The theoretical systematic error for this method is not as well understood, but we estimate this at $\pm 0.5\%$ once one further order in QCD perturbation theory (N^3LO) is calculated.

The most accurate current values of α_s come from lattice gauge theory. Higher-statistics lattice estimates and calculation of additional terms in lattice perturbation theory should decrease the current uncertainties over the next decade to 0.3%. These improvements will come together with improvements in the values of the quark masses, as discussed in 3.4.1.

3.6.3 Electroweak corrections to hadron collider processes

The quest for few-percent accuracy in predictions for hadron colliders brings new elements into play. In particular, it requires that QED and electroweak corrections be included in all predictions for LHC.

Three elements are needed here. Electroweak corrections at NLO order are generally comparable to NNLO QCD corrections. Thus, calculations intended to be accurate to NNLO should also include electroweak corrections of order α_w and, if possible, the mixed corrections of order $\alpha_w\alpha_s$. Electromagnetic corrections to hadronic reactions cannot be consistently included without a set of PDFs derived from formulae that include NLO QED corrections. This requires a nontrivial modification of PDF fitting programs in order to introduce a photon PDF for the proton. Photon-induced reactions can contribute to LHC processes at the few-percent level, increasing to the 10% level at higher pp energies.

Finally, at energies of a TeV and above, electroweak Sudakov effects — negative corrections to two-particle production proportional to $\alpha_w \log^2 s/M_W^2$ — can become important. These are 10% corrections for Drell-Yan processes producing 3 TeV dilepton systems. At pp colliders of energy 33 TeV and above, these double logarithmic corrections must be resummed systematically.

3.6.4 High-precision calculation

In the past decade, a revolution in calculational technique has made it possible to derive formulae at NLO for the QCD cross sections for complex multiparton processes such as $pp \rightarrow W + 4$ jets and $pp \rightarrow t\bar{t} + 2$ jets. This has reduced the size of the theoretical errors in these cross sections from order 1 to 10-20%. Methods are now being developed to evaluate general two-parton processes and even some three-parton production processes to NNLO, to reduce these theoretical errors to the few-percent level.

We have already made reference to NNLO calculations of $t\bar{t}$ and two-jet production. A very important target here is the cross section for Higgs boson production in association with one or more jets. Many Higgs measurements at the LHC include jet vetoes to control background from $t\bar{t}$ production and other sources, so explicit accounting for emitted jets is necessary. These cross sections often require terms to NNLO for stable summation of the perturbation series.

Beyond the fixed-order perturbation theory, many other aspects of higher-order computation remain to be understood. NNLO computations often display large logarithms, which should be systematically resummed. The merging of Monte Carlo event generators with NLO QCD calculations is incompletely understood, and new difficulties arise at NNLO. We are optimistic that Higgs boson production and other QCD processes can be computed to few-percent accuracy, but many challenges remain.

3.7 Fully Understanding the Top Quark

The top quark is the heaviest quark and, indeed, the heaviest elementary particle known today. Its large mass gives it the strongest coupling to the Higgs boson and to other possible particles of the Higgs sector. The mass of the top quark seems to be anomalously large—though it is sometime argued that it is the masses of all other quarks and leptons that are anomalously small. For all of these reasons, the top quark merits thorough experimental investigation.

The Tevatron experiments that discovered the top quark produced, in all, about 100,000 of these particles. The LHC experiments have already produced 10 million and aim for many billions of top quarks by the end of the HL-LHC. Future lepton colliders will bring new precision tools to the study of the top quark. In this section, we will discuss what can be learned from these observations. More details on all of these topics can be found in the working group report [40].

3.7.1 Top quark mass

Like α_s discussed in Section 3.6.2, the top quark mass is a crucial input parameter for many SM predictions. It is already the most accurately known quark mass; a 2 GeV uncertainty on this quantity corresponds to a measurement with 1% precision. An accurate top quark mass is needed for precision electroweak fits, with an error of 600 MeV on the top quark mass yielding, for example, an error of 5 MeV in M_W . The top quark mass is also an important input to the question of ultimate vacuum stability in the SM [41]. Figure 3-4 shows that the measured masses of the Higgs boson and the top quark seem to place the vacuum of the SM on the very margin of stability. More precision, especially on m_t , might indicate whether this is an accident or a hint concerning the parameters of the Higgs sector.

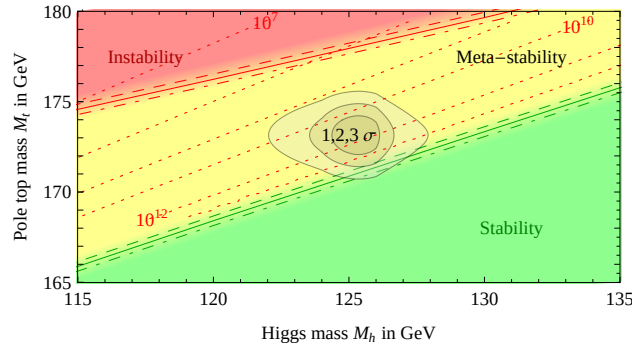


Figure 3-4. Regions of metastability and instability of the Higgs potential of the SM, as the top quark and Higgs boson masses are varied, from [41].

The top quark mass is most precisely defined as an $\overline{MS}$ quantity, evaluated most conveniently at the $\overline{MS}$ top quark mass value itself. However, experimental determinations of the top quark mass are typically done by kinematic fitting to templates based on leading-order QCD and containing hadronization effects whose uncertainties are poorly controlled. Thus, the precision determination of the top quark mass requires, first, defining an observable that can be explicitly related to the $\overline{MS}$ top quark mass, and, then, measuring that quantity accurately.

One solution to this challenge at the LHC is an idea from CMS [42] to measure the top quark mass from the endpoint of the distribution of the mass of the jet plus lepton system, $m(b\ell)$, that is the observable product of a semileptonic top quark decay. In top quark pair production events, ℓ is identified as an isolated lepton and the jet is specified as a b -tagged jet defined by the anti- k_T or a jet clustering algorithm. With these definitions, the distribution of $m(b\ell)$ can be computed in QCD perturbation theory in terms of the perturbative pole mass, which can be related to the $\overline{MS}$ mass with an error of the order of 200 MeV. The results are largely insensitive to new physics contributions to top quark production. The endpoint feature in the distribution is sharp and strongly dependent on m_t . We expect that this method can reach a total uncertainty of 500 MeV with the statistics of the HL-LHC. Other methods for measuring the top quark mass at the LHC are discussed in [40].

At lepton colliders, the cross section for top quark pair production near the threshold has a distinctive rise sensitive to the position of the lowest (unstable) $t\bar{t}$ bound state. Extensive theoretical work has evaluated this cross section to NNLO, with resummation of all large logarithms. The threshold position can be measured to 35 MeV at the ILC and somewhat better at TLEP and muon colliders. The conversion to the $\overline{MS}$ mass gives a total uncertainty of about 100 MeV. This very accurate value of m_t is well matched to the precision electroweak programs at lepton colliders described in Section 3.5.

3.7.2 Strong and electroweak couplings

The production and dynamics of top quark pairs at colliders offers many opportunities to test the strong and electroweak couplings of these particles. At hadron colliders, the dominant pair production mechanism is through QCD. The current agreement between the predicted and measured values verifies that the absolute strength of the QCD coupling to the top quark is equal to the value of α_s measured for light quarks and gluons to about 3% accuracy.

Changes in the form of the top quark coupling to gluons might be induced by new resonances associated with top quark compositeness. Possible magnetic or electric dipole couplings can be probed from the kinematics of top final states to better than 1% at the LHC with 300 fb^{-1} . Though it is difficult to measure the absolute size of the top quark width at a hadron collider, the W boson helicity fractions in top quark decay are sensitive to modifications of the top quark coupling to the W boson, with similar sensitivity. The cross section for single top quark production provides a measure of the weak interaction mixing matrix element V_{tb} , which should reach an accuracy of 2.5% at 300 fb^{-1} . Couplings of the top quark to the photon and Z boson are constrained by measurements of radiation from a $t\bar{t}$ state. The HL-LHC is expected to reach sensitivities of a few percent for the photon couplings and 15-20% for the Z boson couplings.

At lepton colliders, $t\bar{t}$ pairs are produced through virtual photons and Z bosons, with large interference effects that depend on the beam polarization. The ILC and CLIC, which can take advantage of large beam polarization, expect to reach sensitivities below the 1% level for both photon and Z couplings. Randall-Sundrum models [43] and other models with top quark and Higgs compositeness predict shifts of the Z boson couplings to $t\bar{t}$ at the few percent level; these effects could potentially be discovered in the linear collider programs.

3.7.3 Rare decays

The large samples of top quarks available at the LHC allow deep searches for flavor-changing top quark decays. Neutral current decays of the top quark such as $t \rightarrow \gamma c$ or $t \rightarrow gc$ are utterly negligible in the SM, with predicted branching ratios smaller than 10^{-12} . These decays can appear with branching ratios as large as 10^{-4} in models with an extended Higgs sector or R-parity-violating supersymmetric couplings that bring in two structures of flavor mixing. Searches for these decays at the HL-LHC can be sensitive to branching ratios below 10^{-5} .

Lepton colliders can also access these flavor-changing couplings in single top quark production, for example, through $\gamma^*, Z^* \rightarrow t\bar{c}, t\bar{u}$. Searches for these processes can reach sensitivities close to 10^{-4} even in experiments at 250 GeV, below the $t\bar{t}$ threshold, and below 10^{-5} in the full ILC program at 500 GeV.

More details on the specific estimates for each possible neutral current coupling can be found in [40].

3.7.4 Searches for new particles related to the top quark

The motivation that we have given for new particles at the TeV scale in Section 3.2.2 directly implies the presence of exotic partners of the top quark. Examples of these particles are top squarks, in models of SUSY, and Kaluza-Klein excitations of top quarks, in models with extra space dimensions. Searches for these particles have been a very high priority in the LHC program and will continue to be pursued intensively as more data accumulate.

Searches are designed individually for each type of exotic particle. The most powerful searches make use of the fact that top quarks resulting from the decay of the partner particle have different polarizations from those typically produced in SM pair-production. This is reflected in the kinematic distributions of the $t\bar{t}$ final states. For particles with masses of 1 TeV and above, the preferred method for identifying final-state top quarks is as single jets with high jet mass and a three-jet substructure [44]. This “boosted top” identification is quite insensitive to the pileup associated with high luminosity.

Top squarks in SUSY might be as heavy as other supersymmetric particles. However, the naturalness arguments we have given in Section 3.2.2 indicate that they might, alternatively, be among the lightest supersymmetric partners. The LHC experiments have searched extensively for direct pair-production of top squarks that decay to the lightest supersymmetric particle $\tilde{\chi}^0$ through $\tilde{t} \rightarrow t\tilde{\chi}^0$ and $\tilde{t} \rightarrow b\tilde{\chi}^+$. Current searches exclude a top squark up to about 650 GeV in the limit of light electroweak superpartners. The sensitivity should advance to about 1.0 TeV at 14 TeV and 300 fb⁻¹, and to 1.2 TeV with 3000 fb⁻¹.

In models with extra space dimensions and models with composite Higgs bosons and top quarks, the expected partners of the top quark are fermions with vectorlike couplings. The searches for these particles are similar to those for hypothetical fourth-generation quarks, but they involve more complex decay patterns, with $T \rightarrow Wb$, $T \rightarrow tZ$ and $T \rightarrow th$. Searches for these particles that are comprehensive with respect to the decay mode currently exclude vectorlike top partners up to masses of about 800 GeV. The 14 TeV stages of the LHC will be able to discover these particles at masses of sensitivity to 1.2 TeV for 300 fb⁻¹ and to 1.45 TeV for 3000 fb⁻¹.

Models with composite Higgs bosons and top quarks also typically include resonances in the multi-TeV mass region that decay preferentially to $t\bar{t}$. Randall-Sundrum models, for example, predict a resonance at a mass of a few TeV decaying with high top quark polarization to $t_R\bar{t}_L$. The boosted top quark identification described above was developed for the problem of discovering such states and is indeed expected to be very effective. Applying the same methods to larger data sets, we expect a sensitivity to such resonances up to 4.5 TeV for the 14 TeV LHC with 300 fb⁻¹ and up to 6.5 TeV with 3000 fb⁻¹.

Additional examples of new particle searches involving top quarks are described in [40].

3.8 The Path Beyond the Standard Model - New Particles, Forces, and Dimensions

Models of new physics associated with the TeV mass scale contain a wide variety of new particles. These include the particles of an extended Higgs sector discussed in Section 3.4.5 and partners of the top quark discussed in Section 3.7.4. Many of the schemes discussed in Section 3.2.1 for explaining Higgs condensation are based on far-reaching principles that require a spectroscopy of new particles containing heavy partners for all SM particles. This includes additional strongly interacting particles, particles with only electroweak interactions, and new vector bosons. Some of these particles may have lifetimes long enough that their decays are not prompt in a collider experiment. One or more of these particles could be constituents of the cosmic dark matter.

New particles could also introduce new flavor-changing interactions. Observation of such interactions of new particles can complement searches for flavor and CP violation in rare processes. This subject will be discussed in Section 3.9.

The LHC experiments have been able to search for new particles very robustly using a broad range of techniques. In this section, we will discuss how higher energies and luminosities at hadron colliders and new capabilities of lepton colliders will extend these searches.

Models of Higgs condensation and other TeV-scale phenomena based on the various underlying principles discussed in Section 3.2.2 make qualitatively different predictions for the quantum numbers and mass relations of the new particle spectrum. Thus, the first discovery of a new particle beyond the Standard Model will define a direction for an extensive research program, one that will be carried out over decades with multiple complementary experiments. In this section, we will emphasize the comparative reach of proposed collider

programs to make this first discovery. Examples of the consequences of such a discovery will be given in Section 3.11. We will have room to discuss only a limited number of examples. The full range of searches for new particles accessible to TeV energy experiments is described in the New Particles and Forces working group report [45].

The dependence of search reach on luminosity deserves comment. Away from kinematic limits for a given collider energy, parton-parton luminosity functions scale such that increasing the parton-parton center-of-mass energy by a factor 2 decreases the luminosity by a factor of 10. This rule, which implies that a factor of 10 in luminosity increases the search reach by a factor of 2 in mass, must break down at masses near the kinematic limit. At the 14 TeV LHC, the reach increase falls off from the canonical factor of 2 for pair-produced particles with masses well above 1 TeV.

3.8.1 New Vector Bosons

Some predicted particles would show up in collider experiments as distinct resonances. An example is a color-singlet vector boson associated with an extension of the Yang-Mills symmetry group beyond that of the SM. Such bosons are required in many contexts, including models with left-right symmetric weak interactions at high energy, models of the Higgsino mass in SUSY, and models with extra dimensions. Models of Higgs composite structure often require breaking of a larger gauge group to the SM symmetry group.

Searches for vector bosons are conducted at hadron colliders by looking for narrow dilepton resonances. A typical benchmark is sensitivity to the “sequential SM” Z' , a boson with the couplings of the Z but with a higher mass. Current results from the LHC require the mass of such a particle to be above 2.5 TeV. With 14 TeV, it will be possible to discover such a resonance at 4.5 TeV for 300 fb^{-1} , and at 7 TeV for 3000 fb^{-1} . The values of the production cross section and the leptonic forward-backward asymmetry (with respect to the direction of production) give information on the couplings of the Z' . At higher pp energies, the discovery reach increases to 12 TeV for 33 TeV and 30 TeV for the 100 TeV VLHC.

Lepton colliders are sensitive to new vector bosons that interfere with the s -channel virtual photon and Z boson in two-fermion production $\ell^+\ell^- \rightarrow f\bar{f}$. The reach for discovery of a sequential Z' at the ILC at 500 GeV is also about 7 TeV and scales proportional to the center of mass energy for higher energy colliders. Measurements of the Z' signal with two possible beam polarizations and with individual lepton and quark final states gives a large amount of information that may help identify the quantum numbers of the new boson.

3.8.2 Supersymmetry

Searches for supersymmetry (SUSY) encompass a wide range of strategies aimed at different particles of the SUSY spectrum.

The most generic searches assume that supersymmetric partners of the SM particle carry a conserved quantum number, called R-parity. If the lightest supersymmetric particle is neutral, it will typically be weakly interacting and will not be observed in a collider detector. Events are then characterized as containing several hadronic jets, associated with decay to the lightest particle plus missing transverse momentum. In the following discussion, we will call these “jets+ $\cancel{E}_T$ ” events. No significant excess of such events has yet been observed. The results of the searches are then parametrized by limits on the gluino mass and on a squark mass, assumed common to all squark flavors. Current LHC results exclude jets+ $\cancel{E}_T$ events up to

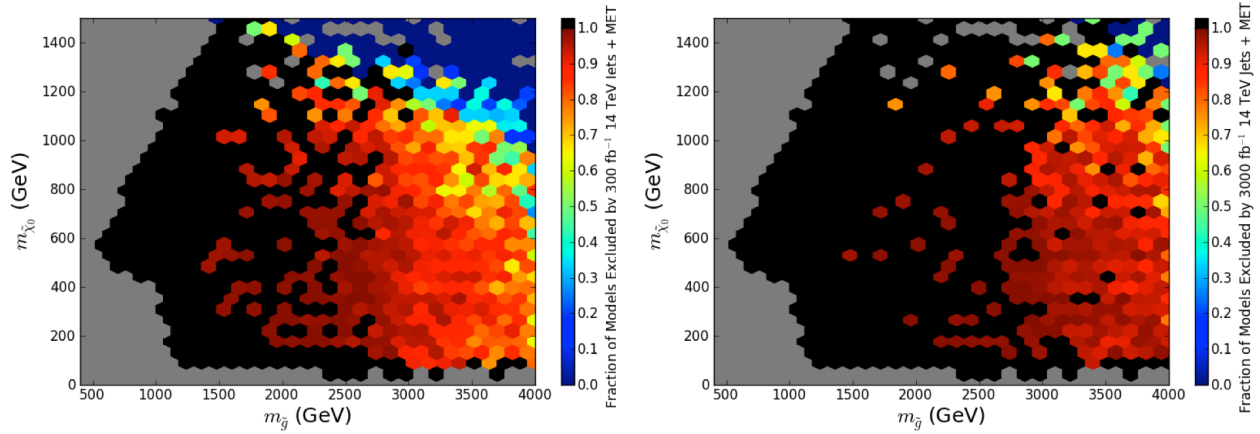


Figure 3-5. Projections for coverage of the pMSSM 19-parameter model space in searches at the LHC at 300 fb^{-1} (left) and 3000 fb^{-1} (right), shown in the plane of gluino mass versus lightest particle mass. The pixel colors show the fraction of pMSSM models excluded at that stage of the LHC program: Black is complete exclusion; green is exclusion of 50% of the models with the given mass values. From [49].

gluino masses of 1.0 TeV and, independently, up to squark masses of 1.3 TeV. For the future stages of the LHC, we expect to be able to discover such events up to gluino masses of 1.9 TeV and squark masses of 2.3 TeV with 300 fb^{-1} , and to 2.3 TeV and 2.7 TeV with 3000 fb^{-1} . This reflects more than a factor of 2 in increased search power at the 300 fb^{-1} stage, and another 20% with the additional factor of 10 in luminosity. The gluino discovery reach increases to 4.8 TeV for a 33 TeV pp collider and to 10.2 TeV for the VLHC.

It is possible that the first signal of SUSY would not be given by the generic search just described, but would require a more specialized analysis. Special search techniques are needed in models in which mass gaps in the SUSY spectrum are relatively small (“compressed spectrum”), so that hard jets are not emitted in particle decays, and models in which only the partners of top quarks, or perhaps only color-singlet supersymmetric particles, are produced at accessible energies. Strong theoretical motivations have been given for models of this type. A compressed spectrum is needed in many models of supersymmetric dark matter particles to allow “coannihilation” to produce the correct dark matter density [46]. They are also needed in models in which only the specific particles restricted by the naturalness bounds in Section 3.2.2 lie below 2 TeV. In such models, the first signal of SUSY would come from direct top squark pair production or gluino pair production with decay to heavy flavor. Reach estimates for top squark pair production were given above in Section 3.7.4.

Models in which SUSY discovery is more difficult at the 8 TeV LHC thus benefit more from the increase in luminosity provided by the HL-LHC program. Models for which the first signal of SUSY would be the partners of W and Z bosons can be searched for at the 14 TeV LHC, with discovery expected up to masses of about 500 GeV. The factor of 10 luminosity increase to HL-LHC increases the reach by a factor of 2 in the analyses [47, 48].

Another way to look at this issue is shown in Fig. 3-5. The figures show a survey of a large number of SUSY models [49] plotted in the plane of gluino mass versus lightest superparticle mass. The color-coding gives the fraction of models excluded by LHC searches at 14 TeV, with 300 fb^{-1} on the left, and with 3000 fb^{-1} on the right. The figures show that the boundary at which most models are excluded shifts to the right by about 30%, but also that a large number of exceptional cases to the left of this boundary are addressed at higher luminosity.

One more exception should be noted. The supersymmetric partners of the Higgs boson automatically have small mass splitting of a few GeV. The direct pair-production of these particles through electroweak interactions is essentially invisible at the LHC, except through searches for initial-state radiation plus invisible particles, described in Section 3.8.4. In a scenario in which these are the lightest supersymmetric particles, the ability of lepton colliders to be sensitive to very small energy depositions in decay would be crucial to observe and study these particles. Studies of Higgsino pair production at the ILC are described in [50].

3.8.3 Long-lived particles

The searches we have described so far assume that all new particles decay promptly at the pp collision point. However, there are many models that give exceptions to this. ATLAS and CMS have carried out dedicated searches for tracks associated with long-lived massive particles and for particles decaying in the detector, perhaps out of synchronization with the bunch crossings. Current limits are stronger than those in searches for promptly decaying particles. For example, ATLAS places limits of 310 GeV on a tau slepton, 600 GeV on a top squark, and 985 GeV on a gluino. Should such long-lived particles exist, the LHC detectors trap a sample of them for detailed studies of their decay modes and lifetimes.

3.8.4 Dark matter

The search for jets+ $\cancel{E}_T$ events discussed above for SUSY applies to a broader class of theories. In Section 3.2.3, we introduced WIMP dark matter as a general motivation for new particles with TeV scale masses. Any model with a new TeV spectroscopy characterized by a new quantum number can give rise to a dark matter candidate particle. The requirements are that the lightest new particle is neutral and that the quantum number is conserved sufficiently that this particle is stable over the age of the universe. Heavier states carrying QCD color will decay to this lightest particle, producing events with jets and missing transverse momentum. If the partners of quarks are fermionic rather than bosonic, the production cross section will be higher. The generic SUSY search described above can then be interpreted as a robust search for models predicting a TeV spectroscopy and dark matter.

It is possible that the heavier states of the TeV spectrum are out of reach kinematically. Then the discovery of dark matter would require observing direct pair-production of the essentially invisible dark matter particles. This can be done using the fact that the production of particles in a hard scattering process sometimes also produces gluons or photons radiated from the initial-state particles. If the particles mediating the pair-production reaction are heavy enough, this initial-state radiation can be at higher momentum than, and therefore distinguishable from, the ordinary particle production in typical collisions.

Reach estimates for the discovery of dark matter pair-production have been studied systematically in [51], using an effective operator formalism to describe the coupling of dark matter to SM particles. This formalism also allows cross sections measured at colliders to be related to rates for direct and indirect detection of dark matter. The paper [51] also included limits on an explicit model with a lighter Z' mediator. Some results of the effective field theory analysis, comparing limits on the dark matter-nucleon cross section from LHC and higher energy pp colliders to limits from direct detection, are shown in Fig. 3-6. It is noteworthy that the VLHC can place limits on the dark matter particle mass above 1 TeV, close to the unitarity limit for thermal production of such a particle in the early universe.

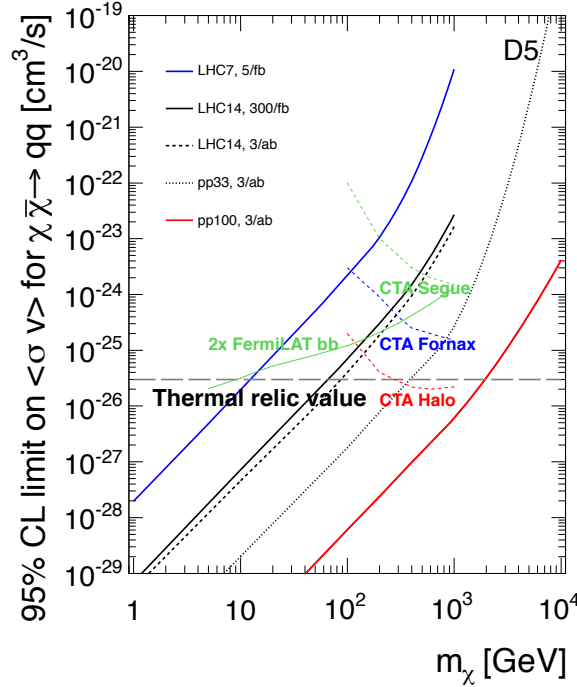


Figure 3-6. Comparison of 95% confidence limits on the WIMP pair annihilation cross section in models of dark matter, analyzed in an effective operator formalism, for current and proposed facilities. The collider limits are constraints on the operator coefficient from searches for missing energy events. The limits from gamma ray telescopes are constraints from searches for dark matter annihilation in galaxies of the local group. From [51].

3.9 Flavor Mixing and CP Violation at High Energy

The explanation of Higgs condensation may or may not give insight into the theory of flavor. It is a mystery why quarks and leptons have a hierarchical mass spectrum and why the weak interactions are not diagonal in flavor and violate CP. In the SM, these features are parametrized by the fermion-Higgs Yukawa couplings. The idea that flavors are distinguished only through terms of the structure of the Yukawa couplings is called “Minimal Flavor Violation.”

Models of new physics at the TeV scale introduce a large number of new couplings. These in principle can be proportional to flavor-violating couplings with completely new structures. Such structures are not needed to build a model of Higgs condensation. In fact, many specific models—most notably, SUSY with gauge-mediated breaking [58]—are flavor-blind. It is possible also that some principle, analogous to the GIM mechanism [59], requires that new flavor couplings are related to the Yukawa couplings. In these cases, there are no new flavor-changing effects beyond the SM arising from the TeV scale.

However, it is also possible that flavor couplings among new particles have a different pattern. Such couplings can generate rare flavor-changing weak decays. They can also affect the phenomenology of the new particles themselves, requiring new strategies for searches. In this section, we discuss examples of models of this type.

There are many possibilities, only a few of which can be discussed here. A more complete catalog is given in the working group report [52].

3.9.1 SUSY with Flavor-Dependent Soft Masses

In Section 3.8, we discussed reach estimates for models of TeV spectroscopy that were either blind to flavor or singled out only the third generation. More general forms of the new particle spectrum are allowed. The most important difficulties with flavor observables come when squarks with the same gauge quantum numbers, *e.g.*, the partners of d_R and s_R , have different masses. But there is little difficulty in giving the partners of d_R and d_L smaller masses than those of the other squarks. This weakens the experimental limits on the lightest squark mass. In fact, it is possible, with other mechanisms to suppress flavor effects of new physics, to allow only the partner of c_R to be light. This has been explored in detail for SUSY models [53, 54]. The current limit on the charm squark in such models is about 300 GeV.

3.9.2 R-parity Violating SUSY

It is possible that the R-parity conservation law that should keep the lightest SUSY partner stable is violated by new interactions. These interactions necessarily have a complex structure in flavor. One possible form of the R-parity violating interaction is

$$\lambda_{ijk}^1 U_i D_j D_k, \quad (3.17)$$

where U_i , D_i are the right-handed quarks or their squark partners and $i = 1, 2, 3$ is the generation number. This violates baryon number conservation, allowing a squark to decay to two antiquarks. The interaction must be antisymmetric in color, and this requires it also to be antisymmetric in the flavors of the two down-type quarks. Other possible R-parity violating interactions have the forms

$$\lambda_{ijk}^2 L_i L_j \bar{E}_k, \quad \lambda_{ia}^3 L_i H_a \quad (3.18)$$

where L_i , $\bar{E}_i$ are left-handed leptons or antileptons or their slepton partners and H_a is a Higgs or Higgsino field. These interactions violate lepton number conservation. The coefficients of these operators are usually taken to be small to avoid unwanted flavor-changing rare decays. In particular, either the operator (3.17) or the lepton number violating operators must be highly suppressed to avoid rapid proton decay.

In R-parity violating models with the operator (3.17) only, SUSY decay chains typically end with the lightest SUSY particle decaying to jets. These jets must have a nontrivial flavor structure, possibly with a b jet always included.

R-parity violation with the Higgs operator in (3.18) can produce neutrino masses through the “Type III seesaw”: A neutrino converts to a Higgsino, which then converts back to an antineutrino, possibly of a different flavor. The difference between the quark and lepton flavor mixing patterns is explained by the statement that the mass matrices come from unrelated operators. In such models, the branching ratios of the Higgsino are related to the neutrino mixing angles, and this relation can be confirmed by direct measurement [55]. In more general seesaw models of neutrino mass, it is possible that the seesaw scale could be the TeV scale, setting up other relations between TeV mass neutral leptons and the neutrino mixing matrix [56].

3.9.3 Models with Electroweak Baryogenesis

To explain the asymmetry between the numbers of baryons and antibaryons in the universe, a new source of CP violation is needed beyond that of the CKM phase. One natural place to look for this is in an extended Higgs sector. A second requirement is that the electroweak phase transition be first-order. In this case, the transition takes place via the expansion of bubbles that contain vacuum with a nonzero Higgs field value into the region with zero Higgs field value. When the bubbles fill all of space, the phase transition is complete. During the period of bubble expansion, top quarks scatter from the bubbles because they have zero mass outside but are massive inside where the Higgs condensate is present. A CP phase in the Higgs sector makes this scattering asymmetric between matter and antimatter. Both criteria can be satisfied in models with multiple Higgs fields [57]. Measurements of the Higgs self-coupling and of CP violation in Higgs decays, described in Sections 3.4.2 and 3.4.3, test models of this type.

3.10 Scientific Cases for Future Colliders

In the previous sections, we presented the physics opportunities for the next steps in the Energy Frontier in terms of individual particles and research areas under study. It is also interesting to assemble these topics in terms of the experimental program that each accelerator in the list given in Section 3.3.2 will provide. Integrating over the topics in this way, we see that many of these proposed accelerators have very substantial physics programs that will explore the TeV energy scale across a broad range of measurements.

In this section, we present the cases for the various accelerators as if each accelerator stood on its own, with no further physics discoveries between now and the time that it begins operation. However, one should always keep in mind the possibility of discoveries would open up the study of physics beyond the SM. We have argued already that the likelihood of the discovery of new particle is very high even for the coming runs of the LHC at 14 TeV up to 300 fb^{-1} . Such a discovery would need to be followed up by further exploration that would benefit from accelerators with complementary capabilities or higher energy. This might, in the end, be the most important benefit of building the accelerators that come later in the timeline below. We will expand on this idea in Section 3.11.

3.10.1 LHC in this decade: 300 fb^{-1}

First of all, we emphasize the many opportunities that will be provided by the coming run of the LHC at 14 TeV. Operation of the LHC at 14 TeV to collect 300 fb^{-1} of data offers a tremendous increase in the power of new particle searches. The increase is close to a factor 2 in mass in most channels. Many of these searches, such as the search for the gluino almost to 2 TeV and the search for vectorlike top partners above 1 TeV, access ranges of the masses that are strongly motivated in models of Higgs condensation.

This impressive capability is only one aspect of a broad program that will be carried out at the LHC over the next ten years. The LHC with 300 fb^{-1} will:

1. Clarify Higgs boson couplings, mass, spin, CP to the 10% level.
2. Provide the first measurement of the top quark-Higgs boson coupling.
3. Measure the W boson mass measurement with a precision below 10 MeV.
4. Make the first measurements of VV scattering.

5. Measure the top quark mass in a theoretically and experimentally precise way, to 600 MeV.
6. Measure top quark couplings to gluons, Z and W bosons, and photons with a precision potentially sensitive to new physics — a factor 2–5 better than today.
7. Search for top squarks and top partners and $t\bar{t}$ resonances predicted in models of composite top, Higgs.
8. Provide data for a new generation of PDFs with improved gluon and antiquark distributions.
9. Measure electroweak cross sections in pp collisions, and provide a well-determined photon PDF.
10. Extend by a factor of 2 the sensitivity to new particles, including SUSY particles, Z , top partners — key ingredients for models of the Higgs potential — and the widest range of other possible TeV-mass particles.
11. Carry out deep searches for dark matter particles accompanied by initial state radiation.

3.10.2 High-Luminosity LHC: 3000 fb⁻¹

The second high luminosity running of the LHC will be designed for instantaneous luminosities of 5×10^{34} cm⁻²s⁻¹. This running with 3000 fb⁻¹ of accumulated data truly inaugurates the high-precision electroweak era at LHC with few percent precision for most Higgs boson couplings as well as the 5 MeV threshold in M_W mass determination. The LHC with 3000 fb⁻¹ will:

1. Begin the precision era in Higgs boson couplings, with sensitivities to 2-10%, and 1% for the ratio of $\gamma\gamma$ and ZZ couplings.
2. Measure rare Higgs boson decays, $\mu^+\mu^-$ and $Z\gamma$, with 100 M Higgs bosons.
3. Provide the first evidence of the Higgs boson self-coupling.
4. Carry out powerful searches for extended Higgs bosons.
5. Measure the W boson mass to a precision ± 5 MeV.
6. Carry out precise measurements of VV scattering with access to Higgs sector resonances.
7. Measure the top quark to a precision of ± 500 MeV.
8. Carry out an intensive search for rare, flavor-changing, top quark couplings, with 10 billion top quarks.
9. Search for top squarks and partners in models of composite top quarks and Higgs bosons in the expected range of masses.
10. Improve q , g , and γ PDFs to higher x and Q^2 .
11. Provide a 20-40% increase in mass reach for generic new particle searches, which can be as much as a 1 TeV step in mass reach.
12. Extend by a factor of 2 the mass reach for particles produced by the electroweak interactions.
13. Follow up an earlier discovery at LHC — or in dark matter or flavor searches.

3.10.3 ILC, up to 500 GeV

The ILC would run at 250 GeV, 350 GeV, and 500 GeV, in a program that could begin as early as the second half of the next decade. It would study the properties of the Higgs boson, the top quark, and possibly also newly discovered particles, in very fine detail. The ILC, up 500 GeV center-of-mass energy, will:

1. Study tagged Higgs bosons using the reaction $e^+e^- \rightarrow Zh$. This gives model-independent Higgs boson width and branching ratio measurements, and direct study of all Higgs decay modes, including invisible and exotic decays.
2. Measure Higgs boson couplings in a model-independent way with percent-level precision necessary to probe for new physics beyond the reach of LHC direct searches.
3. Study the CP properties of the Higgs boson in fermionic channels (e.g., $\tau^+\tau^-$).
4. Carry out a Giga-Z program for EW precision measurements, and measure the W boson mass to 4 MeV and beyond.
5. Improve the measurement of triple vector boson couplings by a factor 10, to a precision below expectations for models with Higgs sector resonances.
6. Provide a theoretically and experimentally precise top quark mass to ± 100 MeV.
7. Carry out sub-% measurement of top couplings to γ and Z , with precision well below expectations in models of composite top quarks and Higgs bosons.
8. Search for rare top couplings in $e^+e^- \rightarrow t\bar{c}, t\bar{u}$.
9. Improve the uncertainty in α_s from the Giga-Z program.
10. Search unambiguously for new particles in LHC blind spots – Higgsino, stealth stop, compressed spectra, WIMP dark matter.

3.10.4 ILC at 1 TeV

An extension of ILC to 1 TeV will access additional Higgs boson reactions for precision study and, possibly, also reach new particle thresholds. The ILC at 1 TeV will:

1. Provide a precision measurement of the Higgs boson coupling to the top quark, to 2%.
2. Measure the Higgs boson self-coupling to 13%.
3. Discover any extended Higgs boson states coupling to the Z , up to 500 GeV.
4. Improve the precision of triple gauge boson couplings by a factor of 4 over 500 GeV results.
5. Carry out a model-independent search for new particles with coupling to γ or Z to 500 GeV.
6. Search for Z using $e^+e^- \rightarrow f\bar{f}$ to masses of about 5 TeV, a reach comparable to LHC for similar models, and provide multiple observables for diagnostics of the Z couplings.
7. Follow up any previous discovery of a new particle, with a search for electroweak partners, a 1% precision mass measurement, observation of the complete decay profile, model-independent measurement of cross sections, branching ratios, and couplings with polarization observables, and a search for flavor and CP-violating interactions.

3.10.5 CLIC: 350 GeV, 1 TeV, 3 TeV

Extremely high energies in e^+e^- collisions will likely require technologies beyond that envisioned for the ILC. CLIC is proposed as an e^+e^- collider capable of multi-TeV energies. It would probe Higgs boson self-couplings and exotic scattering of both standard model particles and any new particles found or hinted at in earlier machines. CLIC will:

1. Provide a precision measurement of the Higgs boson coupling to top, to 2%.
2. Measure the Higgs boson self-coupling to 10%.
3. Discover any extended Higgs boson states coupling to the Z , up to 1500 GeV.
4. Improve the precision of triple gauge boson couplings by a factor of 4 over 500 GeV results.
5. Make precise measurement of VV scattering, sensitive to Higgs boson sector resonances.
6. Carry out a model-independent search for new particles with coupling to γ or Z to 1500 GeV, the expected range of masses for electroweakinos and WIMPs.
7. Search for Z using $e^+e^- \rightarrow f\bar{f}$ accessing masses above 10 TeV.
8. Follow up any discovery of new particles with the elements listed for the 1 TeV ILC, but accessing higher mass states.

3.10.6 Muon Collider: 125 GeV, 350 GeV, 1.5 TeV, 3 TeV

A muon collider holds promise as a technique for reaching very high energies in lepton-lepton collisions and for s -channel production of the Higgs boson and possible additional Higgs states. Studies of the muon collider are not yet mature, particularly in designing a detector that can overcome the background from decays of the muons circulating in the ring. However, promising first results on physics analysis including machine backgrounds were reported at Snowmass. A muon collider will:

1. Provide capabilities similar to those of ILC and CLIC described above.
2. Produce the Higgs boson, and possible heavy Higgs bosons, as s -channel resonances. This allows a sub-MeV Higgs boson mass measurement and a direct Higgs boson width measurement.

3.10.7 Photon Collider

Another technique for producing Higgs bosons in the s -channel is to convert an electron collider to a photon collider by backscattering laser light from the electron beams. This allows resonance studies at 80% of the electron center-of-mass energy. Photon colliders will:

1. Produce Higgs or extended Higgs bosons as s -channel resonances, offering percent-level accuracy in $\gamma\gamma$ coupling.
2. Study CP mixture and violation in the Higgs sector using polarized photon beams.

3.10.8 TLEP, Circular e^+e^-

An e^+e^- collider in a very large tunnel offers the possibility of very large integrated luminosity samples at 250 GeV and below and reasonable integrated luminosity at 350 GeV, especially if multiple detectors can be used simultaneously. TLEP will:

1. Offer the possibility of up to 10 times higher luminosity than linear e^+e^- colliders at 250 GeV, with corresponding factor of 3 improvements in boson couplings measurements.
2. Carry out precision electroweak measurements that could improve on ILC by a factor of 4 in $\sin^2 \theta_{eff}$, a factor of 4 in M_W , and a factor of 10 in M_Z .
3. Provide a theoretically and experimentally precise top quark mass to ± 100 MeV.
4. Search for rare top couplings in $e^+e^- \rightarrow t\bar{c}, t\bar{u}$.
5. Offer a possible improvement in α_s by a factor of 5 over Giga-Z, to 0.1% precision.

3.10.9 VLHC, at 100 TeV

One of the ideas that gained momentum at Snowmass was renewed interest in a Very Large Hadron Collider (VLHC). Our study recommends reinvigorating R&D toward realization of a VLHC. A VLHC will:

1. Provide high rates for double Higgs boson production, and a measurement of Higgs boson self coupling to 8%.
2. Search sensitively for new Higgs bosons and states associated with extended Higgs sectors at 1 TeV.
3. Dramatically improve the sensitivity to vector boson scattering and multiple vector boson production.
4. Increase the search reach for new particles associated with naturalness—including SUSY particles, top partners, and resonances—by almost an order of magnitude in mass over LHC. This corresponds to two orders of magnitude in fine-tuning.
5. Search for WIMP dark matter up to TeV masses, possibly covering the full natural mass range.
6. Follow up any discovery at LHC — or in dark matter or flavor searches — with more detailed measurements, and with searches for related higher-mass particles. Both luminosity and energy are relevant.

3.11 Discovery stories

Another way to survey the physics topics presented in Sections 3.4–3.9 is to consider the consequences of a discovery of new physics at the LHC later in this decade. We have emphasized, first, that the presence of new particles at the TeV scale is necessary to build a physics explanation of electroweak symmetry breaking, and, second, that the coming run of the LHC, up to 300 fb^{-1} , will improve the depth of searches for new particles by more than a factor of 2. The conclusion from these statements is that the discovery of new physics at the LHC is likely. This means that we should have a plan for following up this discovery and

exploring its implications. This program of course depends on the nature of the particle discovered, so a full analysis would be presented as a large number of case studies.

In this section, we give two illustrative examples of specific discoveries that might be made at the LHC in the coming decade, and the physics programs that would follow from them. Further examples of these “discovery stories” are presented in the working group reports.

3.11.1 Well-Tempered Neutralino in SUSY

The New Particles and Forces working group [45] considered in some detail the consequences of a particular SUSY model that could be discovered at the LHC with 300 fb^{-1} . This particular model has a gluino at 1.9 TeV, squarks ranging in mass from 1.3 TeV to 2.6 TeV, and bino and Higgsino states near 200 GeV. The bino and Higgsino are assumed to mix so that the lightest supersymmetric particle (LSP) would be a dark matter particle with the correct thermally generated cosmic density. This dark matter scenario, originally developed in [60], is called the “well-tempered neutralino” [61].

The LHC at 300 fb^{-1} would observe a robust jets+ $\cancel{E}_T$ signal. The signal would be dominated by the decay of the lighter squarks to a quark jet plus the unobserved LSP. The mass difference could be measured from kinematic distributions. Assuming that the LSP was light, this would also give an estimate of the squark mass. With the measured cross section, this would favor SUSY over models with fermionic partners.

The HL-LHC would produce some of the heavier squarks and the gluino. Detailed kinematic measurements would identify at least one more mass scale in the spectrum and give further evidence for the SUSY hypothesis. The direct production of electroweak states would not be observed at the LHC, because these states have a compressed spectrum.

A lepton collider with center-of-mass energy of 500 GeV would be able to pair-produce the Higgsinos and observe their decays to the LSP. Measurement of the polarized cross sections would give information about the quantum numbers of the electroweak states. It would also give an indirect determination of the mass of the electron-type slepton (750 GeV in this model) to 10 GeV. Using this information, it would be possible to evaluate the LSP annihilation cross section and show that it was consistent with that required for a dark matter particle.

Experiments at higher-energy colliders would be needed to discover the heaviest sleptons (at 3.3 TeV) and squarks (at 2.6 TeV). Eventually, the complete SUSY spectrum would be determined, and the data on the mass spectrum could be used to deduce the pattern of SUSY breaking.

3.11.2 $t\bar{t}$ Resonance in a Randall-Sundrum model

An alternative scenario is based on a Randall-Sundrum model with top and Higgs compositeness [43]. The first evidence of this model would be the discovery of a resonance in pp collisions that decayed to $t\bar{t}$. Such a resonance at 3 TeV would be discovered at the LHC with 300 fb^{-1} . Study of kinematic distributions of the $t\bar{t}$ final state would reveal that the top quarks were highly polarized, a prediction of this model.

The HL-LHC would discover an electroweak singlet top quark partner, and, possibly also, a doublet of quarks with vectorlike coupling to the electroweak interactions. It is possible that very accurate studies

of the $t\bar{t}$ spectrum would also reveal the presence of a color-singlet resonance, somewhat below 3 TeV. Its higher-statistics study of the TeV resonance might reveal a decay to $t\bar{c}$ with branching ratio 10^{-3} .

A lepton collider at 500 GeV would observe a significant 3% enhancement of the right-handed top quark coupling to the Z boson. The pattern of Higgs boson couplings would be shifted by the influence of the new top quark partners, with a substantial increase in the $t\bar{t}$ coupling, a 4% decrease in the $b\bar{b}$ coupling, and a 2% increase in the $\gamma\gamma$ coupling. The last of these effects would be observed by combining the high statistics measurement of $BR(\gamma\gamma)/BR(ZZ^*)$ from the HL-LHC with the precise measurement of the Higgs coupling to Z at a lepton collider.

These measurements would give a tantalizing first glimpse of the structure of the underlying composite Higgs model. Experiments at higher energy colliders capable of producing resonances up to 20 TeV in mass would be needed to explore the full structure of the spectrum of states.

3.12 Messages from the working groups

In this section, we collect the summary statements from each of the six working groups.

3.12.1 Higgs: the message

The conclusions of the Higgs Boson working group can be summarized as follows:

1. Direct measurement of the Higgs boson is the key to understanding electroweak symmetry breaking. The fact that the Higgs boson appears as a light, apparently fundamental, scalar particle needs explanation. A research program focused on the Higgs couplings to fermions and vector bosons and achieving a precision of a few percent or less is required to address these questions.
2. Full exploitation of the LHC is the path to few percent precision in the Higgs coupling and to a 50 MeV precision in the determination of the Higgs mass.
3. Full exploitation of a precision electron collider is the path to a model-independent measurement of the Higgs boson width and a sub-percent measurement of the Higgs couplings. Such precision is necessary to probe for new physics beyond the reach of LHC direct searches.

3.12.2 Electroweak: the message

The conclusions of the Electroweak Interactions working group can be summarized as follows:

1. Precision measurements of the W and Z bosons have the potential to probe indirectly for new particles with TeV masses. This precision program is within the capabilities of LHC, linear e^+e^- colliders, and TLEP.
2. Measurement of vector boson interactions will probe for new dynamics in the Higgs sector. In such theories, we expect correlated signals in triple and quartic gauge boson couplings. The LHC and linear colliders will have sensitivity into the mass region above 1 TeV.

3.12.3 QCD: the message

The conclusions of the QCD working group can be summarized as follows:

1. Improvements in PDF uncertainties are required. There are strategies at LHC for these improvements. QED and electroweak corrections must be included in PDFs and in perturbative calculations.
2. An uncertainty in α_s of order 0.1% may be achievable through improvements in lattice gauge theory and precision experiments.
3. Advances in all collider experiments, especially on the Higgs boson, require continued advances in perturbative QCD.

3.12.4 Top quark: the message

The conclusions of the Top Quark working group can be summarized as follows:

1. The top quark is intimately tied to the problems of electroweak symmetry breaking and flavor.
2. Precise and theoretically well-understood measurements of top quark masses are possible both at LHC and at e^+e^- colliders, matching, for each, the needs of the precision electroweak program.
3. New top quark couplings and new particles decaying to top quarks play a key role in models of electroweak symmetry breaking. LHC will search for the new particles directly. Linear collider experiments will be sensitive to predicted deviations from the SM in the top quark couplings.

3.12.5 New particles and forces: the message

The conclusions of the New Particles and Forces working group can be summarized as follows:

1. TeV mass particles are needed in essentially all models of new physics. The search for them is imperative.
2. LHC and future colliders will give us impressive capabilities for this study. Future programs target new physics at the all-important TeV scale, as can be seen in Fig. 3-7.
3. The search for TeV mass particles is integrally connected to searches for dark matter.

3.12.6 Flavor: the message

The conclusions of the Flavor and CP working group can be summarized as follows:

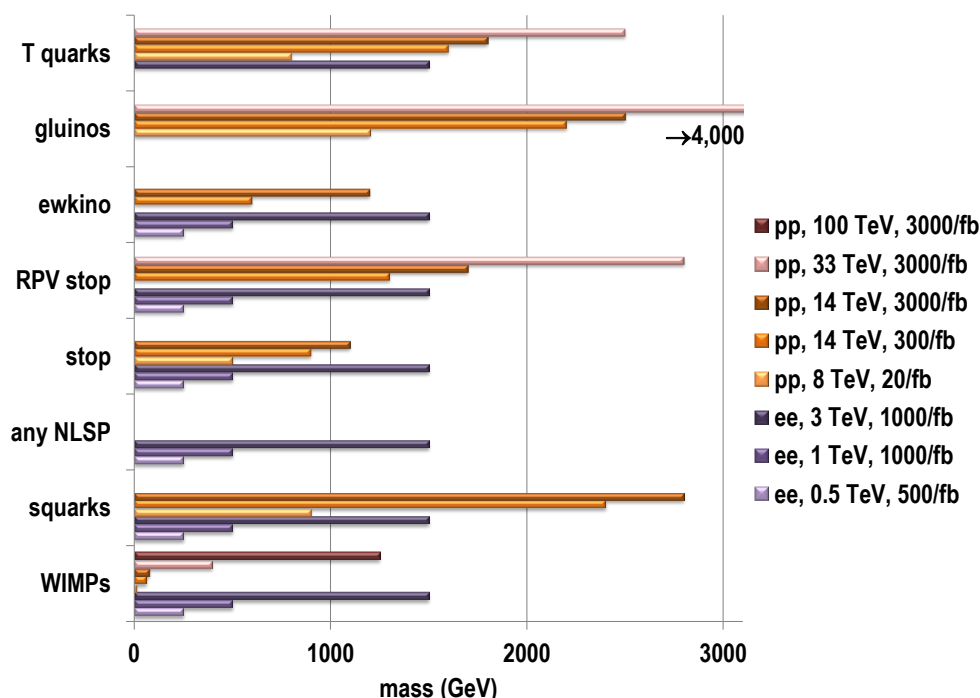


Figure 3-7. Examples of 95% confidence upper limits for new particle searches at proposed pp and e^+e^- colliders.

1. TeV mass particles may or may not introduce couplings with new types of flavor violation. These possible new couplings affect the search methods for new particles, in many cases, requiring new strategies.
2. The search for new particles is integrally connected to searches for rare flavor changing decays.

3.13 Conclusions

In this report, we have described the future program of research at high-energy colliders and summarized the efforts of six Snowmass 2013 Energy Frontier working groups. The detailed results that we have reviewed lead to a set of points that deserve special emphasis.

The search for new particles with masses of order 1 TeV is a central issue in particle physics. The mysteries associated with the Higgs field and dark matter give compelling arguments for a new particle spectroscopy at the TeV mass scale. Such particles must exist to provide a physics explanation for the Higgs condensation and symmetry breaking. We need to know their nature, and their implications for the laws of physics at very short distances. We have given many examples in which these particles address other major questions of particle physics, including the questions of flavor, dark matter, and the unification of forces.

The central capability of high-energy collider experiments is to produce massive elementary particles directly. In Energy Frontier experiments, we observe the W and Z bosons, the top quark, and the Higgs boson as real particles whose production and decay we can study in detail. The same will be true

for any new particles that we can create. This is a unique, direct, and powerful method to learn about the laws of physics.

There are three essential aspects in the exploration of the physics of the TeV mass scale:

1. We must study the Higgs boson itself in as much detail as possible, searching for signs of a larger Higgs sector and the effects of new heavy particles.
2. We must search for the imprint of the Higgs boson and its possible partners on the couplings of the W and Z bosons and the top quark.
3. We must search directly for new particles with TeV masses that can address important problems in fundamental physics.

This program can be realized at accelerators now envisioned to operate in the coming years.

1. We have emphasized the great opportunity that is being provided by the coming operation of the LHC at 14 TeV. The next stage of the LHC will double the range of searches for new particles and give similar leaps in capability for other probes of TeV physics.
2. We have projected quantitatively what will be achieved in running the LHC at high luminosity, to ultimately acquire 3000 fb^{-1} of data per experiment. For some physics topics, the gain is incremental. For others, in particular, the precision study of Higgs couplings and the search for new particles with only electroweak interactions, the high luminosity moves us to a qualitatively new level. Viewed as a whole, this is a compelling physics program.
3. We have listed many essential contributions to the exploration of the TeV scale that can be provided by lepton colliders. These include precision studies of the Higgs boson, the W and Z bosons, and the top quark, capable in all cases of discovering percent-level corrections to SM predictions expected as the effects of TeV mass particles. The construction and operation of the ILC in Japan will realize these goals.
4. We have emphasized that the quest to understand the TeV scale will not be finished with the results of accelerators of the next generation. It is likely that the discovery of new particles at the next stage of collider physics will open a definite path for exploration to still higher energies. Our study called attention, in particular, to the capabilities of a VLHC for further exploration of the TeV mass scale. The journey to still higher energies begins with renewed effort to bring advanced accelerator technologies to reality.

We emphasized in our introduction that the discovery of the Higgs boson changes everything. This discovery points to potentially profound modifications of the laws of physics at energies relatively close to those we now access at accelerators. The quest for new phenomena and the insights they will provide has just begun.

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Cosmic Frontier

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Executive summary

Investigations at the Cosmic Frontier use the Universe as a laboratory to learn about particle physics. Our understanding of the Universe has been transformed in recent years. In particular, experiments at the Cosmic Frontier have demonstrated that only 5% of the contents of the Universe are well understood, with the rest composed of mysterious dark matter and dark energy. As a result, the Cosmic Frontier now plays a central role in the global particle physics program, providing overwhelming evidence for new particles and new interactions, as well as powerful, unique opportunities to address many of our most fascinating questions: What is dark matter? What is dark energy? Why is there more matter than antimatter? What are the properties of neutrinos? How did the Universe begin? What is the physics of the Universe at the highest energies?

To identify outstanding scientific opportunities for the coming 10 to 20 years, the Cosmic Frontier Working Group was organized into six subgroups: 1. WIMP Dark Matter Direct Detection, 2. WIMP Dark Matter Indirect Detection, 3. Non-WIMP Dark Matter, 4. Dark Matter Complementarity, 5. Dark Energy and CMB, and 6. Cosmic Particles and Fundamental Physics. In several cases, these subgroups were further divided into topical working groups. The work of these groups was carried out through teleconferences and meetings, including the Cosmic Frontier Workshop at SLAC, March 6–8, 2013 (<http://www-conf.slac.stanford.edu/cosmic-frontier/2013>), and the Community Summer Study 2013 meeting in Minnesota, July 29–August 6, 2013 (<http://www.hep.umn.edu/css2013>). More complete summaries and references than can be presented here may be found in the talks from these meetings, and the subgroup and topical group summaries [1, 2, 3, 4, 5, 6, 7, 8, 9, 10, 11, 12, 13, 14, 15].

The Λ CDM standard model of cosmology provides the backdrop for much of Cosmic Frontier research. In this model, the Universe underwent a very early epoch of accelerated expansion (inflation), which was followed by eras in which the Universe was dominated successively by radiation, cold dark matter (CDM), and dark energy (Λ). At present, the known particles make up only 5% of the energy density of the Universe, with neutrinos contributing at least 0.1%. The rest is 25% dark matter and 70% dark energy. Remarkably, incisive measurements that explore all of the key components of the model are now within reach. The leaps in sensitivity of the new facilities bring us to a time with strong discovery potential in many areas. Further surprises are likely in this rapidly advancing area, with potentially far-reaching consequences.

Dark matter

The work of Snowmass highlighted the coming decade as one of particular promise for the goal of identifying dark matter. Evidence for particle dark matter has been building for 80 years through the study of galaxy clusters, galactic rotation curves, weak lensing, strong lensing, hot gas in galaxy clusters, galaxy cluster

collisions, supernovae, and the cosmic microwave background (CMB). However, all evidence so far is based on dark matter’s gravitational interactions, and its particle identity remains a deep mystery.

Among the many dark matter candidates, one well-known possibility is weakly-interacting massive particles (WIMPs) with masses in the 1 GeV to 100 TeV range. Particles with these masses are strongly motivated by particle physics, where they appear in many models designed to address the gauge hierarchy problem (the great discrepancy between the weak and Planck mass scales), and by cosmology, where they may obtain the correct relic density either through thermal freeze-out or through an asymmetry connecting their number density to that of baryons.

WIMP direct detection experiments search for the interactions of WIMPs with normal matter. WIMPs may scatter elastically off nuclei, producing recoil energies in the 1–100 keV range, which can be detected through phonons, ionization, scintillation, or other methods. There are daunting backgrounds, and so direct detection experiments must be placed deep underground. In the last several years, however, this field has seen a burgeoning of innovative approaches to discriminate signal from background, including experiments incorporating dual-phase media, self shielding, pulse shape discrimination, and threshold detectors.

The first two decades of direct detection experiments have yielded a diverse and successful program, resulting in “Moore’s Law”-type progress, with sensitivities doubling roughly every 18 months. In the coming decade, this rate of progress is expected to continue or even accelerate for both spin-independent and spin-dependent interactions. Upcoming second generation (G2) experiments will improve sensitivities by an order of magnitude, probing the Higgs-mediated cross sections expected for well-known supersymmetric and extra-dimensional candidates, and also extending the sensitivity to both $\sim$ GeV low-mass WIMPs, where possible signals have been reported, and $\sim$ TeV masses that are beyond the reach of colliders. Following these experiments, multi-ton-scale third generation (G3) experiments are expected to improve current sensitivities by up to three orders of magnitude and will either find dark matter or detect background events from solar, atmospheric, and diffuse supernovae neutrinos. Probing beyond this sensitivity will require either background subtraction or techniques such as directional detection or annual modulation. The Snowmass process produced a detailed census of present and proposed direct detection facilities, with uniform treatment of their capabilities and issues, along with a survey of promising technologies.

WIMPs may also be found through indirect detection, in which pairs of WIMPs annihilate, producing Standard Model particles, including gamma rays, neutrinos, electrons and positrons, protons and antiprotons, and deuterons and antideuterons. Detection of these particles may be used to constrain or infer dark matter properties. The expectation that WIMP annihilation in the early Universe determines the dark matter abundance sets a natural velocity-averaged annihilation cross section of $\langle\sigma_{\text{ann}}v\rangle \sim 3 \times 10^{-26} \text{ cm}^3 \text{ s}^{-1}$ for indirect detection experiments.

Gamma rays from dark matter annihilation may be detected by both space- and ground-based experiments. In space, the Fermi-LAT has recently demonstrated the promise of this approach, excluding the natural cross section $\langle\sigma_{\text{ann}}v\rangle$ for dark matter masses below 30 GeV, given certain halo profile and annihilation channel assumptions, and the reach is expected to be extended significantly with additional data. On the ground, VERITAS and other atmospheric Cherenkov telescopes have set significant limits by looking for gamma rays from dark matter-rich dwarf galaxies. Moving forward, the atmospheric Cherenkov telescope community has coalesced to build the Cherenkov Telescope Array (CTA), with sensitivity at the natural cross-section scale for dark matter masses from 100 GeV to 10 TeV, far beyond current or planned colliders, for conservative halo profiles and many of the possible annihilation channels. These projections require U.S. involvement in CTA, which will double the planned mid-sized telescope array and enable critical improvements in sensitivity and angular resolution.

Neutrinos also provide promising means for indirect detection of dark matter. High-energy neutrinos from the core of the Sun would be a smoking-gun signal of dark matter particle annihilation. The signal depends primarily on the spin-dependent WIMP-nucleon scattering cross section, which determines the capture rate. Current bounds from SuperK and IceCube already provide leading limits on this cross section, and the Precision IceCube Next Generation Upgrade (PINGU), an infill array upgrade to IceCube, will extend the sensitivity to lower masses. In the coming decade, IceCube and PINGU, along with Hyper-Kamiokande, will probe cross sections one to two orders of magnitude below current bounds, with sensitivities competitive with those of planned G2 direct detection experiments.

For antimatter, recent measurements of cosmic-ray positrons by the AMS-02 magnetic spectrometer confirm and improve with excellent precision earlier measurements by PAMELA and Fermi. The rising positron fraction could be indicative of positrons created in the decay or annihilation of dark matter. In the near future, AMS-02 will extend its determination of the positron fraction to energies close to 1 TeV, and add important information on cosmic-ray propagation. Given the possibility of astrophysical sources of primary positrons, however, it may be very difficult to definitively attribute the excess positrons to dark matter. Antideuterons provide a signal that is potentially more easily discriminated from astrophysical backgrounds. With a long-duration balloon flight, the General Antiparticle Spectrometer (GAPS) experiment could provide sensitivities comparable to AMS. Last, the production of positrons and electrons from dark matter annihilation also produces secondary radiation. Detection of signals with radio to X-ray frequencies has the potential to probe the WIMP parameter space.

The Snowmass process also evaluated the prospects for non-WIMP candidates, which could be some or all of the dark matter. The axion is particularly well-motivated, as it arises from the leading solution to the strong CP problem of the Standard Model. RF-cavity and solar searches for axions, such as ADMX and IAXO, will probe a large range of axion parameter space, including the cosmologically-favored region, and have strong discovery potential. Sterile neutrinos are also highly motivated by the observed non-zero masses of active neutrinos. In the mass range where sterile neutrinos are dark matter candidates, their radiative decays produce a monoenergetic photon, which may be detected with X-ray telescopes. Many other dark matter candidates were also surveyed, including asymmetric dark matter, primordial black holes, Q-balls, self-interacting dark matter, superheavy dark matter, and superWIMP dark matter.

How do the diverse strategies for identifying dark matter fit together? The Snowmass process produced a clear articulation of how the different approaches — including the direct and indirect detection experiments mentioned above, but also particle colliders and astrophysical probes — each provide unique and necessary information. This complementarity is discussed below and was examined in two theoretical frameworks. First, the discovery prospects were examined in complete supersymmetric models, with randomly selected parameters in the phenomenological MSSM framework. Second, the possibility that only the dark matter particle is kinematically accessible was considered using the framework of dark matter effective theories. In both cases, the complementarity of different approaches was evident at all levels, both to establish a compelling dark matter signal and, just as importantly, after discovery, to determine the detailed properties of the particle or particles that make up dark matter.

Dark energy and CMB

Cosmic surveys — optical imaging and spectroscopic surveys and detailed measurements of the CMB — precisely map the Universe on many different angular scales and over wide ranges of cosmic time. They provide unique information about cosmology and new physics, including inflation, dark matter, dark energy, and neutrino properties. These measurements are challenging, requiring advances in instrumentation and excellent control of systematic effects. Fortunately, these advances are now within reach, thanks to decades of investment and close collaborations between particle physicists and astrophysicists. The payoffs for this effort are large.

Measurements of the distance–redshift relation, first using supernovae and then additional complementary techniques, revealed the expansion history of the Universe, particularly over the past several billion years, and yielded the surprising discovery that the expansion rate has been increasing instead of decreasing. Now we must determine what is causing the cosmic acceleration. This “dark energy” must produce negative pressure to be responsible for the observed effect. One important clue is whether the negative pressure has been constant in time or is evolving. The stage III (the DES and HSC imaging surveys, and the PFS and eBOSS spectroscopic surveys) and stage IV (LSST imaging survey and DESI spectroscopic survey on mountaintops; Euclid and WFIRST-AFTA in space) dark energy facilities will constrain both the value and the evolution of the value with much higher precision, as recommended in previous community studies, but they will also do much more. We must also check whether our description of gravity is correct, and this is where measurements of the growth of structure, over a wide range of distance scales using both imaging and spectroscopic surveys, are needed.

There are several alternatives to general relativity (GR) that can accurately describe the observed distance–redshift relation, but they also modify the behavior of gravity over different distance scales. The alternative models therefore predict structure growth rates that are different from those in the standard theory. Measuring the structure growth rate over many different distance scales will test GR and the alternative models. Deviation from expectation on just one of these scales will signal new physics. In other words, the upcoming dark energy facilities, particularly at stage IV, where systematic error management is built deeply into the design, will provide many precise tests and will characterize the behavior of dark energy in much richer ways than “just” the overall value and its evolution with time. We will know the strength of the effects in a two-dimensional parameter space of distance and cosmic time, as well as any deviations from expectations in the correlations among of them. Further surprises may await us.

Inflation is the leading paradigm for the dynamics of the very early Universe, and current observations of large-scale structure lend support to this intriguing idea. The most direct available probes of inflation come from CMB observations, and the overall agreement is remarkably good. However, it has not been possible to explore the underlying physics of inflation, until now: The coming generations of CMB experiments will have sufficient sensitivity to falsify large classes of models. The signal is a characteristic pattern with non-zero curl (called “*B* mode”) faintly imprinted on the polarization of the CMB fluctuations, due to gravitational waves produced during the epoch of inflation. The shape of the potential of the scalar field driving inflation directly affects the spectrum of gravitational waves and hence the strength of the imprint, r (the ratio of tensor to scalar power), over characteristic angular scales on the sky. The current generation of experiments is sensitive to $r \sim 0.1$, but over the next 10 to 20 years, improvements of two orders of magnitude are possible by scaling the number of detectors by similar factors, from $\sim 10^3$ (current) to $\sim 10^4$ (generation III) to $\sim 5 \times 10^5$ (generation IV). This would require a change from the way things have been done in the past. Groups would merge into one coordinated effort, and national lab facility design, integration, computing, and management capabilities would be tapped.

Remarkably, future optical and CMB cosmic surveys, as well as future polar-ice neutrino projects (see below), will also provide precise information about neutrino properties, including the mass hierarchy, the number of light neutrinos, and the sum of the neutrino species masses. Combining this information with accelerator- and reactor-based neutrino experiments, as well as other experiments, such as those searching for neutrinoless double-beta decay, will accelerate our understanding of fundamental neutrino properties and enable us to understand the implications of apparent inconsistencies.

Snowmass provided an excellent opportunity to address common problems and to develop a common vision for the potential of cosmic surveys for particle physics. Highlights included advancing detailed strategies to distinguish dark energy from modified gravity; exploiting the complementarity of probes for determining the key cosmological parameters; understanding more deeply the strengths and ultimate limitations of the different techniques; and discussing the planned facilities, which are the result of intensive community

processes over many years. The group articulated a set of goals: (1) Remain a leader in dark energy research, (2) build a generation IV CMB polarization experiment, and (3) extend the reach of cosmic surveys with targeted calibration campaigns, targeted R&D, and support for work at the interface of theory, simulation, and data analysis.

Cosmic particles

Measurements of fluxes of cosmic particles (charged particles, photons, and neutrinos) address many topics in particle physics beyond indirect dark matter searches. Recent results include the detection by IceCube of very high energy neutrinos that are likely to be from astrophysical sources; the observation of the GZK suppression in the cosmic-ray flux above 3×10^{19} eV; the measurement of the positron fraction up to 300 GeV, suggesting the existence of primary sources of positrons from astrophysical processes and/or dark matter interactions; and confirmation that supernova remnant systems are a source of galactic cosmic rays. These and other discoveries were made by the current generation of experiments.

Goals for the coming decade include determination of the origin of the highest energy particles in the Universe, measuring interaction cross sections at energies unattainable in terrestrial accelerators, detecting the GZK neutrinos that arise from the interactions of ultra-high-energy cosmic rays with the CMB, determining the neutrino mass hierarchy, and searching for other physics beyond the Standard Model.

To meet these goals, the group recommends: significant U.S. participation in the Cherenkov Telescope Array (CTA), which is the next-generation ground-based gamma-ray facility; simultaneous operation of Fermi, HAWC, and VERITAS, the current generation of U.S.-led space- and ground-based gamma-ray facilities; construction of the PINGU neutrino detector to lower the energy threshold to a few GeV and enable the determination of the neutrino mass hierarchy using atmospheric neutrinos; continued operation of the Auger and Telescope Array air shower arrays with upgrades to enhance the determination of the composition of the flux of cosmic rays around the GZK suppression region; construction and deployment of the JEM-EUSO mission aboard the International Space Station to extend observations of the cosmic ray flux and anisotropy well beyond the GZK region; and construction of a next-generation ultra-high-energy GZK neutrino detector, which will either detect GZK neutrinos (and constrain the neutrino-nucleon cross section at ultra-high energy) or exclude all but the most unfavorable parts of the allowed parameter space. A detailed census of present and proposed cosmic particle measurement facilities was produced during the Snowmass process.

Conclusions

In synergy with the other Frontier areas, the Cosmic Frontier provides particle physics clear evidence for physics beyond the Standard Model; profound questions of popular interest; frequent new results; surprises, with broad impacts; a large discovery space with unique probes; important cross-frontier topics; and a full range of project scales, providing flexible programmatic options. For each area of the Cosmic Frontier, the Snowmass study identified essential technologies and facilities, the advances required in theoretical models, and experiments with great promise. The largest projects are, appropriately and necessarily, international. The U.S. is still the leader in many areas of the Cosmic Frontier, but this field is evolving quickly and other regions with intensive interest in this physics are advancing rapidly.

4.1 Direct detection of WIMP dark matter

Deciphering the nature of dark matter is one of the primary goals of particle physics for the next decade. Astronomical evidence of many types, including cosmic microwave background (CMB) measurements, cluster and galaxy rotation curves, lensing studies and spectacular observations of galaxy cluster collisions, all points

to the existence of CDM particles. Cosmological simulations based on the CDM model have been remarkably successful at predicting the actual structures we see in the Universe [16]. Alternative explanations involving modification of Einstein’s theory of GR have not been able to explain this large body of evidence across all length scales.

WIMPs are strong candidates to explain dark matter. They represent a class of dark matter particles that froze out of thermal equilibrium in the early Universe with a relic density that matches observation. This coincidence of scales — the relic density and the weak force interaction scale — provides a compelling rationale for WIMPs as particle dark matter. There are many particle physics theories that provide natural candidates for WIMPs, but they do not limit the search parameters very much, leaving a search region with a range of 1 GeV to 100 TeV in mass and 10^{-40} to 10^{-50} cm² in interaction cross sections with normal matter.

Direct detection experiments are designed to identify the interaction of WIMPs with normal matter. Since WIMPs should interact with normal matter by elastic scattering with nuclei [17], this requires detecting nuclear recoil energies in the 1–100 keV range. These low energies and cross sections represent an enormous experimental challenge, especially in the face of daunting backgrounds from electron recoil interactions and from neutrons that mimic the nuclear recoil signature of WIMPs. The unambiguous detection of signal above these backgrounds would confirm the existence of WIMPs in our galaxy and begin to unravel the mystery of their identity, especially with complementary information from production in colliders and signals from annihilation in our galaxy or in the Sun.

Direct detection experiments must be located in deep underground laboratories to avoid effects of cosmic-ray interactions that produce energetic neutrons that could mimic WIMPs. The experiments must also shield the detectors from the decay products of radioactivity in the environment and in the materials of the experiment itself. This is especially important for neutrons resulting from fission or (α, n) reactions, since a single scatter from a neutron produces a nuclear recoil that is indistinguishable from that produced by a WIMP. Luckily, some portion of the neutrons will scatter multiple times in the detector, and so neutron events can be identified and rejected on this basis. In most cases, the experiments also define an ultra-pure active “fiducial” volume that is shielded from radioactive decay products produced by impurities in the materials surrounding the detection material.

The basic methodology for direct detection experiments is to search for elastic scattering of a WIMP from a target nucleus. The rate of candidate nuclear recoils is converted into a cross section for WIMP-nucleon interactions following a standard prescription that includes the effects of nuclear physics and astrophysical properties [18]. Experiments can be sensitive to both nuclear spin-independent (SI) interactions and spin-dependent (SD) interactions. For the momentum exchange range of interest, the SI interaction is expected to be approximately coherent across the entire nucleus, so for a WIMP with equal coupling to protons and neutrons, the rate scales with the square of the atomic mass of the target nucleus. Current experiments are therefore more sensitive to SI dark matter than SD dark matter. Experimental results are usually presented as a plot of WIMP-nucleon cross section versus WIMP mass to allow comparison among experiments. Figure 4-1 shows the current SI landscape, with strict upper limits for higher mass WIMPs and some closed contours for lower mass WIMPs. The SD interaction is generally divided into proton and neutron couplings; the results to date are summarized in Fig. 4-2. Only direct detection can provide limits on neutron couplings, but solar neutrinos from WIMPs that annihilate in the Sun are stronger for proton coupling. Other types of interactions are possible, and it is important to run multiple experiments with different targets, both to cover the parameter space for discovery and to study the interaction type when signals are found.

Nuclear recoils from WIMP scattering result in a featureless energy spectrum, rising exponentially as the energy decreases. Experiments typically do not directly measure the nuclear recoil energy. Instead, the energy deposited by a particle interaction must be reconstructed from the experimental measurements as

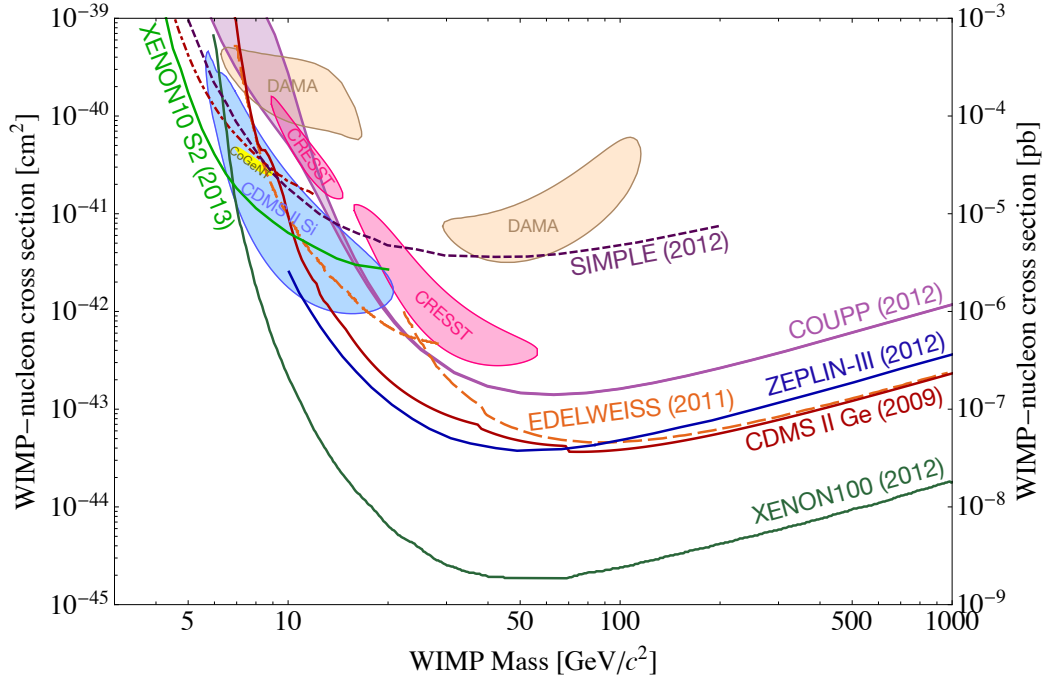


Figure 4-1. Constraints on spin-independent WIMP-nucleon cross sections as a function of WIMP mass as of Summer 2013 [19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31, 32].

either nuclear-recoil (keV_r) or electron-recoil (keV_{ee}). Conversion between the two energies is dependent on the target and experimental technique, and must be calibrated by each experiment. Typically, radioactive gamma sources or in-situ doped beta emitters are used to provide calibration for electron recoils, and neutron sources provide a source of nuclear recoil events. Since the nuclear recoil calibration is difficult at low energies, there is significant uncertainty in the low WIMP mass exclusion limits, sowing some controversy when comparing limits and discovery contours between targets.

A complete list of the current targets and technologies in use for direct detection of WIMPs can be found in the CF1 Summary [1]. One of the key features of modern experiments is the use of discrimination to select nuclear recoils and reject backgrounds that are dominated by electron recoils. Often the signal is split into two components that respond differently to nuclear and electron recoils. Noble liquids, such as argon and xenon, make use of ionization and scintillation light. Solid targets, such as silicon and germanium at cryogenic temperatures, use heat (or phonons) plus ionization. Crystals, such as CaWO_4 , contrast phonons and scintillation light. Two components are not necessary if an experiment can achieve internal purities and excellent fiducial isolation, or can use pulse-shape discrimination (*e.g.*, liquid argon), or is insensitive to electron recoil backgrounds (*e.g.*, threshold detectors).

Another method to deal with backgrounds is to exploit the fact that the Earth is moving through the dark matter that surrounds our galaxy, yielding a “WIMP wind” that appears to come from the constellation Cygnus [38]. This should, in principle, create a small “annual modulation” in the detected WIMP rates, as well as a somewhat larger daily modulation. However, if such effects were detected in an experiment, there would still have to be a convincing demonstration that there are no such modulations in background sources. Since backgrounds are expected to be isotropic, detection of a signal with a preferred direction could provide a powerful additional discriminant against backgrounds. Directional detectors attempt to exploit this effect

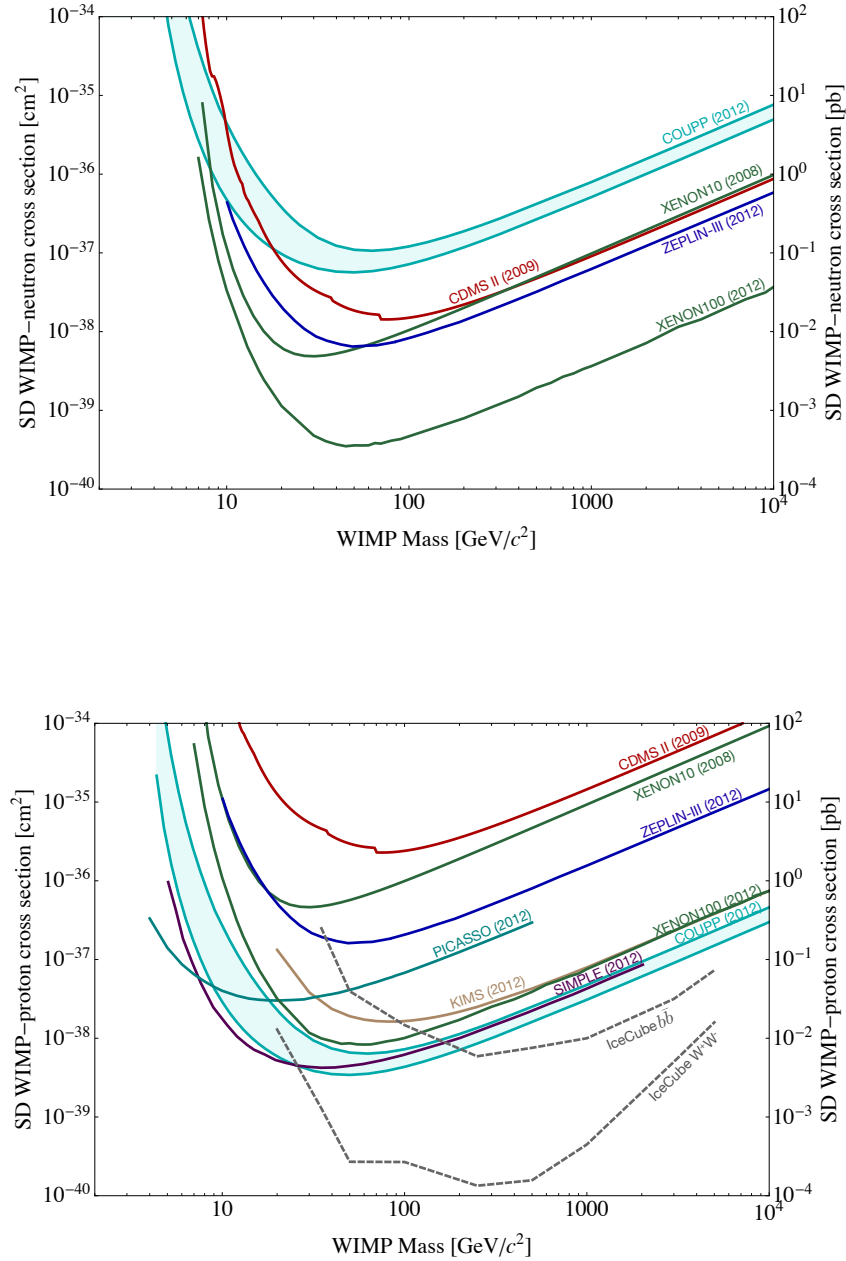


Figure 4-2. Spin-dependent WIMP-neutron (left) and WIMP-proton (right) cross section limits as functions of WIMP mass for direct detection experiments [33, 34, 28, 35, 36, 23, 22], as well as IceCube results (model-dependent) [37], as of Summer 2013.

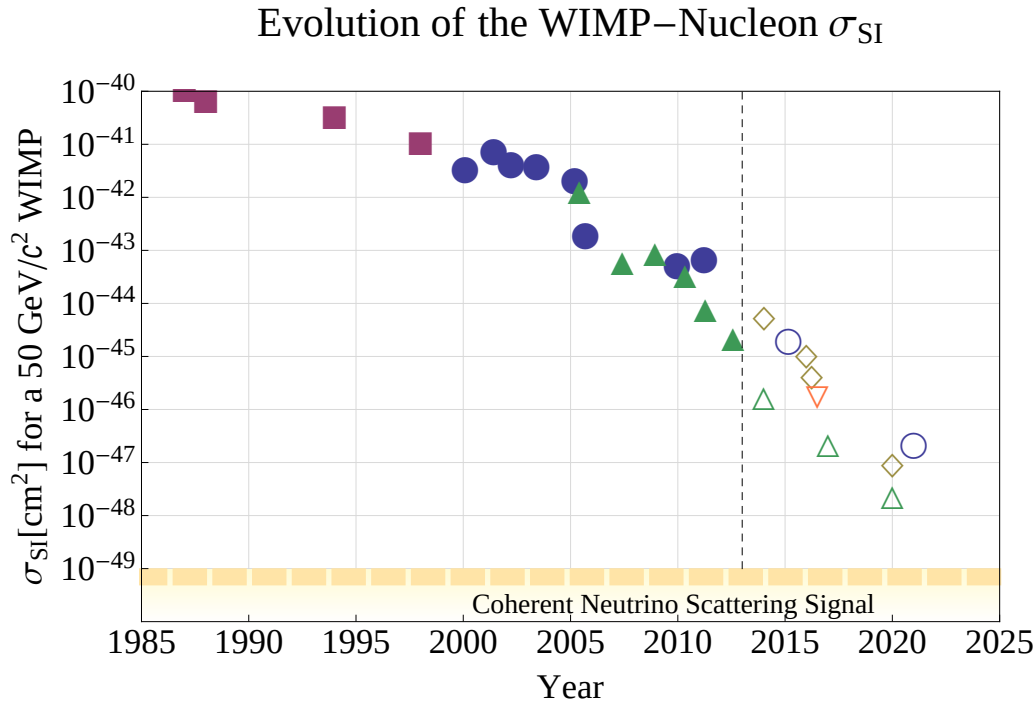


Figure 4-3. Spin-independent limits for the major WIMP direct detection experiments (solid) and their projected sensitivity (open) for spin independent cross sections for a $50 \text{ GeV}/c^2$ mass WIMP. The shapes correspond to technologies: cryogenic solid state (blue circles), crystal detectors (purple squares), liquid argon (brown diamonds), liquid xenon (green triangles) and threshold detectors (orange inverted triangle).

by sensing the vector direction of nuclear recoils, and they are usually looking for a daily sidereal difference as the Earth rotates relative to the WIMP wind.

The first two decades of dark matter direct detection experiments have yielded a diverse and successful program, although not yet definitive evidence for WIMPs. Starting with just a few experiments using solid-state targets, the technologies used for these experiments have grown considerably. There has been a remarkable improvement in WIMP sensitivities, especially in the range where the WIMP mass is comparable to the atomic mass of the target nuclei. A selection of spin-independent results from the first two decades of these experiments, and projections for the coming decade, is shown in Fig. 4-3 for a $50 \text{ GeV}/c^2$ WIMP mass. A “Moore’s law”-type improvement is particularly evident for such WIMP masses, with a sensitivity doubling time of roughly 18 months. Note that direct detection experiments have sensitivity to much larger WIMP masses as well, surpassing what is accessible at the LHC. More recently, there has also been rapid progress in sensitivity to WIMPs with masses at $10 \text{ GeV}/c^2$ and below.

Sensitivity projections are subject to uncertainties from many factors, including technical issues with the experiments, the appearance of unexpected backgrounds, and delays in funding. Despite these uncertainties, the history of the field gives us confidence that progress will continue unabated through the next decade. There is, however, an irreducible background caused by solar, atmospheric, and supernova neutrinos. The ^8B component of the solar neutrinos provides the dominant neutrino coherent scattering rate at low recoil energies. The ^8B spectrum can mimic a WIMP with a mass in the range of $5\text{--}10 \text{ GeV}/c^2$ (depending on the target) and a nucleon cross section of $\sim 5 \times 10^{-45} \text{ cm}^2$. For experiments targeted at larger WIMP masses,

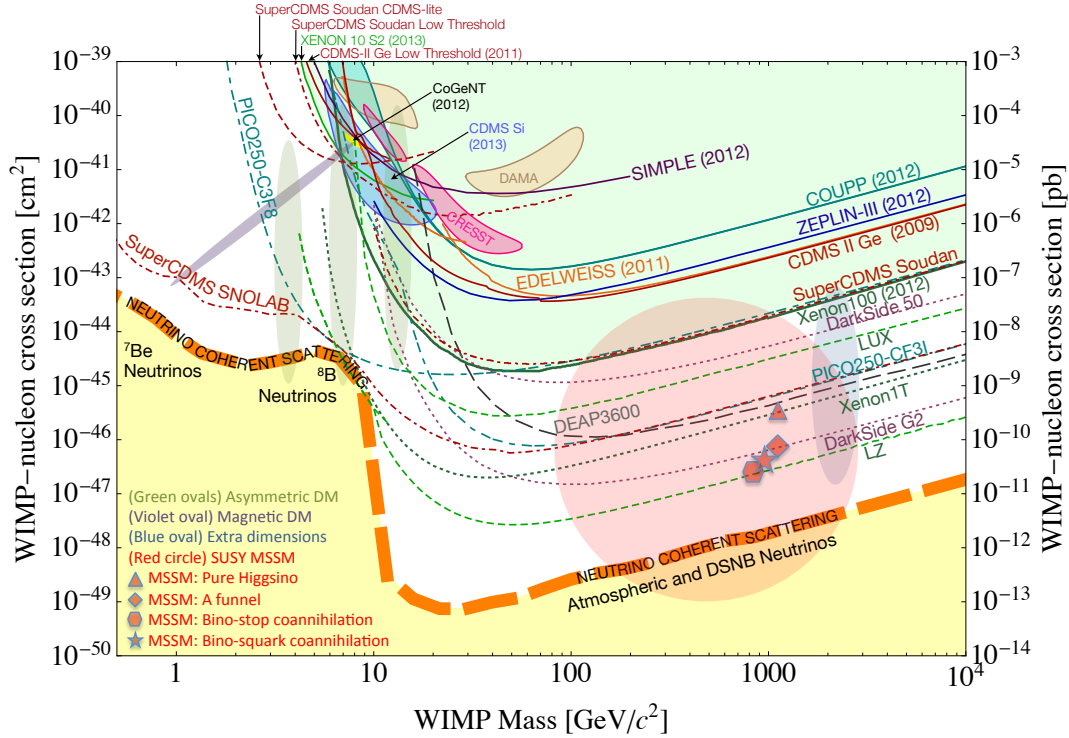


Figure 4-4. A compilation of WIMP-nucleon spin-independent cross section limits (solid curves), hints for WIMP signals (shaded closed contours) and projections (dot and dot-dashed curves) for U.S.-led direct detection experiments that are expected to operate over the next decade. Also shown is a band indicating the cross sections where WIMP experiments will be sensitive to backgrounds from solar, atmospheric, and diffuse supernovae neutrinos.

the solar neutrinos give way to the more energetic atmospheric neutrinos and diffuse supernovae background. The flux of these neutrinos is much lower, and exposures with sensitivities to WIMP-nucleon cross sections of $\sim 1 \times 10^{-48} \text{ cm}^2$ are required to be sensitive to this neutrino component. Depending on the particular WIMP mass under consideration, these neutrino backgrounds can have a recoil spectrum that is very similar to an authentic WIMP signal. Given the Poisson fluctuations from the neutrino signal and their relatively large total flux uncertainties, this creates a challenge to improving the sensitivity of WIMP searches much beyond such cross sections [39]. Figure 4-4 shows not only the current landscape, but also the projected sensitivities of proposed experiments superimposed on the neutrino background, where coherent neutrino scattering will begin to limit WIMP sensitivity. This will eventually require either background subtraction or techniques such as directional or annual modulation to press beyond this background in the absence of a positive WIMP sighting.

4.2 Indirect detection of WIMP dark matter

If thermal decoupling is the mechanism that sets the abundance of WIMP dark matter in the early Universe, WIMPs generically are predicted to annihilate today as well, especially in regions of high dark matter density, producing Standard Model particles, including gamma rays, neutrinos, electrons, positrons, protons, antiprotons, deuterons, and antideuterons. Utilizing these Standard Model “messengers” to constrain or infer properties of the dark matter is the ultimate goal of indirect detection.

The dark matter annihilation signal depends both on particle physics properties, such as the annihilation cross section, and on the density distribution of dark matter. The latter can be determined either by N -body simulations of structure formation in the Λ CDM standard model of cosmology or by direct dynamical measurements that derive the enclosed mass (luminous and dark) by measuring the velocity dispersion of stars or molecular clouds. In the absence of baryons, CDM simulations indicate that dark matter halos are cuspy (with densities approximately scaling as $\rho \sim r^{-1}$ in their centers [16, 40, 41]), with a central concentration dependent on the mass of the halo and the halo formation time [42, 43]. Small halos tend to form earlier than big halos and also to be much more densely concentrated. However, the places where we have the highest dark matter annihilation rates (*i.e.*, the centers of halos) are also the places where baryons settle and dominate the potential well for typical halos. There is not yet a consensus on how baryons affect dark matter halos [44, 45, 46, 47, 48]. Fortunately, even when one makes the most conservative assumptions about the halo profiles (*e.g.*, cored profiles constrained by dynamics), indirect detection methods still constrain the viable WIMP parameter space.

4.2.1 Gamma-ray experiments

The ability to detect the dark matter signal from a given target depends critically on its dark matter density distribution and on J , the integral of the square of the dark matter density along the line of sight to the source. The ideal targets for dark matter annihilation searches are those that have both a large value of J and relatively low astrophysical γ -ray foregrounds. These criteria have motivated a number of galactic and extragalactic targets including the galactic center (GC), dwarf spheroidal satellite galaxies of the Milky Way (dSphs), and galaxy clusters.

In and near the GC, the dark matter-induced γ -ray emission is expected to be so bright that one can obtain strong upper limits at the level of the target annihilation cross section $\langle\sigma_{\text{an}}v\rangle \sim 3 \times 10^{-26} \text{ cm}^3 \text{ s}^{-1}$, even after excluding regions around the bright source at the GC and surrounding region. With the improved angular resolution of future γ -ray experiments, the astrophysical foregrounds will be more easily identified and separated from the diffuse annihilation signal, and if, *e.g.*, the GC source is a point source, the dark matter sensitivity of future experiments could exceed the predictions presented here (see Fig. 4-5).

The Milky Way is also known to host at least two dozen dSphs that are typically very dark-matter-dominated, with mass-to-light ratios of 100 to 1000, and are thought to have very small background emission of high energy γ -rays. About half of the dSphs were discovered in the Sloan Digital Sky Survey (SDSS). A large number of additional dSphs are believed to exist in the Milky Way halo [49, 50]. Big, deep, wide-field surveys like the Dark Energy Survey and, critically, LSST, should find more. In particular, these experiments should help to find additional nearby galaxies in the southern hemisphere, since SDSS did not explore this area. These objects are of particular importance to the future southern CTA experiment that is described below.

The Fermi-LAT data already exclude the target annihilation cross section for masses below 30 GeV, but only for the most optimistic phenomenological minimal supersymmetric standard model (pMSSM) models.

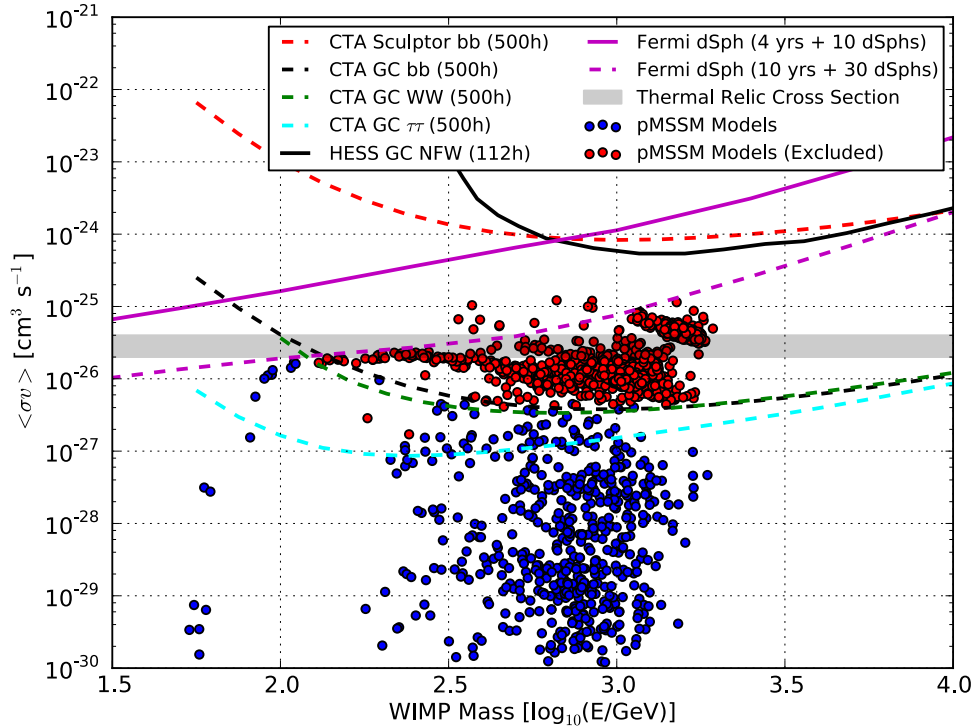


Figure 4-5. Constraints on the total annihilation cross section from γ -ray experiments, including the anticipated sensitivity of Fermi obtained by stacking 10 yrs of data on dSphs, and the sensitivity of the augmented CTA instrument, using an annulus about the GC.

In the low-energy, background-dominated regime, the Fermi-LAT point source sensitivity increases roughly as the square root of the integration time. However, in the high-energy, limited-background regime (where many pMSSM models predict signals), the Fermi-LAT sensitivity increases more linearly with integration time. Thus, 10 years of data could provide a factor of $\sqrt{5}$ to 5 increase in sensitivity.

VERITAS has produced some of the best dark matter constraints at energies above a few hundred GeV through observations of one of the nearest, most dark-matter-dominated dwarf galaxies, Segue 1 [51]. This analysis was based on about 40 hours of data, but the current VERITAS program, augmented by recent upgrades, aims to observe dark matter targets for several hundred hours each season. This offers one of the strongest avenues available for WIMP dark matter detection and, in the absence of discovery, can provide severe constraints on many dark matter models.

For the next generation experiment, improving dark matter searches in the GC depends on stronger astrophysical background rejection through improved angular resolution. CTA [52] is a future ground-based γ -ray observatory that will have potentially game-changing sensitivity over the energy range from a few tens of GeV to a few hundreds of TeV. Doubling the size of the proposed southern CTA telescope, a device with the required resolution and good sensitivity to the GC and southern sources, will dramatically improve dark matter sensitivity. To achieve the best sensitivity over this wide energy range, CTA will include three distinct telescope sizes. Over this energy range, the point-source sensitivity of CTA will be at least one order of magnitude better than current generation imaging atmospheric Cherenkov telescopes (IACTs). CTA

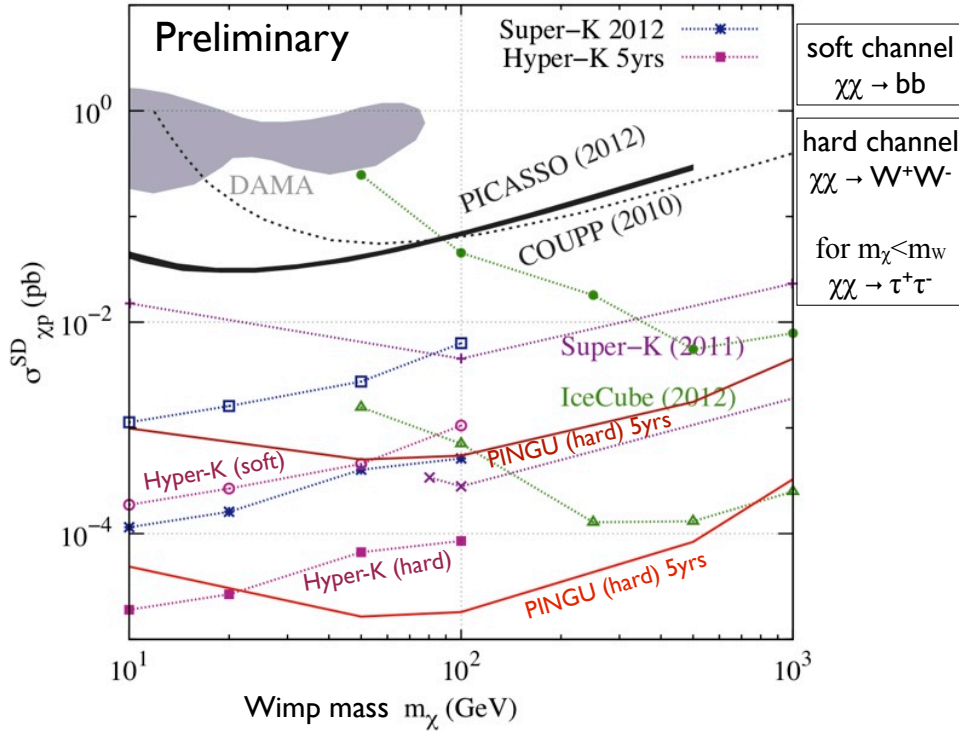


Figure 4-6. Existing constraints on the spin-dependent WIMP-proton scattering cross section from various experiments, along with projected future sensitivities for Hyper-Kamiokande and PINGU with five years of data.

will also have an angular resolution at least 2 to 3 times better. In the following sensitivity estimates, we consider an augmented version of the CTA array (ACTA) with a U.S. enhancement, which has twice as many medium-sized telescopes (~ 60 in total) and triple the sensitivity.

Figure 4-6 shows the projected sensitivity of ACTA to a WIMP particle annihilating through three possible final states: $b\bar{b}$, W^+W^- , and $\tau^+\tau^-$. For an observation of the GC utilizing a 0.3° – 1.0° annular search region, ACTA could exclude models with cross sections significantly below the target annihilation cross section. Overlaid in the figure are WIMP models generated in the pMSSM framework that satisfy all current experimental constraints from collider and direct detection searches. Approximately half of the models in this set could be excluded at the 95% confidence level in a 500-hour observation of the GC.

ACTA, with the critical U.S. enhancement, will provide a powerful new tool for searching for dark matter, covering parameter space not accessible to other techniques. ACTA will provide new information to help identify the particle nature of the dark matter and determine the halo profile. With support levels comparable to the complementary searches of the G2 direct detection experiments, the U.S. dark matter program would have the realistic prospect of both detecting dark matter in the lab and identifying it in the sky.

The HAWC detector will complement existing IACTs and the space-based γ -ray telescopes with its high-energy sensitivity and its large field-of-view. The instrument has peak sensitivity to annihilation photons from WIMP dark matter with masses between about 10 TeV and the unitarity limit ~ 100 TeV. Such large

masses can still satisfy all cosmological and particle physics constraints for a generic WIMP [53]. Much like Fermi, HAWC will survey the entire northern sky with sensitivity roughly comparable to existing IACTs and will search for annihilation from candidates that are not known *a priori*, such as galactic substructure. Furthermore, HAWC can search for sources of gamma rays that are extended by 10 degrees or more and can constrain even nearby sub-halos of dark matter.

4.2.2 Charged cosmic-ray/antimatter experiments

Dark matter may annihilate through a number of channels (*e.g.*, $t\bar{t}$, $b\bar{b}$, W^+W^- , ZZ , $\tau^+\tau^-$) with similar branching ratios. These annihilation channels can eventually lead to cascades producing secondary particles, such as $e^\pm$, γ , and ν . Annihilation can also lead to the production of baryons, such as cosmic-ray protons and antiprotons and even deuterons and antideuterons.

Cosmic-ray electrons and positrons provide a unique astrophysical window into our local galaxy. To a good approximation, the magnetic fields in the galaxy randomize cosmic-ray directions. However, a small anisotropy may remain due to contributions from local sources, such as dark matter subhalos or nearby pulsars. The identification of a dark matter signal from cosmic rays thus requires the detection of a spectral feature that stands out against the background.

Recent measurement of cosmic-ray positrons by the AMS-02 magnetic spectrometer [54] confirms with excellent precision earlier measurements by PAMELA [55] and Fermi [56] showing a rising positron fraction, $e^+/(e^+ + e^-)$, for energies between 10 and several 100 GeV [56, 55]. The most widely used models of propagation of cosmic rays in the galaxy [57] predict that secondary positrons would give a positron fraction falling well below the observed value. The rising positron fraction could be indicative of positrons created in the decay or annihilation of dark matter [58, 59]. Under this interpretation, the dark matter particles must have mass greater than 350 GeV. AMS-02 is capable of identifying cosmic-ray positrons with energies up to ~ 1 TeV. With several more years of data, AMS-02 should be able to extend its determination of the positron fraction to energies close to 1 TeV and add important information on cosmic-ray propagation. However, the possibility of astrophysical sources of primary positrons remains a source of great concern and makes it difficult to clearly attribute the excess positrons to dark matter annihilation [60, 61].

About a decade ago it was pointed out that antideuterons produced in WIMP-WIMP annihilations offered a potentially attractive signature for CDM [62]. The General Antiparticle Spectrometer (GAPS) detector [63] would consist of a detector that identifies antideuterons using a number of planes of Si(Li) solid state detectors and a surrounding time-of-flight system. A long-duration balloon flight of the proposed GAPS instrument could provide upper limits competitive with those obtained by AMS-02, and would provide two independent approaches to making this important measurement.

4.2.3 Neutrino measurements

Like their γ -ray counterparts, neutrino telescopes can potentially detect the products of WIMP annihilations pointing back to their origin in the GC, the galactic halo, from galaxy clusters, and from dSphs. Neutrino telescopes are also sensitive to WIMP annihilations in the core of the Earth or Sun, regions that are inaccessible to γ -ray telescopes. The WIMP source in the Sun has built up over solar time, averaging over the galactic dark matter distribution as those WIMPs that scattered elastically with solar nuclei and lost enough momentum became gravitationally trapped. Typically a sufficient density of WIMPs has accumulated in the

solar core that equilibrium now exists between WIMP capture and annihilation. Then, given a WIMP mass and decay branching ratios, one can unambiguously predict the signal for a neutrino telescope. The IceCube Collaboration continues to take data and has performed or is performing searches for neutrino signals from WIMPs in the center of the Earth [64], the solar core [37], the galactic halo [65] and center [66], galaxy clusters, and dSphs [67].

The PINGU detector is proposed as a new in-fill array for IceCube. PINGU will instrument an effective volume of several million metric tons for neutrinos with energy $E_\nu \sim 5\text{--}15$ GeV. The final geometry for the detector is still under study but will probably be comprised of 20 to 40 new strings with 60–100 modules per string. PINGU is focused on other physics goals but has sensitivity to neutrinos produced by WIMP annihilations. Together IceCube and PINGU will be able to probe a WIMP mass region that is of considerable interest, given intriguing results from other experiments that are consistent with a WIMP mass of a few GeV. In particular, WIMP properties motivated by DAMA’s annual modulation signal [68] and isospin-violating scenarios [69] motivated by DAMA and CoGeNT signals will be tested. The predicted sensitivities of Hyper-Kamiokande and PINGU are shown in Fig. 4-6. Depending on the WIMP mass, IceCube or PINGU could detect a smoking-gun signal of dark matter (a high-energy neutrino signal from the Sun) as well as place competitive limits on the spin-dependent nuclear recoil cross section, compared with planned G2 direct detection experiments.

4.2.4 Astrophysical multiwavelength constraints

Pair annihilation of WIMPs will result in a conspicuous non-thermal population of energetic electrons and positrons from the decays of charged pions produced by the hadronization of strongly interacting final-state particles, as well as from the decays of gauge bosons, Higgs bosons, and charged leptons. This non-thermal $e^\pm$ population loses energy and produces secondary radiation through several processes, covering a wide range of the electromagnetic spectrum from the radio to the γ -ray band. Several recent studies have made it clear that searches in the radio [70] and X-ray frequencies [71] have the potential to reach sensitivities to the relevant WIMP parameter space that are comparable to, and in some instances broader than and complementary to, the sensitivities of γ -ray experiments. A detailed performance comparison depends critically on assumptions about propagation, energy losses, and the astrophysical environment where the secondary radiation is emitted [70]. An accurate definition of benchmarks is crucial for this field.

4.2.5 Indirect detection conclusions

The primary methods of indirect detection include γ -ray measurements of the center of our own galactic halo, in nearby dwarf galaxies and in clusters; observation of charged cosmic-ray antimatter; searches for high energy neutrinos from annihilation in the Sun; and astrophysical signatures, such as radio and hard X-ray emission. Just as the next generation of direct detection experiments is approaching the natural range for WIMP scattering cross sections, γ -ray experiments are reaching the sensitivity required to exclude the natural range of model predictions for WIMP annihilation cross sections. Given the close relation of the γ -ray production cross section to the total annihilation cross section, the bulk of the likely parameter space for SUSY WIMPs (or, in fact, any weakly interacting massive thermal relic) is focused in a relatively narrow band of cross sections compared to the nuclear recoil scattering cross section. Comparing direct and indirect detection, the dominant systematic uncertainty for direct detection from the nuclear recoil cross section might be even larger than the uncertainties in indirect searches from halo models. Even the conservative, cored halos give an observable signal for future γ -ray experiments.

A future experiment like CTA, with U.S. enhancement, could reach most of the parameter space in well-explored theoretical frameworks, such as the pMSSM, through observations of the GC for all but the most pessimistic assumptions about the halo profile or new astrophysical backgrounds. U.S. involvement in CTA is critical. This involvement would result in a doubling of the planned mid-sized telescope array with substantial improvements in sensitivity and angular resolution that are much more than an incremental improvement. Perhaps of equal importance, involvement of the U.S. HEP community would provide not only the technical expertise but also the scientific impetus to devote a large fraction of CTA's observing time to dark matter targets.

New discoveries of nearby dwarf galaxies and new analysis techniques are likely to result in Fermi observations cutting into the pMSSM parameter space to energies up to tens of GeV. Above a few hundred GeV, VERITAS will come within an order of magnitude of the natural annihilation cross section for a combined dwarf analysis. IceCube searches for high-energy neutrinos from the Sun will continue to provide some of the most sensitive constraints on the spin-dependent scattering cross section and offer the potential for a smoking-gun discovery. The proposed PINGU extension to IceCube would provide sensitivity to WIMP masses favored by DAMA.

This is an exciting time for dark matter research, where direct detection experiments may see a hint of a signal, the LHC may find new physics, γ -ray measurements may identify the particle and measure its distribution in galactic halos, and neutrino measurements may provide a smoking-gun detection from the annihilation signal from the Sun. Without all of these avenues for research, the story would be incomplete.

4.3 Non-WIMP dark matter

In solving the mystery of dark matter, it is sensible to assume the properties of the dark matter particle candidate and predict its non-gravitational interactions before the particle can be identified. It is common to predict the properties of dark matter based on either compelling theoretical arguments or experimental and astrophysical hints. The relative significance of different arguments and hints cannot easily be evaluated objectively, which makes it difficult to rank dark matter candidates in importance. Although a combination of certain theoretical arguments, advanced direct search experimental techniques, and connection with collider experiments makes WIMPs attractive candidates, they are by no means the only appealing possibility.

Furthermore, there is no reason to believe that dark matter is comprised of only one type of particle. It is possible that the structure of the dark sector is as complex as that of the visible sector, or even more complex. A broad, extensive, and multifaceted approach to solving the mystery of dark matter is more likely to yield exciting discoveries than a focused pursuit of one dark matter candidate. At the same time, the enormous breadth of possibilities forces us to identify some directions as the most promising. Theoretical plausibility and experimental feasibility are commonly used in the community for this purpose.

The landscape of non-WIMP candidates is sketched in Fig. 4-7. There are a large number of possibilities. To be directly detected, the dark matter needs to interact in some way with normal matter and radiation. Figure 4-8 shows the incredible range of interaction strengths and masses of the different candidates. Generally, the lighter the candidate, the more feeble its allowed interactions. Very heavy dark matter particles can have quite strong interactions. This wide range of coupling strengths and masses calls for an equally wide range of detector technologies. For the most feeble interactions of the axion, the detection technology is based on quantum-limited detection of radio photons. The rare interactions of the more massive candidates, such as Q-balls, produce distinctive signatures in large detectors. Finally, a number of candidates cannot be found in laboratory experiments in the foreseeable future, and so X-ray and gamma-ray telescopes, as well as gravitational lensing techniques, provide the best opportunities for their detection.

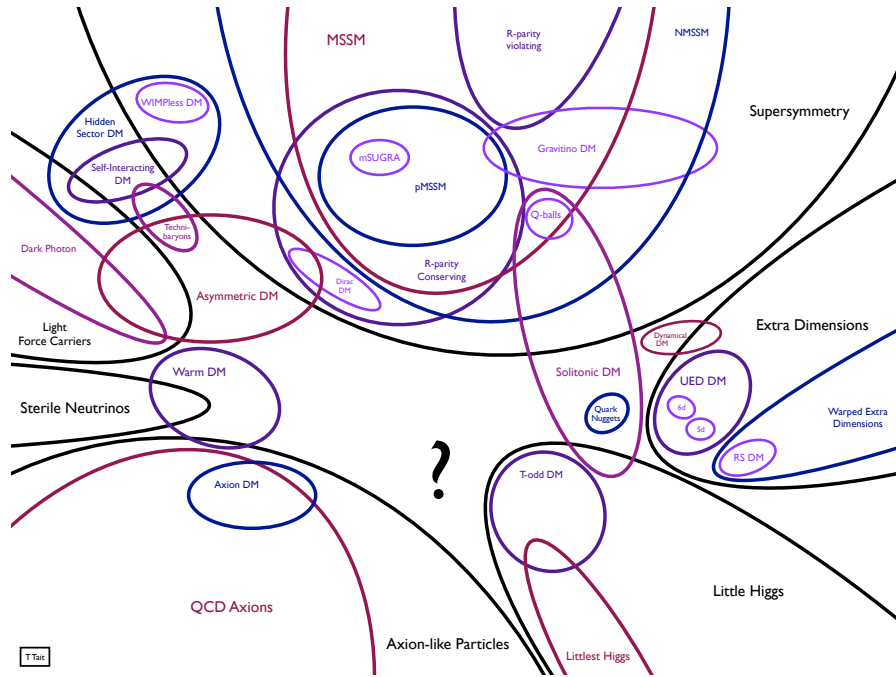


Figure 4-7. The landscape of dark matter candidates [from T. Tait].

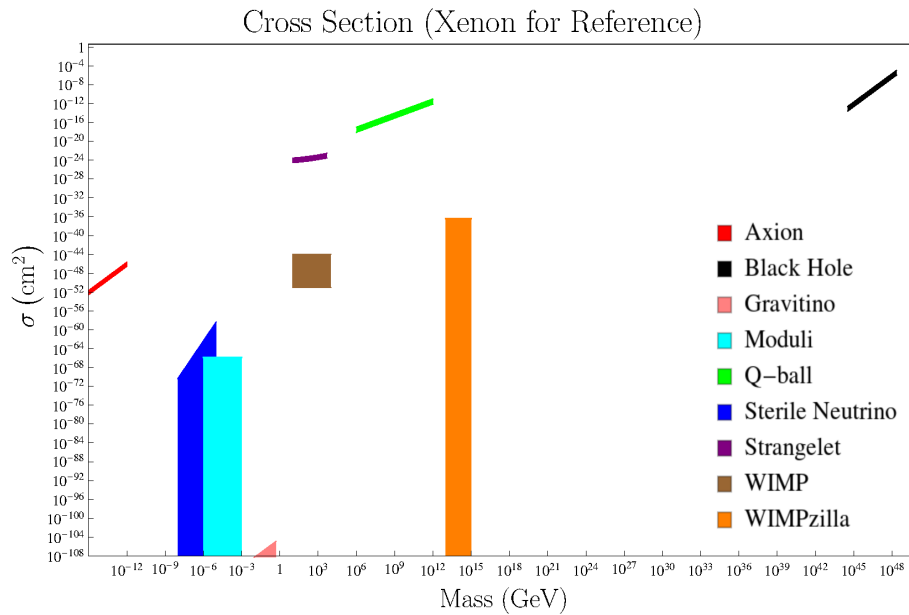


Figure 4-8. The range of dark matter candidates' masses and interaction cross sections with a nucleus of Xe (for illustrative purposes) compiled by L. Pearce. Dark matter candidates have an enormous range of possible masses and interaction cross sections.

The following non-WIMP dark matter candidates were highlighted in the course of Snowmass. This list is not a complete list of possible candidates, but it includes candidates for which there was substantial interest and identifiable experimental and theoretical research paths, and which were discussed in white paper contributions.

- **Asymmetric dark matter** is motivated by the fact that the abundances of ordinary matter and dark matter in the Universe are comparable [72, 73]. The two quantities can be similar and related if the two types of matter are produced simultaneously, or if they are generated from two separate but similar processes in the early Universe. Since the abundance of ordinary matter is controlled by the baryon asymmetry of the Universe, a similar asymmetry may be responsible for the observed dark matter abundance. In contrast with WIMPs, asymmetric dark matter candidates can have masses and interaction cross sections that are very different from the naive expectations motivated by the freezeout of a thermal relic.
- **Axions** arise from an elegant (and, arguably, the only viable) solution to the strong CP problem in the Standard Model [74, 75, 76, 77]. The vacuum of quantum chromodynamics (QCD) is characterized by a parameter θ_{QCD} , which is independent of the phase θ_q in the quark mass matrix. It is a mystery why the sum of these two unrelated parameters, which controls CP violation in strong interactions, should vanish to an experimentally-measured precision of one part in ten billion. Peccei–Quinn theory explains this mystery and predicts a new light degree of freedom, the axion, which can also be dark matter. There are several promising experimental techniques to search for axions, and this program was viewed to have a strong discovery potential. A variant candidate is the axion-like-particle, a light particle with the axion’s quantum numbers, but having mass and couplings not linked by the Peccei–Quinn theory. Searches for such particles were discussed by both Cosmic Frontier and Intensity Frontier working groups.
- **Primordial black holes** can form in the early Universe [78, 79, 80], for example, from the same dynamics that govern cosmological inflation [81]. Although theoretical models invoke fine-tuning of parameters to explain the dark matter abundance, primordial black holes are a viable dark matter candidate that can be discovered using gravitational lensing observations [82].
- **Self-interacting dark matter** can explain some inconsistencies between the predictions of N -body simulations and observations [83, 84, 85, 86, 87].
- **Sterile neutrinos** are motivated by the fact that active neutrinos are massive, which is typically explained by introducing right-handed, gauge-singlet fermions [88, 89]. If one or more of these fermions is relatively light, as occurs in a number of different models [90, 91, 92, 93], the dark matter can consist of sterile neutrinos. In a part of the parameter space allowed for dark matter, the same sterile neutrinos can explain the observed pulsar velocities, since these sterile neutrinos would be emitted anisotropically from a cooling neutron star born in a supernova explosion [94, 95]. The most promising detection strategy is based on the radiative decays of sterile neutrinos, which can produce a line detectable by X-ray telescopes [96].
- **Superheavy dark matter** must have a low number density, which requires the use of very large detectors. Indirect detection of decay products can lead to discovery. Several candidates fall in this category, including quark nuggets [97, 98] and WIMPzillas [99].
- **Supersymmetric non-WIMP candidates**, including SUSY Q-balls [100, 101] and products of their decays, as well as superWIMP dark matter [102] are well-motivated dark matter candidates in theories with supersymmetry. The search for these candidates is possible using direct and indirect techniques targeting specific properties of these candidates.

Among all these candidates, the axion deserves special mention. For a dark matter candidate, the QCD axion has the rare advantage of a fairly well-bounded parameter space. Although there are ways to evade the bounds, the axion-photon couplings $g_{a\gamma\gamma}$ over the range of benchmark models extend over an order of magnitude. The upper end of the QCD axion mass range is set at a few meV by the limit from SN1987A, and the lower end, limited by the requirement that axions not overclose the Universe, is set at around a μeV . A particularly promising approach to detect the QCD axion is via the RF-cavity technique [103]. Although the expected conversion into RF power within the cavity is extraordinarily weak, experiments will shortly start taking data for a definitive search, which will either find the QCD dark matter axion with high confidence, or exclude it at high confidence. These experiments will sensitively explore the first two decades of allowed QCD axion mass, where the dark matter QCD axion is expected to be found. These searches have a large discovery potential over the next decade. There are axion and axion-like-particle alternatives to the QCD axion, and this opens a vast and largely unexplored search space. Much of this space, including the third decade of allowed QCD axion mass, could be explored by large next-generation detectors. For instance, the proposed solar-axion experiment IAXO would explore this space, including where there are astrophysical hints of new physics, yielding a good discovery potential. The axion might be found anywhere within the allowed parameter space, and there are arguments for both higher-mass axions (which would then be of the non-Peccei-Quinn type) or lower-mass axions (which could be QCD axions whose primordial abundance is explained by anthropic selection).

It may also be that the dark matter consists of several different particles. The range of possibilities is enormous. Aside from axions, proposals for special-purpose non-WIMP dark matter detectors and programs are less well-developed. However, existing experiments and astronomical instruments offer a number of serendipitous opportunities.

Once the axion or other dark matter particle is identified, it will mark a new beginning. For instance, one virtue of the RF-cavity experiments is that they measure the total energy of the axion — mass plus kinetic energy — and there may be fine structure to the signal due to the flows of dark matter in the halo. This contains a wealth of information about the history of the formation of our Milky Way galaxy and will mark the beginning of a new field of astronomy [104]. On the other hand, if dark matter is made up of sterile neutrinos, the narrow spectral line from their decay could provide information about the redshift, allowing us to map out dark matter in the Universe and to use the redshift information for measuring the cosmological expansion [89]. Much the same can be said about discovering any of the dark matter candidates: The identification of dark matter will be a revolutionary discovery that will open the door to a new chapter in our understanding of nature.

4.4 Dark matter complementarity

All current evidence for dark matter is derived solely through its gravitational interactions. The presence of dark matter has been quantified on length scales that range from the solar neighborhood to the horizon of the Universe. The history extends back to Jan Oort, who proposed using stars above the plane of the Milky Way as a way to minimize the influence of the stellar disk [105], and Fritz Zwicky who postulated dark matter in 1937 from the large measured velocity dispersion of galaxies in the Coma cluster [106]. Subsequent progress in the 1970's in measuring the rotation curves of nearby galaxies in both optical and radio frequencies [107, 108] really brought the idea of dark matter home to many astronomers and physicists. The support for dark matter in the form of cold relic particles came about through revolutions in cosmology including galaxy correlations [109, 110], the CMB [111, 112], and measurements of hot gas in clusters [113]. Visceral evidence for dark matter is provided by strong and weak lensing measurements [114, 115], including the famous Bullet Cluster [116]. Together, these data provide overwhelming evidence that the energy in

dark matter is roughly a quarter of the total energy in the visible Universe and about five times the energy in normal matter.

To understand dark matter, we need to answer some basic questions: (1) How many particle species make up the dark matter? (2) What are their masses and spins? (3) How do they couple to the Standard Model and other new (dark sector) particles? Our primary conclusion is that the broad range of possibilities for dark matter (more than one of which may be correct) argues strongly for multiple search strategies, including direct searches, indirect searches, collider searches, and astrophysical probes, to answer these basic questions.

The Dark Matter Complementarity working group highlighted the essential synergies of these different approaches. These approaches, as well as the complementarity of experiments within each approach, are discussed in greater depth above in Secs. 4.1, 4.2, and 4.3, and in the reports of the Cosmic Frontier dark matter working groups and the Energy Frontier new physics working group.

4.4.1 Dark matter candidates and the need for a multi-pronged search strategy

Several classes of particles are strong dark matter candidates. The most familiar dark matter candidates are WIMPs, discussed in Secs. 4.1 and 4.2, which are produced in the hot early Universe and then annihilate in pairs. Those that survive to the present are known as “thermal relics” [117, 118, 119, 120]. Such particles are generically predicted in models of physics beyond the Standard Model, including models with supersymmetry [121, 122] or extra spatial dimensions [123, 124]. If these particles interact through the weak interactions of the Standard Model, the resulting thermal relic density is close to the observed dark matter density. This coincidence, known as the “WIMP miracle,” provides strong motivation for dark matter with masses up to about a TeV.

As discussed in Sec. 4.3, however, there are also many alternative scenarios. For example, in the case of asymmetric dark matter [125, 126, 127, 128, 129], there is a slight excess of dark particles over dark antiparticles in the early Universe. These annihilate until only the slight excess of dark particles remains. In many models, the dark matter asymmetry is related to the normal matter–antimatter asymmetry, and one expects the number of dark matter particles to be similar to the number of protons, implying a dark matter particle mass of ~ 10 GeV, assuming this particle is the dominant component of dark matter. Asymmetric or thermal relic dark matter may be in a so-called hidden sector, which has its own set of matter particles and forces, through which the dark matter interacts with other currently unknown particles.

The non-gravitational interactions of the above dark matter candidates may be with any of the known particles or, as noted above for hidden sector dark matter, with other currently unknown particles. A complete research program in dark matter therefore requires a diverse set of experiments that together probe all possible types of couplings, as shown in Fig. 4-9. At a qualitative level, the complementarity may be illustrated by the following observations that follow from basic features of each approach.

- **Direct Detection.** Prime examples are the detection of WIMPs through scattering off nuclei and the detection of axions in RF cavities. The methods relying on scattering off nucleons are relatively insensitive to dark matter that couples to leptons only, or to WIMP-like dark matter with mass ~ 1 GeV or below, while they are powerful for dark matter that couples to charged particles or gluons.
- **Indirect Detection.** When pairs of dark matter particles annihilate, they produce high-energy particles in the cosmic rays. For example, antimatter from local annihilation events can be found by AMS-02, neutrinos from annihilations in the Sun can be detected at IceCube, and photons from annihilations at the GC or in other galaxies can be seen by γ -ray telescopes. Alternatively, dark matter

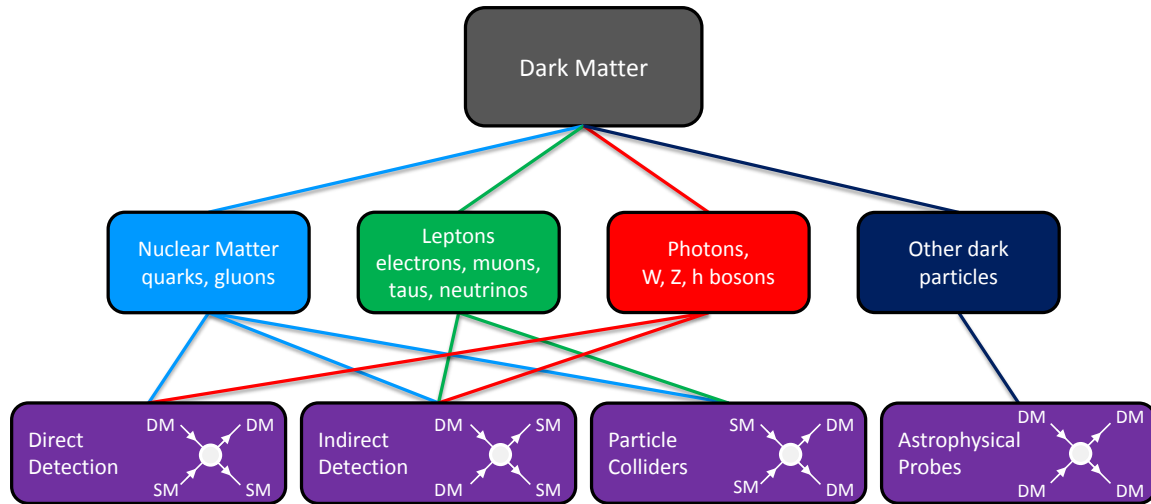


Figure 4-9. Dark matter may have non-gravitational interactions with one or more of four categories of particles: nuclear matter, leptons, photons and other bosons, and other dark particles. These interactions may then be probed by four complementary approaches: direct detection, indirect detection, particle colliders, and astrophysical probes. The lines connect the experimental approaches with the categories of particles that they most stringently probe. The diagrams give example reactions of dark matter (DM) with Standard Model particles (SM) for each experimental approach. From Ref. [130].

may be metastable, and its decay may produce the same high-energy particles. These indirect detection experiments (together) are sensitive to the interactions with all Standard Model particles and probe the annihilation process suggested by the WIMP miracle. Experimental sensitivities are expected to improve greatly on several fronts in the coming decade but some modes require good understanding of astrophysical backgrounds. Further, the signals are typically subject to uncertainties in the spatial distribution of dark matter (which is often not directly constrained) and may be absent altogether whenever the dark matter annihilation is insignificant now, *e.g.*, in the case of asymmetric dark matter or *P*-wave suppressed annihilation.

- **Particle Colliders.** Particle colliders, such as the Large Hadron Collider (LHC) and proposed future lepton colliders, produce dark matter particles that escape the detector, but are discovered as an excess of events with missing energy or momentum. LHC experiments are sensitive to the broad range of masses favored for WIMPs (especially if they couple to quarks and/or gluons), but are relatively insensitive to dark matter that interacts only with leptons. Collider experiments are also unable to distinguish missing momentum signals produced by a particle with lifetime ~ 100 ns from one with lifetime above 10^{17} s, as required for dark matter.
- **Astrophysical Probes.** The particle properties of dark matter are constrained through its impact on astrophysical observables. Dark matter distributions and substructure in galaxies are unique probes of the “warmth” of dark matter and hidden dark matter properties, such as its self-interaction strength, and they measure the effects of dark matter properties on structure formation in the Universe. Examples include the self-interaction of dark matter particles affecting central dark matter densities in galaxies (inferred from rotation velocity or velocity dispersion measures), the mass of the dark matter particle affecting dark matter substructure in galaxies (inferred from strong lensing data), and the annihilation of dark matter in the early Universe affecting CMB fluctuations. Astrophysical probes

are typically unable to distinguish various forms of CDM from one another or make other precision measurements of the particle properties of dark matter.

4.4.2 Discovery complementarity

We illustrate the qualitative features outlined above in three ways. First, we consider a fairly model-independent setting by considering dark matter that interacts with Standard Model particles through four-particle contact operators. These interactions are expected to work well to describe theories in which the exchanged particle mass is considerably larger than the momentum transfer of the physical process of interest. To illustrate aspects of the complementarity of the different searches, we assume the dark matter is a spin-1/2 particle that couples in a generation-independent way to the Standard Model leptons (effective operators involving quarks or gluons were also considered in Ref. [130]). The relevant piece of the Lagrangian is $M_\ell^{-2} \bar{\chi} \gamma^\mu \chi \sum_\ell \bar{\ell} \gamma_\mu \ell$, where M_ℓ is the scale of the new physics. The complementarity of different experimental approaches in probing low masses (≤ 10 GeV), moderate masses, and high masses (> 300 GeV) is evident in the top left panel of Fig. 4-10, which shows dark matter discovery prospects in the mass vs. annihilation cross section plane for current and future direct detection experiments [131], indirect detection experiments [132, 133], and particle colliders [134, 135] for dark matter coupling to leptons. A discovery in the yellow region of this plot (below the dot-dashed line) would indicate that the dark matter (if assumed to be a thermal relic) has other significant annihilation channels that are waiting to be discovered.

The interactions with quarks and gluons are here illustrated through a concrete example of a complete supersymmetric model. The dark matter candidate is the lightest neutralino $\tilde{\chi}_1^0$, which is its own antiparticle. Even within the general framework of supersymmetry, there are many different model scenarios, distinguished by a number of input parameters (~ 20). A model-independent approach to supersymmetry is to scan over all those input parameters and consider models that pass all existing experimental constraints and have a dark matter candidate that could explain at least a portion of the observed dark matter density [136]. Results from such model-independent scans with over 200,000 points are shown in the top right panel of Fig. 4-10, where each dot represents one particular supersymmetric model and its color indicates whether the model is observable in future direct searches (green), indirect searches (blue), or in both (red). The models that escape both direct and indirect searches, but are discoverable with current LHC data, are shown in magenta. The top right panel of Fig. 4-10 demonstrates that these three different dark matter probes combine to discover a large fraction of the supersymmetry models in this scan.

The bottom panel of Fig. 4-10 is for an asymmetric dark matter candidate that is charged under a hidden sector broken U(1) that has kinetic mixing with the Standard Model photon. The broken U(1)'s fine-structure constant is taken to be 10^{-2} for illustration and the hidden sector gauge boson's mass is in the 1–100 MeV range. Bounds from supernova cooling and beam dump experiments restrict the value of the kinetic mixing parameter to be below about $\epsilon = 10^{-10}$. For this mixing parameter, the gauge boson also safely decays before big bang nucleosynthesis. The dark matter self-interaction cross section in the blue shaded region is large enough (0.2 to 20 barns/GeV) to affect dwarf galaxies on observable scales. The effect is to lower the densities (see, *e.g.*, Ref. [137]) and create constant density cores (see, *e.g.*, Refs. [138, 139]) in a manner that is preferred by observations. Also shown are bounds from Bullet Cluster observations and requirements that self-interactions not make galactic halos too round; consistency with observations excludes the parameter space above the indicated contours. Direct detection is sensitive to a large region of parameter space that is preferred by astrophysical observations as shown by the contour for XENON1T sensitivity. We see that astrophysical observables probe low masses, while direct detection experiments probe mid-range and high masses, illustrating the complementarity of these two approaches in this asymmetric dark matter framework. The same model also allows for symmetric dark matter that freezes out with the right relic density (like

WIMPs), where the qualitative complementarity features are the same but with further implications for indirect searches.

4.4.3 Post-discovery complementarity

As important as a broad program of complementary searches is to establishing a compelling signal for dark matter, for several reasons it becomes even more important after a signal has been reported.

First, as is well known, many tentative dark matter signals have already been reported. The potential identification of a quarter of the Universe will require extraordinary proof in the form of verification by other experiments. Second, each search strategy has its limitations, as noted earlier in Sec. 4.4.1. Last, the discovery of dark matter will usher in a rich and decades-long program of dark matter studies, requiring a multi-pronged approach.

Consider the following scenario: Direct search experiments see evidence for a 100 GeV WIMP, and missing energy searches at an upgraded LHC also see a consistent signal. However, further LHC and ILC studies constrain the neutralino's predicted thermal relic density to be half of Ω_{DM} , implying that it is not a thermal relic, or that it makes up only half of the dark matter. The puzzle is resolved when axion detectors discover a signal that is consistent with axions making up the rest of the dark matter. Progress in astrophysical theory, simulations, and observations eventually leads to a consistent picture with dark matter composed entirely of CDM and extends our understanding of the Universe back to 1 ns after the big bang. Direct and indirect detection rates are then used to constrain the local dark matter density, halo profiles, and substructure, establishing the new fields of neutralino and axion astronomy. We see from the above example that even for well-studied dark matter candidates, information from multiple approaches is required to fully understand the dark matter. This is even more true if the dark matter sector is richer than that imagined above.

A balanced program with components in each of the four approaches is required to cover the many well-motivated dark matter possibilities, and their interplay will likely be essential to realize the full potential of upcoming discoveries.

4.5 Dark energy and CMB

Maps of the Universe when it was 400,000 years old, from observations of the CMB and over the last ten billion years from galaxy surveys, point to a compelling cosmological model. This model requires a very early epoch of accelerated expansion, known as inflation, during which the seeds of structure were planted via quantum mechanical fluctuations. These seeds began to grow via gravitational instability during the epoch in which dark matter dominated the energy density of the Universe, transforming small perturbations laid down during inflation into nonlinear structures such as million-light-year-sized clusters, galaxies, stars, planets, and people. Over the past few billion years, we have entered a new phase, during which the expansion of the Universe is accelerating, presumably driven by yet another substance, dark energy.

Cosmologists have historically turned to fundamental physics to understand the early Universe, successfully explaining phenomena as diverse as the formation of the light elements, the process of electron-positron annihilation, and the production of cosmic neutrinos. However, the Standard Model of particle physics has no obvious candidates for inflation, dark matter, and dark energy. The amplitude of the perturbations

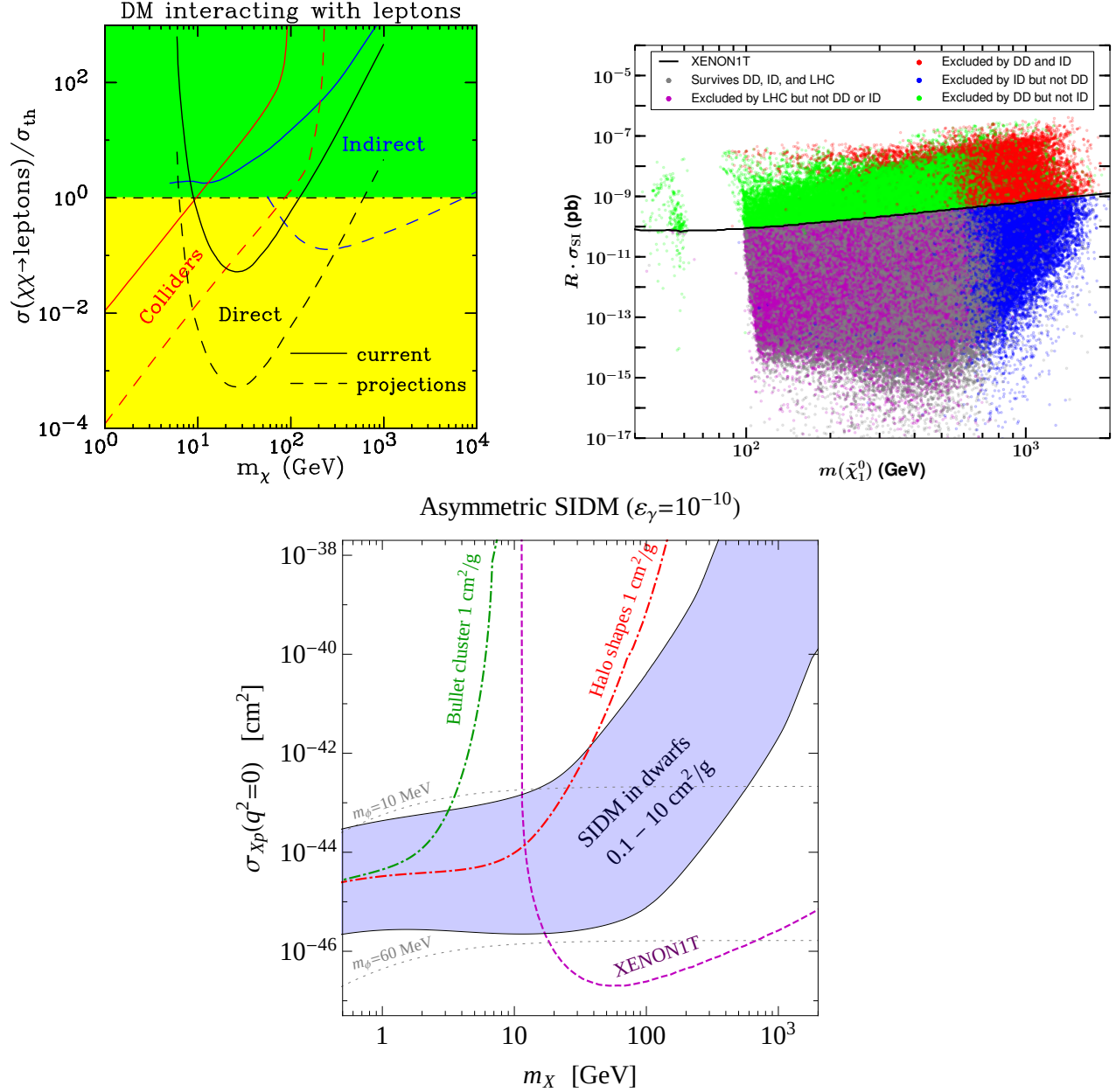


Figure 4-10. Top left: Dark matter discovery prospects in the mass vs annihilation cross section plane in the effective operator framework for dark matter coupling to leptons [130]. Top right: A model-independent scan of the full parameter space in the minimal supersymmetric model (MSSM), presented in the mass vs. dark matter-nucleon spin-independent cross section plane [140]. Bottom: Prospects for direct detection of asymmetric dark matter that couples to quarks via gauge kinetic mixing with $\epsilon = 10^{-10}$, showing complementarity with astrophysics in the mass vs. dark matter-nucleon spin-independent cross section plane [141].

suggests that the natural scale for inflation is at ultra-high energies¹, so understanding the physics driving inflation could lead to information about the ultraviolet completions of our current theories. There are arguments that naturally link the dominant dark matter component to new physics hovering above the electroweak scale; the powerful experiments aiming to find this component have been discussed above in Secs. 4.1, 4.2, 4.3, and 4.4. Apart from the dominant component, neutrino oscillation experiments already inform us that neutrinos constitute a non-negligible fraction of the dark matter. One of the key points of this chapter is that experiments usually associated with dark energy and inflation are ideally suited to pin down the sum of the masses of the neutrinos and the cosmic existence of any additional (sterile) species.

The situation with dark energy is more complex. A cosmological constant (Λ) has effective pressure and energy density related by $p = -\rho$ (equation of state $w = -1$), consistent with preliminary measurements, but in supersymmetric theories, for example, the most natural scale for Λ is at least 100 GeV. A cosmological constant with this value would produce a Universe accelerating so rapidly that the tips of our noses would be expanding away from our faces at a tenth the speed of light. If Λ is responsible for the current epoch of acceleration, its value is many orders of magnitude smaller than this, but curiously just large enough that it began dominating the energy density of the Universe only recently. The mechanism driving the current accelerated expansion of the Universe remains a profound mystery.

The quest to understand dark energy, dark matter, and inflation is, then, driven by a fundamental tension between the extraordinary success of the model that explains our Universe and the failure of the Standard Model of particle physics to provide suitable candidates for the dark sector that is so essential to our current view of the Universe. Experiments on the Cosmic Frontier have demonstrated that the Standard Model is incomplete; the next generation of experiments can provide the clues that will help identify the new physics required.

4.5.1 Dark energy

Physicists have proposed a number of different mechanisms that could be responsible for the accelerated expansion of the Universe. None is compelling, but some of them have been predictive enough to fail, while others have led to a deeper understanding of field theory and gravity. Apart from the cosmological constant solution itself, all are predicated on the assumption that there is some (unknown) mechanism that sets Λ to zero.

One possibility is that there is a previously undiscovered substance that contributes to the energy density of the Universe in such a way that the expansion accelerates. In the context of GR, acceleration (the positive second derivative of the scale factor a) is governed by Einstein's equations, which reduce to

$$\frac{\ddot{a}}{a} = -\frac{4\pi G}{3} [\rho + 3P] , \quad (4.1)$$

where G is Newton's constant, ρ the energy density, and P the pressure. The substance that drives acceleration, therefore, must have negative pressure, or equation of state $w \equiv P/\rho < -1/3$. A nearly homogeneous scalar field whose potential energy dominates over its kinetic energy satisfies this requirement, leading to *quintessence* models with potentials designed to fit the data. In almost all viable models, however, the mass of the field must be less than the Hubble scale today, of order 10^{-33} eV. Embedding such a field in some extension of the Standard Model therefore is challenging, as one would expect the scalar mass to get loop corrections many orders of magnitude larger than this. This mass stability problem is an indication

¹Roughly, the measured amplitude of the density perturbations $\delta\rho/\rho \simeq 10^{-5} \sim (E_{\text{inf}}/m_{\text{Planck}})^2/\sqrt{\epsilon}$, where $\epsilon \simeq 0.01$ is a small parameter in slow roll inflation.

of just how hard the problem is: The mass that is protected is some 44 orders of magnitude smaller than the Higgs mass that lies at the center of the electroweak hierarchy problem. It seems clear that the new physics is an infrared phenomenon, as opposed to all prior hints of new physics, which have entered from the ultraviolet.

With no real guidance from theory, quintessence models are nevertheless appealing because they open up the parameter space: Most models have $w \neq -1$, and many have evolving equations of state so that dw/da is also non-zero. This class of models therefore offers a clear, and arguably more appealing, alternative to the cosmological constant that would be favored if surveys find a deviation from $w = -1$. Although the quintessence field does not clump on scales smaller than its (very large) Compton wavelength, it should clump on the largest scales. This is yet another difference from the cosmological constant model. Finally, some models allow for episodic dark energy domination, so that the present accelerating era is not particularly special. Indeed, an early epoch of inflation is one such epoch, but there may have been others, for example, during phase transitions. Measuring the effects of dark energy in a series of redshift bins is therefore necessary to distinguish among the many possibilities.² This is an area where the CMB, whose lensing maps are sensitive to structure from redshift $z = 10^3$ until today, can be profitably combined with galaxy surveys, whose lensing maps probe structure at a sequence of lower redshifts.

Quintessence models explicitly introduce an extra degree of freedom in the form of a new scalar field. An alternative is to modify Einstein's theory of gravity so that it produces acceleration even in the absence of dark energy. Early attempts [142] to modify gravity in this way (the "left-hand side of Einstein's equations") also implicitly introduced an extra degree of freedom [143], but in a different way than do quintessence models. Technically, these models differ from quintessence in that the coefficient of the Ricci scalar R in the Jordan frame in the action depends on the new field.³ Different scalar-tensor models can therefore produce the variety of predictions found in quintessence models, but they extend the possibilities in a new direction: The non-canonical coupling to the Ricci scalar propagates to the equations that govern the evolution of perturbations. This leads to the general conclusion that modified gravity models typically predict that structure grows at a different rate (*e.g.*, Ref. [144]) than in models based on GR. Differentiating between modified gravity and GR-based dark energy therefore will require measurements of cosmological distances to pin down the background expansion, and then measurements of the growth of structure to distinguish between them.

Although there have been many proposals for how to modify gravity, the most interesting recent development traces back to an idea first proposed by Fierz and Pauli [145], that the graviton has non-zero mass. Qualitatively a massive graviton seems like an appropriate way to decrease the strength of the gravitational force on very large scales and hence to explain the acceleration of the Universe. In practice, the theory runs into two problems, both related to the fact that a massive spin-2 particle carries degrees of freedom beyond those of the massless graviton. These extra degrees of freedom typically lead to modifications of GR in the solar system, modifications that are excluded by the tight limits on post-Newtonian parameters. The second challenge for massive graviton models is to avoid Boulware-Deser ghosts [146], the instability of one of the extra degrees of freedom.

To satisfy the solar system constraints, the extra degrees of freedom need to be screened, *i.e.*, heavily suppressed by limiting their range of interaction or effective coupling to matter in environments like the solar system. Any successful modified gravity model needs a screening mechanism. One possibility is Vainshtein screening [147], which arises due to non-linearities in the Fierz-Pauli potential. In general, these non-linear terms do not avoid the ghost problem, but recently [148] the set of terms in the potential that are

²Redshift $z \equiv a^{-1} - 1$; high redshift corresponds to early cosmic time.

³Theories can be written either in the Jordan frame, in which matter couples to the metric only through $\sqrt{-g}\mathcal{L}_m$, or in the Einstein frame, in which the gravitational part of the action remains Einstein-Hilbert and there are additional couplings of the new degrees of freedom to matter.

safe from ghosts has been identified. These *Galileon* models offer a potentially attractive way of addressing the acceleration of the Universe within a consistent framework. Beyond this theoretical breakthrough, the Vainshtein screening intrinsic to this model (and other proposed mechanisms) opens up yet another axis of tests: How and where do modified gravity theories transition to normal Newtonian gravity in the solar system? The full suite of methods to test modified gravity models is still under development.

The modern view of the formation of structure in the Universe has been confronted with a growing array of precise tests, including both the temperature and polarization anisotropy spectrum of the CMB; light curves of distant supernovae; abundances of galaxy clusters; clustering of galaxies, quasars, and Lyman alpha systems; gravitational lensing; and cross-correlations between different pairs of these observations. The basic framework, with inflation, dark matter, and dark energy at its core, has been confirmed repeatedly over the past decade. We need to keep pushing: Either the basic picture will break or the agreement will become even more remarkable. Thinking in parameter space, the next decade will enable us to reduce the uncertainty in the equation of state w by a factor ten. Historically in physics, precision measurements of this sort have been pursued for these ends: Will the simple theory hold up or will it need to be replaced by something more profound? In the case of the cosmic acceleration, where there is no appealing fiducial model, the push for greater precision takes on an even greater importance.

Since scientists in the United States discovered evidence for cosmic acceleration over a decade ago, the U.S. has been the leader in the field of dark energy studies. The 2006 Dark Energy Task Force report [149] provided a systematic discussion of experimental approaches to dark energy and identified a sequence of “Stage III” and “Stage IV” dark energy experiments to build on those then in progress. Stage III surveys were designed to address systematic uncertainties, with the goal of statistics-limited constraints from four independent probes. Each of these probes will be developed over the ensuing decade to the point at which Stage IV surveys will enable sub-percent level consistency checks for all probes, some of which will have reached their cosmic variance limit, *i.e.*, the limit imposed by the fact that we have only a single observable Universe to probe. This staged categorization was reiterated in the more recent Community Dark Energy Task Force Report [150], which particularly emphasized the importance of complementing planned imaging experiments with spectroscopic experiments. Figure 4-11 illustrates the timelines for several of the major dark energy experiments in which U.S. scientists are playing an important or leading role. Further details provided in a separate document [8].

Major gains beyond the current road map of dark energy projects will require advances on one of a number of fronts. One potential avenue is to develop new techniques or probes of cosmology and new physics that have not yet been developed. Data from many cosmological surveys have been used for tests not envisioned when those experiments were first designed, and we anticipate that that trend will continue [15]. Another path is to obtain complementary information that will enhance planned experiments. For example, a targeted program of spectroscopic redshift measurements can enhance the dark energy constraints from LSST compared to its baseline capabilities [151]. Finally, new instrumental capabilities could enable powerful new dark energy experiments to be conducted at reduced costs. Possible examples include methods that suppress flux from emission lines from the night sky, which are much brighter than distant galaxies at infrared wavelengths, or new detector technologies that would measure photon energy rather than just quantity. Modest investments in these avenues may yield large potential payoffs in the future.

The following sub-sections summarize the physical probes and measurements that address dark energy science. Each of these approaches is detailed further in a separate document [10, 13, 15, 14]. Taken together, these measurements and the projects that enable them constitute an all-out attack on the problem of cosmic acceleration. The program planned over the coming decade guarantees continuing U.S. leadership in this field.

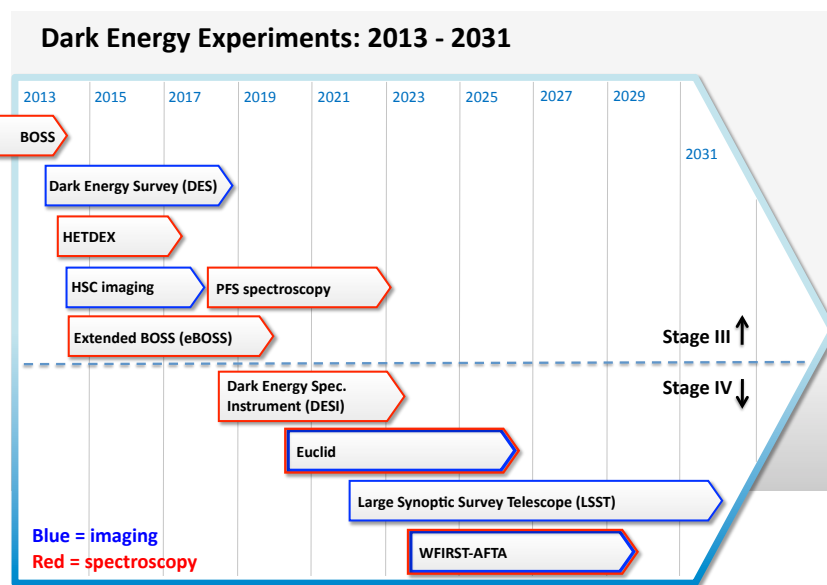


Figure 4-11. A timeline of Stage III and Stage IV dark energy experiments — photometric and spectroscopic — in which U.S. scientists are playing an important or leading role. Most of the projects are ground-based with either U.S. leadership (BOSS, DES, HETDEX, eBOSS, DESI, LSST) or active participation (HSC, PFS). The two space missions are Euclid, led by the ESA with a NASA-sponsored team of U.S. participants, and WFIRST, led by NASA.

4.5.1.1 Distances

The relationship between the redshift and distance of an object is one of the primary tests of the expansion history of the Universe, and therefore played a key role in the discovery of the accelerating Universe. The simple graph of the distance scale of the Universe as a function of redshift, indicating the evidence for cosmic acceleration, has become an iconic plot in the physical sciences. The data for this plot so far come from measurements of Type Ia supernovae and baryon acoustic oscillations (BAO), and these will be the sources of streams of data in coming years. DES and LSST will provide an essentially limitless supply of supernova — thousands, then hundreds of thousands. The DES collaboration and LSST-DESC will coordinate the spectroscopic classification of a fraction of these objects. The challenge is to make sufficiently thorough measurements to mitigate systematic problems, especially those that depend on redshift. Detailed studies of nearby supernovae are beginning to provide clues for how to do this. Much would be gained if observations could be made from space, but a substantial gain will be also achieved if we make ground-based observations that avoid the atmospheric lines in the near infrared.

The subtle pattern of anisotropy in the CMB, just one part in 10^5 , is mapped at the two-dimensional boundary of a three-dimensional feature, the fluctuations in matter density throughout space. The counterpart of the oscillations in the CMB power spectrum is a peak in the correlation between the densities at points separated by 153 Mpc, measured relative to the scale size of the Universe, which is left behind by BAO in the early Universe. This very large meter stick can be observed as far out as redshifts $z = 1.6$ using galaxies

as traces of matter density, and even out to $z = 3$ by observing correlations in the troughs of spectra from distant quasars [152]. The current Baryon Oscillation Spectroscopic Survey (BOSS) is likely to report a distance measurement soon with 1% accuracy. The eBOSS survey is designed to extend the reach of BAO measurements to higher redshifts. The Stage-IV BAO experiment, Dark Energy Spectroscopic Instrument (DESI), should provide more than 30 similarly accurate independent distance measurements.

Figure 4-12 shows the current and projected future constraints on cosmic distances using these two techniques. If our basic understanding is correct, the supernova and BAO measurements should be in absolute agreement. The distance-versus-redshift curve of the Universe is fundamental, and exploring it with completely different techniques is essential. These stunning measurements will allow for percent-level determination of the equation of state and will be extremely sensitive to evolution of the dark energy at earlier times. By pinning down the distance-redshift relation, they will also allow for apples-to-apples comparisons of modified gravity versus dark energy models using the growth of structure.

4.5.1.2 Growth of structure

The quantity and quality of cosmic structure observations have greatly accelerated in recent years, and further leaps forward will be facilitated by imminent projects. These will enable us to map the evolution of dark and baryonic matter density fluctuations over cosmic history. The way that these fluctuations vary over space and time is sensitive to several pieces of fundamental physics: the primordial perturbations generated by GUT-scale physics, neutrino masses and interactions, the nature of dark matter, and dark energy. Here we focus on the last of these, and the ways that combining probes of growth with those of cosmic distances will pin down the mechanism driving the acceleration of the Universe.

If the acceleration is driven by dark energy, then distance measurements provide one set of constraints on w , but dark energy also affects how rapidly structure grows. Upcoming surveys are therefore designed to probe w in two distinct ways: direct observations of the distance scale and the growth of structure, each complementing the other on both systematic errors and dark energy constraints. A consistent set of results will greatly increase the reliability of the final answer.

If cosmic acceleration is driven by modified gravity, then probes of structure become even more important. Generically, modified gravity models are able to reproduce any expansion history that can be attained in dark energy models, but at the cost of altering the growth of structure. How rapidly structure grows is quantified by the dimensionless *growth function* $D(a)$. Figure 4-13 illustrates how different models make predictions that differ from those of GR even though the distance-redshift relation in all these models is identical. The growth of structure then will be able to distinguish modified gravity from dark energy as an explanation for the cosmic acceleration.

Figure 4-13 projects constraints from a spectroscopic survey that measures the local velocities of galaxies by observing redshift space distortions. Similarly powerful constraints are projected from photometric surveys that are dedicated to measuring the shapes of galaxies and therefore are sensitive to the signal from weak gravitational lensing.

Achieving these powerful constraints will require both wide field imaging and spectroscopic redshift surveys, as depicted in Fig. 4-11. The results will pin down far more than the equation of state to percent-level accuracy, although this in itself will be an important clue as to whether the cosmological constant or an alternative is driving the acceleration of the Universe. We will also learn whether the equation of state is varying with time and whether dark energy was relevant at high redshift. The surveys will probe cosmological perturbations as a function of both length and time, opening up dozens of possible failure modes for GR-

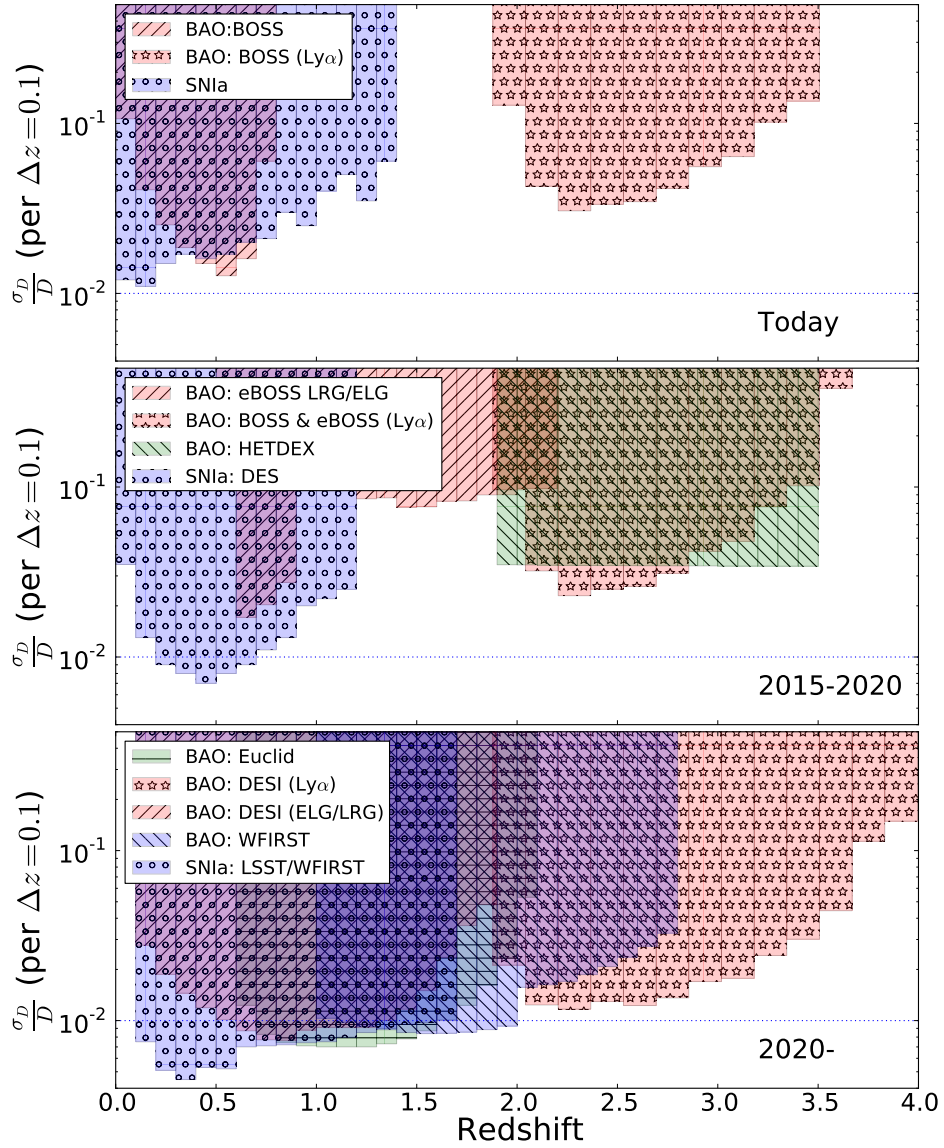


Figure 4-12. Current and projected future uncertainties on cosmic distance as a function of redshift.

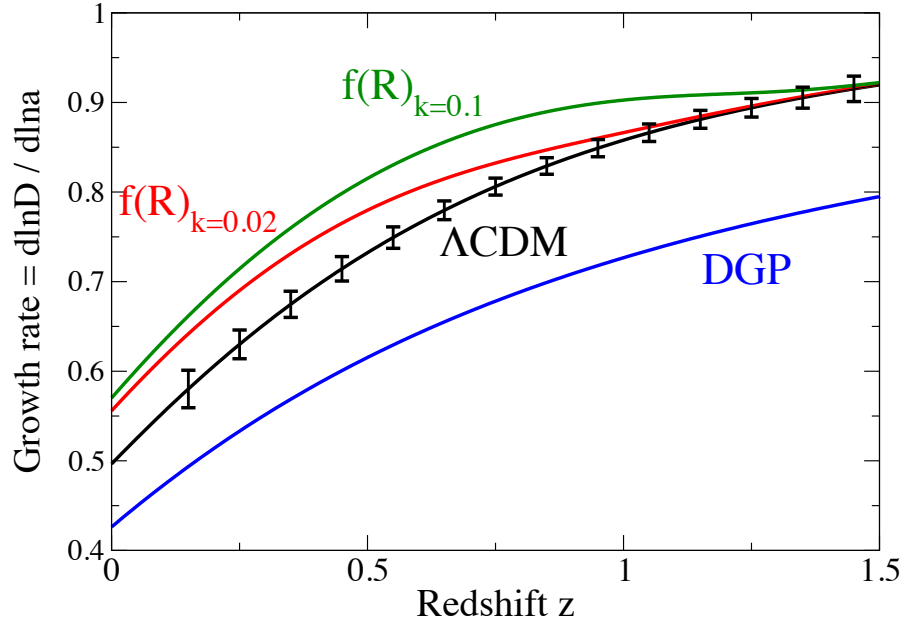


Figure 4-13. Constraints on the growth of density fluctuations in the Universe with errors projected from DESI. The curves show the derivative of the logarithmic growth with respect to logarithmic scale factor — a quantity readily measured from the clustering of galaxies in redshift space — as a function of redshift. We show theory predictions for the standard Λ CDM model, as well as for two modified-gravity models, the Dvali-Gabadadze-Porratti (DGP) model [153], and the $f(R)$ modification to Einstein action [154]. Because growth in the $f(R)$ models is generically scale-dependent, we show predictions at wave numbers, $k = 0.02 \, h \, \text{Mpc}^{-1}$ and $k = 0.1 \, h \, \text{Mpc}^{-1}$. LSST projects to impose constraints of similar excellent quality on the growth function $D(a)$.

based dark energy. If any of the possible deviations from GR predictions is reliably established, we will have a revolution on our hands.

4.5.1.3 Novel probes

Surveys enabling the twin probes of distances and structure can distinguish between modified gravity and dark energy on cosmological scales. It has become apparent over the last few years that non-cosmological tests can also play an important role in determining the mechanism driving the acceleration of the Universe. The basic idea is that gravity is known to reduce to GR on solar system scales, so any modified gravity model must have a screening mechanism, wherein the additional forces operative on large scales are suppressed in the solar system. Indeed, many of the models have screening built into them, so the solar system constraints can be naturally satisfied. The key issue is the nature of the transition from large (modified gravity) to small (GR) scales, and how that transition can be observed.

Most screening mechanisms utilize some measure of the mass distribution of halos, such as the density or the Newtonian potential, to recover GR well within the Milky Way. This leaves open the possibility that smaller halos, the outer parts of halos, or some components of the mass distribution, are unscreened and therefore experience enhanced forces. For a given mass distribution, unscreened halos will then have internal

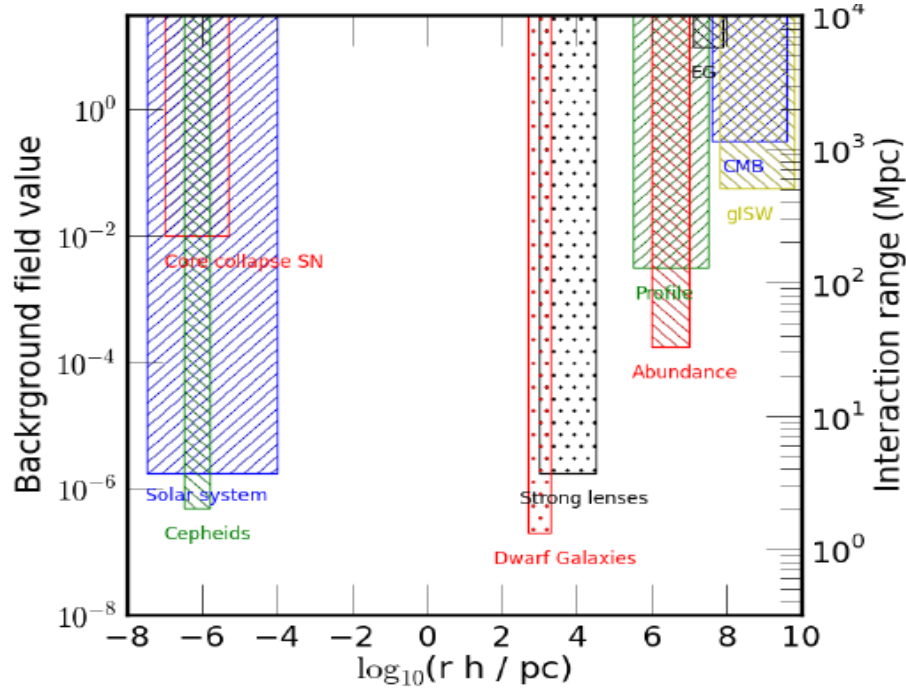


Figure 4-14. Astrophysical [155, 156, 157] and cosmological [158, 159, 160] limits on chameleon theories, in particular $f(R)$ models. The x -axis is the distance in units of h^{-1} pc, where h parametrizes the Hubble constant. The spatial scale on the x -axis gives the range of length scales probed by particular experiments. The parameter on the y -axis is the value of the scalar field responsible for acceleration in units of the Planck mass or, equivalently, the range of the additional force mediated by this field. The rectangles show excluded regions; the two rectangles with dots indicate preliminary results from ongoing work.

velocities and center-of-mass velocities larger than predicted by GR. Deviations from GR are typically at the ten percent level, with distinct variations between different mechanisms in the size of the effect and the way the transition to GR occurs. It is important to note that observable effects are typically larger on galaxy scales than on large (cosmological) scales or at high redshift. The comparison of dynamical and lensing masses provides a powerful test that is being implemented on a wide range of scales, from individual galaxies to large-scale cross-correlations (see Secs. 4.5.1.2 and 4.5.1.4).

Although the Vainshtein screening mechanism mentioned earlier arises in some models, the chameleon mechanism, where the extra degree of freedom is suppressed in high density regions, is prevalent in others. Figure 4-14 shows a range of tests that have been implemented in one particular model, $f(R)$ gravity, with chameleon screening. The screening in this model, as in many others, depends on the value of a field in the region of interest. The y -axis in Fig. 4-14 depicts the value of this field in Planck units divided by a coupling constant, while the x -axis shows that the model has been probed on scales ranging from the solar system all the way out to cosmological scales, with all tests to date verifying GR. In the next decade essentially the entire accessible parameter space of chameleon theories can be probed using the tests shown in the figure. Tests for the other important screening mechanism, Vainshtein screening, are at early stages with potential for rapid progress.

The program of testing gravity theories via these novel probes is in its infancy, but it is becoming increasingly clear that even modest investments in non-cosmological observations have enormous potential to contribute to the cosmic acceleration problem.

4.5.1.4 Cross-correlations

To extract the most information possible about dark energy from galaxy surveys and CMB experiments, scientists will be forced to combine probes. The multi-probe approach is not simply a matter of multiplying uncorrelated likelihoods. For example, gravitational lensing and large-scale structure are highly correlated probes, with the lensing signal enhanced behind over-dense regions and the density enhanced (due to magnification) behind regions with large lensing signals. The likelihoods are therefore not independent, and there is significant information contained in the cross-correlations. Beyond improvement in statistical power, these cross-correlations will enable us to isolate systematic uncertainties. The four principal probes — supernovae, clusters, lensing, and large-scale structure — are correlated with one another in ways that will need to be accounted for as we strive for percent-level accuracy in dark energy parameters.

The four probes enabled by optical surveys will be supplemented by data at other wavelengths. One of the most promising for the purposes of dark energy will come from the oldest probe, the CMB. To date, CMB experiments have supplemented galaxy surveys mainly by constraining parameters that would otherwise be degenerate with those that characterize dark energy. Since the CMB is sensitive to early Universe physics, it probes dark energy only through the geometric projection from the surface of last scattering. The photons from the last scattering surface, however, experience deflections due to gravitational lensing and to Compton scattering off free electrons. These deflections show up as *secondary anisotropies*, which often do carry information about dark energy.

Galaxy clusters are perhaps the quintessential example of the value of observing cosmic phenomena at different wavelengths. One of the key uses for clusters in dark energy studies is to compare their abundance above a given mass threshold as a function of redshift with theoretical predictions. The key uncertainty remains the mass determination, and it is in this regard that multi-wavelength studies become particularly important. By observing clusters in optical surveys, in the microwave frequencies via the Sunyaev-Zel'dovich effect that follows from Compton scattering off hot electrons in the clusters, and in the X-ray region when those same electrons emit radiation, we obtain many possible mass proxies. Taken together, they offer a powerful attack on the dominant systematic uncertainties in dark energy cluster studies. Figure 4-15 shows an example of these different views of a galaxy cluster.

4.5.2 Inflation

Cosmic inflation is the leading theory for the earliest history of the Universe and for the origin of structure in the Universe. Current observations of the large-scale distributions of dark matter and galaxies in the Universe and measurements of the CMB are in stunning agreement with the predictions of inflation. The next generations of experiments in observational cosmology are poised to explore the detailed phenomenology of the earliest moments of the Universe.

Although the landscape of possible models for inflation is large, the theoretical underpinnings are well understood, and we are able to make concrete predictions for observable quantities. One key prediction is the existence of a background of gravitational waves from inflation that produces a distinct signature in the polarization of the CMB (the *B*-modes in Fig. 4-16). Under the so-called Lyth condition [162], all

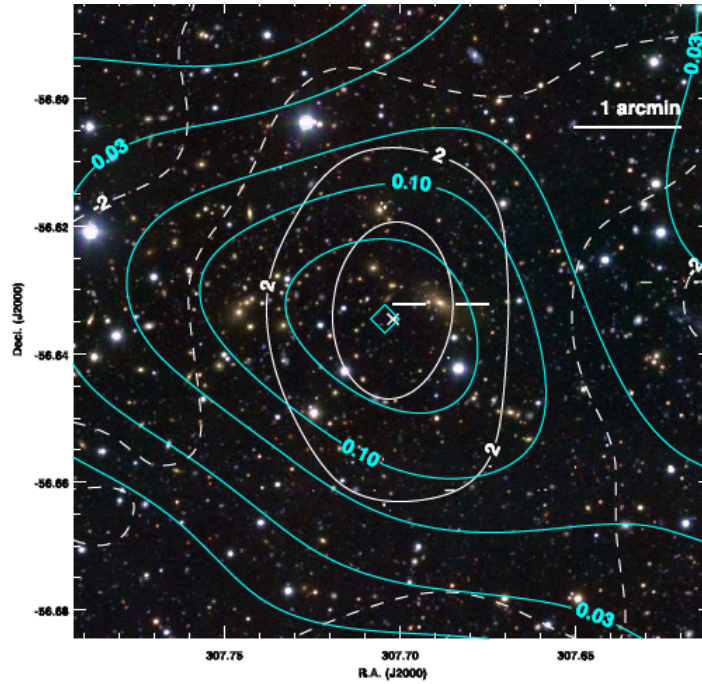


Figure 4-15. Map of a galaxy cluster [161] using three probes: (i) Weak gravitational lensing (blue contours with labels showing the projected density κ); (ii) hot gas as measured by the Sunyaev-Zel'dovich distortion of the CMB (white contours with labels giving signal to noise); and (iii) galaxies as observed in three optical bands (background).

models in which the field driving inflation varies by an amount of order m_{Planck} will produce gravitational wave (tensor) fluctuations that are at least 1% of the amplitude of density-fluctuation (scalar) power in the CMB ($r = 0.01$ in the figure). Definitive evidence as to the presence or absence of tensor modes with amplitudes at or above this level would therefore be a window on an infinite sequence of Planck-suppressed operators. Such scales require a quantum gravity treatment, and this will test string-theoretic mechanisms for large field inflation. If not detected, these observations at least decide between two broad classes of models, since they are sensitive to Planck-scale effects. This motivates the design of a next-generation CMB experiment with the sensitivity and systematics control to detect such a signal with at least 5σ significance, thus ensuring either a detection of inflationary gravitational waves or the ability to exclude large classes of inflation models [9].

There are several other ways to probe the physics of inflation. Inflation generically predicts small deviations from a scale-invariant spectrum, and current measurements confirm this prediction at the 5σ level. DESI projects to obtain a 15σ detection, thereby further reducing the range of allowed models. BOSS, eBOSS, and DESI will potentially constrain the *running* of the primordial spectrum (deviation from a pure power law) at the 0.2% level, a factor of five tighter than current constraints [163].

The non-gaussian distribution (non-gaussianity) of the primordial perturbations can take many forms. The search for one — so-called *local* non-gaussianity — is particularly important because single-field models of inflation generically predict negligible local non-gaussianity [9], so any detection will falsify a large class of models. Planck has placed strong upper limits on this and other forms of non-gaussianity consistent with these predictions. The upcoming surveys eBOSS, DESI, and LSST will constrain a variety of forms

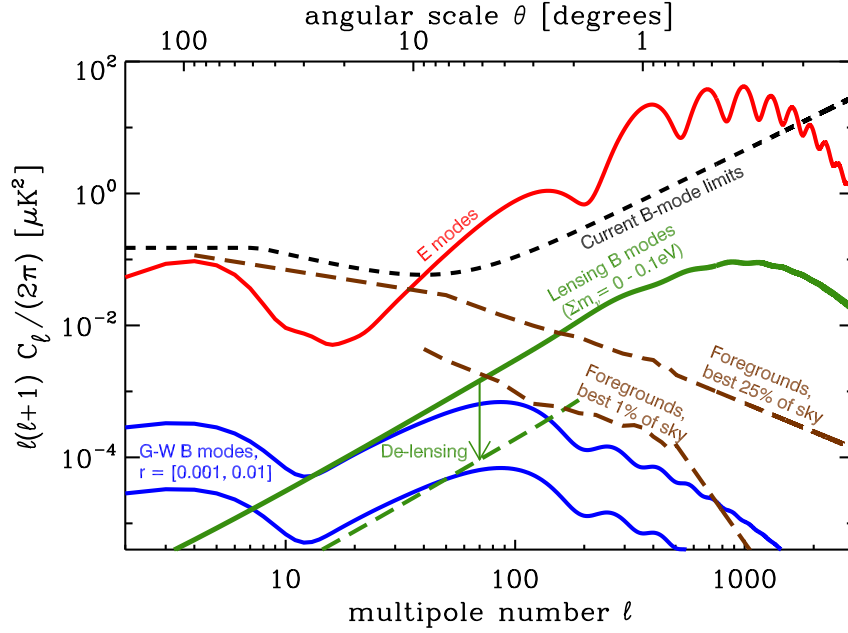


Figure 4-16. Expected signal levels for the CMB Polarization E-mode (solid red), inflationary gravity-wave B-mode (solid blue), and lensing B-mode (solid green) signals. The gravitational wave B-mode signals are shown for tensor-to-scalar ratios of $r = 0.001$ (the Stage IV goal) and $r = 0.01$ (the boundary between small-field and large-field inflation models). The lensing B-mode signal is shown as a band encompassing the predicted signal for values of the sum of neutrino masses $0 \leq \sum m_\nu \leq 0.1$ eV. De-lensing by a factor of 4 in amplitude is shown schematically by the green arrow, with the residual signal at $\ell \leq 200$ (where the de-lensing is critical to the constraint on r) shown by the green, long-dashed line. The black, short-dashed line shows the level of current 95% upper limits on B-modes from WMAP, BICEP, QUIET and QUAD experiments. The brown, long-dashed lines show the expected polarized foreground contamination at 95 GHz for the cleanest 1% and 25% of the sky.

of primordial non-gaussianity on different spatial scales and be subject to different systematic uncertainties than the CMB. They will therefore pave the way for even more stringent bounds on inflationary models.

4.5.3 Neutrinos

One of the most remarkable aspects of physical cosmology is that the study of the largest physical structures in the Universe can reveal the properties of particles with the smallest known cross section, the neutrinos. At the simplest level, this cosmological sensitivity to neutrino properties is due to the fact that the neutrino cosmological number density is so large as to be second only to CMB photons. More specifically, the properties of neutrinos alter the effective energy density of cosmological radiation and therefore the amplitude, shape and evolution of matter perturbations, leading to changes in observables in the CMB anisotropies and in measures of large-scale structure.

The CMB and large-scale structure measured in galaxy surveys are sensitive to the sum of the neutrino masses and to N_{eff} , the number of species produced in the early Universe. These observations are there-

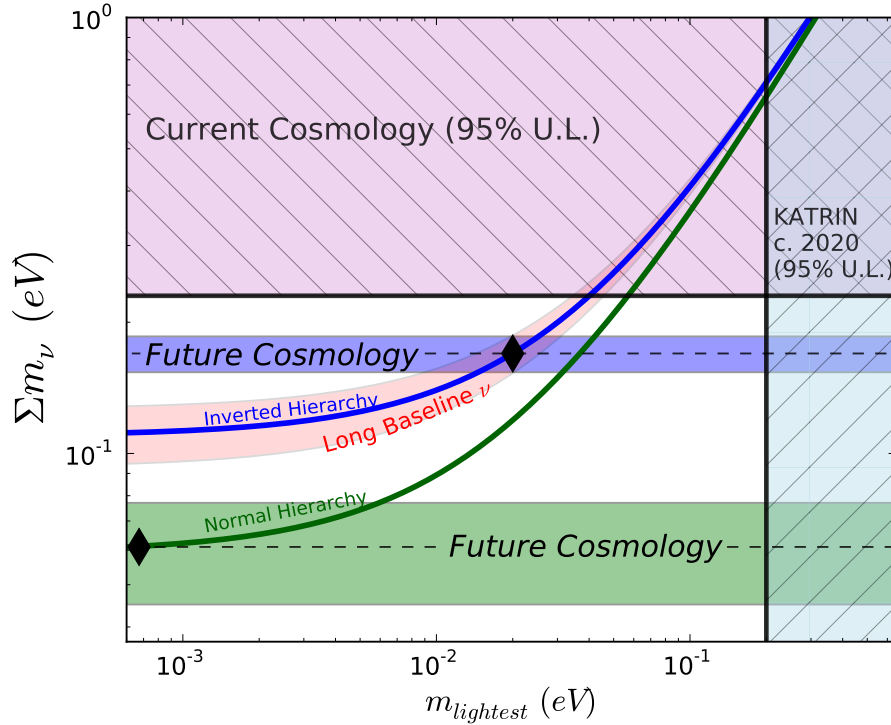


Figure 4-17. Current constraints and forecast sensitivity of cosmology to the sum of neutrino masses. In the case of an “inverted hierarchy,” with an example case marked as a diamond on the upper curve, future combined cosmological constraints would have a very high-significance detection, with 1σ error shown as a blue band. In the case of a normal neutrino mass hierarchy with an example case marked as a diamond on the lower curve, future cosmology would still detect the lowest $\sum m_\nu$ at greater than 3σ .

fore complementary to laboratory probes of neutrinos that measure mass differences and, potentially, CP violation. CMB experiments are sensitive enough to the neutrino energy density to exclude $N_{\text{eff}} = 0$ at more than 10σ ; *i.e.*, these experiments have already indirectly detected the cosmic neutrinos. Together with existing data on the redshift-distance relation from galaxy surveys like BOSS, these experiments also place a stringent upper limit on the sum of the neutrino masses, currently around 0.23 eV [164].

A global fit to solar and atmospheric neutrino flavor oscillations in the standard 3-generation model determines two mass differences. A third parameter, which can be taken as either the sum of the masses or the lightest mass, is, therefore, unknown. Due to a sign ambiguity in one of the mass differences, there are two discrete possibilities, normal and inverted hierarchy, for the relationship between these two parameters. As indicated in Fig. 4-17, upcoming CMB experiments and large-scale structure surveys will unambiguously detect the sum of the masses if the hierarchy is inverted and will likely do so at greater than 3σ significance, even if nature has chosen the normal hierarchy. Measures of the power spectrum from DESI, combined with Planck data, could improve the current constraint on $\sum m_\nu$ to 17 meV, and a Stage IV CMB survey combined with BAO measurements from DESI could achieve similar sensitivity [11]. Figure 4-17 highlights another aspect of the complementarity of laboratory experiments and cosmological probes: If the sum of the masses is found to be 0.15 eV by the cosmological probes, then it will take the lab experiments to determine the lightest neutrino mass by identifying whether the hierarchy is normal or inverted.

Short-baseline neutrino oscillation results hint at a richer neutrino sector than three active neutrinos participating in flavor oscillations, with one or more sterile flavors also participating [165, 166, 167, 168]. Future CMB experiments will achieve a 1σ error of $\Delta N_{\text{eff}} = 0.027$, which will complement future sterile neutrino searches and inform model building, since sterile neutrinos might be detected in the laboratory even though they were not produced in the early Universe and vice versa.

4.5.4 Dark energy and CMB conclusions

Cosmological surveys are sensitive to fundamental physics. To date, this has led to the discovery of the accelerating Universe, strong evidence for an epoch of early acceleration near the GUT scale, the indirect detection of the cosmic neutrino background, and the most compelling evidence for non-baryonic dark matter. However, surveys to date have measured only a fraction of all information available. If the Universe were contained in an area the size of the United States, galaxy maps so far would have surveyed the city of Birmingham, Alabama. CMB experiments have provided low-noise maps of the temperature down to angular scales of order a tenth of a degree. Strategic, valuable information remains unmined in higher-resolution temperature maps and the virtually uncharted polarization field. We have outlined the projections for how this extra information will constrain dark energy, neutrinos, and inflation, and these projections are extraordinary. But even they ignore the very real possibility that future experiments on the Cosmic Frontier will do just what their predecessors have done: Discover something fundamentally new!

The community has rallied behind previous reports [149, 169, 150] which are consistent with the current consensus to support the following key steps:

- **Remain a leader in dark energy research.** The U.S. played the leading role in discovering the acceleration of the Universe, as was recognized by the 2011 Nobel Prize. The acceleration remains a mystery, whose solution may usher in a revolution in either our theory of gravity or our understanding of particle physics. Different classes of theories make different predictions for the growth of structure, given a redshift-distance relation. A combination of spectroscopic and photometric surveys can determine both distances and structure growth, and so will help pinpoint the new physics driving the acceleration of the Universe. The current suite of surveys, Stage III, will be the first to implement the vision of multiple probes and small systematic uncertainties. This vision will be realized fully with the Stage IV surveys (LSST and DESI) when they reach the level where exquisite-precision dark energy constraints from different probes, in some cases approaching the cosmic variance limit, can be checked for consistency. Therefore, the community strongly supports continuing the program of Stage III and Stage IV dark energy experiments, and moving forward as quickly as possible with the construction of LSST and DESI.
- **Build a generation IV CMB polarization experiment.** CMB experiments can measure the sum of the neutrino masses and the energy scale of inflation, as well as constrain exotic physics such as early dark energy and extra neutrino species. After the current generation of small-scale experiments complete data-taking near the end of the decade, the community understands that the next generation experiment — one that can pin down neutrino masses and the scale of large field inflationary models — requires a nationwide coherent effort. Moving from thousands to hundreds of thousands of detector elements will require the involvement of the national laboratories working together with the university community.
- **Extend the reach.** With small additional investments, the dark energy program can be augmented in three important ways. A targeted spectroscopic campaign designed to optimize and calibrate methods

of redshift estimation from imaging surveys can enhance their science returns beyond their nominal capabilities [12]. Second, a continued investment in instrumentation R&D will allow the community to do more science for less money and to be ready to capitalize on future discoveries. Finally, a suite of novel probes of gravity and dark energy can discover signatures of modified gravity and new physics in the dark sector. We have the opportunity to catalyze the next generation of tests by supporting work at the interface of theory, simulation and data analysis, and making small enhancements to the dark energy survey program.

4.6 Cosmic particles and fundamental physics

Over the past decade we have witnessed a revolution in our understanding of the high-energy Universe. This includes several key discoveries.

- Supernova remnants have been shown to be a source of galactic cosmic rays [170].
- Very high energy neutrinos that are likely to be astrophysical in origin have been observed [171].
- The GZK suppression in the cosmic-ray flux above 3×10^{19} eV has been observed [172, 173].
- The positron fraction of the cosmic rays has been measured up to 300 GeV, providing solid evidence for a high-energy primary source of positrons in the galaxy, either from dark matter annihilation or astrophysical processes [55, 54].
- Many sites of astrophysical particle acceleration have been directly observed, from supermassive black holes and merging neutron stars, to rapidly spinning neutron stars and supernova remnants in our galaxy [174].

These discoveries have been driven by the current generation of experiments: the IceCube neutrino detector at the South Pole, the Fermi gamma-ray observatory and the PAMELA and AMS experiments orbiting the Earth, the High Resolution Fly’s Eye and Pierre Auger Observatory ultra-high-energy cosmic ray experiments, and the HESS, VERITAS, MAGIC and Milagro experiments in very-high-energy gamma rays.

Looking forward, a new generation of instruments with greater sensitivity and higher resolution holds the promise of making large advances in our understanding of astrophysical processes and the fundamental physics studied with astrophysical accelerators. The goals for the coming decade are:

- Determine the origin of the highest energy particles in the Universe and understand the acceleration processes at work throughout the Universe.
- Measure particle cross sections at energies unattainable in Earth-bound accelerators.
- Measure the highest energy neutrinos that arise from interactions of the ultra-high-energy cosmic rays with the CMB.
- Measure the extragalactic background light at wavelengths between 1 and 100 μm to understand the star formation history of the Universe.
- Measure the mass hierarchy of the neutrinos.
- Search for physics beyond the Standard Model encoded in cosmic messengers as they cross the Universe.

- Understand the origins of the matter-antimatter asymmetry of the Universe.
- Probe the fundamental nature of spacetime.

In many of these areas, future progress will depend upon either the detailed understanding of particle acceleration in the Universe or the development of methods for controlling systematic errors introduced by our lack of understanding of these processes. High-resolution gamma ray measurements (spectral, angular, and temporal) of many objects and classes of objects are needed to find the source-invariant physics that is the signal for physics beyond the Standard Model. Such measurements, in conjunction with measurements at other wavelengths and with measurements of cosmic rays, neutrinos, and gravity waves, will enable us to understand Nature's particle accelerators. We propose several recommendations to achieve these goals.

- **Significant U.S. participation in the CTA project [175].** U.S. scientists developed the imaging atmospheric Cherenkov technique. Continued leadership in this area is possible with the development of novel telescope designs. A U.S. proposal to more than double the number of mid-scale telescopes would result in a sensitivity gain of 2 to 3, significantly improving the prospects for the indirect detection of dark matter, understanding particle acceleration processes, and searching for other signatures of physics beyond the Standard Model.
- **Simultaneous operation of Fermi, HAWC, and VERITAS.** Understanding particle acceleration and separating astrophysical processes from physics beyond the Standard Model requires observations over a broad energy range. The above three instruments will provide simultaneous coverage from 30 MeV to 100 TeV. HAWC and VERITAS will simultaneously view the same sky, enabling prompt follow-up observations of transient phenomena.
- **Construction of the PINGU neutrino detector [176] at the South Pole.** U.S. scientists have been leaders in the field of high-energy neutrino observations. PINGU, by densely instrumenting a portion of the IceCube Deep Core array, will lower the energy threshold for neutrinos to a few GeV. This will allow for a measurement of the neutrino mass hierarchy using atmospheric neutrinos.
- **Continued operation of the Auger and Telescope Array (TA) air shower arrays with upgrades** to enhance the determination of the composition and interactions of cosmic rays near the energy of the GZK suppression, **and flight of the JEM-EUSO mission [177]** to extend observations of the cosmic ray spectrum and anisotropy well beyond the GZK region.
- **Construction of a next-generation ultra-high-energy GZK neutrino detector** either to detect GZK neutrinos [178] and constrain the neutrino-nucleon cross section at these energies [179], or exclude all but the most unfavorable parts of the allowed parameter space.

4.6.1 Ultra-high-energy cosmic rays

HiRes [172], Auger [180], and TA [181] have established the existence of a suppression of the spectrum at the highest energies (above $\sim 3 \times 10^{19}$ eV), as predicted by Greisen, Zatsepin, and Kuzmin (GZK) in 1966 [182, 183]. The GZK suppression is an example of the profound links between different regimes of physics, connecting the behavior of the highest-energy particles in the Universe to the CMB, and can be explained by the sub-GeV-scale physics of photo-pion production occurring in the extremely boosted relativistic frame of the cosmic ray. A similar phenomenon occurs for primary nuclei due to excitation of the giant dipole resonance, resulting in photo-disintegration. For iron nuclei, this occurs at about the same energy per particle as the photo-pion process does for protons.

The composition of cosmic rays and their interactions with air nuclei may be probed by studies of the depth of shower maximum, $X_{\max}$ [184]. The mean value of $X_{\max}$ rises linearly as a function of the logarithm of the cosmic ray's energy, and depends on the nature of the primary particle, the depth of its first interaction, and the multiplicity and inelasticity of the interactions as the shower evolves. Lower energy observations of $X_{\max}$ indicate that the composition becomes lighter as the energy increases toward $\sim 10^{18.3}$ eV [185], which suggests that extragalactic cosmic rays are mainly protons. However, at higher energies the Auger Observatory's high-quality, high-statistics sample exhibits the opposite trend, along with a decreasing spread in $X_{\max}$ with increasing energy [180, 186]. Using current simulations and hadronic models tuned with LHC forward data, this implies the composition is becoming gradually heavier above $10^{18.5}$ eV. A trend toward heavier composition could reflect the apparent GZK suppression being in fact the endpoint of cosmic acceleration, in which there is a maximum magnetic rigidity for acceleration, resulting in heavy nuclei having the highest energy per particle.

Cosmic rays can be used to probe particle physics at energies far exceeding those available at the LHC. An alternative explanation for the observed behavior of $X_{\max}$ is a change in particle interactions not captured in event generators tuned to LHC data. Auger measurements using three independent methods find that these models do not describe observed showers well. For example, the observed muon content of showers measured in hybrid events at Auger is a factor 1.3 to 1.6 larger than predicted [187]. TA also observes a calorimetric energy that is about 1.3 times higher than that inferred from their surface detector using these models. An example of a novel phenomenon that may explain these observations is the restoration of chiral symmetry in QCD [188].

A critical step in fully understanding the $X_{\max}$ observations is to identify and correct the deficiencies in the beyond-LHC physics used in modeling showers. This requires continued operation of current hybrid detectors such as Auger and TA, with upgrades to enable improved multi-parameter studies of composition and interactions on a shower-by-shower basis. Enhancements of the surface detectors are particularly valuable because of the tenfold higher duty cycle than for fluorescence or hybrid operation.

Observations from space can extend studies of the spectrum and anisotropy beyond the GZK region with high statistics. Measurements at ground-based observatories hint that cosmic ray arrival directions may be correlated with the local distribution of matter [189, 190], but higher statistics trans-GZK observations are required to identify sources. In addition, answering the question of whether the spectrum flattens again above the GZK suppression or continues to fall will distinguish between the GZK and acceleration limit scenarios. The JEM-EUSO mission has an instantaneous collecting area of ~ 40 times that of existing ground-based detectors [177] and, taking duty cycle into account, will increase the collecting area above the GZK suppression energy by nearly an order of magnitude.

4.6.2 Neutrinos

IceCube has recently reported the detection of two neutrinos with energies above 1 PeV and 26 events above 30 TeV, with characteristics that point to an astrophysical origin [171]. These exciting results herald the beginning of the era of high-energy neutrino astronomy and initiate the study of ultra-long baseline high-energy neutrino oscillations. Neutrino data complement observations of cosmic rays and gamma rays due to the origin of neutrinos in the decays following high-energy hadronic interactions and their weak couplings. Several acute issues in particle physics and astrophysics can be addressed by neutrino experiments.

GZK neutrinos. GZK neutrinos are created in the weak decays of the mesons and neutrons produced in the interaction of ultra-high-energy cosmic rays with the CMB [178]. The production of these neutrinos takes place via well-known physics at high Lorentz boost, so robust predictions of the neutrino flux can be made.

The flux depends on the composition of the primary cosmic rays (being lower for a heavier composition) and the evolution of the cosmic ray source density with redshift. Unlike many searches, there is a lower limit on the expected flux. Current detectors such as IceCube, Auger, RICE, and ANITA have begun to probe the highest predicted values of the neutrino flux. Next-generation experiments such as ARA, ARIANNA, and EVA can increase our sensitivity by about two orders of magnitude, and will either detect GZK neutrinos or exclude much of the parameter space. If GZK neutrinos are detected, the event rate as a function of zenith angle can be used to measure the neutrino-nucleon cross section and constrain models with enhanced neutrino interactions at high energy [191].

Atmospheric neutrinos and the neutrino mass hierarchy. PINGU is a proposed high-density infill of the IceCube detector with a reduced energy threshold of a few GeV, employing the rest of IceCube as an active veto. PINGU has sensitivity to atmospheric ν_μ over a range of values of the length-to-energy ratio L/E spanned by the variation in the distance to the production region as a function of zenith angle and the energy spectrum of atmospheric neutrinos. Preliminary studies of PINGU indicate that over these values of L/E , atmospheric ν_μ oscillations can be used to determine the neutrino mass hierarchy with $3\text{--}5\sigma$ significance, given two years of data with a 40-string detector [176].

Supernova neutrinos. The measurement of the time evolution of the neutrino energy and flavor spectrum from supernova bursts has the potential to revolutionize our understanding of neutrino properties, supernova physics, and measure or tightly constrain non-standard interactions. Collective oscillations of neutrinos leaving the neutron star surface imprint distinct signatures on the time evolution of the neutrino spectrum that depend on the neutrino mass hierarchy and the mixing angle θ_{13} . Although the spectral distributions of anti-electron neutrinos for the normal and inverted mass hierarchies are not so distinct, the electron neutrino spectra are dramatically different [192]. Therefore, a large underground liquid argon detector, such as the Long Baseline Neutrino Experiment (LBNE) [193], which is predominantly sensitive to electron neutrinos, could determine the neutrino mass hierarchy if a supernova occurred within our Galaxy.

4.6.3 Gamma rays

High-energy gamma rays provide a unique view into the most extreme environments in the Universe, probing particle acceleration processes and the origin of the galactic and extragalactic cosmic rays. Active galactic nuclei (AGN), supermassive black holes emitting jets of highly relativistic particles along their rotation axis, have been shown to be sites of particle acceleration [174]. Outstanding issues in the acceleration processes include: the nature of the accelerated particles (hadronic or leptonic), the role of shock acceleration versus magnetic reconnection, and the formation and collimation of astrophysical jets [194]. Answers to these questions will come from higher resolution measurements in the GeV–TeV regime, multi-wavelength campaigns with radio, X-ray, and gamma-ray instruments, and multi-messenger observations with gamma rays, ultra-high energy cosmic rays, neutrinos, and potentially gravitational waves. Understanding these extreme environments and how they accelerate particles is of fundamental interest. In addition, these high-energy particle beams, visible from cosmologically interesting distances, allow us to probe fundamental physics at scales and in ways that are not possible in Earth-bound particle accelerators.

Recently, Fermi [170] and AGILE [195] measured the energy spectra of the supernova remnants W44 and IC44. The decrease in the gamma-ray flux below the pion mass in these sources is clear evidence for hadronic acceleration. This is the clearest evidence to date that some galactic cosmic rays are accelerated in supernova remnants. The detection of high-energy neutrinos from a cosmic accelerator would be a smoking-gun signature of hadronic acceleration. In the absence of multi-messenger signals, multi-wavelength energy spectra (X-ray to TeV) can test both leptonic models, where the high-energy emission is derived from inverse

Compton scattering of the X-ray synchrotron emission, and hadronic models, where gamma rays result from pion decays or proton synchrotron radiation.

Backgrounds to dark matter searches. Understanding the origin of the galactic very high energy gamma rays is critical in the interpretation of some signatures of dark matter annihilation. The recent results from PAMELA [55] and AMS [54] of the increasing positron fraction with energy is a clear signal that the current model of secondary production and transport through the galaxy is not correct. There are three potential explanations for this signal: a new astrophysical source of positrons [196], modified propagation of cosmic rays or secondary production in the source [197], or dark matter annihilation [198]. An astrophysical source of positrons, pulsar wind nebula, is now known to also lead to an increasing positron fraction at high energies. Observations of the Geminga pulsar wind nebula in the TeV band by Milagro [199] have been used to normalize the flux of positrons in our local neighborhood from this source. The calculated positron fraction is an excellent match to the data [200]. Similarly, in searching for dark matter signatures from the GC or galaxy clusters, we must understand and measure the more standard astrophysical processes that may lead to signatures that are similar in nature to those expected from dark matter annihilation.

Extragalactic background light. The extragalactic background light (EBL) pervades the Universe. It is the sum of all the light generated by stars and the re-radiation of this light in the infrared band by dust [201], and it is therefore sensitive to the star formation history of the Universe. In addition to advancing our understanding of particle acceleration and astrophysical backgrounds for dark matter searches, the intense gamma-ray beams generated by AGN and gamma-ray bursts can be used to probe the intervening space and search for physics beyond the Standard Model. Future studies may measure the EBL, use the EBL to measure the intergalactic magnetic fields, and search for axion-like particles (ALPs). The production of electron-positron pairs from photon-photon scattering of the EBL with high-energy gamma rays leads to an energy-dependent attenuation length for high-energy gamma rays [202]. At the same time, cosmic rays, if they are accelerated by AGN, will interact with the EBL and the CMB along the line of sight and generate secondary gamma rays at relatively close distances to the observer [203]. This absorption, with the inclusion of secondary gamma rays, can be used to measure or constrain the EBL [201]. Lower limits on the EBL can be established from galaxy counts. An inconsistency between these lower limits and the measurements of the TeV spectra from AGN would be a signature of new physics. Such a discrepancy could be explained either by the secondary production of gamma rays from cosmic rays produced at AGN or by photon-ALP mixing mediated by the intergalactic magnetic fields.

Intergalactic magnetic fields. The origin of the galactic magnetic fields remains a mystery. Although astrophysical dynamos can efficiently amplify pre-existing magnetic fields, the generation of a magnetic field is difficult [204]. The strength of the magnetic fields in the voids between galaxy clusters should be similar to the primordial magnetic field. Measurements of AGN spectra and time delays in the GeV–TeV region have been used to set both lower and upper bounds on the strength of the intergalactic magnetic field [205]. Current bounds are model-dependent and span a large range of values for the magnetic field. Improved measurements of the EBL, the measurement of variability from distant AGN, improved determinations of the energy spectra, and understanding of the intrinsic AGN spectra are needed to significantly improve these limits.

Tests of Lorentz invariance violation. Experimentally probing Planck-scale physics, where quantum gravitational effects become large, is challenging. A unique signature of such effects would be the violation of Lorentz invariance — a natural, though not necessary, property of theories of quantum gravity [206]. Short, intense pulses of gamma rays from distant objects, such as gamma-ray bursts and active galaxies, provide a laboratory in which to search for small, energy-dependent differences in the speed of light. Current limits have reached the Planck scale if the energy dependence of the violation is linear [207], and 6.4×10^{10} GeV if the violation is quadratic [208] with energy. Future instruments could improve these limits by at least factors of 10 and 50, respectively, and significantly increase the sample size used to search for these effects.

TeV gamma-ray instruments. Ground-based TeV instruments fall into two classes: extensive air shower (EAS) arrays, capable of simultaneously viewing the entire overhead sky, and imaging atmospheric Cherenkov telescopes (IACTs), pointing instruments with high sensitivity and resolution. Current IACTs are VERITAS, MAGIC, and HESS, while the HAWC (under construction) and Tibet arrays are the only operating EAS arrays. CTA, the next-generation experiment, will consist of a large array of IACTs with roughly an order of magnitude greater sensitivity than current instruments. It is expected to discover over 1000 sources in the TeV band [52]. The U.S. portion of the collaboration is proposing to more than double the number of mid-sized telescopes to over 50 using a novel optical design that will improve the sensitivity of CTA by a factor of 2-3 (a result of improved angular resolution of the new telescopes and an increase in the number of telescopes).

4.6.4 Baryogenesis

According to standard cosmology, the current preponderance of matter arises during the very early Universe from an asymmetry of about one part per hundred million between the densities of quarks and antiquarks. This asymmetry must have been created after inflation by a physical process known as baryogenesis. Baryogenesis requires extending the Standard Model of particle physics via some new physics that couples to Standard Model particles and is important during or after the end of inflation. Constraints on the inflation scale and on the reheat temperature at the end of inflation give an upper bound on the relevant energy scale for baryogenesis. The new physics must violate CP, as the CP violation in the Standard Model is insufficient [209]. There are a very large number of theoretical baryogenesis proposals.

Leptogenesis. Theoretically, baryon number violation at high temperatures is rapid in the Standard Model via nonperturbative electroweak processes known as sphalerons. Sphalerons conserve the difference between baryon and lepton numbers, leading to the idea of leptogenesis [210]. The decay of very heavy neutrinos in the early Universe could occur in a CP-violating way, creating a lepton asymmetry that the sphalerons convert into a baryon asymmetry. Whether and how leptogenesis could occur can be clarified by Intensity Frontier experiments that will probe CP violation and lepton number violation in neutrino physics [211]; Cosmic Frontier experiments, which may, for example, probe the high inflation scale required in most leptogenesis proposals; and Energy Frontier experiments [212], which may discover new particles, with important implications for leptogenesis.

Affleck-Dine baryogenesis. In supersymmetric theories, condensates of the scalar partners of quarks and leptons have relatively low energy density and are likely to be present at the end of inflation [213]. The subsequent evolution and decay of the condensates can produce the baryon asymmetry, and in some models, the dark matter [101, 214]. The dark matter could be macroscopic lumps of scalar quarks or leptons known as Q-balls, which have unusual detection signatures in Cosmic Frontier experiments [215]. A critical test of this theory is the search for supersymmetry, primarily via collider experiments. Instruments such as HAWC and IceCube will be sensitive to extremely low fluxes of Q-balls — over two orders of magnitude lower than current limits. Although the non-observation of Q-balls cannot exclude the Affleck-Dine mechanism, the observation of a Q-ball would have a profound impact on our understanding of the Universe.

Electroweak baryogenesis. New particles beyond the Standard Model that are coupled to the Higgs boson can provide a first-order phase transition for electroweak symmetry breaking, which proceeds via nucleation and expansion of bubbles of broken phase. CP-violating interactions of particles with the expanding bubble walls can lead to a CP-violating particle density in the symmetric phase, which can be converted by sphalerons into the baryon asymmetry [216]. The baryons then enter the bubbles of broken phase where the sphaleron rate is negligible. This scenario can be definitively tested by searches for new particles in high-energy colliders

and nonvanishing electric dipole moments for the neutron and for atoms. Non-standard CP violation in D - and B -meson physics may appear in some models. A non-standard Higgs boson self-coupling is a generic consequence. The first-order phase transition in the early Universe can show up via relic gravitational waves.

Other experiments that can shed light on baryogenesis include searches for baryon number violation. Neutron–antineutron oscillations violate the difference between baryon and lepton numbers, and most leptogenesis scenarios will not work if such processes are too rapid. Proton decay would provide evidence for grand unification, which would imply the existence of heavy particles whose decay could be responsible for baryogenesis, and which is a feature of some leptogenesis and supersymmetric models.

4.6.5 Fundamental nature of spacetime

Quantum effects of spacetime are predicted to originate at the Planck scale. In standard quantum field theory, their effects are strongly suppressed at experimentally accessible energies, so spacetime is predicted to behave almost classically, for practical purposes, in particle experiments. However, new quantum effects of geometry originating at the Planck scale from geometrical degrees of freedom not included in standard field theory may have effects on macroscopic scales [217] that could be measured by laser interferometers, such as the holometer [218]. In addition, cosmic particles of high energies can probe departures from Lorentz invariance, as discussed in Sec. 4.6.3, and the existence of extra dimensions on scales above the LHC.

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The Future of U.S. Particle Theory

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5.1 Introduction

Theoretical physics has played a crucial role in particle physics since its earliest days. Interpretation and synthesis of a broad range of experimental results (phenomenology), progress in quantum field theory (“formal” theory)¹, recognition of the role of symmetries (model building), and invention of new calculational methods (perturbative techniques and lattice QCD) were all crucial in developing the Standard Model (SM). Theorists have been the drivers in asking the questions which lead beyond the SM, including: the origin of the hierarchy between the scales of the weak and gravitational interactions, the physics of flavor, the origin of neutrino masses, the particle nature of dark matter, inflationary cosmology, baryogenesis, resolving the tension between quantum mechanics and gravity, and identification and explanation of dark energy.

Since the Second World War, the U.S. has been a world leader in theoretical particle physics. This has remained the case in recent years, despite the move of the Energy Frontier to CERN. This leadership results from a combination of university-based and national lab-based research, supported principally by the Department of Energy and the National Science Foundation.

The DPF Theory Panel was formed with the goal of understanding both the scientific problems and opportunities of the next decade, as well as the challenges involved in sustaining a first-class program in the U.S. Specifically, the panel’s charge included:

1. Enumerate areas of opportunity in particle physics theory research in order to set forth a vision for theoretical high energy physics for the next several years.
2. Establish a range of funding needs for individual PI’s to sustain an effective program (students, postdocs, travel, summer salary, equipment needs).
3. Examine roles and relative funding levels of university and national lab theory groups. This includes examination of Comparative Review processes for these two categories of research groups.
4. Consider if suitable mechanisms are in place to assure funding of young researchers.

To address these questions, we solicited comments and suggestions from the community, held town meetings at the BNL and KITP pre-Minneapolis workshops leading up to the main meeting in Minneapolis, and held two parallel sessions and a plenary session in Minneapolis.

Our basic conclusions and recommendations can be simply summarized. The U.S. should maintain a vigorous research effort in theoretical particle physics, ranging from perturbative and non-perturbative QCD studies,

¹While the term “formal” is often used, we prefer to refer to such work as “research on foundational questions” or “field theory, quantum gravity, and string theory”, and will do so in the rest of this document.

to collider phenomenology, to model building, cosmology, and research in foundational areas. We will provide more detailed recommendations in section 5.7.

Theorists and theory straddle the Energy, Intensity and Cosmic Frontiers throughout the international particle physics program, and have an important role to play in the developing international particle physics program. Theorists continue to provide the overarching intellectual framework for these programs. They also more directly help to enunciate the physics cases for future experimental facilities, and to define many of the analyses to be performed at the LHC and beyond.

The SM is likely complete, in the sense that it is a consistent theory up to very high energies. It is now the background to all of our experimental explorations. There are many questions beyond the SM, and the field now confronts new and serious challenges. In going beyond the SM, we are entering an environment with both high potential rewards and high risks. Theorists will continue to propose possible new phenomena relevant to all three Frontiers — Energy, Intensity and Cosmic — which will help guide future experimental studies and will respond to them. They will extend the structures of quantum field theory and quantum gravity, providing clues as to the possible underlying laws of nature, and they will continue to propose explanations for the hierarchy of energy scales, the origin of dark matter and similar mysteries, with implications for existing and proposed experiments. As they have for decades, they will provide critical input to analysis of experimental data.

Trends in funding endanger this vital enterprise. The U.S. funding agencies anticipate significant future reductions in funding for theoretical physics, and these are likely to harm the depth, breadth, and world leadership of this program. In FY 2013, support for particle physics in the NSF was cut by 10-12%. The DOE is facing a declining budget and is increasing the fraction of its budget devoted to new experimental and accelerator projects at the expense of research funding. Unlike many elements of the experimental effort, theoretical research does not lend itself to “project” designation, so the impact of this shift on theory is more pronounced. The consequences of these cuts may be amplified, if applied uniformly to research groups. This is particularly true for postdocs, as hiring a research associate requires some minimum funding level, and many groups are likely to find their funds fall below this minimum. Additional cuts to graduate student support will lead to shrinking numbers of individuals admitted to study particle theory, as well as a longer time to Ph.D. for those who remain. This will have a significant negative impact on our field for years to come. Preserving postdoctoral support at the present level is essential for the health of the field, since postdocs are the drivers of many challenging problems, and also the future leaders of the field. The recent adoption of a comparative review process in the Department of Energy has the potential to allow more targeted cuts, allowing for some control of the numbers of postdocs and students, but even then, serious harm will occur if current budget trends continue.

5.2 Particle physics and theoretical particle physics today

The past two years have seen a major triumph of theoretical and experimental physics working hand in hand. A scalar particle has been discovered in the mass range expected for the Higgs boson from analyses of precision electroweak data. This is an extraordinary experimental accomplishment. But this success also rests on our exquisite understanding of QCD, and our ability to predict, for the simplest Higgs theory, the production rate and the decay branching ratios with great precision. Early indications are that this particle is in fact the Higgs predicted by the simplest version of the SM, where simple has a precise meaning: it is the minimal number of degrees of freedom consistent with the symmetries of the strong, weak, and electromagnetic interactions, and the principles of quantum mechanics, locality, and special relativity.

Theorists are playing an important role in upcoming tests of the SM interpretation of the Higgs boson, which will be central to the continuing LHC program and to a possible International Linear Collider (ILC). Not only are they providing the required SM calculations for production and decay rates, new ways to test the properties of the Higgs boson, and alternative models against which these measurements can be tested, but they have a framework in which to quantify any would-be discrepancies between the simplest Higgs theory and experiment (using the methods of “effective field theory”).

On a slightly longer time scale, we have seen, over the the past two decades, many other successes of the SM. Among the most striking in recent years has been the experimental verification that the CKM phase explains the observed CP violation in the K and B meson systems. This is a triumph for the SM and for experimental ingenuity. However, it also reflects the development, over the past three decades, of a spectrum of theoretical tools, including: the general framework for weak interaction phenomenology employing the operator product expansion; the recognition of the incisiveness of time-dependent CP asymmetries; novel methods for understanding heavy quark systems; and an extraordinary increase in the ability to compute real physical quantities from lattice gauge theory. Indeed, the progress in lattice gauge theory over the past decade has been astounding, including, for example, calculations of the hadron spectrum yielding precision measurements of light quark masses and computation of decay amplitudes necessary for the precise extraction of CKM parameters from data.

Another success for theory (together with experiment) has been the verification of SM predictions with amazing precision in collider experiments. Experiments at LEP and SLC confirmed the electroweak couplings with great accuracy and tested QCD in a clean environment. Theorists and experimentalists have combined data from HERA and the Tevatron to provide precise knowledge of the momentum distributions of quarks and gluons in the proton. This knowledge, together with improvements in the theory of hadron collider processes, has enabled SM tests and measurements at the Tevatron and the LHC with a precision that was unthinkable even a decade ago. The W mass measurement at the Tevatron, and the tests of the SM electroweak vector boson production rates and kinematic distributions, as well as the top quark pair-production cross sections at the Tevatron and the LHC, are just a few examples of this progress.

The past decade has also seen confirmation of the existence of neutrino masses and the measurement of neutrino oscillation parameters. Theorists have played an important role in every aspect of these developments. They first laid the theoretical foundation for understanding neutrino oscillations, in vacuum and including matter effects, and provided natural frameworks for explaining the extremely small values of the neutrino masses. Theorists also provided a detailed understanding of issues arising in the detection of these phenomena using neutrinos from the Sun, the atmosphere, nuclear reactors, and accelerators.

Our seemingly complete understanding of physics, down to distance scales of order 10^{-17} centimeters, has brought other questions about the laws of nature into sharp focus. Again, theory plays a crucial role in delineating the questions and in suggesting possible answers. Among the questions which theorists have helped to identify and sharpen are:

1. What is the origin of the great disparity in the energy scales associated with the weak and gravitational forces? This is the hierarchy problem. It has two pieces: 1) why is there such a large disparity 2) the problem of fine tuning: any new energy threshold much above the masses of the W and Z bosons, such as the Planck scale or unification scale, tends to destabilize the Higgs boson mass through quantum corrections.
2. Where do the parameters of the SM originate?
3. Do the strong and electroweak forces unify at some energy scale?

4. Why is the strong interaction CP conserving? Is this accounted for by an axion field, and does this axion constitute some or all of the dark matter?
5. The quarks and leptons present many mysteries. Why are there repetitive generations? What accounts for the hierarchical structure of the masses and mixings of the quarks and charged leptons?
6. The discovery of neutrino mass has raised new questions. What is the energy scale associated with the generation of neutrino mass? Are neutrinos their own anti-particles?
7. The observed CP violation in the SM is insufficient to account for the baryon asymmetry of the Universe. What phenomena might account for this? Might they be accessible to experiments at the Energy or Intensity Frontiers?
8. What is the identity of the dark matter which makes up 25% of the energy density of the Universe?
9. What is the origin of the dark energy which makes up 70% of the energy density? Why is it just becoming important at the present epoch of the Universe?
10. What caused the inflationary epoch, and how did the Universe end up in its current state?
11. What is the nature of the quantum theory of gravitation?
12. From what set of principles or structures do the laws of nature originate?

Theorists are vigorously considering all of these questions. Some of them point to particular energy scales and types of experiments. Others questions in the list are more speculative. These questions straddle — as do the interests of most theorists — the Energy, Intensity, and Cosmic Frontiers. Proposals for physics beyond the SM include:

- Supersymmetry, a possible new symmetry of nature relating fermions and bosons, to understand the hierarchy between the Planck scale and the weak scale. In many realizations that theorists have considered, one might have expected its discovery in the first run at the LHC. Still, it remains one of the more plausible explanations, and is the subject of continued experimental and theoretical study.
- Composite Higgs models, technicolor, and Randall-Sundrum models. These provide alternative possible explanations of the hierarchy problem, and are the subject of ongoing experimental searches.
- Dark matter candidates. Weakly interacting massive particles (WIMPs) are natural in supersymmetry and several other theoretical structures; axions were invented to understand the strong CP problem. These are both topics of ongoing theoretical work and extensive experimental searches.
- String theory and other ideas for a quantum theory of gravity. String theory in particular provides a promising model for the unification of gravity and the other forces in a consistent quantum mechanical framework. It has also provided new tools for addressing problems in quantum field theory and in disparate areas of physics including heavy ion physics and condensed matter physics. It has suggested new principles (holography) and inspired ideas for particle phenomenology and physics beyond the SM. It has also inspired the invention of powerful techniques for computing scattering amplitudes.
- Leptogenesis: This is an attractive paradigm for explaining the baryon asymmetry of the Universe, which has an intimate connection with the origin of neutrino masses. Plausible indirect evidence for this mechanism would be the discovery of CP violation in the neutrino sector, the subject of tests in forthcoming long-baseline experiments. Other ideas for baryogenesis have different potential consequences.

In addition to raising questions, theorists have developed powerful perturbative and non-perturbative techniques for performing calculations essential for understanding collider experiments. Theoretical precision that was previously viewed as impossible is now routine. This precision has been, and will remain, crucial in both understanding SM physics and uncovering evidence of physics beyond the SM. This includes understanding Tevatron and LHC data, as well as results from BES III and Belle-II.

Upcoming results from the LHC or from experiments at any of the frontiers in the next decade might provide answers to some of these questions. For example, the hierarchy (or “naturalness”) problem provides the principal argument that new physics should appear at the TeV-scale, and it has inspired a range of proposals for physics beyond the SM.

The results of the first 25 fb^{-1} of integrated luminosity collected at the LHC have placed many of these questions in a new perspective. The current data are consistent with the SM, with a single Higgs doublet and a corresponding particle with mass 126 GeV. Many ideas about the hierarchy problem predict the existence of new colored particles to cancel large contributions to the Higgs mass from the top quark; there are now strong limits on such particles with masses below a TeV. Many specific proposals for new physics at the TeV-scale have been severely constrained by LHC searches, and the paradigm of naturalness has come under increasing scrutiny. However, there remains significant space to explore.

Answering theoretical questions will require experiments across the Frontiers. Intensity Frontier experiments and precision measurements at the Energy Frontier may provide evidence for new physics at slightly higher energy scales. Cosmic Frontier experiments may yield a WIMP candidate or further constrain this paradigm. For other questions, experimental input is likely to be limited for a long while, and theorists will try to put together answers, at first tentative, that combine experimental knowledge with theoretical insight. Aspects of the frontier classification scheme, and its relevance to theory, will be discussed in Section 5.4 below.

5.3 Prospects for advances in theory

During the various Snowmass workshops, and especially in Minneapolis, we asked theorists with a broad range of research interests to outline recent developments and to enumerate areas of opportunity likely to witness significant progress over the next decade. We present some highlights of these discussions. As with most theoretical ideas, several topics discussed below have overlapping boundaries. This list does not encompass all active areas of theoretical particle physics, but does represent a significant fraction of current activity. It is important to note that theorists not only cross the frontiers, but also the areas of activity we enumerate below.

Here we consider five broad areas: phenomenology, field theory calculational methods, model building, astrophysics, and cosmology, and string theory, quantum gravity and foundational questions. These are broad topics; within phenomenology, for example, we consider collider phenomenology, electroweak physics, neutrino physics, heavy quark physics and additional topics; in field theory, we include lattice gauge theory as well as perturbative and semiclassical methods; model building includes models of flavor, supersymmetry, grand unified theories (GUTs), and large or warped dimensions.

5.3.1 Phenomenology

Particle phenomenology plays the crucial role of linking experiments with the various aspects of theory, including model building, perturbative and lattice QCD, and more foundational issues. Over the past decades

it has been central to the extraction of the parameters of the fundamental Lagrangian from experimental data. It played a vital role in the success of the Tevatron and LHC programs, leading to precision determination of the top quark properties and the discovery of the Higgs boson. The successful operation of the B factories led to the confirmation of the Cabibbo-Kobayashi-Maskawa paradigm for quark mixing and CP violation, and the ever more precise determination of the parameters of the flavor sector. The phenomenology community has proposed many new kinematic variables now used on a daily basis by researchers in the LHC collaborations. Collider phenomenologists have also catalyzed an important new area of study at hadron colliders: jet substructure and various other jet and event properties, such as N -jettiness and N -subjettiness, have led to new experimental strategies for searching for pure-jet decays of heavy particles and of the Higgs boson. Phenomenology has also played a crucial role in the direct and indirect dark matter detection experiments. In particular, theorists working on dark matter have proposed that the proper framework of analyzing direct detection experiments is in terms of the couplings of a low-energy effective theory, broadening the possible interpretations of these experiments. Phenomenology will remain critical to the interpretation of continuing and upcoming experiments and for developing plans for the future.

5.3.1.1 Flavor physics

The study of flavor physics was an integral part of the development of the SM. The existence of the charm quark was predicted based on the suppressed decay $K_L \rightarrow \mu^+ \mu^-$. The fact that ν_μ is a distinct state from ν_e was inferred from the absence of $\mu \rightarrow e \gamma$ decay, and the top quark was predicted to be heavy from the measurement of $B_d - \bar{B}_d$ mixing. In the quark sector, the Cabibbo-Kobayashi-Maskawa mixing and CP violation paradigm was confirmed with data from BaBar, Belle, and the Tevatron. These measurements are quite impressive, but experimental and theoretical uncertainties still leave some room for contributions to processes in the B system. Theory has played a crucial role here, beginning with the idea of three generations of quarks, to heavy quark effective field theory and lattice QCD, which provided important form factors with a few percent uncertainty.

Proposals for physics beyond the SM are strongly constrained by flavor physics, whether or not they address directly the big questions of the subject. The scale of new physics that contains generic flavor violations is constrained by present data on rare processes in the muon, kaon, B and D -meson systems to be greater than about 10^4 to 10^5 TeV. For example, the spectrum of supersymmetric particles must exhibit special features. For example, the squark masses might be nearly the same (degeneracy), or they might be approximately diagonal in a basis in which the fermion masses are (alignment), if they are within reach of currently conceivable experiments. In the near future, LHCb and Belle-II will test non-standard theories of quark flavor with higher precision, and theorists will be engaged with analysis and interpretation of the results. Electric dipole moment limits on atoms, nuclei, the neutron and the electron severely constrain new sources of TeV-scale CP violation. Theoretical ideas which attempt to explain the pattern of fermion masses and mixings also typically predict new sources of flavor violation. The scale of flavor dynamics could be low, comparable to the TeV scale, or much higher. If it is not too high, experiments can test these ideas. The theory of flavor is relatively clean in processes such as $\mu \rightarrow e \gamma$ decay and coherent $\mu \rightarrow e$ conversion in nuclei (once the source of flavor violation is specified), but not so clean at the required level of accuracy in muon $g - 2$ for which there is currently a 3.6σ discrepancy between theory and experiment. For the next round of the $g - 2$ experiment, lattice calculations may decrease the theoretical uncertainty so that experimental results can be better compared to the SM prediction.

5.3.1.2 Neutrino physics

Neutrino theory sits at the intersection of particle physics, nuclear physics, astrophysics and cosmology, and as such provides great opportunities and interesting challenges. These are especially relevant today, given that a significant part of the U.S. experimental program over the next decade is likely to be focused on measurement of neutrino properties.

The experimental neutrino physics program has had tremendous success over the past fifteen years, beginning with the discovery of neutrino oscillations in solar and atmospheric neutrinos. Theory has played an important role in each step of these developments, from the calculation of neutrino fluxes from the Sun and from the atmosphere, to the recognition of the importance of matter effects including MSW resonances in neutrino propagation, to the calculation of various neutrino cross sections, to elucidating mechanisms for generating small neutrino masses.

This has focussed the field on measurement and understanding of the neutrino masses and mixings (the PMNS matrix), as well as fundamental issues such as whether neutrinos are their own antiparticles (Majorana particles), the scales of neutrino mass generation, and the possible role of neutrinos in the creation of the asymmetry between matter and antimatter (leptogenesis). The recent measurement of one of the neutrino mixing parameters, θ_{13} , in reactor- and accelerator-based experiments is a major step in pinning down the PMNS matrix. Discovery of CP violation in neutrino oscillations, as anticipated in forthcoming long baseline neutrino experiments in the U.S. and abroad, could provide indirect hints for leptogenesis as a plausible mechanism for generating the baryon asymmetry of the Universe. To unravel answers to the fundamental questions, theorists must undertake the time-consuming task of combining different experimental results, each with its own uncertainties, nontrivial correlations, and parameter degeneracies. Often the needed calculations, such as neutrino-nucleus cross sections, require a strong background in nuclear physics. The three-flavor oscillation paradigm needs to be tested by over-constraining parameters, which in itself is a time-consuming endeavor, requiring years of dedicated efforts. Currently, the U.S.-based theory community working on such topics is relatively small. There is an ongoing effort to enhance this community, led by a Neutrino Theory Task Force. This Task Force proposes new initiatives to attract more researchers into this area, in collaboration with experimentalists and nuclear theorists.

5.3.2 Field theory calculational methods

5.3.2.1 Perturbative and effective field theory methods

In a variety of circumstances, it is possible to extract important results from experiment only through very precise theoretical predictions. For example, a more precise understanding of the physics of heavy hadrons has been enabled by several breakthroughs: first, the operator product expansion for weak transitions; and later, the successive development of heavy quark effective theory, nonrelativistic QCD, and most recently, soft collinear effective theory (SCET). These developments all represent the construction of effective field theories (EFTs) that arise from a separation of physical scales. They have had great practical payoff for the experimental study of B mesons, charmonia, and bottomonia.

Monte Carlo event generators, such as Isajet, Pythia, Herwig, Sherpa, Alpgen and Madgraph, were developed by the theory community and have become indispensable tools for experimentalists to simulate events at all high-energy colliders, but particularly hadron colliders. There has also been enormous progress in computing cross sections relevant to both backgrounds and signals at colliders, at higher order in the strong coupling constant, α_s , and even including electroweak corrections. Next-to-leading order (NLO) QCD predictions

for the LHC are now routine and automated in programs such as MCFM, Rocket, CutTools, GoSam and BlackHat, based in part on an improved theoretical understanding of loop amplitudes. The next order (NNLO QCD) is now in sight for a variety of LHC processes as well, where it will make even more precise experimental tests possible. This progress at fixed order in the coupling has been accompanied by a greatly improved understanding of how to resum large logarithmic corrections: either analytically using, for example, SCET; or within Monte Carlo simulations by merging fixed-order results with parton showers.

Precise parton distributions are essential for all LHC physics, but it was only possible to obtain these once NLO and especially NNLO computations were combined with high precision data from HERA and elsewhere. QCD has truly come into its own as a precise theory for hadronic collisions. One also cannot overstate the importance of precision electroweak theory. The triumph of precision electroweak measurements in predicting the Higgs mass would not have come about if the quantum corrections to the weak mixing angle and weak boson masses had not been known to two loop order. With only the one-loop terms, the inferred Higgs boson mass would have been about 500 GeV higher than where the Higgs was found. Even better theoretical precision will be needed to match the experimental capabilities of an ILC running at the Z pole. Despite all this progress in precision applications of continuum quantum field theory, there is still plenty of room for new theoretical developments, both those with practical applications to experiments, and those that enlarge our understanding of quantum field theory and of the structure of physical law.

5.3.2.2 Lattice QCD

Lattice QCD is our main tool for understanding non-perturbative aspects of QCD. Numerical evaluation of hadronic matrix elements is crucial to progress in many areas of particle physics. The last decade has witnessed an enormous increase in the power of lattice methods. Lattice QCD has enabled the computation of weak matrix elements at the percent level, along with precise determination of the QCD coupling α_s and the quark masses. Computations of the decay constants of the D and D_s mesons provide striking examples of recent progress. Other applications include: 1) studies of condensed matter physics 2) work by the Columbia and Edinburgh lattice QCD groups in collaboration with IBM that resulted in the Blue Gene series of supercomputers 3) the MILC collaboration code has been used as a benchmark for many computer purchases. Breakthroughs are expected over the coming years, including: determination of m_b and α_s to 0.25% or better (important for precise calculation of Higgs decay modes at the proposed ILC); further calculations of weak matrix elements and form factors needed for tests of the CKM paradigm; calculation of the hadronic contributions to the muon magnetic moment, important for the interpretation of the upcoming $g-2$ measurement; nucleon matrix elements needed for prediction of nucleon decay and $n-\bar{n}$ mixing; form factors for quasi-elastic neutrino-nucleon scattering; calculations of strongly-coupled models of physics beyond the SM; and lattice calculations of supersymmetric models. Calculations are increasingly done with up and down quark masses at the average of their physical values. Future calculations will likely include dynamical electromagnetism and isospin breaking. Lattice gauge theory needs increased support at universities as well as laboratories, particularly in order to ensure training of a future generation of students.

5.3.3 Model building

Model building connects theory to phenomenology. Model builders discover new mechanisms, new phenomena, and inspire experiments and new experimental analyses. Here we distinguish TeV-scale model building, and model building involving higher energy scales, such as GUTs, models for neutrino mass and flavor, and string model building. Much of TeV model building has been driven by efforts to solve the hierarchy problem, but an important motivation is enumerating possible signals for experiments in the complicated

environments of the Tevatron and LHC. This will continue to be an important driver of theoretical work in this area in the coming decades. Higher scale model building yields possible solutions to many of our outstanding questions. In some cases, it has led to model building of relevance to lower energy physics, and/or has driven experiments which have shed light in other areas.

5.3.3.1 TeV-scale model building

For TeV model building, the idea of supersymmetry, which can potentially resolve the hierarchy problem, has been an important driver of experiments. Supersymmetry predicts an array of new particles, often with properties which are a priori known, and this has led to a variety of experimental searches looking directly for these new particles. Many of the searches for dark matter via direct detection have focused on the lightest supersymmetric particle, which is a natural candidate for dark matter. The theory of large extra spatial dimensions stimulated short-distance tests of Newton's laws. The axion hypothesis led to microwave cavity experiments and is pointing towards new types of experiments. Warped extra-dimensional models led to searches for the Kaluza-Klein gluon, which in turn was one of the inspirations for new techniques related to jet substructure. New types of models for dark matter led to new classes of experiments searching for dark sectors. Models of leptogenesis provide a very strong motivation to map out the parameters of the neutrino sector in great detail, especially the CP properties.

In the LHC era, in which trigger bandwidth is critical and backgrounds are large, it is important to have models that cover as many signatures as possible. For example, R -parity-violating supersymmetric models inspire new searches for colored states decaying without missing energy. Split supersymmetry suggests quasi-stable gluinos and leads to searches for out-of-time decays. Other models lead to quasi-stable charged particles or displaced vertices. Having a large number of exotic models significantly expands the range of search strategies at the LHC experiments, reducing the likelihood of missing critical signals of new phenomena. This synergy between experiments, phenomenology and model-building can work in both directions. Hints of new physics suggested by experiments, for example the muon $g - 2$ anomaly, have greatly influenced model building as well as phenomenology.

5.3.3.2 Flavor model building

The origin of the quark, lepton, and in recent years the neutrino mass matrices are among the great mysteries in particle theories. Theorists have explored a variety of ideas for understanding the hierarchies observed in the quark and charged lepton mass matrices. These have included, but are hardly limited to, flavor symmetries with small breakings, warped and extra dimension models, and composite and technicolor models. The evolving determination of neutrino masses and mixings poses challenges for many of these ideas, but at the same time, neutrino model building ties closely to possible mechanisms for the origin of the matter-antimatter asymmetry.

5.3.3.3 Grand unified theories

GUTs are proposals to unify the strong, weak, and electromagnetic interactions into a single force. They also unify quarks, leptons, anti-quarks and anti-leptons of each family into common multiplets. Such an arrangement would explain the coexistence of quarks with leptons, their quantum numbers, and the quantization of electric charge. With the assumption of low energy supersymmetry, the three gauge couplings of the SM are found to unify rather precisely when extrapolated to high energies at a scale $M_X \approx 2 \times 10^{16}$ GeV, which may be argued to be a piece of indirect evidence for grand unification and for supersymmetry.

This unification is the principal direct evidence for GUTs. But the study of these theories has had other impacts. GUTs stimulated proton decay experiments, which helped solve the solar neutrino problem, and led to the discovery of atmospheric neutrino oscillations, and to the detection of neutrinos from a supernova.

Even without supersymmetry, the gauge couplings can unify if the GUT permits an intermediate symmetry, as happens in SO(10). SO(10) theories also predicted the existence of the right-handed neutrino, which plays an essential role in the generation of small neutrino masses via the seesaw mechanism. Unification of matter into common multiplets implies that baryon number is not conserved, and that the proton should decay. In the context of low energy supersymmetry, these theories predict that the decays $p \rightarrow \bar{\nu}K^+$ and $p \rightarrow e^+\pi^0$ should occur with rates that are likely to be within reach of the next-generation proton decay search experiments.

Proton decay searches probe distance scales of order 10^{-30} cm, something not possible by any other means. If discovered, proton decay would be a landmark discovery in science.

5.3.3.4 String theory model building

There are many challenges in connecting string theory to the real world, but consideration of string models has profoundly influenced ideas for particle physics models. Some of these ideas will be mentioned below in Section 5.3.5, but further examples include ideas for understanding light Higgs bosons, theorems demonstrating the absence of continuous global symmetries, the role of discrete symmetries, ideas for the origin of repetitive generations, a natural setting for the Peccei-Quinn solution of the strong CP problem, candidate fields and mechanisms for inflationary cosmology, and alternatives to the conventional hot Big Bang theory at early times. We anticipate further progress in this field, exploiting theoretical developments and responding to experimental discoveries and exclusions.

5.3.4 Astroparticle physics and cosmology

The combination of a cosmological constant with a distribution of cold dark matter particles (Λ CDM) after an initial period of inflationary expansion is now considered the SM of cosmology. This model has provided a simple explanation of diverse physical phenomena in the Universe. However, even if this simple picture continues to be borne out, there is much still to explain, including the reason the cosmological constant has the value it does, the origin of cosmological density perturbations, and the nature of dark matter. Theorists have proposed a range of dark matter candidates; indeed, the need for cold dark matter has become a criterion for model building and particle phenomenology. Astrophysical and cosmological theory also plays a key role in the interpretation of a wide variety of current and future measurements, including dark matter experiments, massive galaxy surveys, gravitational lensing observations, and cosmic microwave background data. The hypothesis of inflation came from particle theory, emerging from ideas about grand unification and monopoles. It is now a central part of the Cosmic Frontier and of the Theory program. Recent data from WMAP and Planck have further constrained possible inflationary models. Given the limited amount of data we are likely to have, theoretical ideas will be critical if there is ever to be a single, compelling model underpinning inflation. Such ideas might come, for example, from string theory, or from connecting the degrees of freedom of inflation with those accessible to experiments (e.g., excitations in large extra dimensions or associated with supersymmetry breaking).

5.3.5 String theory/quantum gravity/foundational questions

The SM developed, in significant part, from theorists delving into fundamental questions in field theory. These included the demands of renormalizability and unitarity and the related constraint of freedom from anomalies. Its development and full understanding required that theorists master previously unfamiliar topics in mathematics such as group theory, topology and the theory of fiber bundles. String theory grew out of efforts to understand the phenomenology of the strong interactions, and later blossomed into a unified approach to quantum mechanics and gravity. In particular, the resurgence of interest in string theory in 1984 followed the discovery of anomaly-free string theories that could incorporate the chiral gauge structure of the SM. Since then string theory has been a major source of new ideas in particle physics. It has provided insights into questions of unification, the strong CP problem, black hole physics, supersymmetry, the possibility of flat and warped extra dimensions, and much more. It has had an important indirect impact on particle physics by inspiring new computational approaches to ordinary perturbation theory.

One of the most important recent developments in string theory is the “AdS/CFT” correspondence, or gauge/string duality. This is the startling observation that a quantum gravity theory in Anti-deSitter space is equivalent to a conformal field theory at the boundary of the space. This idea has provided a fundamental new tool for the study of strongly interacting field theories. As such it has provided a new method of studying non-perturbative QCD, has motivated new computations in lattice gauge theory, has found important applications to heavy ion physics, where it was used to predict the viscosity to entropy ratio of the quark-gluon plasma, and is now being widely applied to problems in condensed matter physics.

As a theory of quantum gravity, string theory also has close ties to general relativity and there has been a fruitful interplay between string-theoretic methods and more conventional methods in the study of the properties of black holes. Supersymmetry, alone and in conjunction with techniques developed in string theory, continues to be a powerful tool for unraveling the dynamics of strongly-interacting gauge theories. String theory practitioners have also contributed to advances in our understanding of cosmology (e.g., the systematic study of non-gaussianity), models of flavor physics, and much more. String theory and supersymmetry have also had a broad impact in pure mathematics in areas ranging from algebraic geometry to number theory. It is likely that the ideas and techniques to which string theory has led will be critical to resolving many of the questions we have about nature at the deepest level.

5.4 Particle theory, the Frontier paradigm and cross-disciplinary research

It is worth stressing again that theoretical particle physics, and most theorists, transcend the frontiers of physics. QCD, the theory of the strong interactions, is an essential element in both the Energy and Intensity Frontiers. QCD computations are important for the Cosmic Frontier, for example in evaluating cross sections for the direct detection of dark matter. Perturbative and lattice QCD are essential for understanding experiments in both areas. Furthermore, from a theoretical point of view, the physics of heavy quarks straddles the Energy and Intensity Frontiers, and has received contributions from theorists with a broad range of interests. In building models to describe short-distance phenomena, theorists will consider the collider signatures of their theories, the constraints from and predictions for flavor changing processes, and possible electric dipole moments. They will almost invariably ask whether their models possess a dark matter candidate, and if so, what sorts of experiments might detect it. They will consider cosmological issues, including creation of the dark matter and the baryon asymmetry, mechanisms for inflation and possible distortions of the microwave background, and the like. String theorists have pointed out the possibility of

alternative cosmologies, in which the Universe was not hot in the past, and provided a variety of frameworks for inflation. They have also suggested that axion decay constants might be larger than conventionally supposed, inspiring new cosmologies and exploration of new search strategies. String theorists and others working on more foundational questions have provided tools for scientists working on heavy ion physics (and other areas outside of traditional particle physics), as well as increasing our knowledge of field theory in ways which have proven useful for perturbative and non-perturbative QCD, and for those building models which go beyond the SM.

In addition to overlapping several frontiers, research in particle theory influences, and is influenced by, other areas of physics. The DOE and NSF have historically supported a broad science program, and, as indicated above, this has had significant scientific rewards. The question has arisen whether the traditional breadth of scope of the U.S. program in elementary particle theory should be narrowed in fiscally challenging times. There has been much discussion, in particular, about subjects like AdS/CMT, the application of gauge/string duality to condensed matter systems, with some questioning why the DOE, in particular, should support this activity. We would respond that there is a long history of fruitful cross-pollination of ideas between condensed matter physics and particle theory. For example, the BCS theory of superconductivity, which we now recognize as the first example of spontaneous breaking of a gauge symmetry, provided direct inspiration for Nambu's Nobel Prize winning work on chiral symmetry breaking in strong interactions. The renormalization group and its revolutionary implications for our understanding of quantum field theory had its origins in particle theory. It was then combined with ideas of universality and scaling and applied to understand phase transitions in condensed matter physics; insights gained from these applications then filtered back into particle physics. In the modern era, insights gained from the study of topological objects like solitons and instantons have had an impact on both condensed matter and particle physics. The recent progress in the condensed matter physics of topological insulators owes much to the understanding of anomalies gained by particle theorists. Recently these connections have expanded through gauge/string duality to include both general relativity and string theory.

There has also been increasing interaction between particle theory and areas of pure mathematics, an area of research sometimes referred to as "physical mathematics." For example, there are burgeoning connections between number theory, geometry and the mathematical structure of scattering amplitudes. There has also been a resurgence of interest in the formal structure of supersymmetric gauge theories and their application to areas of mathematics, including knot theory and the structure of low-dimensional manifolds. Dualities in string theory have found a direct connection to elements of the Langlands correspondence, one of the main drivers of research in mathematics. These areas are further from DOE's traditional purview (NSF is able, in some cases, to share such efforts between its Math and Physics Divisions), but may also lead to new tools and ideas with applications to particle physics. They are being pursued by some of the most talented members of the younger generation.

We see many reasons that such activities should be supported. First, the questions which drive our field may require entirely new concepts and methods. As Dirac famously said in the paper in which he introduced the magnetic monopole, "The steady progress of physics requires for its theoretical formulation a mathematics that gets continually more advanced." Second, these subjects have many ideas and techniques in common that often find application in areas far from their original source. Finally, we must note that these connections are at the cutting edge of research in the more foundational aspects of the subject and attract many outstanding young theorists. We will lose these people to other areas or other countries if their research is not supported.

Theoretical physics is in many ways a unified field in which the U.S. has been the dominant force in the world. Our universities and advanced graduate programs still lead the world but are under severe threat from funding cuts. We currently attract many of the top students from abroad. These students not only receive a world-class education, but also become familiar with our culture of scientific excellence. Other

countries understand the power of educating the best minds in the world. The panel believes it would be a great loss if the U.S. gave up this role.

5.5 The roles of national laboratories and universities

A significant share of research in theoretical physics is performed in National Laboratories, with a roughly equal level of funding for labs and universities. The number of PI's in the labs is significantly smaller than in universities, and the funding balance results, in part, from the fact that lab PI's work throughout the year on research (and professional service), without obligations to teach and to university service.

The U.S. national labs are critical players in the national theory effort. They support excellent theoretical programs, which provide leadership in research on QCD and Higgs physics relevant to collider physics, and essential support for ongoing experimental analyses and planning for future facilities. Essentially all of the U.S. expertise in parton shower Monte Carlos resides at the labs, and they have a strong focus on lattice and perturbative QCD as well.

Concerns about declining funding have led some to suggest an examination of the balance of lab and university funding, and we have discussed this among ourselves and with the agencies. These individuals have pointed out that, while the national labs represent about 20% of PI's nationwide and a comparable fraction of total output (as measured by publications, impact factors and similar measures) they represent about 48% of the total expenditure of DOE theory funds. This results from the fact that universities, as knowledge producers as well as educational institutions, pay for much of the research time of their faculty. This balance has served physics education and the field of particle physics well for many decades.

Still, declining funding has serious implications for research in labs and universities, both for the present cadre of researchers and research activity, and for future hiring in the field. We encourage HEPAP to establish a subcommittee to look at the complex question of university/lab balance.

5.6 Sustainability: theory funding

The U.S. has sustained a vigorous program in theoretical particle physics for many decades. Support for this effort has come principally from the DOE and the NSF, with modest additional funding from private sources. We have had extensive conversations with representatives of the DOE and NSF, and have been gratified by their strong appreciation of the value of theory. This is, however, a challenging period for funding of all aspects of particle physics; we have focused on a number of issues particular to theory. We are especially concerned, as we will describe in greater detail in this Section, with the consequences of anticipated future cutbacks for postdoctoral fellows and graduate students in national laboratories and universities, and for hiring of new faculty in theoretical particle physics at U.S. universities.

DOE and NSF funding has supported the following activities central to the theory effort:

1. **Training of students.** U.S. universities still lead the world in attracting the strongest students in many subfields of theoretical particle physics. Besides direct support for graduate student research through grants to PI's, support has also been provided through the DOE graduate student fellowship and the Fermilab visiting student program, and the LHC-TI graduate student fellowship funded by the NSF. The Theoretical Advanced Study Institute (TASI) summer school (for many years held at UC Boulder) has provided, since 1984, a thorough training for advanced graduate students in modern

particle theory concepts and methods. It is an extremely valuable component of the U.S. particle theory funding portfolio, and is the most highly regarded and competitive summer school anywhere in the world. Many TASI students have gone on to become leading researchers in particle theory. The federally supported training of graduate students directly supports the future of particle theory in the U.S. However, the effects of training students are broader than simply their impact on particle physics. Many students eventually leave the field, typically put their training to use in the “knowledge economy” of the U.S., whether it is in other fields of research, in teaching at undergraduate institutions, Silicon Valley, Wall Street, or other technical endeavors.

2. **Salaries of postdoctoral fellows.** Postdocs enable much of the particle theory research performed in the U.S. and constitute the future leadership of the field. While a substantial fraction go on to positions in universities and national labs, others move out of particle physics and contribute to the broader technical work force in the U.S.
3. **Travel to conferences and workshops and for collaboration.** Even in the internet era, face-to-face communication between theorists is essential for propagating ideas to others and for developing collaborations.
4. **Summer salary for investigators.** Summer salary frees faculty from teaching responsibilities in summer months and facilitates concentrated research time. The case for hiring faculty in particle theory at U.S. universities is driven in part by the recognition that they play an important role in nationally supported research. In addition to taking on teaching in the summer to supplement their incomes, faculty lacking summer salary are often assigned increased teaching loads during the academic year.
5. **Theoretical physics in the national laboratories.** Traditionally, the DOE has supported strong research groups at its national laboratories that have performed theoretical research closely related to experiments in particle physics. These theory groups have performed original research and trained postdocs and students. There has been a strong focus on relevance to, and support for, experimental activities, particularly those related to on-site accelerator-based facilities, but also including more general planning of large national and international facilities. While there is at present only one single purpose national laboratory dedicated to particle physics (Fermilab), the other labs maintain research groups in experimental particle physics working on the LHC detectors, on dark matter experiments worldwide, on the LSST, and other experiments. These efforts benefit from the presence of laboratory theory groups.
6. **Institutes running extended workshops.** A very important component contributing to the vitality and success of U.S. particle theory is the set of workshops run by the Aspen Center for Physics (ACP) and the Kavli Institute for Theoretical Physics (KITP at UC Santa Barbara), both partially funded by the NSF. Their workshops focus on a particular topic of current interest. An Aspen workshop typically runs for 3–5 weeks, with around 30 participants per week, while a KITP workshop might run for a few weeks or a whole semester. These programs have become major venues for intense exchanges of ideas, and canalization of new projects. KITP programs provided the impetus for much of the early progress in AdS/CFT, for the development of new methods for perturbative computations, and for numerous other activities across the field. The ACP hosted the discovery of anomaly cancellation in string theory, a major impetus to the field, and was the place where the original idea for the e-print archive arXiv was formulated. Numerous projects in model building, neutrino physics and collider phenomenology had their genesis at the Aspen Center. These institutes have become so successful that many other countries (and subfields) have put similar institutions in place. Examples include the Galileo Galilei Institute (GGI) in Florence, Italy, the Munich Institute for Astro-and Particle Physics (MIAPP) in Munich, Germany, the Mainz Institute for Theoretical Physics (MITP) in Mainz, Germany, the Kavli

Institute for Theoretical Physics China (KITPC) in Beijing, China, as well as the Institute for Nuclear Theory (INT) in Seattle funded by the DOE Nuclear Physics program, and the Simons Center for Mathematical Physics at Stony Brook. In order to maintain the ACP's and the KITP's status as the world-leading institutes for extended workshops, it is very important to keep funding these institutes at appropriate levels, allowing future generations of theorists to take advantage of these excellent resources.

This formula for supporting particle theory in the U.S. has been extremely successful. The theory program in the U.S. has arguably been second to none for many decades. With a changing funding climate, this model is at risk. Funding for DOE-funded groups is expected to decline several percent per year for the next several years; the NSF has seen a severe decline, as we have mentioned, in 2013 alone. This has a number of implications:

1. **Decline in support for graduate students.** In most university groups, particle theory students are currently serving as teaching assistants (TAs) during significantly more than half of the academic terms in which they are enrolled. These TA duties lengthen the time required to complete a Ph.D. thesis, with detrimental effects on education and careers.
2. **Decline in support for postdoctoral fellows.** Postdoctoral fellows play a vital role in particle theory groups, and the postdoctoral period is a crucial one in a research career. To prove that he or she qualifies to become a permanent member of the research community, a postdoctoral researcher must be open to new directions, invent or develop new concepts or methods that push the state of the art in some area, and evolve a unique, personal outlook within the research enterprise. These experiences often guide the researcher throughout his or her whole career. Developing a distinct professional identity is a far greater challenge than that of completing a PhD project with one's adviser. Many of those who succeed at the first level cannot achieve this one; others emerge because the brilliance of their independent work. With the current and anticipated future funding cuts there is a possibility that the number of postdoctoral positions might shrink drastically in a short period of time. Any appreciable decrease in the pool of postdocs would have a significant negative impact on the future of the field.
3. **Severe restrictions on travel.** Travel budgets at universities and the national labs are shrinking substantially. Senior researchers are often forced to choose between travel essential for their research and travel by postdocs and students. It should be noted that, in addition, the approval process at national labs has become increasingly cumbersome and often is now hindering healthy exchanges of scientific ideas.
4. **Elimination of Laboratory Visitor Programs, Fermilab and DOE student fellowships.** One of the roles of theory groups at national labs is to be the national centers of scientific interaction. However, the recent elimination of visitor programs at national labs makes this impossible. This is in stark contrast with CERN, where the theory group is truly the center of European particle theory, mainly due to its visitor program and focused theory institutes. The very successful Fermilab visiting student program ("Fermilab Fellowship for Theoretical Physics") faces an uncertain future beyond the 2013-2014 academic year. Similarly, the DOE graduate student fellowship program has been suspended.
5. **Cap on university summer salaries.** The DOE has instituted a \$15K/month cap per investigator for summer salaries, which was also implemented this year in the NSF theory program. This effects only more highly paid senior faculty, and is preferable to further cuts in postdoc and student support. Significant cuts beyond this level could have negative effects on more junior faculty and overall productivity.

6. **The move towards a higher fraction of projects within the DOE has led to disproportionately large cuts in the theory budget.** At the recommendation of its 2010 Committee of Visitors, DOE HEP is moving towards increasing the fraction of projects in its funding portfolio to a level close to 20%. This is clearly the correct general decision: a vibrant HEP experimental program has to have a large fraction of the budget committed to projects, rather than to general research. However, the implementation of this move has also included the theory budget, which has no projects. This has led to a significantly higher effective cut in support for theory than in some other areas.

Each of these items has significant detrimental implications for the future of the field. Students make significant contributions to the research enterprise, both through their own work and as a significant source of new ideas and stimulation for their mentors; at the same time, it is the training of students that insures the future health and vibrancy of the field. Decreasing the number of postdocs will have a significant negative effect on research productivity, while again shrinking the pool of future investigators. Postdocs, particularly, bring fresh ideas to the research enterprise, and typically bring skills and a willingness to learn new techniques and methods. We are concerned that even modest funding decreases will mean that many groups will drop below threshold to sustain postdoctoral positions, so that a 10% decrease in funding over one to two years could translate into a much larger decrease in the number of postdocs. In the case of graduate students, in the past, decreases in funding could be ameliorated with increased TA responsibilities. While this would slow student progress and decrease productivity, it would at least provide a means of support. However, particularly in public institutions, the number of TA positions has been decreasing in many cases, closing this option.

This past year, the DOE instituted caps on summer salaries, and the NSF is following suit. We agree that this is preferable to further cuts in student and postdoctoral support, but it should be noted that still lower caps will have implications for research productivity, particularly if they reach the level of junior faculty (assistant or associate professor salaries). Many researchers may have to supplement their income with further teaching or other responsibilities in the summers.

5.7 Recommendations

The DOE, in the context of the P5 process, has referred explicitly to the role of theory. In particular, P5 is to address “Fundamental questions for the field and how to inform/connect the Frontiers framework . . . Input from the theory community will be especially important in this area.” We have outlined above our reasoning in support of these statements, describing both the historical role of theory and what we view as its crucial importance to the field going forward. In addition to enumerating what we view as the important contributions of theorists for the national and international particle physics efforts, we have also listed some specific areas where we see the field as particularly vulnerable.

- Seemingly modest cuts in theory funding are likely to lead to much larger declines in the numbers of postdoctoral fellows and students funded. This has implications both for research productivity and for sustaining an outstanding theory program in the U.S. This includes impact on the historic ability of the U.S. to attract outstanding talent from abroad.
- Shrinking numbers and size of theory grants will have impact on university hiring and teaching loads in theoretical particle physics, as well as obvious impacts for national labs.

Our recommendations to deal with these issues can be summarized as follows:

1. It is important to maintain the vitality and international competitiveness of theory programs both at universities and national laboratories.
2. The move to extract funds from research for projects should treat theory differently from other areas, as the damage to the program is more severe. At the same time, a possible class of projects for theory is proposed below.
3. A project category for theory, within DOE, could be the existence of theory *networks*, modeled loosely on such networks in Europe. A proposal would come from multiple institutions in response to a DOE solicitation to establish such networks, open to all subfields. We envision the scale of such networks as including of order five postdoctoral fellows, funds for travel between institutions and resources particular to the project. The projects would have a duration of order five years. They would have well-defined deliverables. Proposals would be peer-reviewed, preferably within a panel structure. Actual topics would emerge from the community, but one might imagine subjects such as neutrino physics (in this context, a proposal might have a component aimed at DOE Nuclear Physics), computations related to collider physics, or topics in more foundational areas (the “String Vacuum Project” of a few years ago, funded by the NSF, is a possible model).
4. The breadth of the topics and research areas supported in particle theory should be maintained. The successful formula of funding the best and most interesting research should not be changed. While in the experimental program there is obviously a need for prioritizing based on an agency’s missions, this is not the case for particle theory. We advocate that programmatic considerations in funding decisions should be kept at a minimal level. It is important not to limit the scope of high-quality theoretical research that is being performed, even if it appears to cross traditional funding agency boundaries.
5. HEPAP should examine the question of the balance of resources between laboratory and university theory groups. Both have been vital, historically, to the theory effort in the U.S. Both are vulnerable in the likely long term funding environment.
6. A target level of average support for funded researchers in university groups should include 1/2 postdoc per PI and 1/2 student (equivalent to fully supported) per PI. It should include travel funds adequate to attend two major meetings per year per PI, as well as one trip for postdocs and attendance every other year by students at summer schools or appropriate conferences. It should include two months of PI summer salary. Levels of support will vary among groups based on assessments by referees, comparative review panels, and agency officials.
7. Support for graduate student research should be increased. Ideally, particle theory students would be supported for three months during the summer, and for 50% of the terms during the academic year.
8. PI summer salary caps should not be lowered significantly below their current levels except to protect postdoc and graduate student support, and only with attention to impacts on research productivity, particularly of more junior PI’s (assistant and associate professors).

In broad brush, our recommendations are similar to the conclusions of the recent update of the European Strategy for Particle Physics on the importance of theoretical physics: “Theory is a strong driver of particle physics and provides essential input to experiments, witness the major role played by theory in the recent discovery of the Higgs boson, from the foundations of the SM to detailed calculations guiding the experimental searches. Europe should support a diverse, vibrant theoretical physics programme, ranging from abstract to applied topics, in close collaboration with experiments and extending to neighbouring fields such as astroparticle physics and cosmology. Such support should extend also to high-performance computing and software development.”

Similar support for our recommendations can be found in the statement, issued by a large group of experimental particle physicists, principally from U.S. institutions: “We, the undersigned experimental high-energy physicists, believe that a strong experimental high-energy physics program requires a vibrant theoretical physics community in the U.S. Over the last 50 years and more, the theoretical and experimental high-energy physics communities have supported and inspired each other. Not only are predictive tools developed by the theoretical community essential for the experimental endeavor, but the combination of experimental discovery and theoretical interpretation has led to revolutions in our understanding of the nature of matter. An experimental program which probes today’s outstanding questions is strongest when stimulated by new ideas from the theory community. When faced with difficult budgeting choices, we urge policy makers to protect the strength of the theoretical high-energy physics community and the balance between the experimental and theoretical programs.” Signatures can be viewed at http://amanda.uci.edu/~daniel/theory_letter.php

In the following subsections, we have included some topics which would be appropriate for consideration by the DOE Committee of Visitors, as well as some further information on the Snowmass Theory Panel.

5.7.1 Suggestions for agency committees of visitors

There are several issues that have come to this panel’s attention, that are more appropriately addressed by the Committee of Visitors at the DOE and the NSF. These include:

1. NSF panels are FACA panels. The DOE Comparative Review panels are not. The DOE COV should examine whether this limits the role of the panels, and consider whether a change in procedures is needed.
2. The DOE Comparative Review process was established, in part, to assure that awards were based on the quality of proposals and not on historical factors, as sometimes was the case in the past. Because of the challenging funding climate, it appears that in some cases, decisions about awards were made based on historical levels of support. The COV should examine whether, in fact, awards are in line with the quality of proposals, or whether there are still distortions due to past funding levels.
3. The COV should examine policies regarding overlapping proposals. For example, it should be possible to propose the same research for a DOE Career award and for a regular DOE grant (clearly with the understanding that only one would be funded). We understand that the agencies have made efforts to allow such overlap, but in the past, in some cases, agency policies have led to weakened proposals, and a review would be appropriate.
4. It appears to have become standard practice in DOE not to fund proposals from first year assistant professors. The DOE COV could consider whether this serves the best interest of the field.

5.7.2 The Theory Panel: additional information

Theory Panel Web Site The theory panel web page can be viewed at:
<http://www.snowmass2013.org/tiki-index.php?page=Theory+Panel>

Theory Plenary Presentations During the Community Summer Study

1. QCD (Kirill Melnikov)

2. Lattice Gauge Theory (Steve Gottlieb)
3. Neutrino Physics (André de Gouvêa)
4. Phenomenology (Tim Tait)
5. Model Building (Ann Nelson)
6. Cosmology (Jonathan Feng)
7. Field Theory and String Theory (David Gross)
8. The view from the NSF (Keith Dienes)
9. The view from the DOE (Simona Rolli)

Slides from most of the talks can be viewed at:

<https://indico.fnal.gov/conferenceTimeTable.py?confId=6890#20130804.detailed>

Accelerator Capabilities

Convener: W. A. Barletta

M. Bai, M. Battaglia, O. Brüning, J. Byrd, R. Ent, J. Flanagan, W. Gai, J. Galambos, G. Hoffstaetter, M. Hogan, M. Klute, S. Nagaitsev, M. Palmer, S. Prestemon, T. Roser, L. Rossi, V. Shiltsev, G. Varner, K. Yokoya

6.1 Introduction

Accelerator-based experiments have been the mainstay and driving force of experimental particle physics ever since Rutherford recognized that radioactive sources provided an extremely limited capability to probe the nature of matter at the shortest time and distance scales. From their earliest beginnings the history of invention in accelerator technology have enabled the history of particle physics discoveries especially on the energy frontier; see Fig. 6-1.

Yet the figure of merit of energy reach in the constituent center-of-momentum frame is far from the entire story of enabling capabilities. For experiments to be completed in a reasonable amount of time, the rate of interesting experimental events must be sufficiently high. That is, the luminosity of the accelerator-target combination must be large enough to explore the physics of interest.

$$\text{Interesting Events} = \text{Cross section} \times \text{Average collision rate} \times \text{Time} . \quad (6.1)$$

Generally high luminosity implies a large charge per bunch, a high collision frequency of beam bunches with the target, and focus of the beam to a small spot. The target can either be fixed or a counter-rotating beam bunch. Even energy-reach and accelerator-target luminosity is insufficient for discovery-level experiments. The accelerator should deliver beam particles with appropriate quantum numbers and with a timing structure which maximizes the signal-to-background noise of the experiment.

Assessing the suite of characteristics of present and proposed accelerators in the light of current interests and priorities of particle physicists has been the task of the Accelerator Capabilities Study of the Snowmass process. For proposed or planned capabilities the study has recommended the critical research and development programs needed.

The study has been a synthesis of individual pre-Snowmass workshops conducted by six working groups culminating with the collective Snowmass meeting of all interested participants. Each working group addressed major challenges foreseen for their respective class of accelerators in their respective pre-Snowmass workshops. The areas of inquiry included but were not limited to the following specific questions:

- How high a luminosity is possible for the LHC?
- How high an energy is possible in the LHC tunnel?
- Could a Higgs factory be built in the LHC tunnel?

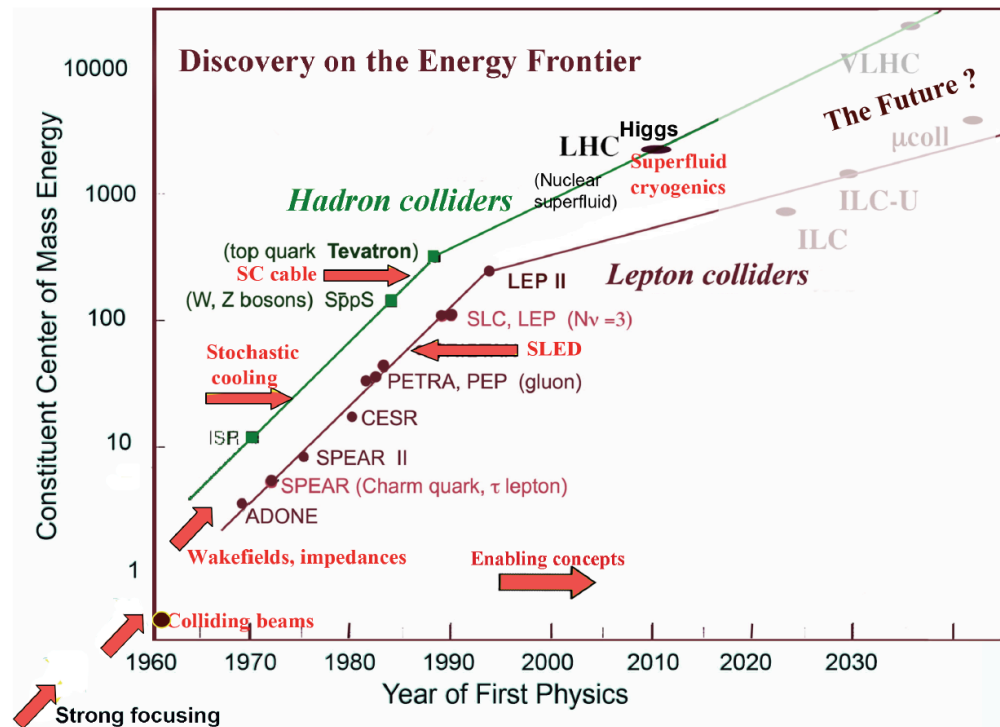


Figure 6-1. The so-called Livingston plot illustrates how history of discovery on the energy frontier has been enabled by the history of invention (red arrows) in accelerator science and technology.

- Can the ILC and CLIC designs be improved using new technologies?
- Can one design a multi-TeV $\mu^+\mu^-$ collider?
- What secondary beams are needed for Intensity Frontier experiments?
- What proton beams are needed to generate these secondary beams, and can these be made by existing machines?
- What accelerator capabilities at heavy flavor factories are required to realize the full range of physics opportunities?
- What are new physics opportunities using high-power electron and positron beams?
- What is the broad range of test capabilities existing or needed for developing accelerator capabilities?

The working groups also considered a set of big questions regarding accelerator capabilities for the long-term future of high energy physics:

- How can one build a collider at the 10 – 30 TeV constituent mass scale?
- What is the farthest practical energy reach of accelerator-based particle physics?
- How would one generate ten or more megawatts of proton beam power?

- Can multi-megawatt targets survive and if so, for how long?
- Can plasma-based accelerators achieve energies and luminosities relevant to particle physics?
- Can accelerators be made an order of magnitude cheaper per GeV and/or per MW?

This report is organized as a collection of the executive summaries of each of the six working groups: Energy Frontier proton colliders, Energy Frontier lepton colliders, Intensity Frontier proton sources, Intensity Frontier electron accelerators, electron-ion colliders, and accelerator research and test-beam facilities.

6.2 Energy Frontier proton colliders

High-energy hadron colliders have been the tools for discovery at the highest mass scales of the Energy Frontier for more than a decade. They will remain so, unchallenged for the foreseeable future. The discovery of the Higgs boson at the LHC announced opened a new era for particle physics. Its measured properties are consistent, within the current uncertainties, with those of the Standard Model (SM) Higgs boson.

After the discovery of the Higgs boson, understanding what is the real origin of electro-weak symmetry breaking becomes the next key challenge for collider physics. This challenge can be expressed in terms of two questions: Up to what level of precision does the Higgs boson behave as predicted by the Standard Model? At what energy are the new particles that could offer some insight into the origin of dark matter, the matter-antimatter asymmetry, and neutrino masses?

6.2.1 The LHC and its upgrades

The approved LHC program, its future upgrade towards higher luminosities (HL-LHC), and the study of an LHC energy upgrade or of a new proton collider delivering collisions at center-of-mass energies up to 100 TeV, are all essential components of this endeavor. High luminosity could increase the precision of several key measurements, uncover rare processes, and guide and validate the progress in theoretical modeling, thus reducing systematic uncertainties in the interpretation of the data. To achieve this end,

the full exploitation of the LHC is the highest priority of the energy-frontier hadron-collider program.

The LHC is expected to restart in Spring 2015 at a center-of-mass energy of 13–14 TeV and reach its design luminosity of $10^{34}\text{cm}^{-2}\text{s}^{-1}$ during 2015. After 2020, some critical components of the accelerator will reach the radiation damage limit, and others will have lower reliability due to vulnerability to radiation. Furthermore, as the statistical gain in running the accelerator without substantially increased luminosity will become marginal, the LHC will need a substantial increase in luminosity [1]. With a high luminosity upgrade (HL-LHC), the LHC should deliver 3000fb^{-1} during a decade of operation [2]. The HL-LHC project is the first priority of Europe, as stated by the Strategy Update for High Energy Physics approved by CERN council in the special session of May 30, 2013 in Brussels.

Superconducting magnets are the single most critical technology for the LHC and for proton colliders beyond the present LHC level of performance. Indeed, the present LHC is based on 30 years of development of magnets using NbTi wire. In the LHC NbTi-based magnets are pushed to their limits both in the collider arcs and in the interaction regions designed and built in collaboration with U.S. and Japanese national laboratories.

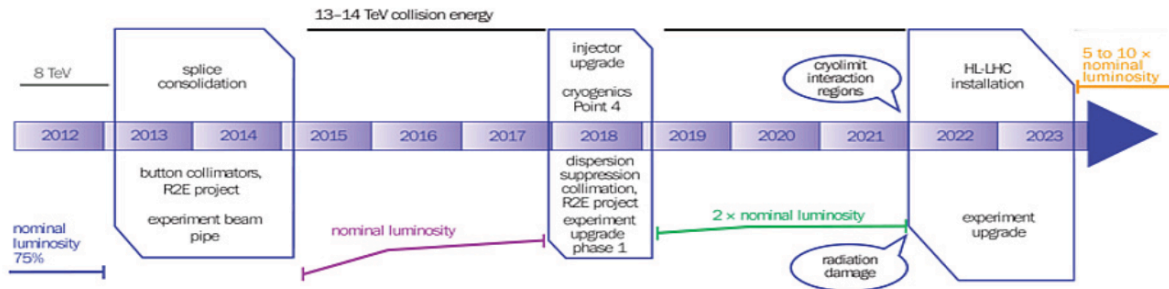


Figure 6-2. LHC baseline plan for the next ten years in terms of energy of the collisions (upper line) and of luminosity (lower lines). The first long shutdown (LS) 2013-14 is to allow design parameters of beam energy and luminosity. The second LS, 2018, is to increase beam intensity and reliability as well as to upgrade the LHC Injectors.

Delivering the beam brightness required for HL-LHC will be a difficult challenge for the upgraded LHC injector chain; also challenging will be preserving it in the LHC storage ring. In light of this limitation, the preferred route towards increased luminosity is reducing the beta (amplitude) function at the interaction point, β^* , by means of stronger triplet magnets with larger apertures in the interaction region. The present design of the HL-LHC interaction region requires quadrupoles with an aperture of 150 mm and with a peak field in excess of 12 T — beyond the capabilities of NbTi conductor. It relies heavily on the success of the advanced Nb₃Sn technology developed by an integrated consortium of U.S. national laboratories under the LHC Accelerator Research Program (LARP). In addition to magnets, many other technologies will be involved in HL-LHC, such as crab cavities, advanced collimators, high-temperature superconducting links, and advanced remote handling. LARP plays a vital leadership role in this work to maximize the discovery potential of the extensive LHC infrastructure at CERN.

A vigorous LARP leading to U.S. participation in the HL-LHC construction project is crucial to the full realization of the potential of LHC to deliver discovery physics to the thousands of U.S. researchers who are engaged at the LHC.

Energy and luminosity play complementary roles in exploring the energy frontier with hadron collisions. Production rates for signals of interest at the collider may be small either because the mass of the produced objects is large or because the coupling strength is small. In the former case, increasing the beam energy is clearly favored. In contrast, higher luminosity may be more effective in probing small couplings at smaller masses. In reaching higher energies, magnet technology will retain a pivotal role.

Given the progress in magnet technology and the maturity that Nb₃Sn is reaching, due largely to the LARP program for HL-LHC, Nb₃Sn magnets are expected to reach a limit of 15.5–16 T in operating field (that is with 15–20% margin with respect to quench) within the next decade, thereby opening a path towards a collider with an energy significantly larger than the LHC. This would open the way towards very high energy hadron collisions with an exceptional potential for probing the Energy Frontier.

6.2.2 Options for higher energy hadron colliders

The first option is a machine housed in the LHC tunnel. The achievable center-of-mass energy depends on the dipole field strength. An energy of 26 TeV is within the reach of proven Nb₃Sn technology, although it requires engineering development. The energy reach would become 33 TeV, if 20 T magnets based on more

futuristic high-temperature superconductors (HTS) were practical and affordable, and if they could fit in the limited space available in the LHC tunnel. In a new, longer tunnel, a 100 TeV proton collider is possible. Studies for a very large proton collider able to deliver center-of-mass energies of 100 to 200 TeV have been conducted over the past two decades. The multi-laboratory VLHC study led by Fermilab in 2001 [3] was noteworthy. The analysis remains valid and argues for a very large (233 km) tunnel that allows the use of NbTi magnet technology.

Continuing an integrated, multi-laboratory program (LARP-like) towards the engineering development of 20 T magnets for HE-LHC or a new collider in a longer tunnel, needs to be an HEP priority. A program focused on engineering readiness, closely coordinated with CERN, would establish the limits of Nb₃Sn technology, investigate new conductor materials, and refine our present concepts for how to manage the enormous stresses produced by such high magnetic fields. The experience from RHIC, SSC, and LHC indicates that the dipoles account for about half of the total collider cost. Therefore, investing in magnet technology is critical and should represent a major focus of the research and development process.

With the renewed interest in the U.S. in a 100 TeV scale collider, the study group recommends participation in the international study for colliders in a large tunnel which CERN is now organizing. The construction of a new tunnel reduces the demand for high dipole field strength, since there is a tradeoff between the tunnel circumference and required field strength (see Figure 3). The target collision energy is 100 TeV for 20 T dipoles in an 80 km tunnel ($R = 13$ km). However, a slightly larger, 100 km circumference tunnel would provide the same collision energy of 100 TeV with field of 16 T, reachable with Nb₃Sn technology that relies on a far more mature and less expensive conductor than HTS. A large tunnel would also open up the possibility of a Higgs Factory based upon an e^+e^- storage ring, if the International Linear Collider is not built [4].

The 100 TeV study should address critical technology and cost feasibility issues and, thus, will provide input for developing a long-term roadmap of high-energy physics. Areas of particular interest to U.S. research groups include beam dynamics, magnets, vacuum systems, and machine protection.

Whether in the LHC tunnel or in a new, larger tunnel a collider with energy beyond the LHC will have to deal with an additional challenge, the emission by the beam of synchrotron radiation at least 20 times greater per meter than the LHC at its nominal parameters. The beam pipe and beam screen will have to absorb that radiation. Although synchrotron radiation is very beneficial for beam stabilization and will make the higher energy collider the first hadron machine dominated by synchrotron radiation damping, the power dissipated in synchrotron radiation must be removed at cryogenic temperature. In LHC it is removed at 5–10 K. One possible solution for the HE-LHC is based on a beam screen at 40–60 K. This approach has no major drawback in principle, although careful design, engineering and prototyping are needed to validate such a design. Merely relying on a beam screen at 10 K similar to the LHC would be a heavy burden for the cryogenics. Handling synchrotron radiation may be even more complex for a 100 TeV machine.

Focused engineering development is no substitute for innovative R&D.

In addition to the focused program of engineering development, the separate programs of long-range research in magnet materials and mechanical structures of magnets must be continued. These programs have provided the intellectual and infrastructure base for the success of LARP. For example, the long-range future of high-energy physics using p-p collisions may need magnet operating fields beyond 16 T. In practice, the decision will rest on a tradeoff between tunnel cost — which scales roughly inversely with dipole field — and the cost of the dipole magnets that scales roughly linearly with dipole field for long magnets. The crossover in cost cannot be decided ab initio as the tunnel cost will depend strongly on the geology of the collider site. A new generation of magnets using high-temperature superconductors will require new engineering materials with small filament size that are available in multi-kilometer continuous pieces. Advanced magnets may offer

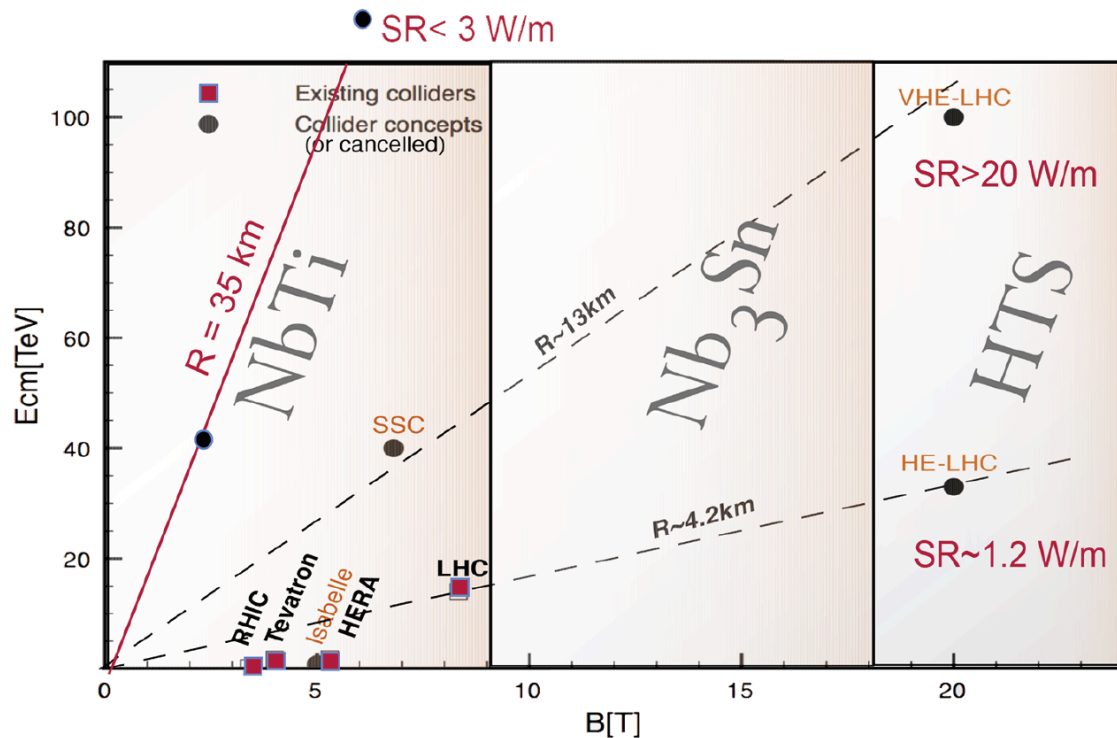


Figure 6-3. Role of the superconductor in the energy reach of hadron colliders as a function of field and bending radius, R , produced by the dipole magnets.

greater temperature margin against quenches due to stray radiation lost from the beam. Higher-field magnets will require innovative stress management techniques, exquisitely sensitive magnet protection schemes, and perhaps novel structural materials.

Although synchrotron radiation damping is not dominant in proton colliders, as it is in electron colliders, it is still important to understand beam dynamics and other effects of marginal synchrotron radiation damping. As the energy stored in the beam becomes several gigajoules, control of tenuous beam halos becomes a pressing issue. Likewise machine protection from accidental beam loss and the design of special beam abort dumps become difficult challenges that demand innovation. An effective long-range research and development program must also consider beam physics of the injection chain, effects of noise and ground motion, and design and technology options for the configuration of the interaction regions.

6.3 Energy Frontier lepton colliders

In its first three years of running experiments, the Large Hadron Collider (LHC) has begun to explore the energy region up to 1 TeV. The LHC experiments have discovered a Higgs boson which, within current experimental precision, is consistent with that of the Standard Model and have measured its mass as about 126 GeV. The discovery of the Higgs boson and the recent release of a technical design report for the International Linear Collider have energized high energy physicists to evaluate in depth the experiments that could be performed with a lepton collider. As described in the report of the Snowmass Energy Frontier Study Group, a future lepton or photon collider would provide a factory for measurements of the properties

of the Higgs boson with ultimate precision. A lepton collider would allow unambiguous searches for new particles that complement searches at the LHC. It would also provide opportunities to probe for and study new physics, both through the production of new particles predicted by models of physics beyond the Standard Model and through the study of indirect effects of new physics on the W and Z bosons, the top quark, and other systems.

6.3.1 International Linear Collider

In June 2013 the Global Design Effort (GDE) published a Technical Design Report (TDR) [5] of the International Linear Collider (ILC), an accelerator that will give these capabilities. The ILC is a superconducting linear e^+e^- collider with a center-of-mass collision energy tunable between 200 and 500 GeV with a luminosity exceeding $10^{34} \text{ cm}^{-2}\text{s}^{-1}$ at 500 GeV, roughly scaling in proportion to the collision energy. The ILC is upgradable in luminosity by a factor of 2 and upgradable in energy to 1 TeV. The Japanese high-energy physics community has named the ILC as its first priority. An experienced cadre of U.S. accelerator physicists and engineers is capable and ready to work on this project.

Our study welcomes the initiative for ILC in Japan. The U.S. accelerator community is capable of contributing as part of a balanced U.S. high-energy physics program.

The key characteristics of the ILC accelerator are the relatively long interval between collisions of bunches, narrow beam energy spread, beam position and energy stability, and the ability to polarize both electrons and positrons. The design and technical details of the ILC have been developed over more than 20 years and incorporate extensive US leadership contributions in beam dynamics, damping ring design, electron and positron sources, superconducting RF (SRF) technology, and beam delivery system design. As described in the TDR, the ILC represents a mature collection of technologies that are ready to proceed to construction. The 1.2 GeV VUV Free-Electron-Laser FLASH facility at DESY has provided a critical integrated system test experience. FLASH technology is quite similar to ILC, and ILC operational parameters are within the range of FLASH hardware. Tests at FLASH were done with the ILC bunch number, bunch repetition rate, bunch charge and peak beam current. These tests were successful and encountered no fundamental technology issues with operating a superconducting linac with the ILC design characteristics. The research program has successfully demonstrated the goal of 31.5 MV/m in installed cryomodules with beam loading, using niobium cavities with no more than two surface-preparation processing cycles. With the specified accelerating gradient, the total length of the 500 GeV ILC is 31 km.

Industrial and institutional partners of the GDE advised a strong industrialization program from the outset. A key ingredient was the development, within the GDE, of a project-governance and value-based cost-estimating strategy that was balanced regionally and that took advantage of the intrinsic modularity and relative maturity of the SRF technology. The most costly and time-consuming part of industrialization is the construction and commissioning of heavy infrastructure, notably institutional test facilities. Fabrication of superconducting cavities to the ILC design specifications has been industrialized, with qualified vendors in Europe, North America, and Asia.

During the research phase, U.S. national laboratories and industry have contributed substantially to cavity performance, cryomodule design, high-level RF performance, demonstrations of beam dynamics, and specific components and systems. For the construction of ILC, it is reasonable to assume the U.S. contributions will continue along these lines. Specifically, construction and testing of completed cryomodules will contribute the greatest economic value to the project. The US community could also provide detailed accelerator design development efforts as an intellectual contribution. It is expected that the US would also contribute to design and construction of one or more linear collider subsystems. Given that the TDR estimates the

E_{CM}	GeV	250	350	500	500 up	250 up	350 up
Rep. rate	Hz	5	5	5	5	10	8
Bunches/pulse		1312	1312	1312	2625	2625	2625
Total beam power	MW	5.3	7.4	10.5	21.0	21.0	23.5
Luminosity	$10^{34} \text{ cm}^{-2}\text{s}^{-1}$	0.75	1.0	1.8	3.2	3.0	3.2

Table 6-1. Summary of ILC performance in the range 250 to 500 GeV. Columns 3–5 give the TDR values. Columns 6–8 indicate scenarios beyond the parameters discussed in the TDR, but still within its technical scope. Units are specified in the second column.

Center-of-mass energy	% of TDR cost	Power consumption (MW)
250 GeV Higgs factory	70%	120
500 GeV	100%	163 - 204
1 TeV upgrade	150%	240
1.5 TeV upgrade	200%	> 300

Table 6-2. Scaling of ILC cost and power consumption scaling as function of center-of-mass energy.

required labor effort to be 13,000 person years, participation by America’s highly experienced accelerator scientists and engineers will be crucial.

Extension of the ILC to 1 TeV is straightforward. It requires lengthened linac tunnels and additional cryomodules, but would use the original ILC sources, damping rings, final focus and interaction regions and beam dumps. No new technological breakthroughs would be required, although research to develop higher-gradient cavities would permit shorter tunnel extensions and thus cost savings.

6.3.2 Alternative e^+e^- colliders

Alternative approaches to the ILC albeit on a much longer time scale include approaches with the potential to reach multi-TeV energies. One approach, the Compact Linear Collider (CLIC) [6] two-beam accelerator, is based on a high-gradient X-band, warm linac powered by a high current, low energy (about 1 GeV) drive beam. CLIC would stretch for 50 km at an ultimate center-of-mass energy of 3 TeV. In contrast, the muon collider — if proven feasible — would fit a 3–5 TeV collider onto the present Fermilab site. Moreover, the muon approach has a very large overlap with capabilities needed for Intensity Frontier accelerators. Yet another approach would use wakefields driven either by beams or lasers to achieve accelerating fields of 10 to 100 GeV per meter. Many feasibility and practicality issues need to be solved in order to implement any of these programs.

For CLIC options at 350 GeV, 1.5 TeV and 3 TeV are currently being studied. The first and second option use a single drive-beam generation complex to feed both linacs, while in option three each linac would be fed by a separate complex. The CLIC design is based on three key technologies, which have been addressed experimentally: high gradient structures, drive beam generation, and power extraction. The normal-conducting accelerating structures in the main linac have a gradient of 100 MV/m, to limit the length of the machine. The RF frequency of 12 GHz and detailed parameters of the structure have been derived from an overall cost optimization at 3 TeV. Experiments at KEK, SLAC, and CERN have verified

the structure design and established its gradient and breakdown rate. The drive beams run parallel to the colliding beams through a sequence of power extraction and transfer structures, where they produce short, high-power RF pulses that are transferred into the accelerating structures. These drive beams are generated in a central complex. The drive-beam generation and power extraction have been demonstrated in a dedicated test facility at CERN.

Wakefields in plasma-based accelerators can potentially provide a thousand-fold or greater increase in acceleration gradient over standard technologies. Two primary approaches are being investigated: beam-driven wakefields (plasma wake field acceleration, or PWFA) and laser driven wakefields (laser plasma acceleration, or LPA). Experiments at SLAC, LBNL, and at European laboratories have shown that plasmas can accelerate and focus high-energy beams at an accelerating gradient in excess of 50 GeV/m. At present two large R&D facilities are spearheading this research in the U.S., FACET at SLAC (PWFA) and BELLA at LBNL (LPA). Smaller efforts are being pursued at university laboratories.

For any variant of wakefield accelerator to be practical as a linear collider, several feasibility and practicality issues must be resolved in the context of an integrated system test. Most importantly, wakefield accelerators, like standard accelerator modules, must be capable of being staged in a series of phase-locked segments. Both PWFA and LPA approaches must demonstrate simultaneous positron acceleration and focusing in plasma densities consistent with preserving positron beam quality. Both must demonstrate timing, pointing, and focusing control consistent with the high luminosities required in a lepton collider. Finally, both must demonstrate that multi-bunch plasma instabilities (such as zero-frequency, convective hose instability) can be overcome with operation at the tens of kHz repetition rate required for high luminosity. Beyond the feasibility issues are questions of practicality related to overall cost, efficiency, and reliability. These issues may be more demanding for laser-driven concepts as cost and efficiency limitations of drive beam sources are better known.

For the study of the Higgs boson what matters are deliverable luminosity, maturity of design, risk, timescale, and cost. The type of e^+e^- collider, linear or circular, is of little importance. A circular collider might offer a potential alternative to the linear collider. It can benefit from three characteristics of lower-energy circular machines: (1) high luminosity and reliability, (2) the availability of several interaction points, (3) excellent beam energy accuracy. For a given RF power, the luminosity of a storage ring collider rises linearly with its circumference. For a given tunnel size, and assuming the machine operates at the beam-beam tune-shift limit, luminosity rises linearly with the total dissipated synchrotron radiation (SR) power, which is approximately proportional to the total available power. Therefore, the analysis can be scaled to different machine sizes. The energy reach consistent with sufficient luminosity for Higgs studies depends strongly on the machine circumference; a machine of 27 km circumference might reach 240 GeV, while the limit is about 350 GeV for a machine of 80 km. At a given circumference, the energy reach and luminosity are very strongly coupled; detailed characteristics of the design matter greatly.

CERN has begun a design study of the TLEP storage ring, a machine with a circumference of 80 to 100 km circumference and dissipated synchrotron radiation power set at 100 MW. The luminosity would be largest at the Z peak, but fall rapidly as center-of-mass increases beyond that point. The proposed TLEP storage ring would need 1 km of superconducting RF cavities and low β^* insertions, would operate at fixed field and fed by a synchrotron situated in the same tunnel for continuous top-up injection. Multi-bunch operation would be necessary for high luminosity below the top energy, requiring separated beam pipes for electron and positron beams. Beamstrahlung from the collision process can lead to a short luminosity-lifetime and large instantaneous energy spread in the beam of the order of several percent. Beam losses must be tightly controlled even in a lattice of very large dynamic aperture. Maintaining vertical to horizontal emittance ratios considerably smaller than light sources needs further study.

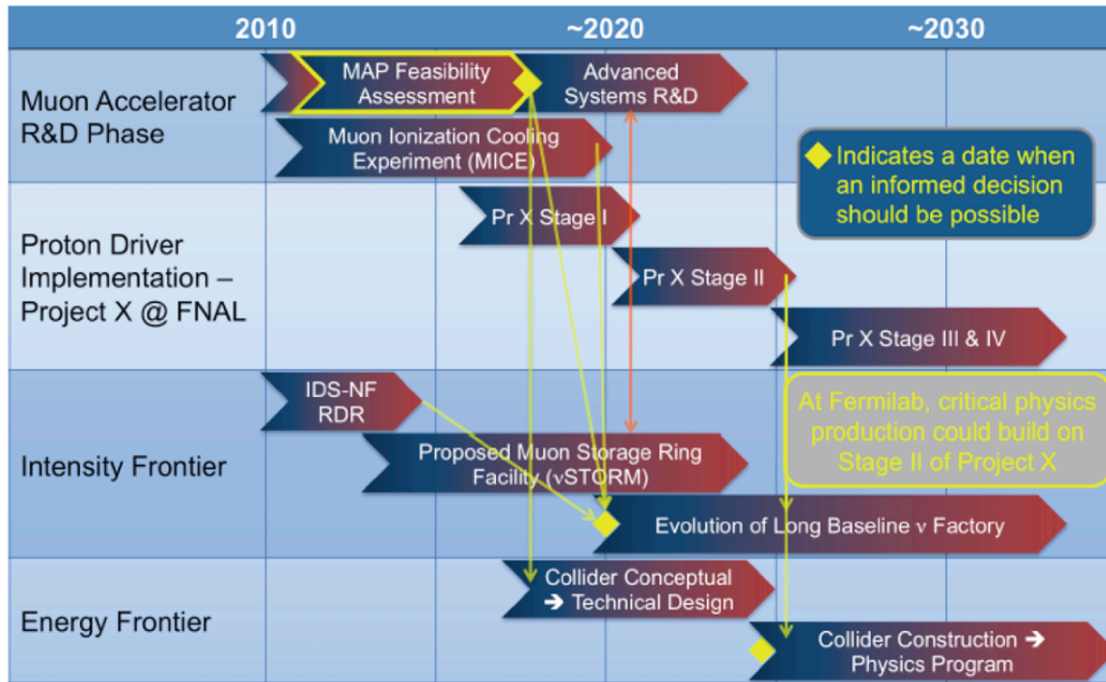


Figure 6-4. Muon accelerator timeline, including the MAP Feasibility Assessment period. It is anticipated that decision points for moving forward with a Neutrino Factory project supporting Intensity Frontier physics efforts could be reached by the end of this decade, and a decision point for moving forward with a Muon Collider physics effort, supporting a return to the Energy Frontier with a U.S. facility, could be reached by the middle of the next decade.

The Chinese high-energy community has also started discussion and first studies of a circular Higgs factory or a proton collider in a very large tunnel [7]. Should the ILC not go forward over the next decade and should the renewed interest in a very large circumference hadron collider be sustained, the possibility of a circular Higgs factory deserves extensive consideration.

6.3.3 Muon accelerators

Muon accelerators [8] have the potential to provide world-leading experimental capabilities for physics at center-of-mass energies from the Higgs at 126 GeV up to the multi-TeV scale. A circular muon collider can potentially reach the higher energy range because the larger mass of the muons means they produce far less synchrotron radiation than electrons. For the same reason, beamstrahlung is suppressed leading to a very small spread of the center-of-mass energies in muon collisions. Muons, however, have a lifetime (at rest) of about $2.2 \mu\text{s}$ and therefore decay in flight. The unstable nature of the muon demands that the sequence of beam creation, manipulation, and acceleration be done rapidly; high-gradient acceleration is essential. Therefore, an Energy Frontier muon collider would necessarily be relatively compact. Even a 5 TeV collider would fit within the Fermilab site.

Research of critical importance to the performance of a muon collider includes: (1) development of a high-power target station including a high-field capture solenoid (nominally 20 T hybrid normal and superconduct-

ing magnet with about 3 GJ stored energy) that is ultimately capable of handling more than 4 MW of power; (2) control of collective effects in the high-intensity beams at the low energy end of the production, capture, and acceleration process; (3) cooling (reduction) of the muon six-dimensional phase space volume by six orders of magnitude to achieve the beam parameters required for the muon collider designs; (4) development of very large-aperture, superconducting dipole magnets with heavy inserts to protect the superconductors from the decay products of the stored muons in the collider. Once items (1) and (2) are resolved positively, a muon accelerator would provide an outstanding high-flux source of well-characterized neutrinos for a range of Intensity Frontier experiments. The potential for muon accelerators to address crucial questions on both the Intensity and Energy Frontiers argues for a robust development program.

6.3.4 Photon colliders

In a Higgs factory photon collider, two electron beams are accelerated to 80 GeV and converted to about 63 GeV photon beams by colliding with low-energy (3.5 eV photons), high-intensity (5 J per pulse) laser beams via the Inverse Compton Scattering (ICS) process. The two high-energy photon beams then collide and generate Higgs particles through the s -channel resonance $\gamma\gamma \rightarrow H$. Among various options for a Higgs factory, a photon collider has the distinct advantage that the 80 GeV energy required for the electron beam is lower than for other colliders. Photon colliders have been discussed as options to accompany proposed linear or circular colliders or as a stand-alone facility. Since the overall acceleration efficiency of any facility is the product of the individual efficiencies of each of the systems involved in the transfer of power from the wall plug to the beam, each of the systems should be as efficient as possible. Innovative research on efficient RF generation would be extremely beneficial to all lepton collider designs. It should be strongly supported as a key technology to reduce the operating costs of future facilities for both the Energy and Intensity Frontiers.

6.4 Capabilities requiring high-intensity proton beams

The beam parameters needed for a variety of measurements of interest in the Intensity Frontier were presented at a community workshop held at Brookhaven National Laboratory in April, 2013 [9]. Experiments included neutrino, kaon, muon, neutron, and proton EDM measurements. From the secondary beam specifications, the workshop identified the proton beam requirements for the different proposed Intensity Frontier measurements. Although the resulting sets of primary proton beam requirements were generally not based on detailed calculations but rather on general guidelines, they can be used as a guide for matching the desired measurements with existing or proposed proton facilities. The study compared 20 existing proton beamlines and 14 planned upgrades with the requirements of 19 secondary beam requests filled out by experiment advocates and drew the following conclusion:

The next generation of Intensity Frontier experiments will require proton beam intensities and timing structures beyond the capabilities of any existing accelerators.

The major proton accelerator facilities surveyed included U.S. capabilities at FNAL, BNL, LANL, and SNS. International representation included CERN, J-PARC, PSI, ISIS, and TRIUMF. New and proposed proton accelerator facilities included the European Spallation Source (ESS), the Chinese Neutron Spallation Source (CSNS), DAE δ ALUS, and Project X. In addition, existing facilities typically have upgrade proposals — both near and long term — which have been included in the study. High proton beam powers of 1 MW or more are desired for most proposed experiments. The beam energy requirement typically exceeds 1 GeV. The

required time structure of the beam varies significantly, depending on the nature of the measurements and the specifics of the measurement conditions; for example, underground detectors have special requirements.

Accelerators with current and anticipated future programs of Intensity Frontier experiments include FNAL, J-PARC, and CERN. The LANL linac is undergoing a refurbishment and in a few years this beam would operate at 100 Hz and provide bunches with a 1 ms duration available for potential users. Cold neutron applications may be able to use this beam structure, as well as others. Existing accelerators such as J-PARC and SNS might provide MW proton beams, with energy less than 3 GeV with very low duty factor (less than 10^{-4} to minimize background from cosmic sources) for lower-energy muon and electron neutrino studies. ISIS (in the UK) has been used for this purpose in the past, and still has a cavern 10 m from their TS-1 target. The ultimate technical potential of linac technology is suggested by the example of LEDA. The normal-conducting LEDA linac, which produced a 6.7 MeV, 100 mA CW beam, operated at Los Alamos from 1999 to 2001.

6.4.1 Proposed new accelerators

Proposed new accelerators for intensity frontier applications include Project X, DAE δ ALUS, and nuSTORM. Of the three, Project X [10] has the broadest scope and, if required, could be structured as a multi-phase proposal. Project X is a high-intensity proton facility that will support a world-leading Intensity Frontier research program over the next few decades at Fermilab. When compared to other facilities in the planning stages elsewhere in the world, Project X would be unique in its ability to deliver, simultaneously, up to 6 MW of site-wide beam power to multiple experiments, at energies ranging from 233 MeV to 120 GeV, and with flexible beam formats. Project X is designed to support a wide range of experiments based on neutrinos, muons, kaons, nucleons, and nuclei. It will also lay the foundation for the long-term development of a neutrino factory and/or a muon collider.

Project X capitalizes on the very rapid development of superconducting RF technologies over the last 20 years, and their highly successful application to high-power H acceleration at the Spallation Neutron Source (SNS) at Oak Ridge National Laboratory. As a result of the accelerator research leading to and supporting SNS construction and operation, excellent simulation and modeling tools exist for designing the Project X facility with high confidence that the performance goals can be achieved. The primary supporting technologies required to construct Project X exist today.

The Project X Collaboration is engaged in a comprehensive development program aimed at mitigating technical and cost risks associated with construction and operations. The major elements of the risk reduction program include an integrated, 25 MeV front-end system test, the Project X Injector Experiment (PXIE). To allow for beam accumulation in the Main Injector, the Project X linac will accelerate H^- ions. Optimizing the superconducting RF cavities will improve the cost effectiveness of the final accelerator design. Achieving the performance required of high-power targets is expected to be challenging. Therefore, high-power target technology will be a key component of research in support of the project.

The overall scope and goals of the Project X development program are based on being prepared for a 2017 construction start. Essentially all elements described above are required to implement the first stage of Project X.

DAE δ ALUS [11] is a neutrino research program based on “decay-at-rest” sources. Pions are produced by interaction of 800 MeV protons on a suitable target. This energy is sufficiently above threshold for good pion yield, and low enough that pions will stop in the target before decaying. The DAE δ ALUS configuration consists of three nearly identical sources of neutrinos. The highest-power accelerator, located 20 km from

the detector would provide 10 mA of protons at 800 MeV to the neutrino-generating targets. A current of 10 mA is approximately a factor of 5 times the highest achieved current at PSI, the world's leading high-power cyclotron today. Accelerating H_2^+ ions rather than protons has the potential for reducing space-charge issues at injection. Extraction of the beam at 800 MeV with a stripper foil minimizes the necessity for clean turn separation at the outer radii, only requiring a proton extraction channel with sufficiently large momentum acceptance to transport ions stripped from several overlapping turns. The use of H_2^+ acceleration represents a novel approach to reducing beam losses at extraction from the 800 MeV cyclotron.

To date, the DAE δ ALUS feasibility arguments are made by scaling from existing low-energy H^- commercial cyclotrons as well as from the PSI high-power proton cyclotron. Since the high-power H_2^+ concept is quite novel, a systematic research is being conducted to address challenges of meeting the required performance. The most critical elements are (1) ion-source development to achieve very bright, vibrationally cold H_2^+ beams of at least 50 mA CW, (2) injection into cyclotrons with emphasis on bunching efficiency, space requirements and space-charge dynamics, (3) end-to-end simulations to evaluate beam stability and uncontrolled loss, and (4) atomic physics experiments to measure stripping and beam-gas scattering cross sections and possibly techniques for Lorentz dissociation of vibrational states in high-field (about 25 T) magnets in the transport line between the injection and the second-stage cyclotrons. As is the case for Project X, target systems are challenging and will require sustained research.

The first phase of DAE δ ALUS is IsoDAR [12], a compact 600 kW cyclotron proposed to be located 15 m from the KamLAND detector for a definitive search for one or two sterile neutrinos. The IsoDAR cyclotron would also be a prototypical injector for the superconducting ring cyclotron of DAE δ ALUS. At present space charge effects at injection are being studied experimentally in a collaboration with industrial partners.

The nuSTORM facility is designed to deliver beams of electron and muon neutrinos from the decay of a stored muon beam with a central momentum of 3.8 GeV/c and a momentum acceptance of 10%. The facility will be able to (1) conduct highly sensitive searches for sterile neutrinos, (2) provide definitive measurements of neutrino-nucleon scattering cross sections with percent-level precision for the electron and muon neutrinos, and (3) take the crucial first step in developing of muon accelerators as a powerful new technique for particle physics.

nuSTORM is the simplest implementation of the neutrino factory concept. It utilizes protons from the Main Injector, has a conventional (NuMI-like) target station, pion collection with a NuMI-style horn, a pion transport line, and a 480 m circumference muon decay ring. The nuSTORM facility can also provide an intense pulses of 10^{10} low-energy muons, appropriate for future 6-dimensional muon ionization cooling experiments. This beam is available during operation of the neutrino physics program and operates in a completely parasitic mode. Finally the muon decay ring provides the opportunity for accelerator R&D on beam instrumentation and potentially on muon acceleration technology.

6.4.2 General accelerator research needs

In addition to project-specific needs, the Snowmass workshop identified more general research needed to advance Intensity Frontier capabilities. That research includes but is not limited to the following [13]:

- Understanding and controlling beam loss: Very precise simulations of beam dynamics involving accurate predictions of beam halos are essential for enabling high-power multi-megawatt-scale machines to run without inducing excessive amounts of radioactivity. To benchmark simulation codes, reliable measurements of very weak beam halos will be required with a large (10^4 – 10^5) dynamic range.

- Superconducting RF technology: Most modern high-power proton facilities rely on linacs utilizing superconducting radio frequency (SRF) acceleration. The initial development of SRF technology emphasized maximizing the accelerating gradient to limit the size of the accelerators. Since many Intensity Frontier applications require the delivery of high-duty-factor beams, the figure of merit is shifting from high-gradient to very high unloaded quality-factor, Q_0 . Cavity fabrication and processing techniques must minimize cost, while reliably producing cavities with gradients of roughly 20 MV/m at Q_0 in excess of 10^{10} to support future facilities.
- High-quality, high-current injection systems: Multi-MW linacs and cyclotrons will need high-current, low emittance sources of ion beams, effective beam chopping, and control of strong space-charge effects at low beam energies. While suitable ion sources are close to being available, effective beam chopping is beyond the present state of technology and is under active development. The challenge is to develop choppers to provide full beam deflection on a time scale less than the beam micro-bunch spacing, and/or to cleanly transport partially deflected beam.
- Isochronous ring cyclotrons: Isochronous ring cyclotrons (including fixed-field alternating gradient accelerators) are also good candidates for high continuous (CW) power. Clean extraction of the beam requires well-separated turns. This sets the scale of both the ring diameter and the energy gain per turn. The alternative is to use an ion such as H_2^+ that can be extracted with a foil. The large superconducting magnet sectors require some development beyond the present RIKEN magnets before costs can be confirmed.

6.4.3 High-power targets

High-power target development and its implications for high-power proton accelerators should not be underestimated [14]. Three of the present high-power accelerator facilities (SNS, J-PARC-MLF, and FNAL-MINOS) have been limited in beam power during recent operational periods by target concerns, not by the accelerator capability. The cost of these systems is a non-negligible portion of high-power accelerator facilities, and the resources needed to operate and maintain them are also significant (remote maintenance, disposal of radioactive wastes, control of personnel exposure to radiation). Plans for new facilities should include the impacts of handling the proposed high beam powers.

Of the many identified challenges of high-power targets, radiation damage is of special note because it is complex, difficult to evaluate, and time-consuming to investigate. Atom displacements and gas production are the main underlying damage mechanisms, the particulars of which depend on primary beam characteristics, target materials, and specifics of the mechanical design. In the regimes of beam energy and intensity foreseen for Intensity Frontier experiments, one cannot simply scale from experience with nuclear power reactors. Moreover, the formation and growth of defects in target materials in extreme conditions is a mesoscale physics problem that is presently beyond present modeling and computational capabilities. In addition, techniques of accelerated testing are frequently not validated and/or appropriate irradiation facilities may not be available.

Sustained, focused research into target technology in the context of a broad international collaboration of interested laboratories will be essential.

Experiments on the Intensity Frontier offer a broad range of scientific opportunities accompanied by a similarly broad range of technical challenges that must be overcome to conduct these experiments in acceptable time frames. The technical challenges will require sustained, focused efforts. The time to begin is now.

6.5 High-intensity electron and photon beams

To offer new opportunities for physics discoveries heavy flavor factories must accelerate and store beams with characteristics that will be challenging to deliver. Two types of flavor factories are being constructed or proposed worldwide: super B factories and super tau-charm factories. SuperKEKB, a super-high-luminosity B factory, is an upgrade to the KEKB B -factory currently under construction in Japan, with commissioning scheduled to commence in January 2015. U.S. national labs and universities have made important contributions to the design. Two super tau-charm factories have been proposed: one at Frascati (Tor Vergata) in Italy and one at Novosibirsk in Russia. Both machines are two-ring, symmetric-energy machines, with provisions for longitudinally polarized beams.

For SuperKEKB to achieve its target luminosity of $8 \times 10^{35} \text{ cm}^{-2}\text{s}^{-1}$ — a 40-fold increase over that of KEKB — it will require beam currents approximately twice as high as used at KEKB and vertical bunch sizes at the collision point about 20 times smaller than those achieved at KEKB. Although it would be premature to contemplate increases in the design luminosity of a machine that has not yet started commissioning, there is performance headroom in machine subsystems for increasing luminosity beyond its baseline design value [15]. Continuing U.S. collaboration would strengthen the SuperKEKB project, and would support efforts to increase the luminosity to well above that targeted in the existing design. This result would mirror the operational experience with both KEKB and PEP-II.

Enhanced collaboration could also enable polarized beams in the collider. Polarization in SuperKEKB will involve preparing and injecting a beam of polarized electrons in the correct spin orientation parallel to the local precession axis at the injection point. While polarized sources with the required intensity exist (e.g., the SLC polarized gun), they are not low-emittance polarized sources. BNL has developed a candidate gun which is ready for commissioning and testing. The alternative would be to use a polarized DC gun followed by a damping ring to lower the emittance. The physics case needs to be made for both higher luminosity and polarized beams.

The technology development for super flavor factories exploits strong synergies with light sources and damping rings. Relevant areas of synergy include high top-up rate to compensate for low lifetimes, beam stability and control, control of electron cloud and fast ion instabilities, coherent synchrotron radiation issues with short bunches, timing jitter and the attendant energy jitter, and beam instrumentation. Many of these areas are important to the design of damping rings for a linear collider and would certainly apply to the design of a circular Higgs factory. Participation of U.S. accelerator personnel in commissioning and machine studies would help to maintain a high level of U.S. expertise with high-current, low-emittance electron storage rings, such as TLEP.

Three plans or proposals address physics opportunities using high-power electron and positron beams. The DarkLight experiment plans to use the 100 MeV, 10 mA electron beam of the Jefferson Lab free electron laser (FEL), impinging on a hydrogen target, to search for gauge bosons associated with Dark Force theories. The gauge bosons are predicted to decay into e^+e^- , leading to the final state $e^- + p + e^+ + e^-$. DarkLight will be unique in its ability to detect all four particles in the final state. The leptons will be measured in a large high-rate time-projection chamber (TPC) and a silicon detector will measure the protons. A 0.5 T solenoidal magnetic field provides the momentum resolution and focuses the copious Møller scattering background down the beam line, away from the detectors. A first beam test has shown the FEL beam is compatible with the target design and that the hall backgrounds are manageable. The DarkLight experiment has been approved by Jefferson Lab for first running in 2017. High-intensity electron beams may also be instrumental in fixed target searches for light, weakly-coupled particles including dark matter in the MeV to GeV range. One proposed experiment would require 10^{20} to 10^{23} electrons with energies of ≥ 1 GeV striking stationary nuclei in a beam dump to produce new weakly interacting states, which pass through shielding

material and scatter tens of meters downstream in a relatively small detector, one cubic meter or less. A more speculative proposal would use a high-intensity, low-emittance positron beam, impinging on a plasma target, to generate muon/anti-muon pairs. The beam energy should be just above the $\mu^+\mu^-$ production threshold with maximal beam energy asymmetry, i.e., a ~ 45 GeV positron beam interacting on an electron target. A low-power experimental test of this muon production rate may be possible at the FACET facility at SLAC.

6.6 Electron-ion colliders

Several designs of future electron-ion colliders have been under consideration in recent years. All of them are based at an existing accelerator facility. The collider configurations include both ring-ring and linac-ring options. Center-of-mass energies of the proposed colliders range from 14 GeV to 2000 GeV. Most of the collider concepts share enabling technologies. In all designs superconducting RF cavities must be able to operate with high average and high peak beam currents, providing effective damping of high-order modes. The cryomodule design must be able to accommodate containing high beam power. The hadron beam transverse and longitudinal emittances must be small to achieve a high electron-ion collider luminosity. In the designs with medium hadron energy, this requirement calls for the application of powerful cooling techniques. The low interaction region designs for all proposed colliders face the issues of strong focusing of beams at the collision point and fast separation of beams after the collision. The synchrotron radiation fan produced by electrons in the interaction region magnets must be kept from hitting the beam pipes whether inside or in the vicinity of the detectors or in superconducting magnets. Requirements imposed by the detector integration must be satisfied and chromatic corrections to the beam optics have to be implemented while maintaining an acceptable dynamic aperture. Approaches considered include the following:

- Special design of interaction region magnets, including Nb₃Sn superconductor technology.
- Collimators, absorbers, and masks at appropriate locations to provide protection from synchrotron radiation.
- Integrated dipole field in the detector design.
- Modified crossing angle geometry (in some designs), including crab-crossings.

The linac-ring designs utilize a high-current polarized electron source, with the average current ranging from 6 mA to 50 mA. A Gatling gun arrangement or large-size cathode gun can likely produce up to 50 mA current. The linac-ring scheme introduces non-standard beam-beam effects, which must be explored to understand the limits on the luminosity and the beam parameters. The effects include the electron beam disruption, the hadron beam kink instability, and the effect of the electron beam parameter fluctuation on the hadron beam. Other shared technologies include polarized beam sources and techniques to preserve beam polarization. In ring-ring colliders spin matching and the harmonic correction techniques have to be investigated further with the goal of minimizing the beam depolarization due to synchrotron radiation, especially in the presence of spin rotators and solenoidal detector magnets.

6.7 Accelerator R&D and test beam capabilities

This study identified a broad range of test capabilities present in existing or needed for proposed Energy and Intensity Frontier accelerators. The first category of test facilities permits testing beam physics or accelerator

components essential to manage technical risks in planned projects. A second category integrates accelerator systems to provide proof-of-practicality tests. The third category provides tests of physics feasibility of concepts and/or components. The study identified 35 existing test facilities [16] in the U.S. and overseas both with and without beam testing capability. Although these facilities provide substantial readiness to move forward with the highest priority accelerators for high-energy physics, the long-range future of high energy physics needs a few additional dedicated test capabilities in the near term.

6.7.1 Energy Frontier accelerators

Hadron colliders have dominated the high-energy physics landscape for several decades and are expected to continue in that status over many more years of the LHC operation. Several Energy Frontier hadron colliders are under consideration now, including upgrade of LHC luminosity to $5 \times 10^{34} \text{ cm}^{-2}\text{s}^{-1}$, upgrade of the LHC energy to more than 25 TeV, and an 80 to 100 TeV VLHC in a 80–100 km or longer tunnel. The most crucial technology issues for hadron colliders are in the area of superconducting magnets which are the largest drivers of the cost and of the energy and luminosity reach of these colliders. Of greatest challenge are Nb₃Sn 15 T arc dipoles and possible HTS superconductors such as YBCO and Bi-2212 capable of supporting dipole fields well above 20 T. For the VLHC, cheaper lower-field magnets and innovative low-cost tunneling technologies are of utmost importance to establish the financial practicality of a collider. Superconducting crab-cavities and very high-power beam dumps need to be developed for beams of such high energy.

Many engineering challenges will be associated with the expected few GJ of beam energy in colliders beyond the present LHC. Increased levels of synchrotron radiation will affect beam dynamics, machine design, and operating costs and complexity. These issues will require research to identify methods of efficient collimation and control, novel vacuum chamber designs, and development of the photon stops to protect the superconducting magnets and other equipment.

Many requisite technology developments for Energy Frontier hadron colliders described previously can be carried out at the existing superconducting magnet and RF test facilities at the DOE national laboratories, as well as at similar facilities overseas, foremost of which is at CERN. Some of these facilities may need upgrades to accommodate the research needs of specific projects (cable tests, transmission line magnet tests, etc.). Many beam dynamics studies concerning collimation and coherent instabilities can be performed at the LHC and its injectors, at the Fermilab Main Injector and Booster, and at RHIC. Most Project X research and development activities and facilities will directly address many issues related to the injector chain for a hadron collider beyond the LHC. Several promising, novel accelerator schemes such as resonance-free, integrable optics, space charge compensation with electron devices, and optical stochastic cooling require a dedicated test accelerator for evaluation. The Integrable Optics Test Accelerator (IOTA) at the Advanced Superconducting Test Accelerator (ASTA) facility at Fermilab is a suitable match for the task.

Although a primary challenge of any Energy-Frontier e^+e^- linear collider project is value engineering to reduce costs, specific technical challenges remain for design of the positron source and the beam delivery system. These issues are being addressed at facilities including ATF2 at KEK, FLASH at DESY, and ASTA at Fermilab. A CLIC-type system has additional challenges to develop and industrialize the accelerating structures and major components. These questions are being actively addressed at CesrTA, ATF2 and CTF3.

At energies less than 250 GeV per beam, in tunnels with sufficiently large circumference, circular colliders may potentially provide a high luminosity while drawing on a base of mature technology with rich experience. Primary challenges for rings are the large synchrotron power, beamstrahlung, and large momentum accep-

tance required. Many of these issues are being addressed in large part at flavor factories, diffraction-limited light sources, and beam test facilities.

Photon collider designs allow access to the CP property of the Higgs boson, and they can study it at a lower electron beam energy with a high degree of polarization without the need for positrons and ancillary systems. Primary challenges for photon colliders relate to generating sufficient photon flux with high-energy lasers as well as new interaction region design. Appropriate laser technology is being developed within the international consortiums and at LLNL in the U.S.

A muon collider scales well to high energies because the relatively large mass of the muons allows for tight bends and compact accelerator geometry. The primary challenge for muons is creating and cooling them to sufficient phase space density to allow for high enough luminosity. The Muon Accelerator Program is coordinating the R&D into the major issues with experiments primarily at FNAL and RAL.

The main challenges for plasma accelerators include demonstrating a concept for positron acceleration and demonstrating staged acceleration with the required beam brightness and efficiency, and those are being addressed through multi-institution collaborations conducting experiments at dedicated and proposed facilities BELLA/BELLA-II at LBNL and FACET/FACET-II at SLAC.

A major challenge in applying plasma wakefield accelerator concepts to high-energy colliders is staging successive drive beams to accelerate a sufficiently intense witness beam to the desired energy. Staging requires the generation of high-intensity, multiple-drive electron/laser beams capable of high-repetition-rate operation. Tailoring the longitudinal characteristics of a drive beam may effectively increase the energy transfer efficiency from the drive beam to the witness beam, thus reducing the number of required stages. Issues of wakefield acceleration in dielectric structures include identifying materials with appropriate breakdown voltage characteristics, identifying optimal structure geometries, and demonstrating staging with the required efficiency. Relevant research is being conducted at the AWA at ANL, ATF at BNL and FACET at SLAC. The NLCTA facility at SLAC hosts a dedicated beamline studying the primary issues for laser-driven microstructures including damage limits, coupling geometry, staging, and maintaining phase tolerances.

Several facilities in the U.S. are exploring wakefield accelerators. AWA at Argonne combines multiple 75 MeV drive beams with a 15 MeV witness beam. BELLA at LBL uses a 1 PW laser to conduct experiments that synchronize two laser beamlines to accelerate a single, sub-GeV electron beam. FACET at SLAC uses a 30 GeV beam from the SLAC linac to investigate positron acceleration, high-transformer ratio configurations and the “afterburner” concept. Multiple drive beam generation and pulse shaping is one of the major goals for the proposed FACET II upgrade.

6.7.2 Intensity Frontier accelerators

The short- and medium-term needs of the U.S. Intensity Frontier program will employ Fermilab proton accelerators — power-upgraded Booster and Main Injector augmented by the “muon campus” rings ($g-2$, $\mu 2e$, nuSTORM, etc.). Longer-term aspirations require construction of the proposed Project X, described in Section 6.4.1. Project X could support a wide range of experiments based on neutrinos, muons, kaons, nucleons, and nuclei and also can form a basis of the front-end of a future Neutrino Factory. The proposed DAE δ ALUS experiment calls for construction of several very high-power, 1 to 5 MW, 800 MeV proton cyclotrons. The primary design constraints in high-power hadron accelerators for the Intensity Frontier are minimizing beam loss (< 1 watt/m, or $< 0.01\%$ total beam loss), and flexible beam delivery in experiment-specific time formats ranging from quasi-CW for rare particle decays to single few-ns-long bunches. Additional challenges facing designers of Intensity Frontier accelerators include: (1) generating high-current,

high-quality beams, (2) efficiently accelerating high beam currents to high energies, (3) controlling emittance and minimizing beam halo, (4) manipulating beams on a bunch-by-bunch basis for parallel experiments, (5) maintaining low loss during beam extraction, and (6) creating target systems for extreme power densities and extreme radiation environments.

Project X R&D activities at the PXIE facility and beam studies with the SRF cryomodules at Fermilab's ASTA facility will directly address many issues listed above. Many beam experiments on efficient collimation, coherent instabilities, and novel extraction methods can be performed at Fermilab's Main Injector and Booster, at the ORNL's SNS, or at the LHC and its injectors. Studies of promising novel accelerator techniques — like halo suppression in the integrable optics and space charge compensation with electron devices — can be performed at the IOTA at the ASTA facility. A dedicated facility is needed for development and tests of high-power targets, though some target-related issues can be explored at existing accelerators. Many tests and technology demonstrations to support the DAE δ ALUS experiment design work can be performed at the existing facilities, such as INFN-Catania, RIKEN, ORNL, or in the private sector at medical cyclotron companies.

6.8 Summary remarks

The approved LHC program, its future upgrade towards higher luminosities (HL-LHC), and the study of an LHC energy upgrade or of a new proton collider delivering collisions at center-of-mass energies up to 100 TeV, are all essential components of exploring the Energy Frontier with proton colliders. Therefore the priority recommendation of our study is full exploitation of LHC. Doing so requires a strong LHC Accelerator Research Program sponsored by the OHEP that transitions to a US-LHC high luminosity construction project. During the project period we recommend continuing a focused, integrated, laboratory program that emphasizes the engineering readiness of technologies suitable for a 26 TeV upgrade of the LHC or a machine of higher energy in a larger tunnel. The most critical technology development toward higher-energy hadron colliders is the next generation high-field Nb₃Sn magnets (limited to about 15 T) and adequate beam control technology to assure machine protection.

Our study welcomes the initiative for ILC in Japan. The U.S. accelerator community is capable of contributing as part of a balanced U.S. high-energy physics program. As described in its Technical Design Report (TDR), the ILC is technically ready to proceed to construction. The TDR incorporates leadership U.S. contributions to machine physics and technology in superconducting RF (SRF), high-power targets for positron production, beam delivery, damping ring design, and beam dynamics such as electron cloud effects. Plasma-based accelerators (beam or laser driven) providing GV/m accelerating fields would open the way to linear colliders in the multi-TeV range with a relatively small footprint if the preservation of beam quality during multi-stage acceleration can be demonstrated. A circular muon collider, if feasible, could provide discovery-level experimental capabilities at energies from the Higgs *s*-channel threshold at 126 GeV up to the multi-TeV scale. Such a collider could probe narrow physics features with extreme precision, because the larger mass of the muon suppresses synchrotron radiation during the collision process that would spread the energy of the beams.

Required accelerator capabilities for Intensity Frontier experiments are more diverse than for the energy frontier. The common characteristics required are (1) average beam power > 1 MW delivered with (2) a flexible, experiment-dependent time structure. The overarching conclusion of the study is that the next generation of intensity frontier experiments requires beam intensities and timing structures beyond the capabilities of any existing accelerator. Fermilab's proposed, multi-stage Project X would yield a world-leading facility that would simultaneously deliver a flexible “on-demand” beam structure that could serve multiple experiments over an energy range 0.25 – 120 GeV. Our study heard exciting possibilities for

DAE δ ALUS, a capability of narrower experimental scope than Project X. For both proposed capabilities high-power targets are a difficult challenge that limits facility performance. One cannot simply scale the performance with high-energy beams from experience with nuclear reactors. Computed radiation effects in inhomogeneous materials, subject to time-varying irradiation, need validation with controlled, instrumented in-beam tests.

The relevant technologies of super flavor-factories exploit strong synergy with light sources and damping rings for lepton colliders. Doubtless the operation of such machines such as SuperKEKB, the super-high-luminosity B factory, will inform the final design of damping rings for a energy frontier lepton collider. All electron-ion colliders studied in recent years would be based at an existing accelerator facility. The collider configurations include both ring-ring and linac-ring options; center-of-momentum energies range from 14 GeV to 2000 GeV. Most of the collider concepts share enabling technologies. Superconducting RF cavities must be able to operate with high average and high peak beam currents, providing effective damping of high-order modes.

Although existing accelerator test-bed facilities provide substantial readiness to advance the highest priority accelerators for high-energy physics, the long range future of high energy physics needs a few additional dedicated test capabilities in the near term, most notably, for high power targetry, muon accelerators, and high-intensity accelerator systems. The group did not examine the process in basic accelerator physics and modeling that must accompany any long-range research program. Such research bears fruit beyond the confines of specific projects and is essential to the long-term health of the field.

The Snowmass Accelerator Capabilities Study offers the following summary findings and recommendations:

- To maximize the exploration of the Energy Frontier, the full exploitation of the LHC is the highest priority of the hadron-collider program.
- As described in its Technical Design Report (TDR), the ILC is technically ready to proceed to construction.
- A vigorous, integrated U.S. research program toward demonstrating feasibility of a muon collider is highly desirable. The current funding level is inadequate to assure timely progress.
- The overarching conclusion is that the next generation of Intensity Frontier experiments requires beam intensities and timing structures beyond the capabilities of any existing accelerator.
- Fermilab's proposed, multi-stage Project X would yield a world-leading facility that could serve multiple experiments over an energy range 0.25 – 120 GeV.
- Sustained, focused research into high-power target technology in the context of a broad international collaboration of interested laboratories will be essential to frontier accelerator facilities.
- The relevant technologies of super flavor-factories exploit strong synergy with light sources and damping rings for lepton colliders.
- All electron-ion colliders studied in recent years would be based at an existing accelerator facility with center-of-momentum energies range from 14 GeV to 2000 GeV.
- Innovations in acceleration and beam transport techniques such as plasma and dielectric wakefield acceleration have a significant potential to reduce the size of future accelerator facilities.
- Long-term research including fundamental accelerator and beam physics theory and simulation will expand the technical options for any future accelerator-based facility. Focused engineering development is no substitute for innovative R&D.

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Underground Laboratory Capabilities

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7.1 Introduction

Many of the most compelling scientific issues in particle physics can only be addressed with experiments operating at underground facilities. For example, large underground facilities are needed for direct searches for dark matter and neutrinoless double-beta decay ($0\nu\beta\beta$), and the scale and complexity of these experiments will continue to increase for the foreseeable future. The importance of underground experiments using solar, reactor, atmospheric, supernova neutrinos and neutrinos from accelerators to elucidate neutrino properties is also expected to grow over the next decade and beyond.

Underground facilities are located in North America, Europe, Asia and Antarctica (ice). In the last few years, new underground facilities have become operational in Canada (SNOLAB), China (China Jinping Deep Underground Laboratory – CJPL), Spain (Canfranc Laboratory, or Laboratorio Subterráneo de Canfranc – LSC) and in the United States (Sanford Underground Research Facility – SURF). Experiments continue to be operated or assembled at older underground facilities in Asia, Europe, the United States and in ice at the South Pole. The world-wide particle physics community plans to expand underground capabilities over the next few years. Significant expansions are underway or planned in China (CJPL and JUNO), Korea (RENO50 and Yangyang Laboratory), Japan (Hyper-K), France (Modane extension), India (India Neutrino Observatory – INO), South America (Agua Negra Deep Experiment Site – ANDES) and possibly in Finland (Center for Underground Particle Physics – CUPP). If all of these plans are realized, general-purpose space for underground experiments would roughly double by the end of the decade. There would be major new facilities for reactor experiments at moderate depths (JUNO and RENO50) and for a new class of very large facilities for long-baseline and atmospheric neutrino experiments, proton decay and other physics. New experiments, which would require substantial facility improvements, are also proposed at the South Pole.

Plans for expansion of underground facilities in the United States are less developed. Currently, there are no approved plans with federal funding for expansion of underground capabilities at the Kimballton Underground Research Facility (KURF), the Soudan Underground Laboratory, the Sanford Underground Research Facility (SURF), or the Waste Isolation Pilot Plant (WIPP). The Long-Baseline Neutrino Experiment (LBNE) has provisional approval to be located on the surface at SURF, but design work is underway in anticipation of achieving a global collaboration to allow LBNE to be sited deep underground at SURF.

The first-generation experiments in direct dark matter detection (G1) are those operating and producing results by 2013. G2 is the generation of experiments under construction now or in the near future, and G3 the larger experiments being considered for construction late in this decade. All of the next-generation (G2) dark matter experiments can be accommodated by existing or planned underground facilities, assuming no reduction in these facilities for the rest of the decade. Most G2 experiments are at facilities outside the U.S. U.S. physicists are participating in most of the G2 experiments, and they started and are leading many of them. A G3 experiment is likely to be 5–10 times the volume of the G2 experiment of similar technology and

mass reach. It seems likely that a facility with depth ≥ 3400 mwe (e.g., LNGS) will have sufficient depth for a G3 experiment but improved simulations and results from G2 experiments will be definitive. The U.S. does not now have an underground hall large and deep enough to house a large G3 experiment. Such a new hall in the U.S. would most naturally be at SURF. It is premature to develop plans for facilities dedicated to a large directional experiment.

Several $0\nu\beta\beta$ experiments are already under construction at existing underground facilities, all but one of these outside the U.S. U.S. involvement is currently strong in many of these experiments. Next-generation (“tonne scale”) neutrinoless double-beta-decay experiments can likely be accommodated by existing and planned facilities, but may face competition for space from G2/G3-scale dark matter experiments. One next-generation experiment with large U.S. involvement (which may or may not be sited within the U.S.), with potential participation in others, is the current U.S. planning, under the auspices of DOE nuclear physics. Depth requirements for tonne-scale $0\nu\beta\beta$ experiments depend on the choice of technology and are not yet entirely known. New information may be available on a 6-month to 2-year timescale. The path beyond tonne-scale experiments is not well defined but may require expanding underground facilities.

There are worldwide efforts towards reactor experiments at medium baseline (~ 50 km) and short baseline (~ 10 m). Detectors for reactor experiments at >100 m baseline require medium-depth underground laboratories (several hundred mwe overburden). There has been strong U.S. involvement in recent reactor experiments overseas (KamLAND, Daya Bay, Double Chooz). Planning and R&D towards future reactor experiments overseas is underway with funding commitments from the host countries (RENO-50, JUNO). There may be U.S. involvement in these experiments. There may be synergistic efforts with nuclear non-proliferation activities in the U.S. In this regard, a new U.S. remote reactor monitoring initiative requires a 500-5000 mwe site to demonstrate sensitivity to reactor antineutrinos using a large Gd-water-Cherenkov detector. The 1600 mwe Fairport mine near Cleveland Ohio and the 2800 mwe Boulby mine in England are viable options. A kiloton-scale device will have world-class supernova sensitivity. Upgrading to liquid scintillator may enable geo-antineutrino measurements.

There is an international effort to search for CP violation in the lepton sector. A massive detector in a neutrino beam is required. The search for nucleon decay is one of the most important topics in particle physics. Atmospheric neutrinos, observable in a large underground detector, may be sensitive to all of the currently unknown neutrino oscillation parameters. Some of the same detectors could be used to advance the search for nucleon decay, the study of atmospheric neutrinos, and other physics if the detector is located underground. This is the plan for Hyper-K (Japan) and LBNO (Finland). It would be a lost opportunity if this condition cannot be satisfied with LBNE. The use of cyclotrons (or dedicated reactors) might increase the number of potential facilities for neutrino oscillation experiments in the U.S.

There is a tremendous opportunity for physics and astrophysics from detection of a supernova neutrino (SN) burst. SN detection capability typically comes for free if the detector is underground but is very difficult on the surface. Bursts are rare (only every ~ 30 years): it is therefore critical to gather as much information as possible from each event. Diverse flavor sensitivity is important for physics. The unique sensitivity of LBNE to electron flavor will be lost if the far detector is not sited underground. Broader low-energy neutrino/nuclear physics experiments (large-scale solar neutrinos, geoneutrinos, low-energy nuclear astrophysics) may require new underground spaces at existing facilities and perhaps new facilities.

Underground space should be reserved for materials assay and storage. Selection of radiopure materials for shielding and detectors is a common need. The majority of such measurements must be done underground, requiring sensitive detectors, expert personnel, and long-term storage of materials (e.g., Cu) sensitive to cosmogenic activation. Experimental needs worldwide far outstrip current assay capability. Operation as a user facility across multiple sites with existing expertise is the most efficient use of resources and personnel. In addition, underground space should be reserved for small prototype testing and generic R&D. New

experiment technologies need to go underground to validate background performance. There is enough U.S. infrastructure space for the future if existing U.S. underground labs are maintained. Substantial past agency investment and future leverage of state, university, private and other agency (e.g., non-proliferation) funds make it cost effective and attractive to maintain these sites for smaller experiments, generic R&D, and as elements of a centrally managed materials assay consortium.

7.2 Facilities for large underground proton decay and neutrino experiments

7.2.1 Physics goals

The main physics motivations for large underground detectors are for neutrino physics — both oscillation physics and study of astrophysical sources — and searches for baryon number violation. These topics span event energies ranging from less than an MeV to the TeV scale.

- Long-baseline neutrino oscillation: The current generation of neutrino oscillation experiments has made measurements of two mass-squared differences, Δm_{21}^2 and $|\Delta m_{23}^2|$, and now all three mixing angles. The remaining unknowns in the three-flavor picture are the mass hierarchy, the CP phase angle δ , and the octant of θ_{23} . Long-baseline neutrino beams of $\sim$ GeV energies coupled with large detectors can address all these unknowns, and can addition improve precision on parameters and search for beyond-the-SM physics (e.g., non-standard interactions, sterile neutrinos). Shorter-baseline pion decay-at-rest neutrinos can in addition address CP-violation physics (e.g., DAE δ ALUS).
- Atmospheric neutrinos: The atmospheric neutrino flux covers a wide range of energies and provides neutrinos over several orders of magnitude in baseline. Atmospheric neutrinos, observable in a large underground detector, are sensitive to all of the currently unknown oscillation parameters.
- Baryon number violation: To date, the search for nucleon decay has not yielded any positive evidence, but the absence of nucleon decay, now extended beyond a lifetime of 10^{34} years, has provided stringent constraints that must be addressed by any proposed grand unified theory (GUT). Additional exposure will probe and constrain these models.
- Solar neutrinos: If the large underground detector has a sufficiently low energy detection threshold, then the detector could add to our knowledge of solar neutrino oscillations and potentially probe exotic physics. Study of solar neutrinos may also improve knowledge of solar astrophysics.
- Supernova neutrinos: Detection of a burst of core-collapse supernova neutrinos would bring a wealth of information on both neutrino physics and astrophysics. There may in addition be oscillation parameter information to be had (e.g., mass hierarchy) by, e.g., comparing neutrinos from the burst that have traveled through Earth matter with those that have not.
- Relic supernova neutrinos: A massive underground detector would also greatly enhance the search for relic supernova neutrinos, i.e., those from supernovae throughout the history of the universe. These neutrinos are sensitive to cosmology (e.g., star formation rate) and the supernova explosion mechanism (e.g., typical neutrino temperatures) as well as exotic scenarios like neutrino decay.
- Geoneutrinos: Detectors with very low energy threshold and low levels of background can probe neutrinos from radioactive decays in the Earth, connecting to geophysics.

- Short-baseline oscillations: Searches for sterile neutrinos using radioactive sources coupled with large low-energy-threshold detectors have been proposed.
- Neutrinoless double-beta decay: Candidate isotopes may be dissolved in detector material to enable searches for neutrinoless double-beta decay in large detectors. Detection or limits on this rare process will be critical for understanding the Majorana or Dirac nature of the neutrino.

7.2.2 Large underground detectors

In general, the same large underground detectors built for neutrino physics can be used for baryon number violation searches — indeed, historically, the original motivation for large underground detectors was to find proton decay predicted by SU(5). Detectors come in several types: iron trackers, water Cherenkov detectors, liquid argon time projection chambers, and liquid scintillator detectors. Each has pros and cons.

- *Water Cherenkov detectors* (e.g., Super-K, Hyper-K) have the advantage of cheap detector material making large volumes possible, and a technology proven at multi-ton scale. Directional information is available from the Cherenkov cone, allowing multiparticle reconstruction. Events with energies from a few to hundreds of GeV are accessible. Disadvantages are the relatively low Cherenkov light yield and limits on reconstruction of low-energy particles due to the Cherenkov threshold. Water Cherenkov detectors also exist in the form of long strings of photomultipliers deployed in water or ice (e.g., IceCube, Antares). Such detectors have Mton-scale masses and sensitivity to very high-energy neutrinos but are not capable of precision reconstruction (although future upgrades, e.g., PINGU, ORCA, MICA, may mitigate this).
- *Liquid argon time projection chamber detectors* (e.g., Icarus, LBNE) have the advantage of excellent tracking and reconstruction, enabling high efficiency for high-energy interactions and some proton decay modes (e.g., νK^+). However LArTPCs are unproven at multi-kton scale and entail technical challenges associated with large volumes of cryogenic liquid. Low-energy physics may be challenging.
- *Scintillator detectors* (e.g., Borexino, KamLAND, LENA) have the advantage of very high light yield, resulting in potentially very low energy thresholds and excellent energy resolution. The technology is also well proven. The disadvantages and challenges are relatively high expense of detector material, stringent cleanliness requirements associated with low energy thresholds, and difficulty in reconstruction of high-energy multiparticle events due to isotropy of light emission. These detectors are most suitable for low-energy physics.
- *Tracking detectors* (e.g., Soudan II, MINOS, INO, OPERA, MIND) have the advantages of proven technology, good reconstruction capability at high energy, and ability to employ a magnetic field to sign-select interaction products. Disadvantages are relatively high-cost detector material and low sensitivity at low energy.

7.2.3 Underground requirements and issues for large detectors

While large detectors for long-baseline neutrino experiments can be located on or near the surface due to tight event timing cuts, nucleon decay detectors must be located underground to shield them from constant cosmic-ray background. Excavations for these large detectors are costly and must be situated in regions of high rock quality to minimize construction costs. Depending on the detector technology, the infrastructure

requirements can include cryogenic systems, water purification systems, air conditioning, radon control, low-background construction materials, etc. Once this laboratory infrastructure is established, other experiments, such as those for direct dark matter detection and double-beta decay, which also require this underground environment, can be located there. There are many potential sites that can house large detectors. Since overburden is not a crucial factor for long-baseline experiments, almost any surface or near surface site in the neutrino beam would be usable. Examples here include the possible surface installation of the LBNE LAr detector and the proposed use of a water-filled mine pit in Minnesota (CHIPS). The overburden requirement for large detectors for nucleon decay and other physics depends to some degree on the chosen technology and the specific physics topic: see Table 7-1. The low-energy thresholds needed for such physics as solar neutrino and relic supernova neutrino detection require depths where the cosmic-ray muon-induced dead times and backgrounds are sufficiently reduced.

Physics	Water	Argon
Long-baseline accelerator	1000 mwe	0–1000 mwe
$p \rightarrow K^+ \nu$	> 3000 mwe	> 3000 mwe
Day/night ^8B solar ν	~4300 mwe	~4300 mwe
Supernova burst	3500 mwe	3500 mwe
Relic supernova	4300 mwe	>2500 mwe
Atmospheric ν	2400 mwe	2400 mwe

Table 7-1. *Estimated depth required to study various neutrino physics and proton decay processes, with a water Cherenkov detection or with a liquid argon detector, from [1].*

Underground sites that could potentially accommodate a new large detector include the Homestake mine in South Dakota, which will be in the LBNE beam, the Pyhasalmi mine in Finland, which could be in the proposed LBNO neutrino beam, and the Kamioka mine in Japan in the T2K neutrino beam. Other sites include the WIPP facility in New Mexico, the INO site in India, the LNGS laboratory in Italy, and the Soudan site in Minnesota. The first two have no planned neutrino beam; the latter two have beam now but will have no beam later. Table 7-2 gives a summary.

We note that detectors proposed for further reactor experiments (e.g., JUNO) with significant overburden could also be considered in the large detector category. We additionally note that the DAE δ ALUS and IsoDAR concepts could be coupled with a large inverse-beta-decay sensitive detectors.

7.3 Facilities and infrastructure for low-background experiments

7.3.1 Summary of requirements

Unlike neutrino experiments, rare event searches such as dark matter and $0\nu\beta\beta$ decay place less emphasis on cavern size and more on background reduction. Thus, they tend to prefer deeper sites with a smaller footprint, but require more in the way of radiopurity infrastructure. Since dark matter and $0\nu\beta\beta$ decay are exploratory programs which cover large unknown parameter space, they require multiple targets and complementary techniques. A healthy discovery program for dark matter must include a few large G2 experiments that probe favored WIMP models to the highest sensitivity; smaller experiments that explore alternate mass regimes, targets, and interaction processes; and required auxiliary efforts, such as low-background screening and storage. One needs to account for several similarly large neutrinoless double beta decay experiments worldwide that compete for the same space, but might share infrastructure such as radiopurity assay.

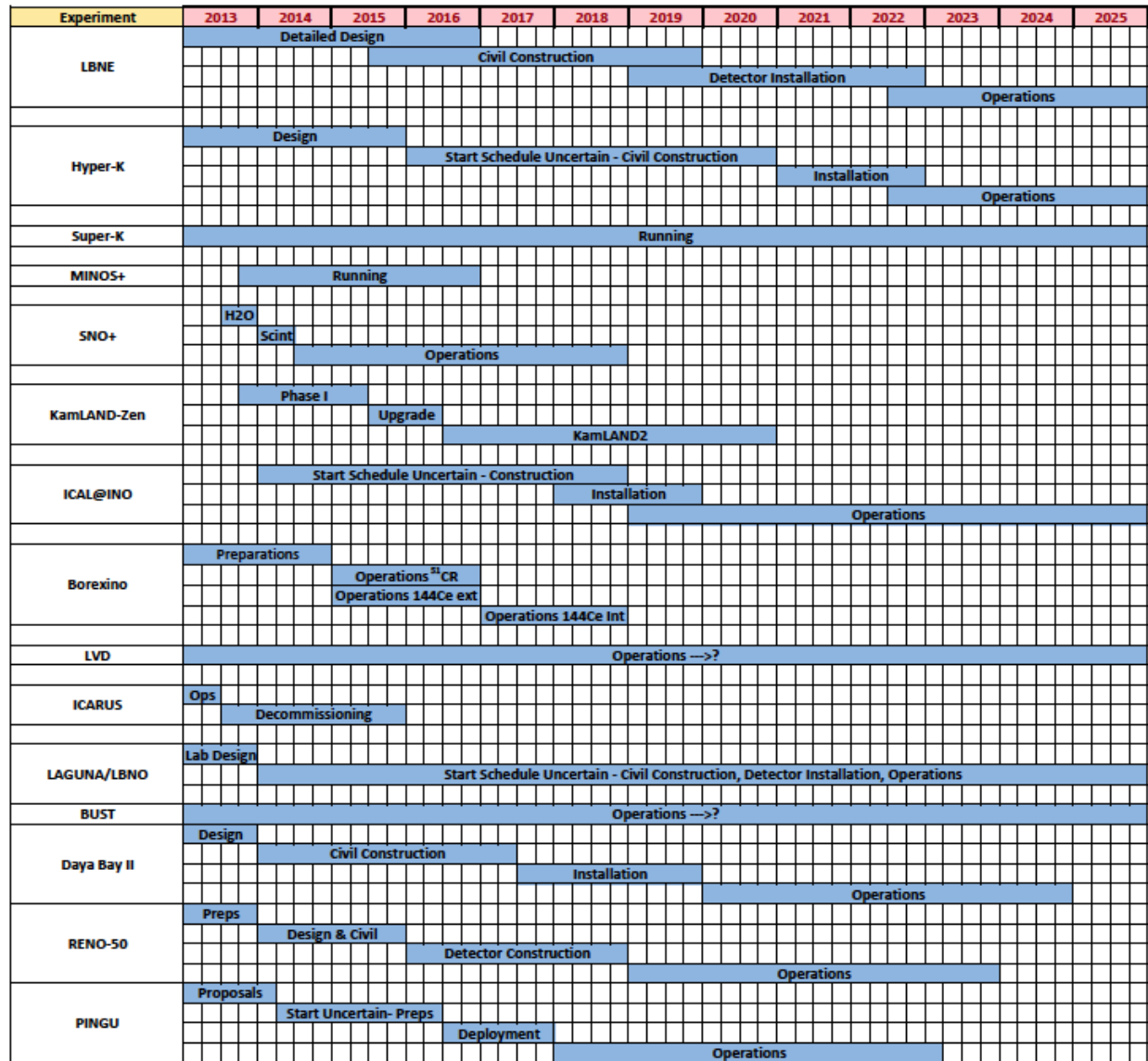


Figure 7-1. Approximate timelines of underground neutrino and proton-decay experiments.

Facility	Location	Overburden (mwe)	Past/existing large underground detectors	Proposed/planned large underground detectors	Beam	Ref.
Homestake	USA	4290	Homestake chlorine detector	LBNE (LArTPC)	Future from FNAL	[2]
Soudan	USA	2090	Soudan II, MINOS		Current from FNAL	[3]
LNGS	Italy	3800	LVD, Borexino, ICARUS, OPERA		Recent from CERN	[4]
Pyhäsalmi	Finland	3900		MEMPHYS, LENA, GLACIER, MIND	Future from CERN, Protvino	[5]
SNOLAB	Canada	6010	SNO, SNO+			[6]
Kamioka	Japan	2700	Kamiokande, Super-K, KamLAND	Hyper-K (Tochibora or Mozumi site)	Current and future beam from J-PARC (T2K, T2HK)	[7]
INO	India	3500		ICAL		[8]
Baksan	Russia	850	BUST	BUST upgrade		[9]
Antares	Mediterranean	2500	Antares-12 String	ORCA		[10]
IceCube	South Pole	1500	IceCube-86 strings	PINGU, MICA		[11]

Table 7-2. *Underground sites for large proton decay and neutrino detectors.*

Currently there appears to be enough underground space worldwide for several relatively large G2 dark matter experiments, as well as smaller complementary efforts. The pinch points are the limited assay infrastructure worldwide and space for the future G3 dark matter experiments which can probe the irreducible neutrino background. These G3 experiments will be physically large due to the substantial shield/veto systems required to mitigate radiogenic and cosmogenic backgrounds. The DUSEL S4 engineering studies provide a rough envelope for the size of these experiments ($\sim 25\text{m} \times 15\text{m} \times 15\text{m}$). Moderate depths (> 2000 mwe) can be tolerated by the use of a large, carefully designed active shield/veto, but the risk of unexpected cosmogenically-induced backgrounds is greater at shallower depths. Sites at deeper locations also require a smaller footprint for G3, since their active shielding could be more compact. The Cryopit and Cube Hall caverns at SNOLAB are large enough, and their 6010 mwe depth is suitable for both dark matter and $0\nu\beta\beta$ decay, but their availability on the timescale of such experiments is not clear. The Laboratori Nazionali del Gran Sasso (LNGS) has extensive underground space capable of hosting G3 experiments at a 3000 mwe effective depth. The Laboratoire Souterrain de Modane (LSM) has a depth of 4800 mwe. A major expansion to $17,500\text{ m}^3$ is expected to become available in roughly 2017 which could house one G3 experiment. Jin Ping II in China will be a site of substantial depth (7000 mwe) and is expected to have $100,000\text{ m}^3$ of space in a format large enough for G3 experiments on the 2016 timescale. The India Neutrino Observatory (INO) may have an available volume for non-neutrino experiments of about 26000 m^3 , but there is some

uncertainty about the 2018 timescale. It should be clearly noted that there are no U.S. sites capable of hosting experiments of this size. The available caverns at the Soudan Lab (2100 mwe) and the Sanford Underground Research Facility at Homestake (4200 mwe) are too small and would require new excavation.

The next-generation dark matter and $0\nu\beta\beta$ decay experiments require unprecedented levels of radiopurity in their detector, target, and shielding materials. These needs will require significant investment in the tools needed to measure such radiopurity, both new techniques to improve sensitivity and increased throughput in moderate-sensitivity production screening. While mass spectrometry and atom trap techniques can be done above ground, the vast majority of assay techniques require shielded and low-background environments to obtain the required sensitivity. Therefore it is absolutely essential to reserve space underground dedicated to screening and associated functions like sample preparation and radon-free storage. Space for long-term storage of activated materials, such as copper and germanium, is also beneficial.

Space underground also needs to be reserved for R&D, which includes small prototypes of proposed experiments, novel screening techniques, and general physics related to understanding backgrounds for experiments. The latter projects may be measurements of muon, gamma, or neutron backgrounds as a function of geology and depth, and often include direct comparison to simulation with the purpose of improving the Monte Carlo physics models. The reserved space should be reconfigurable, but some on-site infrastructure will enable small-scale projects that cannot afford this overhead to realize their goals, as well as reducing the cost for larger projects. Useful infrastructure includes gamma-ray and neutron shielding to reduce radiogenic backgrounds from the rock, an umbrella muon veto for shallower sites, cryogenic liquids or nitrogen and helium liquifiers, radon reduction systems, or even water purification and a multi-purpose water shielding tank.

7.3.2 Assay needs

Determining the number and sensitivity of the assay capabilities which should be available to the field must take account of the long lead time required for low-level assay and the screening needs preceding the commissioning of each experiment, typically by 3–5 years, as the assays inform design, as well as establish quality control of all pre-installed components. Screening needs were collected for all dark matter collaborations by the Cosmic Frontier WIMP Direct Detection group (CF1) as part of the Snowmass process. The results of the survey [12] indicate that the current suite of assay techniques and screeners are already inadequate for current experiments in both sensitivity and throughput. The type of assay required depends on the material to be screened, whether it is a passive or active element, and the required level of sensitivity. This implies that a variety of techniques and facilities will be necessary, which must be matched to the needs of the experiment and to the capabilities of existing screeners.

As a function of depth and increasing sensitivity, the situation in the U.S. today is a decentralized set of detectors dedicated to specific research projects at surface sites, a few shallow depth sites which have evolved into user facilities, and a few deep sites which are challenged to provide the ultra-low counting needs for the next generation of double-beta decay, neutrino, and dark matter experiments. These ultra-sensitive screening detectors can also be used by geology, microbiology, environmental science, and national security applications to identify radioisotopes, date samples, and measure tracers introduced into hydrological or biological systems. After the initial dialog among the different disciplines identified by the DUSEL effort, there has not been much subsequent dialogue with other fields that might also benefit from the ultra-low counting sensitivities available at deep sites. Indeed the geophysics and geomicrobiology fields have been looking for physics leadership to establish the sensitive assay capabilities. While this is recognized as a

means to enhance the user base and increase capability, it has only been possible to achieve in large national laboratories with historical ties to other communities, notably homeland security applications.

Although the background is reduced significantly by moving screening detectors underground, it is not possible to exploit extreme depths without additional effort, since backgrounds internal to the detectors themselves may become the limiting factor beyond an overburden of typically 1500 mwe. The low-background counting facility in Gran Sasso has demonstrated its ability to achieve exceptionally low backgrounds in the detectors themselves, and consequently benefit from their additional overburden. Moreover, for many practical applications, the counting time itself would become challengingly long. Thus, many underground screening detectors do not benefit from placement at great depths without the commensurate attention to internal backgrounds. The use of muon veto systems can increase the effectiveness of very shallow sites such as PNNL and Oroville.

7.3.3 Existing infrastructure

Existing U.S. facilities which could be extended to users range from radiochemical and mass spectroscopy techniques on the surface to increasing levels of sensitivity moving from shallow to deep underground sites. Table 7-3 provides information on assay infrastructure in the U.S.

In other countries, the assay infrastructure is much more developed. It has been built up in conjunction with centralized laboratories such as SNOLAB in Canada and LNGS in Italy, and thus has the advantage of co-location with many of the experiments it services. The existence of centers for assay creates the critical mass of experts and organization to extend the services to other fields, producing, in turn, a broad user community capable of producing a self-sustaining business model. Tables 7-4 and 7-5 provide information on foreign assay infrastructure.

The original vision for DUSEL included the same attention to assay infrastructure as is manifested in the investments of Europe, Canada, and Japan (and now China). However, without such a National Underground Laboratory to concentrate screening in one location and to justify the expense, we may require a new model. Over the last decade, the gap between the needs of rare event physics searches and available screening has widened considerably, since it has been left to individual experiments to propose individually what should be common to all. This has led to a shortage of screening infrastructure overall and uneven distribution of existing resources.

7.3.4 Assay techniques required

High-purity germanium gamma ray spectroscopy (HPGe) is a mature technology which serves as the prime tool for material selection. Moderate sensitivity to the ubiquitous background from radioisotopes of potassium and the uranium/thorium chains is routinely achieved using commercially available detectors. A large number of such moderate sensitivity, production screeners are needed to deal with the high throughput of samples expected over the next decade. To meet this need, additional HPGe stations should be located at the three U.S. underground sites, which already have the staffing and expertise to produce quick turn-around for multiple users.

For the most sensitive applications, R&D in ultra-sensitive HPGe technology should be pursued at several sites where expertise already exists. With appropriate choice of low-background cryostat and shielding, multiple crystal coincidence detectors with pulse shape discrimination can reach $\sim 10^{-12}$ g/g. HPGe with

Facility	Depth (mwe)	Suite of detectors and technology
Berkeley LBCF	Surface	2 HPGe (1 with muon veto) managed by LBNL, 100% use by others NaI, BF ₃ counting, shielded R&D space
PNNL	Surface	ICPMS: Dedicated instrument and clean room facilities for low-background assay 6 commercially shielded HPGe detectors considering use by others
PNNL ULab	30	Copper electroforming and clean machining 14-crystal HPGe array considering use by others multiple commercial HPGe for various stakeholders
Oroville (LBNL)	530	1 HPGe managed by LBNL 100% use by others Large shielded R&D space
Kimballton (KURF)	1450	2 HPGe managed by UNC/TUNL 50% use by others
Soudan	2100	1 HPGe managed by CDMS 10% use by others 1 HPGe managed by Brown, dedicated to LUX/LZ 6000 m ³ lab lined with muon tracker + 2 muon-correlated neutron detectors Large R&D space with muon tag provided
Homestake (SURF)	4300	1 HPGe managed by CUBED, priority to LZ, Majorana, other users by negotiation

Table 7-3. *U.S. assay resources [12, 13].*

thin windows and low thresholds are useful for extending background characterization into the x-ray region (useful for solar neutrino projects), understanding cosmogenic activation, and surface contamination.

The signal from many trace isotopes can be enhanced using neutron activation analysis (NAA) to observe counts from short-lived isotopes produced by epithermal neutron capture. Such enhanced signals do not need a shielded underground site to achieve ppt sensitivity and can be counted by the production screeners discussed above. This also goes for pre-screening of unknown materials, which might contaminate more sensitive screeners.

There is also a need for alpha and beta screening for contaminants that are not accompanied by gamma emission. ²¹⁰Pb and its progeny do not have a penetrating signature and are deposited on all surfaces exposed to radon. Such radon plate-out plagues all rare-events searches, since it creates a patina of contaminant that causes nuclear recoils, beta-emission, and alphas. In addition, since Pb is often used in circuitry, its alpha-decay to ²¹⁰Po can cause single-site upsets. Surface contaminants, such as ⁴⁰K, and anthropogenic

Facility	Depth (mwe)	Suite of detectors and technology
Japan	Surface	1 HPGe (with active veto) managed by KamLAND currently 100%, but may consider use by others 1 HePG managed by CANDLES in Osaka (sea level) currently 90%, with 10% use by others
Kamioka Observatory Japan	2700	Each experimental group has their own devices for screening and assay, but will consider use by others 1 HPGe managed by KamLAND, currently 100% 1 HPGe under construction by KamLAND: 100% 1 HPGe under construction by CANDLES: 100% 3 HPGe (2 p-type, 1 n-type) 100% SuperK and XMASS Underground ICP-MS and API-MS managed by SuperK and XMASS (100%) Many radon detectors to measure radon emanation of materials, managed by SuperK and XMASS (100%)
CanFranc (LSC) Spain	2450	5 HPGe p-type 100% usage by LSC Outside collaboration possible 2 HPGe p-type to be installed by end of summer 2013
Boulby Mine England	2805	1 HPGe 2kg p-type for UK groups in SuperNEMO, LZ, DRIFT (75%), 25% use by others. 2 n-type LO-AX detectors for radiometric dating: 80% Boulby/Scottish Universities Environmental Research Centre, 20% use by others 1 new broad energy p-type HPGe (mid-2014) 100% dedicated to SuperNEMO, LZ, DRIFT Expanded assay capability with new lab construction
STELLA at LNGS Gran Sasso Italy	3800	10 HPGe operated by INFN as a user facility 1 HPGe with 100% usage by XENON and GERDA, (DARWIN in future), radon mitigation underway
LSM (Modane) France	4800	15 HPGe with 6 dedicated to material selection: 2 detectors, 100% usage by SuperNEMO 1 detector 100% usage by EDELWEISS 3 detectors 100% dedicated to experiments installed in Modane 2 detectors may be available to others at level of 5-10%

Table 7-4. *International assay resources [12, 13].*

Facility	Depth (mwe)	Suite of detectors and technology
SNOLAB Canada	6010	1 PGT coax HPGe 54% usage by Canadian based experiments, 34% usage by U.S. based experiments, 12% usage by SNOLAB 1 Canberra well HPGe , 100% by SNO+ and DEAP 11 Electrostatic counters (alpha counters), 100% usage by EXO, in future SNO+, PICASSO and MiniCLEAN 8 alpha-beta counters, 100% usage by SNO+ available for other experiments on request 1 Canberra coax HPGe (currently being refurbished) The SNOLAB facilities are used by SNOLAB based experiments, but can be negotiated during down time
CJPL (JinPing) China	6800	1 HPGe managed by PandaX, 100% for PandaX 1 HPGe managed by CDEX, 90% usage by CDEX 2 HPGe to be installed by end of 2013: 70% CDEX, ~30% availability reserved for others.

Table 7-5. *International assay resources (cont.) [12, 13].*

contaminants, like ^{125}Sb and ^{137}Cs , are also detectable by beta screening. R&D into more sensitive counters should be supported.

Radioisotope identification can be done using mass spectrometry on the surface. While machines capable of probing to ppb levels are typically found in universities and commercial analytic labs, the ppt levels achieved by inductively-coupled plasma mass spectroscopy (ICPMS) are only realized at a few locations where radiochemical expertise and careful attention to quality assurance protocols are augmented by novel dissolution and digestion techniques which can process a wide variety of samples. Since ICPMS provides an alternative to counting, it must be taken into account in any determination of future assay needs, as well as included in any centralized scheduling apparatus.

Laser cooling techniques can be used to trap atoms, excite them to a metastable state, and then detect their fluorescence, thus determining abundances by directly counting atoms. AtomTrap Trace Analysis provides a fast turn-around method of measuring radioactive background from ^{85}Kr and ^{39}Ar to a few parts in 10^{-14} and could be installed underground to screen user samples, as well as aid in the purification of Ar, Ne, Xe.

While an augmented suite of sensitive production screeners can provide the bulk of the assay, orders of magnitude improvement in sensitivity is required for some materials close to detectors and for active elements. An ultra-sensitive whole body screener in a water tank at depth would provide an ultimate check on the total activity from all isotopes in the material, including short-lived isotopes which are impossible to detect chemically. The goal is bulk assay of large amounts of material at the 10^{-13} – 10^{-14} g/g U/Th level. Such designs have been explored in NUSL [14] and DUSEL [15] reports and generally resemble the Borexino Counting Test Facility [16] with a top-loading sample changer.

7.3.5 R&D space

Reconfigurable space must be reserved underground for small scale experiments, storage, and background studies. Prototypes using new technologies for next-generation dark matter and neutrinoless double-beta decay experiments must be operated underground to establish background rejection and evaluate sensitivity. While muons can be efficiently tagged on the surface, muon-induced neutrons where the parent muon does not intersect the active shield produce an irreducible background that makes it impossible to determine the level of rejection achieved by the new technique. By the same rationale, new assay techniques also require underground R&D space to evaluate their sensitivity before deployment.

Underground space is not low-background unless it is also shielded against radiogenic backgrounds from the rock or the shotcrete which covers the rock. Most users need a combination of high-Z material for gamma reduction, hydrogenous material for neutron absorption, and copper liners for the cleanest inner shell. Since shielding is often the most expensive element in the experiment, the shielding should remain underground to be re-used as part of the site infrastructure. This also has benefits with respect to long-term reduction in cosmic activation. If the hydrogenous material is thick enough and pure enough, it is sufficient on its own. This implies that the most cost-effective shielding for multiple users would be a large water tank surrounding the R&D space, sharing water purification infrastructure already needed for the neighboring ton-scale dark matter experiments.

A better quantification of backgrounds underground is important to all experiments and should be part of the supplied infrastructure. For example, each site should be able to provide spectral and flux characterization of their muon, gamma, and neutron backgrounds to the experiments located there. Compilation of these data over multiple geologies and depths provides the means to better model the physics, as well as take the burden from individual experiments. Although the rock overburden reduces the flux of cosmic-ray muons by several orders of magnitude, a residual flux of highly penetrating muons creates difficult-to-shield high-energy neutrons. The neutrons themselves can imitate WIMP interactions, and gammas from their inelastic scatters produce gamma background for neutrinoless double-beta decay experiments. Data on the flux, spectrum, and composition of such cosmogenic neutrons underground is therefore vital, but since the rate is so low, experiments must rely on extensive Monte Carlo simulations for background estimates/subtraction, shielding design, and veto efficiencies. Large vats of gadolinium or boron-loaded water or liquid scintillator can provide the data to compare to simulation and improve the physics models. These can serve multiple purposes, including shielding for experiments and R&D, as well as neutrino and homeland security applications.

7.3.6 Strategic Infrastructure Planning

Immediate investment in assay infrastructure is required to ensure the successful deployment of the next-generation dark matter, neutrino, and neutrinoless double-beta decay experiments. The availability of screeners worldwide is not sufficient for the suite of experiments currently approved and proposed. This is an international shortage, so sending samples overseas for assay is not the solution, although experiments with significant international collaborators will certainly employ sensitive screeners at European and Asian labs for a subset of samples. Since sensitive counting techniques require overburdens matched to existing U.S. underground facilities, space in U.S. laboratories should be reserved for this function. There is enough space for future expansion of assay capabilities, and for new small-scale experiments designed to test new technologies or expand sensitivity to new WIMP regimes, as long as the full suite of existing U.S. laboratories is exploited. This would also take advantage of considerable past investment, existing infrastructure, and

expert personnel. Using U.S. sites will also minimize surface activation during shipping and contamination during handling.

Increased U.S. assay capability is best achieved by organizing the existing underground facilities under a single umbrella organization with representatives from those sites, which could then be funded as a consortium or network, rather than as a competition between experiments. Such an entity can then propose increased investment in capability as a validated representative of the larger community, including centralized screening at the deep U.S. site to take advantage of co-location and unique smaller efforts at other locations. In the meantime, inter-site scheduling tools can be developed to achieve improved efficiency with existing screeners. The same organization can manage R&D space underground, including scheduling use, underground storage of cosmogenically-activated materials, and multi-user shielding. The larger community thus served would form a base for innovative funding schemes which include industry partners.

A staged transition from an experiment-specific model to a multi-site user coordinated network is in line with current budgetary constraints, yet would retain capabilities that have been built up over many years in the service of individual projects. Such organization will promote rapid and broad dissemination of research results. When combined with access to assay and a database of previously assayed materials, this will significantly increase scientific reach and reduce risk for all rare event searches underground.

7.4 Conclusions

In summary, a substantial expansion in non-U.S. underground capabilities is very likely to occur by the end of this decade. The U.S. has leading roles in many of the future dark matter, neutrinoless double-beta-decay, and neutrino experiments. It is critical that U.S. scientists continue to be supported well enough to take full advantage of international and domestic underground facilities housing these experiments. Improved coordination of the underground facilities (including domestic infrastructure) is required to maintain this leading role. An open access policy for all major underground facilities is needed. As the scale (and thus cost) of underground experiments grows, it will become more important to foster open, competitive access as much as possible. The best way for the governments to support the international system of underground experiments is for each major country (or region) to support at least one major underground laboratory capable of hosting the forefront experiments. It is not clear whether it would be possible to sustain this international support if one country chose to take a major role in the research without supporting any facility.

Our conclusions related to the upcoming U.S. planning process are:

1. Locate LBNE underground to realize its full science potential. This step would also provide a natural base for additional domestic underground capabilities at SURF in the future.
2. The U.S. has leading roles in many of the future dark matter, neutrinoless double beta decay and neutrino experiments.
3. More coordination and planning of underground facilities (overseas and domestic) is required to maintain this leading role, including use of existing U.S. infrastructure and closer coordination with SNOLAB as the deepest North American Lab.
4. Maintaining an underground facility that can be expanded to house the largest dark matter and neutrinoless double beta decay experiments would guarantee the ability of the U.S. to continue its strong role in the worldwide program of underground physics.

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Instrumentation Frontier

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8.1 Introduction

Innovation in instrumentation is central to discovery science. Progress in understanding the laws which govern the most fundamental building blocks of nature has been achieved largely through technological advances in instrumentation that have made increasingly sophisticated experiments and analysis of the data from these experiments possible. The field of high-energy physics (HEP) is viewed as an incubator of innovation, which has led to fundamental discoveries in particle physics as well as breakthroughs in other fields of science.¹ In many cases, novel physics experiments have been enabled by advances in technology,² whereas in others, physics needs have driven necessary technological development.

¹High-energy physics and particle physics are used interchangeably in this Chapter.

²To quote Freeman Dyson: “New directions in science are launched by new tools much more often than by new concepts. The effect of a concept-driven revolution is to explain old things in new ways. The effect of a tool-driven revolution is to discover new things that have to be explained [1].”

The physics requirements of many experiments in the Energy, Intensity and Cosmic Frontiers entail very large-scale detectors that use technologies that cannot be scaled up without the cost becoming prohibitive. The very large costs involved in building and operating many existing HEP detectors, combined with increasingly tight budgetary constraints, make it difficult for the HEP community to provide substantial investment in new technologies, many of which have the potential to make significant contributions to the future HEP program but which carry a substantial risk of failure.

Newer, riskier technologies will almost certainly need to be developed to enable study of particle interactions with high statistics at higher energies, to look for extremely rare decays or ultra-low cross section processes, and to obtain and analyze increasingly large data samples. The need to develop new, transformational technologies has been recognized in recent government panel recommendations that are promoting the need for riskier but higher reward research [2]. Investment in the development of new technologies is an investment in the sustainability of the field. It has therefore become very important for the HEP community to establish a mechanism for developing and implementing a coherent vision for the future of *generic* HEP instrumentation development.

The Snowmass Instrumentation Frontier is an outgrowth of a process initiated by an American Physical Society Division of Particles and Fields (DPF) task force established in 2010. This task force was charged with addressing the needs, organization, and health of instrumentation in U.S. particle physics. The major task force recommendation was that the DPF form a standing committee to address a number of outstanding issues related to instrumentation [3]:

- Make available up-to-date information on elements of the national detector R&D program.
- Expand coordination among the national laboratories, and provide a forum (and information) for enhanced access to selected resources at the national laboratories for university groups.
- Stimulate detector R&D through workshops and studies in a coordinated way.
- Stimulate new ideas, especially those of a scale that requires substantial collaboration, particularly between laboratories and universities.
- Instigate a concerted effort to involve industry in workshops and studies with the intent of later involvement in R&D.
- Coordinate joint educational activities at schools and other community events.
- Explore opportunities for attracting and providing careers for high-energy physicists in instrumentation.
- Act as a resource for the funding agencies by helping them establish SBIR categories, improve the response to program solicitations, and investigate other opportunities.

In response to this recommendation the DPF established the Coordinating Panel for Advanced Detectors (CPAD). This committee, supplemented by liaisons to the physics frontiers and technologies, was the basis of the organization of the Instrumentation Frontier and will continue its work after the Snowmass process is complete.

There are at least two time scales to be considered in formulating a national vision for instrumentation: (1) the intermediate term (< 10 years), where the community has a reasonable understanding of the physics goals, and (2) the longer term, where technologies being developed now will ultimately become the enablers of future experiments which are either technologically inaccessible or prohibitively costly today. We must be able to effectively communicate these needs to the funding agencies.

This report is organized as follows. Sections 8.2, 8.3 and 8.4 describe major experiments with significant U.S. involvement in the Energy, Intensity, and Cosmic Frontiers, and the technologies currently employed by these experiments. We identify existing technologies that need to be refined and new technologies that need to be developed in order to address the current or near term needs of these experiments. When possible, we also describe projected needs beyond the next decade. In section 8.5 we describe several widely-used, key technologies identified during the Snowmass process, in which future R&D would enable the U.S. HEP community to maintain leadership in detector development in a global HEP community and build a strong, future experimental research program. Section 8.6 lists the existing facilities, and partnerships available for instrumentation are described in section 8.7. In section 8.8 we describe issues which must be addressed to maintain detector R&D leadership in the future. Finally, in section 8.9 we provide some conclusions based on the material presented in the previous sections.

8.2 The Energy Frontier

8.2.1 Hadron colliders

8.2.1.1 Current and planned projects

At this time, the Energy Frontier is embodied by the Large Hadron Collider (LHC) and the two large detectors ATLAS and CMS. The primary physics goals of the LHC detectors in the next 5 years are (1) to measure branching ratios and related couplings of the recently discovered Higgs boson and to characterize its properties in as much detail as possible to determine whether or not it is the Standard Model Higgs, (2) to search for physics beyond the Standard Model including supersymmetry, dark matter, and extra dimensions, and (3) to carry out a measurement of WW scattering. This implies maintaining detector performance both at low-energy scales for Higgs studies and the highest scales in searches for new physics. The LHC will resume running in 2014 at 13 or 14 TeV center of mass energy with an average instantaneous luminosity of $5 \times 10^{33} \text{ cm}^{-2}\text{s}^{-1}$, which is already beyond the design parameters of the experiments.

ATLAS and CMS are both large multipurpose detectors. CMS is more compact, with a 4T central field. It includes an all-silicon tracker, a lead-tungstate electromagnetic calorimeter, a brass/scintillator sampling hadron calorimeter, and a muon system with drift tubes, resistive plate chambers, and cathode strip chambers. The CMS compact design and high magnetic field enables the use of high-resolution crystal electromagnetic calorimetry and all-silicon tracking [4].

ATLAS has a lower, 2T, field with an inner silicon tracking section, an outer straw tube transition radiation tracker, and a liquid argon sampling electromagnetic calorimeter with an outer scintillator-tile hadronic calorimeter. The ATLAS muon system utilizes air core toroid magnets with both barrel tracking chambers and tracking chambers in an inner and outer wheel configuration at each end. The ATLAS liquid argon calorimeter is radiation-hard and provides excellent pattern recognition. The ATLAS muon system provides an independent measurement of muon momenta [5].

Both the CMS and ATLAS experiments performed very well in the initial physics runs. Both experiments have devised operations, trigger, and analysis strategies to cope with high occupancies caused by the 50 ns bunch-crossing interval and high instantaneous luminosity. The experiments have also identified significant physics limitations. Both trackers are more massive than optimal. The CMS endcap crystals are beginning to suffer from radiation damage. Both pixel detectors will suffer losses due to buffer overflows at higher luminosity. Triggers for both experiments are close to saturation.

Both ATLAS and CMS are currently being upgraded [6]. These Phase-I upgrades include improvements to the trigger and DAQ systems and replacement of the inner pixel detectors [7]. The new detectors will utilize low mass mechanics and CO₂ cooling to reduce tracker mass. Hybrid photodiodes are being replaced by silicon photomultipliers, increasing the granularity of the CMS hadron calorimeter. These changes will enable continued operation of these detectors to integrated luminosities of 300 fb⁻¹, expected to be reached by the year 2022.

8.2.1.2 Future outlook and needs

The European Strategy calls for an upgrade of the LHC instantaneous luminosity to 5×10^{34} cm⁻²s⁻¹ (HL-LHC). ATLAS and CMS will need to operate at an average of ~ 140 interactions per 25 nsec beam crossing time and expect to obtain 3000 fb⁻¹ of integrated luminosity. To achieve their physics goals the experiments will need to maintain at least the current values of resolution for tracking and calorimetry and maintain the ability to selectively trigger on specific physics events at reasonable thresholds.

The projected increase of the instantaneous luminosity at the HL-LHC by an order of magnitude will lead to nonlinear increases in raw trigger rates. Improved rejection power will be needed at each trigger level. Both CMS and ATLAS plan to utilize the increasing bandwidth, storage, and processing capabilities of modern electronics to enable robust and flexible triggering. Plans include increases of the level 1 (L1) and level 2 (L2) trigger bandwidth and an increased L1 trigger latency. In addition, both CMS and ATLAS foresee access to the full granularity of the detector at L1, which enables increased sophistication of trigger algorithms, and a L1 track trigger utilizing either on-detector correlations or regions of interest. All of this will need low-power, high-speed, radiation-hard optical data links.

Measurements of WW scattering and Higgs self-coupling require efficient identification of jets at high pseudorapidity η .³ This is an enormous challenge, with singles rates on the order of 5×10^9 Hz at the inner radius of a forward calorimeter. This will require high-rate, large-area, radiation-hard detectors. Ideally, a calorimeter would provide ~ 100 ps time tags to separate primary vertices, allow for dual readout of electromagnetic and hadronic energies, and provide for detailed pattern recognition.

Current detectors and electronics used for inner vertex layers are not sufficiently radiation-hard and will either have difficulty or will be unable to handle projected luminosity increases. Further R&D on 3D, diamond, or thin silicon sensors is necessary for layers at small radii. Silicon will have to be kept near -20°C to avoid reverse annealing and to control the leakage currents. Similarly, inner layers will need radiation-hard electronics with acceptable single-event upset rates and good performance after exposure to doses approaching a Grad.

The most powerful new tools envisioned for the HL-LHC upgrades are the L1 track triggers. An L1 track trigger, which includes the ability to identify primary vertex z location, can eliminate much of the pileup background. This requires a pixelated tracker with pixels that can correlate information with other detectors. In the CMS track trigger design, track stubs greater than 2 GeV can be found by using hit correlations between silicon sensors separated by $\approx 1\text{--}4$ mm. This requires unprecedented connectivity and intelligence in the tracker electronics. The design of the high-speed, complex, on-detector electronics is in turn directly limited by the power that can be delivered by DC-DC converters or serial power. The increased pixelization of the tracker and increasing data bandwidths will continue to increase power density and cooling demands. New support structure and cooling technologies will be needed to control mass and therefore preserve the resolution of the trackers and calorimeters.

³Pseudorapidity is defined as $\eta \equiv -\ln(\tan(\frac{\theta}{2}))$ where θ is the usual polar angle from the beam direction.

8.2.1.3 Beyond the next decade

Discoveries at the Large Hadron Collider (LHC) may point to hadron machines with energies beyond lepton collider capabilities, such as a Very Large Hadron Collider (VLHC). Experiments at these facilities indicate clear instrumentation needs. New physics signals from additional Higgs bosons, supersymmetric particles, or other unexpected particles will likely be increasingly buried in QCD background. This points to the need for experiments in high-luminosity environments with selective triggers and robust data acquisition bandwidth. This in turn implies increased pixelization in the tracking detectors and calorimeters to enable experiments to identify the correct primary interaction and reduce total hit occupancy. Multiple interaction backgrounds can also be mitigated by picosecond-level timing or by limiting the region of the interaction diamond where the shower of particles originated. Almost all system components must be radiation-hard.

Advances in electronics and sensor technology can start to address these issues. Novel integrated-circuit packaging technologies hold the promise for large-area sensor front-end readout electronics bonding at very fine pitch, which could be considerably less expensive than conventional bump bonding. Sensors can be designed to have lower capacitance, which will decrease noise, increase speed, and decrease power consumption. Silicon sensors can be operated in avalanche or Geiger mode, substantially increasing speed while eliminating the need for high currents in front-end amplifiers. Light sensors can also be pixelated using Silicon PhotoMultiplier (SiPM) or Large Area Picosecond Photodetector (LAPPD)-style technologies. Power can be reduced towards 10 pJ/bit and bandwidth increased in on-detector data transmission utilizing new radiation-hard optical transmission technologies or silicon photonics. New semiconductors are being developed for commercial DC-DC conversion which may provide both radiation hardness and increased voltage step-down ratios at high efficiency. Advances in opto-photonics may lead to completely novel detectors.

8.2.2 Lepton colliders

8.2.2.1 Current and planned projects

Lepton colliders appear to be the machines of choice for precision measurements of the properties of the Higgs boson and Standard Model particles, to elucidate the nature of supersymmetry, extra dimensions, or other new physics discovered at the LHC. The candidates for e^+e^- colliders are an International Linear Collider (ILC) which can be tuned for a center-of-mass energy between 200-500 GeV, with a potential energy limit of 1 TeV; CLIC, a higher energy but less well-developed e^+e^- collider; and TLEP, a Higgs factory in a ≈ 80 km ring [8, 9, 10]. An alternative is a muon collider, which has the advantage of being both much more compact, because of the lower radiation losses of the muons, and being scalable to higher energy.

One application for an e^+e^- collider is measuring the properties of the Higgs boson, including its couplings to fermions and other bosons. Furthermore, an e^+e^- collider allows a precise determination of the properties of the top quark, and study of possible signals of new physics through the electroweak production of new particles and through signals of these interactions in W , Z , and two-fermion processes. e^+e^- collider experiments will be sensitive to supersymmetric partners of known particles, new heavy gauge bosons, extra spatial dimensions, and particles connected with strongly-coupled theories of electroweak symmetry breaking.

There has been considerable design work carried out for the silicon detector (SiD) and the International Large Detector (ILD), which are designed for the ILC but are also being used as a basis for possible CLIC detectors, where beam backgrounds will be substantially greater. SiD is a general purpose detector with (1) a multilayer barrel and endcap silicon pixel vertex detector, (2) a multilayer barrel and endcap silicon strip

tracker, (3) a multilayer electromagnetic calorimeter consisting of thinner inner and thicker outer layers of sheets of tungsten absorber interleaved with silicon wafers with hexagonal 12 mm^2 silicon pixels, (4) a highly segmented hadronic calorimeter, (5) a 5T solenoidal magnet, and (6) an iron flux return instrumented with either scintillator or resistive plate chambers. ILD is less compact than SiD and features a large TPC for tracking. Both detectors are designed to be used as a complete system for Particle Flow Analysis (PFA) to measure jet energies, charged leptons, photons, and missing energy [11]. CLIC has higher energy, more beamstrahlung, and 0.5 ns bunch spacing and will require faster detectors and electronics as well as thicker calorimetry [12].

8.2.2.2 Key issues and next steps

The SiD and ILD detector design concepts for the ILC are fairly mature. Key issues that will need to be addressed and solved for ILC detectors are: fabrication of low mass vertex detectors and trackers, power delivery and low mass supports, and the design and affordable implementation of precise particle flow or dual readout calorimetry.

Both the ILC and CLIC aspire to measure the energy of hadron showers with an unprecedented resolution that can separate hadronic W and Z decays. The particle flow technique, which aims at a di-jet mass resolution of better than 5%, uses highly pixelated calorimeters to identify energy deposited by neutral hadrons and adds that energy to the energy of tracks in a jet. An alternative dual readout technique, which shows promising results in a collimated test beam and also aims to achieve better than 5% jet energy resolution, attempts to separately measure hadronic and electromagnetic components of a jet and correct the jet energy for losses due to nuclear breakup.

There is an increasing focus on understanding the fundamental limitations to hadronic calorimeter energy resolution to enable optimum resolution calorimeter designs in future detectors. The CALICE collaboration has studied particle flow calorimetry for ILC and has developed test hadron calorimeters which have been extensively tested in the Fermilab and CERN test beams. The CALICE digital hadron calorimeter concept could be the basis for experiments needing highly granulated charged particle calorimeters [13, 14]. R&D on dual readout calorimetry is being carried out by the CERN RD52 collaboration and others. Crystals capable of high-resolution total-absorption calorimetry are also being explored [15].

8.2.2.3 Beyond the next decade

LHC measurements have not yet found any evidence for new physics. If new physics does exist, a multi-TeV lepton collider may provide a path to fully explore the spectrum. e^+e^- colliders have barriers, however, to scaling to the multi-TeV range. ILC technology will be too expensive. Power requirements may place an upper limit on affordable CLIC energies. TLEP cannot reach center of mass energies beyond 350 GeV in an 80 km ring.

These barriers to scaling do not apply to a muon collider. A compact circular $\mu^+\mu^-$ collider can be built with acceptable energy loss and cost if the appropriate cooling and magnet technologies can be developed. However, substantial experimental difficulties must be overcome to deal with the radiation background from muon decays in flight. Decay backgrounds originate primarily from electromagnetic showers in absorbers and beamline components. The overwhelming majority of these backgrounds is composed of low-energy particles, which produce signals at different times than signals produced from particles from the interaction vertex. The key to background rejection is fast, nanosecond-level timing in the tracker and calorimeters. Pixelization is needed both for resolution and to limit background occupancy. Experiments at a muon collider will combine

the radiation hardness needs from the LHC, timing requirements of CLIC, and precision of experiments at the ILC.

8.3 The Intensity Frontier

8.3.1 The neutrino sector

8.3.1.1 Current and planned projects

There are a number of unanswered questions about neutrino properties and about the relationship between three fundamental neutrino eigenstates ν_1 , ν_2 , ν_3 , and the mix of them in the physical neutrinos ν_e , ν_μ , and ν_τ . In particular, it is currently unclear whether neutrinos are Majorana or Dirac, whether or not the masses of ν_1 and ν_2 are greater or less than the mass of ν_3 , and whether neutrino interactions violate CP. In addition, the absolute masses of the neutrinos are unknown.

Reactor neutrino experiments include Daya Bay, which is currently running with eight detectors, and Double Chooz, which is running with one (far) detector out of a planned two detectors [23, 24]. These experiments are explicitly designed to measure the mixing angle θ_{13} .

Neutrinoless double-beta decay experiments are designed to determine if the neutrino is Majorana (its own anti-particle) or Dirac, since neutrinoless double-beta decay is impossible unless the neutrino is Majorana. In addition, if the mass hierarchy is known to be inverted, the absence of a signal below a critical mass provides definite proof that the neutrinos must be Dirac. Nuclear physics non-reactor experiments planned for the near future include three neutrinoless double-beta decay experiments, — SNO+, Majorana, and Cuore — and one additional experiment, Katrin. Katrin is designed to measure, or put a more stringent limit on, the neutrino mass by carefully measuring the tritium beta decay phase space endpoint.

8.3.1.2 Key issues and next steps

Large-mass, high-resolution, cost-effective detectors will be needed for future neutrino and proton decay experiments [25]. To improve the analysis capabilities of neutrino detectors, the field has decided to move away from the workhorse water Cherenkov detector to liquid argon (LAr) tracking detectors, which have a 3D space point resolution of less than 1 mm and energy resolution of less than 10 MeV. Although relatively small LAr Time Projection Chambers (TPCs) have been constructed and are in use in a variety of HEP experiments, considerable detector R&D will be required to insure the required purity of very large volumes of liquid argon,⁴ to generate and distribute the high voltages required for drifting ions in the chamber,⁵ to make the cryostats and cryogenic systems as inexpensive as possible, and to fabricate low noise (possibly cold) readout electronics for very large-volume LAr detectors, that will work reliably for tens of years. It will also be desirable to provide a magnetic field in LAr chambers to help with particle identification. In addition, a number of safety issues need to be resolved before a very large LAr detector can be deployed underground.

A number of technical issues need to be resolved in designing larger LAr detectors. Electronics may be warm or cold. The optimum electronics sampling time for a given detector needs to be determined.⁶ Scintillation

⁴Electronegative contaminants in LAr must be reduced to 100 parts per trillion of oxygen equivalent contamination.

⁵Voltage requirements are ~ 500 Volts/meter over several meters.

⁶Current and planned values are 0.197 μ s for ArgoNeuT, 0.4 μ s for ICARUS, and 0.5 μ s for both MicroBooNE and LBNE.

light readout may use photomultipliers, which require wavelength shifting of the 128 nm LAr scintillation light and which take up space,⁷ wavelength shifter paddles,⁸ or silicon photomultipliers, which are expensive but have a low profile. Finally, liquid and gas two-phase argon detectors are being considered as possible candidates for future large neutrino detectors.

The cost of liquid scintillator is a problem for very large volume liquid scintillator detectors, and the cost of photomultipliers is a problem for both large volume liquid scintillator and water Cherenkov detectors. A brighter and optically cleaner scintillator for future long baseline (JUNO (Daya Bay II) and RENO-50) and future short baseline (SBL) reactor experiments is also highly desirable.⁹

A water-based liquid scintillator, capable of detecting the $p \rightarrow K^+ \bar{\nu}$ channel that is invisible to a Cherenkov detector, has recently been developed at Brookhaven National Laboratory. It is about an order of magnitude cheaper than mineral oil-based liquid scintillator, and might be used to increase the sensitive area of the NO ν A far detector. In addition, new metallic loading methods in water-based scintillators provide metallic ions, which can customize the properties of future scintillation detectors [26] in novel ways. For example, the SNO+ experiment has taken advantage of this new technology, with Te-doped scintillator as a projected neutrinoless double-beta decay source and detector.

The LAPPD program is developing fast, high-spatial-resolution photodetectors which may allow the development of an optical TPC using fast timing and good photon spatial resolution for tracking and the different characteristics of Cherenkov and scintillation light for particle identification. If large-volume, low-noise detectors can be developed, detecting low-energy reactor antineutrinos using coherent neutrino scattering with the corresponding two to three orders of magnitude increase in interaction cross section becomes very attractive. In addition, neutron tagging techniques could help with flavor separation in large water Cherenkov detectors such as the proposed Japanese Hyper Kamiokande detector.¹⁰

Advanced water Cherenkov detectors are also being used by other parts of the scientific community. The WATCHMAN collaboration of universities and national laboratories for nuclear non-proliferation is conducting a site search for a kiloton-scale advanced water detector to passively obtain detailed information on the operation of commercial nuclear reactors.

Plans are being made to scale up the $0\nu\beta\beta$ decay Stanford/SLAC EXO-200 experiment to a 5 ton experiment. The main technological concerns are maintaining the required xenon purity for the larger TPC, identifying large-area low-radioactivity ultraviolet optical detectors to replace the large area avalanche photodiodes used in EXO-200, and developing cold, front-end electronics with acceptably low levels of radioactivity. Another possibility is to detect barium atoms that are produced when Xe undergoes either two-neutrino or neutrinoless double-beta decay. If a next-generation xenon $0\nu\beta\beta$ decay experiment can develop the technology for tagging barium atoms with high efficiency, the experiment will be virtually free of backgrounds from non-xenon decays. In addition, if future xenon double-beta decay experiments use high-pressure xenon gas instead of liquid xenon, there is the possibility of an order of magnitude improvement in energy resolution due to the much lower Fano factor in xenon gas. Better energy resolution would help remove two-neutrino backgrounds (and any remaining non-double-beta decay backgrounds) which are not under the $0\nu\beta\beta$ decay energy peak.

Fermilab is researching the possible use of solid ^{136}Xe as a $0\nu\beta\beta$ detector. A 2.3 MeV energy deposition generates $\sim 10^7$ phonon quanta, which would predict an energy resolution of 0.3% at 1% collection efficiency. This is about one order of magnitude better energy resolution than that of EXO-200. Further R&D on the actual properties of sub-Kelvin crystalline xenon is necessary because few studies have been done under

⁷These are used by MicroBooNE and ICARUS.

⁸These are planned for LAr1 and LBNE.

⁹Short baseline reactor experiments have baselines less than 100 m.

¹⁰Gadolinium doping would allow detection of neutron captures in water via $\text{Gd}(n,\gamma)$ which gives an 8 MeV gamma cascade versus a 2.2 MeV gamma from $\text{H}(n,\gamma)$.

these conditions. In addition, there is a need to grow large transparent xenon crystals and read out signals from large crystals.

The U.S. is currently in a position to take a leadership role in all the areas identified in this section. Fermilab is interested in developing a high power source of neutrinos, and a neutrino factory could be a useful source of intense neutrino beams in order to carry out high statistics neutrino experiments.

8.3.1.3 Beyond the next decade

Double-beta decay experiments benefit greatly from isotopic enrichment of the double-beta decay isotope. It would be very beneficial to develop less expensive ways to enrich these isotopes. In addition, liquid argon detectors used in rare decay experiments would be substantially improved if the ^{39}Ar contamination, which is produced by cosmic rays, could be removed inexpensively from ^{40}Ar .

Optical TPCs using water-based scintillators and inexpensive, fast, high-resolution photodetectors could be enlarged at considerably less cost than cryogenic, noble gas TPCs. Ultra-low-threshold neutrino detectors have the potential for measuring low energy neutrino interactions with a coherent neutrino-nucleus cross section which is larger than a neutrino-nucleon interaction by a factor of roughly the square of the number of nucleons in the nucleus.

There is also a growing need to identify and stockpile construction materials that are free from radioactive contamination to reduce backgrounds in very large detector systems and double-beta decay experiments.

8.3.2 Rare decays

8.3.2.1 Current and planned projects

There are currently two rare decay experiments running, the MEG experiment [30] searching for the lepton flavor-violating reaction $\mu \rightarrow e\gamma$, and KOTO [31] which will attempt to measure the CP-violating reaction $K_L \rightarrow \pi^0 \nu \bar{\nu}$ with enough precision to look for new physics if there is a significant deviation from the Standard Model calculation. Two other rare decay experiments are proposed: Mu2e, which will search for the lepton flavor changing decay $\mu \rightarrow e$ [32], and ORKA, which will search for significant deviations from the Standard Model calculation of the decay $K^+ \rightarrow \pi^+ \nu \bar{\nu}$ [33].

The Muon $g-2$ experiment will make precise measurements of the gyromagnetic ratio of the muon to explore with greater precision the disagreement between previous results and QED calculations [34].

Currently, MEG has published a limit of 2.4×10^{-12} on the branching ratio $\mu \rightarrow e\gamma$, and there is a proposed upgrade to a branching ratio measurement sensitivity of 6×10^{-13} . Key detector requirements for this upgrade consist of improving the rate capability of all detectors and improving energy, angular, and timing resolutions for the positron and photon arms of the detector. A more highly granulated positron tracker is envisioned, along with a highly segmented, fast timing counter array using silicon photomultipliers. The photon arm, which uses the largest liquid xenon detector in the world, will be improved by replacing photomultiplier tubes with arrays of smaller photosensors, such as silicon or multi-anode photomultipliers. This will increase the granularity at the incident face.

KOTO is being constructed at J-PARC. Since there are no charged particles in the reaction of interest, the challenges are to produce a neutron-free, pencil-like beam of K_L mesons and to measure the energy and

time of the photons from the π^0 decay. This experiment needs a highly efficient, finely granulated gamma detector which completely surrounds the decay region.

The Mu2e experiment is currently undergoing the DOE's critical decision (CD) process for final approval and is currently scheduled to begin running in fiscal year 2019. The detector consists of a highly segmented straw tube tracker and a LYSO crystal calorimeter located in the warm bore of a solenoid with a 1 Tesla magnetic field. It is surrounded by a passive cosmic ray veto as well as an active veto using wavelength-shifting fibers embedded in scintillating material. The veto scintillator fibers will be read out by silicon photomultipliers.

The ORKA experiment will use an upgraded detector modeled on the Brookhaven E787 and E949 experiments. The new experiment will increase the sensitivity by a factor of 100 over previous experiments, with a 10 times more intense kaon beam and a 10 times more sensitive detector.

8.3.2.2 Key issues and next steps

All the rare decay experiments benefit from highly segmented trackers and timing counters to reduce backgrounds [25]. Currently, silicon photomultipliers are becoming the photodetectors of choice because of their high photon detection efficiency and high segmentation. The principal limitation is cost in covering large areas. The LAPPD project could potentially provide photodetectors that could deliver precise timing in rare decay experiments.

8.3.2.3 Beyond the next decade

Project X will enable an extensive program of critical tests of the Standard Model in both rare muon and kaon processes. The flagship process is the CP-violating kaon decay $K_L \rightarrow \pi^0 \nu \bar{\nu}$, where a thousand-event experiment may stress the Standard Model understanding of flavor. Simultaneously, lepton-flavor-violation processes involving charge exchanges will reach unprecedented sensitivities, with the exact program determined by progress in the current round of $\mu \rightarrow e \gamma$ and $\mu \rightarrow e$ conversion experiments. These experiments will take advantage of the tremendous flux and exquisite time structure of the Project X beams [25]. To fully capitalize on these accelerator capabilities, associated improvements in detector rate capability and timing resolution will be needed. These experiments will need fast, high resolution electromagnetic calorimetry; low mass, high-resolution, fast tracking; cost-effective, high efficiency photon detection; and high throughput streaming data acquisition systems. The $K_L \rightarrow \pi^0 \nu \bar{\nu}$ calorimeter will need good photon pointing. These experiments would benefit from less expensive, highly segmented photodetectors with high quantum efficiency and immunity to magnetic fields.

8.3.3 Heavy flavor factories

8.3.3.1 Current and planned projects

Heavy flavor detectors are designed to make precision measurements of B , B_s , and D mesons. The physics program includes measurements of their decay and mixing, the study of CP violation and measurement of the CKM angles, and a search for new physics in decays prohibited by the Standard Model or in branching ratios that differ significantly from Standard Model predictions. Three detectors, each with different capabilities, are carrying out precision heavy flavor measurements. The LHCb detector [35] will search for new physics signatures in beauty and charm decays with capabilities that extend to electroweak and top physics and

studies of exotic states. The Belle-II detector [36] will search for new CP-violating phases, flavor-changing neutral currents (FCNC) beyond the Standard Model and other manifestations of new physics in rare B decays. BES III [37] is designed to do precision studies of D decays and mixing, to look for new charmed states, and to provide precision data which can test lattice QCD calculations. The BaBar detector is no longer taking data, but analysis of its data set is ongoing and, in 2012, it uncovered direct evidence for T violation in the B^0 system.

8.3.3.2 Key issues and next steps

Major upgrades are planned for the LHCb and Belle detectors. An upgrade to the LHCb detector has been approved for running at higher LHC luminosities. The LHCb collaboration is considering a triggerless data acquisition (DAQ) system running at 40 MHz for which new, radiation-hardened DAQ boards and high-speed optical links are required. The experiment will need a more highly segmented, radiation-hardened tracking system, a low-mass cooling system, and better capabilities for forming RF shielding foils. The upgrade of the Belle to Belle-II detector consists of installing a new two-layer DEPFET-based pixel detector, complemented by a four-layer silicon tracking system using double-sided silicon sensors. Quartz-based time-of-propagation counters will be used for particle identification in the barrel, read out with waveform sampling electronics. The trigger, data acquisition system, and readout electronics will also undergo a major upgrade. No upgrades are currently being planned for BES III.

The key issues that must be addressed in heavy flavor detector upgrades are almost entirely related to dealing with higher luminosities: LHCb in the range of $10^{34} \text{ cm}^{-2}\text{s}^{-1}$ and Belle II in the range of $10^{36} \text{ cm}^{-2}\text{s}^{-1}$ [25]. Radiation hardness of electronics and vertex detector elements must be improved, faster electronics is needed, and granularity of tracking detectors must be increased to reduce the ambiguity due to increased occupancy. Better triggering and faster data acquisition systems must also be designed. The addition of fast waveform sampling electronics will improve background rejection and particle identification.

8.3.3.3 Beyond the next decade

There is another charm/tau factory under consideration, the BINP Super c/τ Factory with projected beam energies from 1.0 to 2.5 GeV, a peak luminosity of 10^{35} at 2 GeV, and longitudinally polarized electrons at the interaction point [38]. This is one of six projects approved for further development by the Russian Governmental Commission on Innovations and High Technologies.

The United States was planning to be a major player in a Super-BaBar detector in Italy, using recycled parts from the original SLAC BaBar Detector. Now that that project has been canceled by the Italian government, the focus has shifted towards the BELLE-II and LHCb upgrades. The U.S. currently carries a major responsibility for the design, construction and readout of the quartz-based particle identification detector for the BELLE-II detector.

8.4 The Cosmic Frontier

8.4.1 Ultra-high-energy cosmic rays and neutrinos

8.4.1.1 Current and planned projects

There are a number of experiments designed to measure the energy spectrum of ultra-high-energy cosmic rays and neutrinos, to determine their composition and source directionality, and to observe and study the Greisen-Zatsepin-Kuzmin (GZK) cutoff.¹¹

The Pierre Auger Observatory uses an array of 1600 water Cherenkov detectors, spaced 1.5 km apart, and 24 fluorescence detectors to study and characterize extensive air showers resulting from ultra-high-energy cosmic rays [39, 40]. The fluorescence-light-detecting telescopes record fluorescence in the ultraviolet spectrum from shower particle excitation of nitrogen.

The Ice Cube neutrino observatory at the South Pole [41] consists of a surface air shower detector and regularly spaced arrays of 5160 photomultiplier tubes (PMTs) buried in the South Pole ice between 1450 and 2450 meters below the surface. The PMTs record Cherenkov radiation from charged particles from neutrino interactions in the ice. This experiment aims to better understand the origins of ultra-high-energy cosmic rays, search for cold dark matter particles with masses approaching a TeV, search for supersymmetric particles, and study neutrino oscillations over megaparsec baselines. The PINGU upgrade consists of adding more densely packed photomultiplier strings to reduce the neutrino oscillation energy threshold and study matter effects in neutrino oscillations [42].

The Antarctic Impulsive Transient Antenna (ANITA) experiment is also designed to study ultra-high-energy cosmic neutrinos [43]. It uses sensitive microwave antennae to detect electromagnetic showers produced by ultra-high-energy neutrinos in the Antarctic ice through the Askaryan effect.¹² The experiment attempts to locate the source of ultra-high-energy cosmic rays, measure model independent weak interaction cross sections, and look for new physics in neutrino flavor oscillations.

8.4.1.2 Key issues and next steps

The major limitation of experiments that aim to measure the spectrum of ultra-high-energy cosmic rays and neutrinos is the very low flux of high-energy particles from space.¹³ Thus, it is very important to have high detection efficiency. The Pierre Auger Observatory would greatly benefit from low-cost photodetectors with high spectral efficiency between 300 and 400 nm and the capability to survive high background currents.

The ANITA experiment would benefit from reducing the levels of temperature equivalent noise from 70 K to 10 K in affordable amplifiers.¹⁴ The experiment would also benefit from improvements in antenna technology, lower-power waveform digitizers with better dynamic range and throughput, and more flexible, addressable sample windows. Continued R&D on ASIC development for waveform capture is also very desirable.

¹¹The GZK cutoff occurs when protons with energies greater than 5×10^{19} eV interact with cosmic microwave background photons to produce a Δ^+ which decays into p, π^0 or $n\pi^+$.

¹²The Askaryan effect is coherent radio emission from excess negative charge in an electromagnetic shower.

¹³A detector the size of Auger detects ~ 30 cosmic rays with an energy of $\sim 10^{20}$ eV per year.

¹⁴Amplifiers with noise levels of 70 K were made affordable because of the recent revolution in wireless technology.

8.4.2 Dark energy

8.4.2.1 Current and planned projects

The discovery of cosmic acceleration, or dark energy, has spawned a new generation of experiments whose purpose is to elucidate this phenomenon. These experiments fall into two classes: (1) distance measurements, to track the expansion of the universe, and (2) measurements of the growth of cosmic structure, to determine whether the acceleration is due to a cosmological dark energy or modified gravity. Distance measurements either use supernovae as standard candles or baryon acoustic oscillations to compare the length scale of galaxy clusters today to the clustering at the time of matter recombination. Structure measurements either use weak lensing or galaxy cluster counting.

The currently running Dark Energy Survey (DES) uses the 4 meter Blanco telescope with a 570 megapixel charge coupled device (CCD) imager [44]. The Baryon Oscillation Spectroscopic Survey (BOSS) is conducting a five year survey utilizing the Sloan Digital Sky Survey 2.5 meter telescope and a spectrometer, which was upgraded in 2009 [45]. The proposed Mid-Scale Dark Energy Spectroscopic Instrument (MS-DESI), is scheduled to start running around 2018 and will consist of a 4 meter telescope coupled to 5000 robotically controlled fibers. It will obtain spectra for about 10% of the available galaxies with a magnitude limit of 22.5 and a redshift of about 3.5 [46]. Euclid, a joint European-NASA space imager and spectrograph scheduled to launch in 2020, will consist of a 1.2 meter telescope with a visible imager (VIS), near infrared (NIR) photometry and spectroscopy [47]. The Large Synoptic Survey Telescope (LSST), an imager scheduled to begin taking data in 2021, will consist of an 8 meter telescope using six filters for visible photometry [48]. It will have a magnitude limit of ~ 27.5 , will cover $20,000 \text{ deg}^2$ in 10 years, and measure ~ 20 billion galaxies. A key issue for the LSST experiment will be acquiring, managing, and analyzing huge data sets.

8.4.2.2 Key issues and next steps

All dark energy experiments should be able to perform high-resolution ($R = \lambda/\Delta\lambda \geq 1000$) spectral analysis. DES and LSST use multiple filters and large CCD cameras to carry out a 5 or 6 point spectral analysis. The ability to carry out high-resolution spectral analysis with the camera itself would be a major technological step forward in this research.

There is a current R&D effort underway to develop a larger spectrometer using Microwave Kinetic Inductance Detectors (MKIDs). These are superconducting resonators. When a photon strikes the superconductor, it breaks Cooper pairs, which changes the inductance of the resonator, which in turn changes the resonant frequency and its phase. The phase change is then measured, providing a time and energy measurement for each photon. The theoretical resolution R is ~ 100 , and up to 2000 resonators could be multiplexed to the same readout line.

A proposed low resolution MKID spectrometer, Giga-Z will consist of 100,000 MKIDs [49]. The built-in spectral resolution of the MKIDs allows the use of direct lens-coupled, aperture mask spectroscopy as opposed to the fiber-fed spectroscopy. This promises a far more efficient use of telescope time with wavelength coverage extending to the near infrared, which is important for observations of high redshift galaxies. Outstanding issues are getting R closer to the theoretical limit, improving the packaging and RF electronics, improving the quantum efficiency, and developing a geometrically flat RF cable that works at 100 milliKelvin.

8.4.3 Gamma rays

8.4.3.1 Current and planned projects

There are currently two principal techniques used for detecting atmospheric gamma rays in the range from tens of GeV up to ~ 200 TeV: the atmospheric air Cherenkov technique and the water Cherenkov technique. The atmospheric air Cherenkov technique records images of air showers by focusing Cherenkov light onto a telescope consisting of an array of photosensors (~ 1000 pixels) and a large optical reflector. The VERITAS, H.E.S.S, and MAGIC observatories use this technique with multiple telescopes in coincidence to form stereoscopic views of air showers [50, 51, 52, 53]. Existing Imaging Atmospheric Cherenkov Telescopes (IACTs) used in these observatories reconstruct the arrival direction with an accuracy of < 0.1 degree and measure the gamma ray energy with a resolution of 10-15% for cosmic rays with energies from 50 GeV to 50 TeV.

The water Cherenkov technique has a wider field of view and higher duty cycle than the IACTs, but has less sensitivity and poorer angular resolution. The shower particles reaching the water tanks produce Cherenkov light and are detected with photomultipliers. These signals are used to reconstruct the arrival direction and energy of the air shower. The HAWC (High Altitude Water Cherenkov) experiment will consist of an array of 300 water tanks with the ability to observe 50% of the sky in a 24-hour period [54].

The larger Cherenkov Telescope Array (CTA), with ~ 100 telescopes, is currently in the planning stage [55]. It will provide multiple stereo views in shower reconstruction and should be sensitive to energies between 30 GeV and 300 TeV.

These experiments can provide sensitive searches for gamma rays resulting from WIMP dark matter annihilation. They can also indirectly search for axion like particles (ALPs), which couple to gammas, by searching for a gamma-axion oscillation, which produces an anomalous transparency for gammas traversing the galaxies. In addition, they can look for violations of Lorentz invariance by searching for energy-dependent photon speeds using pulsar timing sources and flares from distant blazars.

8.4.3.2 Key issues and next steps

The Cherenkov telescope experiments have been using photomultiplier tubes at the focal point of large optical mirror dishes. Increased-granularity (10,000 - 100,000 pixels) and high-efficiency large area photodetectors with immunity to sudden bright flashes of light are particularly desirable for this application. The recent introduction of novel optical instrument designs, such as dual mirror systems, allows a large reduction in the size of the Cherenkov light images, enabling the use of arrays of smaller photodetectors such as silicon photomultipliers and multi-anode phototubes. These devices, combined with inexpensive and high-density readout electronics with GHz sampling capability, will reduce cost and improve performance of detector arrays.

For the CTA experiment, which is envisioned to have a telescope array covering from 1 to 3 km², a precision (< 1 nsec) clock distribution system with custom chips for array processing is essential for triggering. A wireless data acquisition system would be very useful for recording the data.

The HAWC experiment would benefit from high-efficiency, large-area photodetectors to instrument its 300 water tanks. Precise clock signal distribution in trigger systems and data acquisition systems are also a challenge.

8.4.4 Heavy dark matter - WIMPs

8.4.4.1 Current and planned projects

There are currently several experiments running and planned whose motivation is to directly detect weakly interacting massive particle (WIMP) dark matter by observing collisions of cosmic WIMPs with target nuclei. These experiments fall into six principal categories:

- (1) Solid state detectors with transition edge phonon sensors to measure both the ionization and thermal energies of potential dark matter recoils with nuclei in the detector (CDMS [56])
- (2) Solid state germanium detectors, which use shaped internal electric fields and very small capacitance readout contacts to reduce noise so that low-energy nuclear recoils at a level of about 500 eV can be detected (CoGeNT [57])
- (3) Experiments looking for low mass WIMPs in Charge Coupled Devices (CCDs) (DAMIC [58])¹⁵
- (4) Two-stage liquid/gas xenon TPCs which detect ionization light (S1) from the nuclear recoil in the liquid and electro-luminescence light (S2) from electrons drifting through the gas and use their ratio to distinguish nuclear recoils from electron backgrounds (Xenon 100 [59], LUX [60])
- (5) Two-stage liquid/gas argon detectors (the Darkside family of detectors [61]), which use S1 and S2 light as well as S1 pulse shape discrimination, and single stage liquid argon detectors (the DEAP/CLEAN family of detectors [62]), which use pulse shape discrimination alone to distinguish nuclear recoils in the liquid argon from electron recoils resulting from natural radioactivity at the level of better than a part in 10^8
- (6) Bubble chambers, which adjust their operating pressure to nucleate a single bubble from a nuclear recoil (COUPP) [63].

8.4.4.2 Key issues and next steps

Since the WIMP mass is not known, it is important to increase the sensitivity of dark matter experiments to cover the broadest possible mass range, in particular the region from 1 MeV to 1 GeV. Because the nuclear recoil energies are very small for low mass WIMPs, viable detectors must be sensitive at or near the single electron-ion pair limit. One technique for reducing the WIMP mass detection threshold is to increase the phonon signal in a CDMS semiconductor dark matter detector by operating it at high voltage bias and using the electric potential energy in the detector to produce Luke-Neganov phonons as the electron-ion pairs traverse the detector, in addition to the phonons produced by the recoil itself. This has the effect of discriminating nuclear recoil signals for low WIMP masses from background electron recoils. Neutron backgrounds and small target mass are the most significant problems for DAMIC, and better shielding is required to improve sensitivity. The experiment has moved from the Fermilab Minos tunnel to SNOLab.

Most of the large scale experiments described in section 8.4.4.1 are planning upgrades to greater detector mass. CDMS is planning to upgrade from 9.3 kg of active germanium detector mass using 3" diameter, 1" thick detectors in the Soudan underground laboratory to 100-200 kg of active germanium detector mass using 3.9" diameter, 1.3" thick detectors in SNOLab. In the much longer term, a CDMS detector with

¹⁵The DArk Matter In CCD experiment, DAMIC, uses 10 250 μm thick CCDs with a ten μm pixel size cooled to -150 °C with a total mass of 0.01 kg enclosed in a low-radiation package in a copper box to look for WIMPs in the 1 - 10 GeV mass range, where other direct search dark matter experiments do not have good sensitivity.

1 metric ton of active germanium detector mass using 6" diameter, 2" thick detectors operating either at SNOLab or the Sanford Lab is anticipated.

Both xenon experiments, Xenon100 and LUX, are planning to upgrade. Xenon100 plans to upgrade from 100 kg of xenon detector mass to 1 ton (Xenon1T) [64]. LUX plans to upgrade from 350 kg of xenon with a fiducial mass of 100 kg to a 7 ton xenon detector with a fiducial mass of 5 tons (LZ) [65]. R&D will be necessary to develop the high-voltage, low-radioactivity feedthroughs and high-voltage components to provide the necessary drift field. Both experiments also need to develop xenon purification systems, particularly to remove radon and radioactive krypton from xenon. The LUX/LZ experiment uses low-radioactivity titanium as a principal construction material. Titanium has some advantages in strength and cost over oxygen-free, high-purity copper, which has been widely used in many previous low-background experiments. Radioactivity from photomultiplier tubes and materials used in construction of the TPC also needs to be reduced as much as possible. This detector also needs a neutron shield, and a gadolinium-doped liquid scintillator is proposed to serve as a neutron veto detector.

Next-generation two-phase liquid argon (LAr) dark matter detectors have the same problems with purification and high voltage as the xenon dark matter detectors but they have the additional problem of reducing radioactive ^{39}Ar . Pulse shape analysis can be used to identify ^{39}Ar if rates and backgrounds are sufficiently low. Argon from underground wells, which has much lower concentrations of ^{39}Ar than atmospheric argon, is being used by the DarkSide family of experiments. Detecting the 128 nm scintillation light from LAr requires wavelength shifting and efficient, large area photodetectors. There are other very large (multi-ton) liquid noble gas detectors planned, which combine xenon and argon (XAX, MAX, Darwin) [66]. These experiments will have the same scale-up problems as the next-generation argon and xenon detectors.

The COUPP experiment plans to upgrade from 60 kg of refrigerant, running in SNOLab, to 500 kg with a possible move to Sanford Lab [67]. It does not anticipate major scale-up problems other than keeping the amount of radioactive contaminants in the detector as low as possible.

Another approach to WIMP detection is to look for a sidereal anisotropy signal. Most contemporary approaches, such as the DMTPC [68] and DRIFT [69] collaborations, attempt to visually track images in a low-density TPC. Both the DMTPC and DRIFT collaborations have built and operated track detectors for dark matter with ~ 0.1 kg or more of target material. With this small target mass, DRIFT was able to set limits comparable, at the time, to much larger non-directional detectors owing to these detectors' excellent discrimination ability. Both DRIFT and DMTPC are developing ideas to field detectors at the ~ 1 kg scale, which would be capable of confirming or rejecting purported low-mass WIMP signals. An array of several hundred such detectors could provide clear confirmation to a dark matter detection by a generation-2 counting detector. Construction of such an array would be very challenging, but would be justified by the importance of providing an unambiguous signal for particle dark matter. R&D on both electronic and optical readout will continue for the next several years, at which time the dark matter community will most likely gel around a common method for a large mass array.

R&D on a columnar recombination approach may permit sensing of the nuclear recoil in a dense xenon gas and with a detector gas density ~ 100 times greater than possible with track visualization. This idea has, at the moment, not been experimentally verified and the work is still in an early stage compared with the low-density, low-pressure TPCs described above.

8.4.5 Light dark matter - axions

8.4.5.1 Current and planned projects

Axions are another possible candidate for dark matter. They are well motivated as a solution to the strong CP problem. Axions are predicted to be light pseudoscalars with masses in the range of 10^{-6} to 10^{-2} eV, with the smaller mass preferred from astrophysical considerations, and a coupling to two photons. There is currently one axion experimental search, ADMX, which uses the inverse Primakoff effect¹⁶ in a tunable microwave cavity with an 8 Tesla magnetic field [70]. Since the energy of the outgoing gamma ray from an axion conversion includes the mass of the axion, the experiment searches for a resonant peak in frequency, using a radio receiver, as the cavity is tuned through possible axion masses.

8.4.5.2 Key issues and next steps

The major challenge in the ADMX experiment has been to develop a radio receiver with sufficient sensitivity to measure very low axion conversion rates in the microwave cavity. The current ultra-low noise SQUID-based radio receiver is approaching the quantum limit for sensitivity. A second major challenge has been to maintain a stable, high magnetic field as the microwave cavity is tuned over a large range of possible axion masses. ADMX is well suited to detecting axions in the mass range 10^{-6} to 10^{-5} eV, but larger axion masses are difficult to measure with this technique due to the difficulty in building suitable tunable cavities.

Fermilab is conducting R&D on the use of a solid Xe detector operating at sub-Kelvin temperatures with superconducting sensors to read out photon, ionization, and phonon signals. This type of detector can be used to search for solar axions, which may be created in the core region of the sun. The axion-photon conversion by the strong Coulomb field of atoms in the crystalline Xe detector may cause coherent Bragg scattering of the photons, which depends on the energy and incident angle of the axions. This strong correlation provides sensitivities below an axion mass of 10 eV, providing coverage of a QCD axion parameter space not accessible to other experiments.

8.4.6 Cosmic microwave background radiation

8.4.6.1 Current and planned projects

The Cosmic Microwave Background (CMB) is a critical component for understanding cosmology and exploring the fundamental physics of the early Universe [71]. The focus of current CMB experiments is to characterize the faint CMB B -mode polarization pattern. There are two sources of B -mode polarization in the CMB. The first source, and the majority of the B -mode polarization, is due to gravitational lensing of the intrinsic CMB polarization, which appears at small angular scales (~ 10 arcmins). This lensing signal, which has recently been detected for the first time [72], provides a measurement of the growth of large-scale structure. Precision measurements of CMB lensing will provide constraints on the sum of the neutrino masses at a level relevant for understanding the neutrino mass hierarchy. The second, much harder to detect source of B -mode polarization is caused by the gravitational waves produced at the end of the inflationary epoch. These perturbations produce an angular B -mode power spectrum that peaks at angular scales of ~ 2 degrees. The amplitude of this signal is a measure of the energy scale of inflation, which is expected to

¹⁶ $\gamma + \text{axion} \rightarrow \gamma'$

be $\sim 10^{16}$ GeV, the energy scale favored by Grand Unified Theories. The measurement of the inflationary B -mode signal would not only validate the inflationary paradigm, but would also constrain the physics of inflationary processes, physics not accessible by other means.

CMB experiments are sensitivity-limited. The noise in an individual detector element is dominated by the shot noise of the measured photon flux. Experiments can increase sensitivity only by increasing the number of optical modes that are measured. The world-wide CMB program can be classified into “stages” where each stage corresponds to a 10-fold increase in the number of optical modes and corresponding sensitivity. Stage-I experiments have been deployed and consist of focal planes that measure much less than 1000 optical modes. Stage-I experiments do not have the sensitivity to measure CMB B -modes. Stage-II corresponds to current operating experiments which measure on the order of 1000 optical modes. These operating state-of-the-art instruments have observed CMB B -modes for the first time. Bolometers based on Transition Edge Sensors (TES) dominate the stage-II technologies. Stage-III projects are in the R&D stage and will deploy in the first half of this decade. These proposed experiments will measure on the order of 10,000 modes and will be the first to realize the era of “precision CMB B -mode science.” Technologies being pursued for stage-III experiments include arrays of single-moded TES bolometers coupled through superconducting microstrip and large-area multi-moded semiconductor bolometers. TES-based bolometers continue to be a core technology for stage-III. A stage-IV experiment will measure on the order of 100,000 optical modes across a broad range of angular scales from a few degrees to a few arcmins and large fractions of sky ($\sim 20,000$ deg², or about half of the full sky). TES-based bolometers with single-moded coupling are the only stage-III technology capable of simultaneously probing inflationary B -modes at large angular scales and lensing B -modes at smaller angular scales, which makes single-moded TES-based bolometers a critical technology for realizing stage-IV CMB science.

8.4.6.2 Key issues and next steps

The challenge for implementing single-moded TES-based bolometers for a stage-IV CMB experiment is one of scale. The key issues can be broken down into two classes: (1) stability of fabrication and (2) increased production throughput. Incremental and revolutionary R&D will address both of these challenges, though the challenge of production throughput also requires expanded tools and fabrication resources along with high throughput test facilities. Incremental R&D consists of stabilizing and scaling the fabrication of current detector architectures and materials, (such as detectors coupled to superconducting microstrips made from Nb-SiO_x-Nb trilayers). More revolutionary development consists of pursuing novel materials, fabrication processes, and new detector architectures. In addition to detector R&D, developments in the SQUID-based TES readout and multiplexing also have the potential to further simplify and streamline detector electronics.

8.4.7 Tests of space-time

8.4.7.1 Current and planned projects

An experiment at Fermilab, the Fermilab Holometer, will probe Planck-scale quantum geometry via position measurement with Planck spectral density. Its non-local, bi-directional position measurement will probe non-commutative geometry [73]. The experiment correlates signals of nearly co-located 40-meter Michelson interferometers which can measure motions of $\sim 10^{-18}$ m in $\sim 10^{-6}$ sec. When the ends of the arms of the interferometers are in a nested configuration, space-time volumes will collapse into the same state and be correlated, while when the ends of the arms are in a back-to-back non-overlapping configuration space-time volumes will be uncorrelated.

The holometer is currently under construction. One of the keys to distinguishing “signal noise” from background noise is to look at the frequency spectrum of the noise. The expectation is that signal noise will be frequency independent whereas mechanical background noise will fall off inversely with frequency, and the noise will not have the expected white noise spectrum. To keep electronic and laser noise uncorrelated, independent lasers and electronics are used in each interferometer.

8.5 Technologies

The preceding three sections have demonstrated the range and breadth of the current and planned U.S. experimental HEP program. These experiments share the need for many technologies which are being developed or which are already in use. Accelerator experiments need: fast, highly pixelated, radiation-hardened, low-mass vertex detectors with customized electronics; low-mass trackers with fast, highly segmented readout and customized electronics; cost-effective, highly segmented, radiation-hardened calorimeters with good energy resolution; and high-speed data acquisition systems. Experiments studying particle interactions with small cross sections or rare decays typically need cost-effective large volume detectors with affordable large-area charged particle detection or efficient optical readout systems. A subset of these experiments need materials and sensors with extremely low intrinsic radioactivity, while another subset need high-granularity detectors with fast timing and good energy resolution. Experiments in the Cosmic Frontier frequently need large arrays of ultra-low-noise electromagnetic radiation detectors. The technologies described in the following sections have been identified as being generic to many HEP experiments. They provide examples of areas where investment in detector R&D could increase the physics reach, and/or reduce the cost of experiments in the current and planned HEP experimental program. A more detailed overview is given in a sensor compendium [74].

8.5.1 Pixelated tracking

Low-mass, radiation-hardened pixelated detectors will be needed for ATLAS, CMS, an ILC, CLIC and muon collider in the Energy Frontier, and LHCb and next generation B or τ -factories in the Intensity Frontier. These will also often require sophisticated in-pixel processing such as time stamping, cluster finding, or inter-cell hit correlation. There are often competing demands for low-power, high-speed, electronics density and radiation hardness. The current technology of choice is bump-bonded hybrid pixels with either planar or 3-D silicon.

8.5.1.1 Monolithic pixels

The performance of silicon-based pixelated detectors is defined by the pixel size, density and power consumption of the readout electronics, thickness of the sensor, and capacitance of the sensing nodes. There are a number of technological approaches to the fabrication of detectors which can combine lower mass and power, decreased sensor thickness, and complex functionality for tracking detectors for HL-LHC and beyond.

Monolithic Active Pixel Sensors (MAPS) can evade sensor thickness and pixel size limitations by utilizing a thin (typically 15 μm) epitaxial layer under the CMOS transistor structures as the sensitive medium [75]. These devices can be made very thin, with small pixels, and avoid bump bonding. The main issues for MAPS sensors are radiation hardness and rate capability. Current MAPS sensors typically collect charge by diffusion rather than drift, making them about a factor of ten slower than biased sensors. MAPS sensors used

in commercial cameras have 5 transistors per pixel whereas HL-LHC rates will require logic densities greater than 1000 transistors per pixel. They also suffer from parasitic charge collection from PMOS transistors fabricated as part of CMOS circuitry as well as the coupling of large digital signals from the CMOS readout circuit to the connected sensor region.

Current MAPS R&D has an emphasis on charge collection by drift instead of diffusion, which is mandatory for HL-LHC radiation levels. High-resistivity epitaxial substrates can fully deplete and serve to mitigate radiation damage effects. HV-MAPS capacitively couples a MAPS-based sensor to a standard pixel readout chip, avoiding bump bonding and lowering cost [76]. The T3-MAPS technology uses a buried n-layer to isolate transistors from the substrate and preserves high logic density [77].

Silicon-on-insulator (SOI) sensors utilize a thin (typically 200 nm) CMOS layer, oxide-bonded to a thick silicon handle wafer [78, 79]. An SOI detector utilizes the handle wafer as a sensor connecting to the top electronics by vias etched through the thin oxide. These devices can be fully depleted, have fine pitch and low capacitance, and produce a large signal but are unlikely to be sufficiently radiation hard for HL-LHC.

Three-dimensional integration is a set of technologies for vertical interconnection of wafers including bonding, thinning, and interconnection [80]. It can offer advantages similar to those in SOI and MAPS applications without some of the disadvantages. For pixelated sensors it offers the opportunity for fine pitch (4 μm has been demonstrated) interconnection, low capacitance, and separate optimization of sensor, analog and digital layers. Silicon wafer stacking technology can be used to separate digital and analog circuits from the MAPS sensor, allowing for separation of PMOS and NMOS, eliminating parasitic collection, and allowing the sensors and analog circuits to be shielded from digital noise. Principal issues for R&D are related to the availability of the technology in foundries, and the issue of fabricating such devices with large area and low cost [81]. These developments are relatively recent and happening in several places at once. The U.S. could establish a leading role with modest support for such activities.

8.5.1.2 Radiation-hardened pixel sensors

LHC pixel detectors need to include sensors capable of surviving radiation doses of about $2 \times 10^{16} \text{ n}_{eq}/\text{cm}^2$ or providing easy replacement of the innermost pixel layers.¹⁷ In silicon detectors the primary radiation effects are increased leakage current, introduction of p-type impurities, and charge trapping. Some of the effects of p-type impurity introduction can be mitigated by device and defect engineering, but leakage current increase is a universal effect that can only be addressed by operating at temperatures of $\sim -20^\circ\text{C}$. For radiation doses of about $10^{15} \text{ n}_{eq}/\text{cm}^2$ the trapping distance will be significantly smaller than the thickness of a typical 300 μm thick detector. One important method of mitigating radiation effects in silicon is to reduce the drift distance by reducing sensor thickness. The depletion voltage is proportional to the square of the thickness, so the operating voltage is reduced and the reduced volume also reduces the leakage current. 3-D sensors take advantage of this by etching electrodes through the silicon. These electrodes then collect charge from the full detector thickness, while the small electrode spacing means that the devices can be fully depleted at low voltage [82].

Diamond sensors are still being studied for certain applications where both radiation hardness and moderate-temperature operation are important. There are continuing concerns regarding material availability and decreased signal collection after irradiation due to trapped charges forming a polarization field [83]. The smaller charge collected in diamond with respect to silicon requires the development of new, very low threshold readout chips.

¹⁷ $\text{n}_{eq}/\text{cm}^2$ is 1 MeV neutron equivalent fluence.

8.5.1.3 High-speed sensors

An ideal particle detector has low mass, large area, and includes pixelated tracking layers with both high resolution and high speed. Time resolution scales as $\sigma_t = T_{rise} \times \frac{Signal}{Noise}$, so a large-signal, low-noise system is needed. This can be accomplished by reducing the noise, by decreasing the sensor capacitance (which is not always possible), by increasing the amplifier current, or by increasing the signal. In a silicon sensor, an improved signal-to-noise can be obtained by increasing the detector thickness or by utilizing avalanche multiplication in the silicon itself [84]. A sensor utilizing either proportional or Geiger amplification can provide very good time resolution at low analog power, but these devices typically suffer from high noise rates. Solutions to noise might include cooling the sensors to reduce dark currents or utilizing a 3-D stack of SiPM-like sensors in coincidence to reject random noise.

8.5.1.4 Large-area arrays

Another important issue to resolve is how to build affordable, large-area arrays of pixelated detectors. Readout chips are typically limited to a reticule size of $2 \times 3 \text{ cm}^2$ and by the overall yield, which scales with chip area. Depleted silicon sensors typically require an inactive area larger than 2 to 3 times their thickness to reduce leakage currents near the diced edge. Active edge technology which utilizes 3-D sensors can eliminate this inactive area. Undepleted MAPS sensors also do not suffer from this problem. Technologies that combine 3-D sensors and interconnect technologies may provide low-cost ways of wafer-scale pixel tile production. Fine-pitch interposers of silicon or glass with through-vias can also provide a solution that matches the large areas available in wafer scale sensors and smaller areas associated with chips [85].

Micro-pattern gas detectors (MPGDs) for charged particle tracking and muon detection are an alternative to pixelated silicon vertex and tracking detectors [86]. These low-mass detectors have the potential of economically covering large areas and providing high tolerance against radiation damage, high spatial resolution (of order $10 \text{ }\mu\text{m}$), and good time resolution (of order 1 ns). Future work is needed to reduce the readout cost by developing highly integrated, radiation-hardened front-end readout electronics with at least 4000 channels/chip and high-density detector electronics interconnections with flex-circuit readout electronics directly integrated into the MPGD structure. Work is also required to develop innovative signal induction structures to reduce cost, improve performance, and provide stability against electric breakdown. It is important to develop materials with resistance to aging and radiation damage, as well as cost-effective MPGD construction techniques for high-volume production. There is a promising effort, led by the nuclear physics community, to establish an MPGD industry base in the U.S. to produce large area foils. With the recent increase in activities by both the nuclear and high-energy physics communities, e.g., the planned ATLAS and CMS MPGD upgrades for the HL-LHC, this is a promising area where the U.S. could play a leading role in the further development of this technology, synergistic between the two branches of science.

8.5.2 Mechanics and power

Mechanics and power refer to the interconnected issues of precision support structures, cooling, and electrical and thermal services. The implications and issues have some commonality across the frontiers but also present some specific challenges. The Energy and Cosmic Frontiers need very stable and precise structures, often with minimal mass and high radiation length, to support tracking or imaging sensors. The Intensity and Cosmic Frontiers need large vessels, cryostats, and support frameworks, which may be possible with standard mechanical engineering approaches but may also require very radiopure materials in novel configurations.

Across the Frontiers, issues of placement, metrology, and alignment, both in fabrication and installation, are of considerable significance. Precision mechanical actuator technologies can enable in-place alignment and adjustment of detector and sensor elements, such as tracker modules and fibers and imagers in focal planes. Mechanical actuators also play a role in the assembly and placement process for sensors and modules. The development of new types of probes, novel measurement configurations, and analytical methods are actively being pursued.

The goal of minimizing detector mass and maximizing radiation length requires an integrated system design incorporating cooling and power distribution as primary design considerations. Radiation-hardened trackers may need to operate at temperatures as low as -30°C . For such applications, low-mass, low-Z, high-strength, thermally conductive materials and cooling structures are crucial. A coordinated effort to develop new temperature-tolerant materials is key to science across multiple frontiers and has dramatic spin-off potential.

Carbon composites are one example. The U.S. currently has a dominant role in development and fabrication of carbon composites, with world-leading facilities and expertise at FNAL, BNL, and LBNL. Additionally, thermally conductive carbon foam is a successful U.S. industry SBIR development — it did not exist 10 years ago and is now commercially available. However, mechanical design and construction of composites require highly specialized engineering with a similar degree of specialization as IC design. Maintaining this engineering expertise is the only way to maintain leadership.

Providing power and signals to future particle detectors, particularly front-end detectors in high radiation and high magnetic field environments, is an increasingly challenging cabling problem since thousands of readout chips require tens of thousands of amperes of current at low voltage and large multidrop or point-to-point signal distribution. This requires appropriate cable and insulation materials, including large area flex cables, micro-coaxial cables, low-mass conductors, low mass reliable connectors, and encapsulants.

The main approaches to power distribution include DC-to-DC conversion and serial current flow. DC-to-DC converters at the load point allow power to be delivered at higher voltage and lower current. Radiation hardened DC-to-DC converters with air core inductors can operate in high magnetic fields but are currently limited in step-down ratio and efficiency. For example, the capacity of the CERN DC-DC converter is the central limiting factor in the design of the CMS Phase-II tracker readout chip. There is potential for very significant improvements in voltage ratio and efficiency as well as radiation hardness by utilizing laterally diffused CMOS converters, utilizing gallium nitride transistors, and moving to more efficient designs operating at higher frequency [88]. Serial powering works best when multiple loads all demand the same current. Novel configurations that optimize the current distribution are an interesting area for further study. Circuits and systems to monitor, control, and protect the power distribution network are also critical. Point-of-use circuits to reliably multiplex and monitor the high voltage bias current delivery to sensors can dramatically reduce the number of cable plants servicing large detector arrays and reduce the number of power supplies.

The development of radiopure materials covers both basic materials such as copper, titanium, and semiconductors and the fabrication of novel and specialized components. For example, the emerging 3-D printing technologies, combined with new radiopure polymers and ceramic or metallic powders, could be very relevant to future low-background applications [89]. A community-wide or small business approach to radiopure materials, fabrication, and assay could be very beneficial in this regard.

8.5.3 ASICs

Application Specific Integrated Circuits (ASICs) were first developed for particle physics in the 1980s to read out silicon strip vertex detectors in colliding beam experiments. They have since become one of the core

technologies available to detector designers. A separate Snowmass study has been carried out to evaluate integrated circuit design in the U.S. and its findings and recommendations are reported in the white paper “Integrated Circuit Design in the U.S.” [90]. A summary of the key points is provided below.

A number of factors make ASICs essential to HEP. These include:

- **Small physical size:** The space constraints of many detectors, most notably pixel vertex detectors, require custom microelectronics. Even when commercial electronics can be used, small ASICs can often be positioned closer to the sensor than would otherwise be possible. This reduces input capacitance and improves noise performance. In many cases the size of the cable plant is also reduced (e.g., smaller radiation length, less power dissipation on the detector, lower cooling).
- **Low power dissipation:** The infrastructure required to power and cool on-detector electronics often limits detector performance. Especially in high channel count applications, low power dissipation can be one of the most important specifications.
- **Radiation tolerance:** Many HEP applications require 10 - 100 Mrad total ionization dose tolerance as well as immunity against single event upsets. Future vertex detectors may require Giga-rad tolerance.

ASIC-related R&D is required in a number of areas in order to improve science output or simply to make future experiments in the Intensity, Cosmic, and Energy Frontiers possible. Examples are [81, 85]:

- high-speed waveform sampling
- picosecond timing
- low-noise high-dynamic-range amplification and pulse shaping
- digitization and digital data processing
- high-rate radiation-tolerant data transmission
- low-temperature operation
- extreme radiation tolerance
- low radioactivity processes for ASICs
- low power
- 2.5-D and 3-D assemblies

Major recommendations of the report include the continuation of strong physicist–engineer links, an increase in generic ASIC R&D, education to provide basic literacy in IC technology for physics students, and increased communication and collaboration among U.S. laboratories and universities. The DOE might want to consider an ASIC stewardship role for HEP, analogous to the particle accelerator stewardship role.

8.5.4 Calorimetry

Electromagnetic and hadronic calorimetry is central to many experiments in all Frontiers. At the LHC the principal challenge to detector subsystems is the survivability in the radiation field and at high rates. In addition, the ability to detect and accurately measure electromagnetic showers in the presence of large backgrounds is a major challenge faced in calorimeter designs for the HL-LHC. Intensity Frontier experiments utilize calorimeters designed to detect and measure electromagnetic showers with good resolution at energies from 10 GeV down to several MeV [91]. These devices often need precision timing in the range of a few tens of picoseconds, while having a high tolerance to ionizing radiation. Future lepton collider calorimeters are

required to have excellent energy resolution both for single particles and for jets. Size, cost and containment are critical. For applications in space, calorimeters need to be compact, omni-directional, and able to distinguish between electrons, gamma rays, hadrons, and high-Z nuclei.

In the last two decades there have been revolutionary changes in detector technology, which significantly impact calorimetry. The field has seen a significant increase in the capabilities of front-end electronics, enabling practical systems with a very large number of channels. There has been the development of very dense inorganic scintillators with large scale production capabilities, and enormous progress in understanding radiation damage and its mitigation. In the area of readout, robust, compact photodetectors such as silicon photomultipliers (SiPMs) have been developed, capable of working in high magnetic fields and allowing the construction of hermetic, yet highly segmented calorimeters. There has been continued progress in the development of modeling tools, allowing for detailed analysis of physics processes in the body of the calorimeter and advances in nanotechnology and non-linear optics allowing for major improvements in light collection and detection schemes. These new capabilities have spurred development of promising techniques for hadron calorimetry, such as particle flow and dual readout.

These changes have led to a re-evaluation of our approach to calorimetry. The particle flow technique, pioneered by the CALICE collaboration, is based on utilizing the momentum resolution provided by the inner tracking system to provide the basis for precise jet reconstruction. Information from the tracker can be combined with a fine-grained calorimeter to identify shower components with charged tracks, locate energy deposit from neutral hadrons, and combine this information into the best estimate of jet energy. Simulations have been shown to reconstruct jets at the Z boson mass with a precision of 3%. While this approach was developed in the context of experiments for ILC and CLIC, recently the adaptation of these ideas to the domain of hadron and muon colliders has begun.

A separate development, which has also resulted from a better understanding of the physical processes of hadronic showers, is the concept of dual readout calorimetry. In this approach, pioneered by the DREAM collaboration [15], correlations between the electromagnetic and hadronic fraction of hadronic showers are utilized to correct for energy lost to nuclear breakup. This is done by making separate measurements of Cherenkov and scintillation light in a shower. Thus, by knowing on an event-by-event basis how much energy has been deposited electromagnetically, fluctuations in the hadronic component can be corrected to achieve much better energy resolution than provided by the separate Cherenkov and scintillation measurements.

At the LHC, the CMS detector demonstrated the possibility of operating a crystal calorimeter at a hadron collider even with the lattice damage that occurs with hadron interactions, while the results from the ATLAS liquid argon calorimeters have shown that sampling in depth in a shower helps significantly in reconstructing the physics events.

There are other R&D efforts related to calorimetry technology. Secondary emission detectors and novel radiation-resistant wavelength-shifting or scintillating fibers are two active areas of R&D [92][93].

Brighter, faster and radiation hardened crystals, e.g., cerium doped lutetium oxyorthosilicate (Lu_2SiO_5 or LSO) and lutetium-yttrium oxyorthosilicate ($\text{Lu}_{2(1-x)}\text{Y}_{2x}\text{SiO}_5$ or LYSO), are needed for future HEP experiments at the Energy and Intensity Frontiers. With high density (7.1 g/cm^3), fast decay time (40 ns) and superb radiation hardness, LSO/LYSO crystals are the baseline for the Mu2e experiment at the Intensity Frontier and are being proposed for the CMS forward calorimeter upgrade for the HL-LHC [94].

Fast crystals with sub-nanosecond decay time, e.g., barium fluoride (BaF_2) and YAP:Yb, are needed for future HEP experiments at the Intensity Frontier to cope with the unprecedented event rate. To facilitate π^0 reconstruction, a longitudinally segmented crystal electromagnetic calorimeter (ECAL) would provide a photon pointing resolution in addition to excellent energy and position resolutions.

The cost of the crystals is a crucial issue at future lepton colliders because of the huge crystal volume needed to build calorimeters. UV-transparent crystals, such as PbF_2 , PbFCl and BSO, can achieve unprecedented jet mass resolution and are being considered for dual readout of Cherenkov and scintillation light in a homogeneous hadron collider calorimeter (HHCAL) detector concept.

R&D for future crystal calorimeters thus will proceed along the following lines.

- Investigation on the radiation damage effects induced by gamma-rays, neutrons, and charged hadrons in various crystals to be used in future HEP experiments.
- Development of super-fast crystals with sub-nanosecond decay time.
- Development of dense, UV-transparent, and cost-effective crystals for the HHCAL detector concept.
- R&D on detector design and compact readout devices for a segmented crystal calorimeter with photon pointing resolution.

8.5.5 Photodetectors

Photodetectors provide essential functionality for HEP experiments in a wide variety of contexts. Light can be generated by particle cascades in calorimeters, by scintillation in liquid noble gas detectors, in gas avalanches, by Cherenkov radiation, by scintillation or Cherenkov light in crystals, in extensive air showers, or through scintillating plastics. Requirements vary, but the physics reach of experiments in all three Frontiers can be extended through targeted improvements in three key areas: (1) spatial and time resolution, (2) economical coverage of large areas, and (3) robustness under extreme conditions such as high radiation environments, cryogenics, and high pressures.

Many measurements in particle physics are limited by the time resolution of particle detectors. Conventional photomultiplier tubes are single-pixel detectors with time resolutions of order nanoseconds. Next-generation photodetectors could deliver sub-nanosecond time resolutions and finely segmented spatial resolutions [95]–[100].

Often, the combination of precision timing and spatial resolution is as important as the timing itself, allowing the measurement of event kinematics in terms of 4 vectors rather than 3 vectors. Measurement of the arrival times of particles with resolutions approaching single picoseconds over a drift distance of 1.5 m could be combined with track information to determine particle masses, and thus the quark composition, of hadronic jets. Timing measurements can also be used to match high energy gamma rays to their respective interaction vertices, providing a useful capability for collider experiments interested in measuring certain Higgs, Kaluza-Klein, and gauge-mediated SUSY signatures. Similarly, in neutrino experiments where Cherenkov light is present, the combination of precision position and time information can be used to reconstruct tracks and EM showers with high resolution, which is important in reducing backgrounds for electron neutrino selection, as well as discriminating between gamma rays and electrons [101, 102].

Heavy flavor physics experiments rely on technologies such as Ring Imaging CHerenkov (RICH) and Direct Internally Reflected CHerenkov (DIRC) detectors, where the geometric and timing properties of Cherenkov light emitted by high energy particles passing through thin radiators are used to separate particles of similar momentum but differing mass. DIRC detectors, in particular, rely on timing to correct for chromatic dispersion effects on these Cherenkov rings [99, 103, 104, 105].

Given the large scales of future collider and neutrino experiments, a substantial improvement in the cost-per-area coverage of photodetectors can have a positive impact on large experiments. While this is not the

only factor, improvements that allow better light collection and area coverage per unit cost can address key physics needs within the currently tight budgetary environment. Further gains can be made using light collectors, devices such as Winston cones and wavelength-shifting plastics. These allow for improved light collection with the same number of photosensors. Finally, in the context of large water Cherenkov experiments, as in flavor physics, it is possible to employ full imaging optics such as parabolic and spherical reflective geometries.

A particularly fruitful area for future R&D is the development of novel, high-quantum-efficiency photocathodes, capitalizing on advances in the field of materials science [106]. It may not only be possible to significantly increase the detection efficiency of single photons, but also to tune photocathode responses for custom spectral ranges [107].

Many HEP experiments subject photosensors to extreme conditions, particularly low temperatures, high pressures, and high-radiation environments. Continued work on implosion mechanisms, stress-testing, and containment are important for vacuum-photosensors in deep-water and ice applications. It is also important that photosensors can survive and operate under exposure to cryogenic temperatures, such as those of liquid argon. In most collider-based environments, photosensors will be subject to unprecedented radiation doses. These conditions require development of radiation-hardened materials, such as fused silica and optical glues for DIRC-based detectors, SiPMT-based detectors with a good resistance to large neutron doses [108], photocathodes with resistance to radiation, and many other materials.

The Large Area Picosecond Photodetector (LAPPD) collaboration has developed flat-panel, large-area ($20 \times \text{cm}^2$) micro-channel plate (MCP) detectors with photocathode quantum efficiencies of 20% or better, a gain of between 10^6 and 10^7 , xy position resolution of about one millimeter, and time resolutions approaching single picoseconds. These detectors consist of MCP-based detectors, made by thin-layer deposition of enhanced secondary electron emissive materials on substrates made from drawn glass capillary tubes. The vacuum package is based on flat-panel, sealed-glass technology, with no external pins. The delay-line anode pattern consists of silver micro-striplines, where differential timing is used to measure the hit position in the direction parallel to these striplines and charge centroiding is used to determine the hit position in the transverse dimension. The nominal LAPPD design addresses the entire detector system, including front-end electronics. The LAPPD collaboration has developed a low-power ASIC, capable of sampling fast pulse shapes with 1.5 GHz analog bandwidth and up to 15 Gsamples/sec sampling rates [109].

Another area is the development of novel phototube geometries and electron optics. One example is the Microchannel Plate-based Large Area Photodetector (MLAP) project in China, which uses a perfectly spherical geometry, along with both reflective and transmissive photocathodes to increase photon detection efficiency [110].

Multi-gap resistive plate chamber (MRPC) technology, as pioneered by the ALICE detector, may be useful for very large time-of-flight (TOF) systems. Its TOF resolution is close to $\sigma \approx 50$ ps/track, which is a great achievement, considering that the detector's total area is close to $\approx 150 \text{ m}^2$. It provides a good particle-identification (PID) performance below 1 GeV/c. With more gaps in the MRPC multi-electrode structure, timing resolution could move towards $\sigma \approx 10\text{-}20$ ps/track. The STAR experiment reached a timing resolution of better than 100 ps/track. Its total area was 50 square meters [111, 112, 113].

8.5.6 Data flow

Acquisition of massive amounts of data is a problem in all physics frontiers. However, in spite of significant differences in operating regimes, the data acquisition (DAQ) demands from different frontiers have

similarities that can be generalized to the need of processing many data links at rates of between 3 and 10 Gigabits/second.

Luminosity upgrades to the LHC present significant challenges to existing LHC experiments. Operating the HL-LHC with 25 ns bunch crossing spacing at $5 \times 10^{34} \text{cm}^{-2}\text{s}^{-1}$ implies a pileup of up to 200 minimum-bias events per crossing. This is an order of magnitude greater than the 17 minimum-bias events per crossing at the LHC design luminosity ($10^{34} \text{cm}^{-2}\text{s}^{-1}$), and will degrade all occupancy-dependent trigger algorithms that rely on forms of isolation to identify electrons, muons, taus, and missing-energy signals. This requires a better-performing trigger with additional information, such as tracking data, which is used to reduce the trigger rates against the much higher backgrounds. The size of regions sampled for trigger decisions will need to shrink to handle the increased backgrounds. Therefore, substantial improvements will be needed for the ATLAS and CMS trigger systems for the HL-LHC. The ATLAS and CMS detectors will acquire 2 MB of data every 25 ns, or 10 TB/s. This requires a sophisticated trigger system, since this amount of data cannot be read out. ATLAS and CMS anticipate a Level-1 trigger system that reduces the input data rate to between 0.2 - 1 MHz for readout into the higher-level triggers, which output their data at 5 - 10 kHz. These experiments require studies of high-speed links, field programmable gate arrays (FPGAs), and high-speed backplane infrastructures in order to meet these challenging requirements.

The LSST ~ 3 billion pixel camera needs only a 1 Hz readout rate, but each readout consists of gigabytes of data. An increased bandwidth for the LArTPC readout for LBNE can allow a triggerless readout with minimal front-end sparsification to avoid unnecessary loss of precision and can defer processing to sophisticated algorithms running on online or offline computers. Modular component high speed massively parallel DAQ systems are one possible solution.

A promising modulator-based,¹⁸ high-bandwidth, optical readout in-air system for HEP detectors is in development at ANL. Optical modulators are more reliable, are lower-cost/Gigabit/second, are more radiation tolerant, and operate at lower power than vertical-cavity surface-emitting lasers (VCSELS). There are also small business development projects to develop radiation hardened, low-mass modulators. In addition, studies with wireless RF readout and optical beam power transmission have been carried out to test the application of new wireless readout technologies for use with large detectors or with smaller detectors that require increased mobility, such as detectors deployed to monitor the reactor neutrino flux or measure a neutron flux in security applications. The front-end circuitry, including a high-voltage power supply, is powered wirelessly, thus creating an all-wireless detector system [114].

Chips that can perform waveform sampling at rates of gigasamples/second are also useful in DAQ by allowing experimenters to perform real-time or offline digital signal processing, pulse shape analysis, cross-talk correction, and precision timing extraction. While commercial waveform digitizers can often provide this functionality, custom waveform sampling ASICs have been developed for applications requiring some combination of multi-gigasample/second sampling rates, low-power, and very high-channel density. However, these custom chips need very large buffer depths to handle fast sample rates and relatively slow readout rates, which can be a problem. In addition, many timing calibration constants for the large numbers of delay lines are needed for the fast sampling rates.

8.5.7 Large-volume, fully sensitive detectors

Large-volume, fully sensitive detectors are crucial for both neutrino and dark matter studies. The focus for neutrino physics is liquid argon, but other materials such as xenon, bulk semiconductors, or high-pressure

¹⁸Mach-Zehnder and absorption modulators with forgiving stability requirements.

gases are being studied. Dark matter experiments utilize semiconductors as well as liquid or solid noble gases or crystals.

Future high-mass neutrino detectors are likely to be fully sensitive noble liquid or water-based detectors. In order to fully exploit noble liquid detectors, we need to improve our understanding of the basic physics of the processes involved in producing the observed charge and light signals, as well as the backgrounds that accompany the desired signals. We also need a dedicated program to establish the properties of liquid argon, such as diffusion, electron-ion recombination, and electron attachment to impurities, as they relate to the desired physics capabilities of the detector and studies of other issues related to TPC construction, such as optimal wire spacing, the use of cold electronics, the reduction of backgrounds from contaminants, and shielding from cosmic rays.

Dark matter experiments are ultra-low in rate, less than 10^{-3} /day/kg, and very low in energy, between 10 – 100 keV, making background reduction and rejection the overriding experimental consideration. Most experiments utilize dual signals – light and ionization for noble liquid detectors, or phonons and ionization for semiconductor detectors – to separate the dark matter signal from radioactive backgrounds. Cosmic Frontier experiments span a very large spectrum of technologies and techniques, but they often share the requirement for low and well known background levels. A HEP community-wide or small business approach to radiopure materials and assay is likely to be very beneficial to a variety of experiments. Many dark matter experiments are planning to scale up in area to improve sensitivity. If a signal is found, the next step may be a much larger-scale dark matter observatory. An important capability for any such detector would be the ability to identify the direction of the nuclear recoil and therefore the WIMP wind. One possible approach is to utilize the ratio of recombination and ionization rates in the dense ion trail surrounding a recoil nucleus as a function of external field direction.

8.5.8 Low background materials

Direct-detection dark matter searches and neutrinoless double-beta decay experiments require very low radiation backgrounds and must be constructed with materials that are extremely radiopure. Controlling background radioactivity is one of the major requirements of the next generation of these experiments [115]. In general, this is accomplished by (1) selecting materials that are known by their chemistry to have intrinsically low levels of radioactivity and are free from contaminations of elements such as U and Th ¹⁹, (2) paying special attention to handling these materials and reducing their exposure to sources of radioactive activation and surface contamination, and (3) assaying the materials for radioactive isotopes by gamma counting with high-purity germanium detectors or testing small samples using mass spectroscopy. In addition, modern metallurgical techniques and special attention to polymerization precursors can reduce radioactive contamination in materials. For example, high-Z construction materials can be purified using electrochemical techniques to levels resulting in $< 1 \mu\text{Bq/kg}$ ²³⁸U and ²³²Th activities.

8.5.9 Superconducting sensors

Our capabilities with superconducting technologies have become sufficiently mature that superconducting detectors can be routinely fabricated with nearly quantum-limited performance. The commercial availability of cryogen-free, sub-Kelvin systems has significantly lowered the operational cost and complexity of operating

¹⁹U and Th produce neutrons via spontaneous fission or (α ,n), depending on the matrix in which they are embedded, as well as gamma rays with energies up to 2.614 MeV.

superconducting technology. Thus, it is possible to envision superconducting detectors having profound impact on future HEP science. The principal challenge facing increased incorporation of superconducting detectors into HEP is one of scale. Developing the capability of high volume production and operating a broad spectrum of superconducting detector arrays is an important goal for HEP instrumentation.

There are two superconducting technologies that have the most promise as detector arrays. The first of these is Transition Edge Sensors (TES) technology. Over the past 20 years, our understanding of TES detectors has grown substantially. TES detectors have led the past decade in dark matter direct detection and they have transformed the landscape of Cosmic Microwave Background (CMB) measurements. For both of these applications, the detectors have reached their fundamental background limit, and further improvements in scientific reach are possible only with larger detector arrays. The frontier of research for TES technology is mass production of cutting-edge detectors along with increasing the density of their cryogenic readout.

A second technology, kinetic inductance detectors (KIDs), has emerged at the turn of the century. Progress with KIDs has been rapid, and a detailed understanding of their micro-physics is expected within a few years. KIDs are a particularly attractive technology because they are naturally multiplexed, with a single element for both the detector and readout. This attribute reduces the number of steps required in fabrication, enabling high yield and rapid production of detector arrays.

Developing the techniques for fabricating large arrays of superconducting detectors promises immediate impact at the Cosmic Frontier. Making dark matter detector arrays with 100 to 1000 detectors will enable searches for low-mass WIMPs, along with ton-scale heavy-WIMP searches using superconducting technology. Making CMB bolometer arrays with 10,000 to 500,000 detectors will enable the next generation of CMB experiments, sustaining U.S. leadership in the field of CMB science. These experiments will explore the connection between inflation and Planck-scale physics while also measuring the sum of the neutrino masses. Making this many optical imaging spectrometers will be revolutionary for future studies of dark energy by enabling simultaneous large-aperture imaging and spectroscopy. Investment by HEP into generic superconducting instrumentation is critical for the development of these array technologies for HEP science. The immediate scientific impact of this R&D is at the Cosmic Frontier, though cultivating the expertise and resources for fabricating and reading out multiple kinds of superconducting detector arrays will likely have much broader impact. For example, there are applications of highly-multiplexed TES arrays for neutrino physics via measurement of coherent neutrino-nuclear scattering and detection of the cosmic neutrino background.

The majority of current efforts for developing superconducting detector systems is project-driven. The closest semblance of a broad superconducting detector program is R&D for space-based applications. Globally, there is no effort focused on developing generic superconducting technology for HEP science. The U.S. HEP community is best positioned to lead this development because of its connection to multi-disciplinary labs with the resources and tooling required for fabricating and reading out superconducting devices. Developing the capability and expertise for mass production and operation of superconducting detectors promises quick scientific return at the Cosmic Frontier while also providing access to new technology to the broader HEP community and beyond.

8.5.10 Emerging technologies

There are a variety of emerging technologies and ideas for using conventional technologies to solve current or anticipated problems in HEP. Several of these are based on novel materials, while others are based on using existing technologies in novel ways.

Graphene, a pure carbon planar structure, one atom thick with atoms arranged in a hexagonal structure, is of interest to HEP because of its extremely high electron mobility at room temperature, an optical opacity which is electrically tunable at frequencies up to a terahertz, very high thermal conductivity, very high strength and rigidity, and very low weight. Potential applications include use in ICs, where its high carrier mobility and low noise make it an excellent material for channels in field effect transistors, and in field effect transistors on undoped substrates used as high resolution particle detectors. Graphene's high electrical conductivity and electrically controllable optical transparency make it an excellent candidate for switching optical devices, such as modulators for optical data acquisition systems. Considerable R&D is necessary to produce inexpensive, usable quantities of graphene and devices based on it. Silicene, a silicon analog of graphene, is also being studied for possible similar applications and has the advantage of being an intrinsic semiconductor for electrical applications.

Another material that may be useful for HEP particle detectors is amorphous nanocrystalline thin-film silicon. This material provides the advantages of amorphous silicon, such as its strong absorption in the visible light spectrum, radiation hardness, and ease of uniform deposition over large areas on a variety of substrates, while benefitting from low-cost thin-film fabrication technology. To achieve desirable detector characteristics, such as long carrier lifetime and substantial drift velocity, nanoparticles of crystalline silicon are incorporated into the amorphous silicon, marrying the detector-grade characteristics of crystalline silicon with the inexpensive production methods of large scale amorphous silicon. Preliminary results using hydrogenated amorphous silicon containing silicon nanocrystallite inclusions indicate large enhancements of mobility and recombination lifetime compared to hydrogenated amorphous silicon films.

The plasma display panel (PDP) used in plasma TVs is an existing technology which is being studied for possible use as a large, inherently digital, high gain variant of a micropattern gas detector. The plasma panel sensor (PPS), which will be derived from the PDP, is comprised of a dense array of small, plasma discharge gas cells within a hermetically sealed glass panel, and is assembled from non-reactive, intrinsically radiation-hard materials such as glass substrates, metal electrodes, and mostly inert gas mixtures. The PPS offers the potential to provide low-cost, large-area ionizing radiation detectors with high granularity (1 millimeter), good spatial resolution (300 μm), and fast response time (~ 10 ns). Technology for fabricating these devices with low mass and small thickness, using gas gaps of at least a few hundred micrometers, is being developed.

8.6 Facilities

8.6.1 Test beams

At present, there are two test beam facilities in the United States, one located at Fermilab, and the other at SLAC. Each is heavily utilized for detector development.

The Fermilab test beam can provide 120 GeV protons, 32-66 GeV pions, 1-32 GeV mixed beam, and, when using a steel absorber target, a broadband muon beam. It can provide fluxes from 100 - 5,000 Hz below 8 GeV and up to 100,000 Hz for energies up to 120 GeV. It is instrumented with two Cherenkov detectors, a fixed beam telescope with a ~ 10 micron tracking resolution, four multiwire proportional chambers with ~ 500 micron resolution, two time-of-flight systems — one with 24 psec and one with 160 psec resolution — and lead glass calorimeters and scintillators.

SLAC has several test beams, one of which is appropriate for HEP detector R&D. It is an electron beam with energies between 2.2 and 15.0 GeV, a repetition rate of 120 Hz, and a beam charge between 20 - 250 pC with a typical value of 150 pC. There is a clean secondary beam of electrons which can be tuned to energies

between 2 and 13 GeV with $1 - 10^9$ e/pulse. Two scintillation counters are used as a trigger, and there are three silicon strip layers and lead glass blocks for beam instrumentation.

8.6.2 Irradiation capabilities

Irradiation facilities needed to test detectors up to radiation levels of 10^{16} particles/cm²/sec consist of (1) radioactive sources that typically provide low energy γ or neutron irradiation, (2) accelerated beams that can provide energetic protons, electrons and neutrons, and (3) nuclear reactors that typically provide low energy neutrons. There is also a High-Intensity Gamma-ray Source (HIGS) facility at Duke University, which is a free-electron laser (FEL) based Compton backscattering gamma ray source.

Radioactive source irradiation facilities exist at Argonne and Brookhaven National Laboratories as well as a few universities. In addition, Fermilab has several radioactive sources, including a 200 Ci ^{137}Cs source which generates $\sim 3 \times 10^6$ gammas/sec or 700 rads/hour at a distance of 30 cm. Sandia National Laboratory provides both gamma and neutron irradiation facilities. The Gamma Irradiation Facility (GIF) provides both ^{60}Co and ^{138}Cs irradiation; the Annular Core Research Reactor (ACRR) provides neutron irradiation [116].

Accelerated beam irradiation facilities include the 88 inch cyclotron for protons or ions at Lawrence Berkeley National Laboratory, a proton cyclotron at Massachusetts General Hospital, and the Los Alamos Neutron Science Center (LANSCE) 800 MeV proton beam. In addition there are the MT3 120 GeV proton, the MTA 400 MeV proton, and the 10 MeV neutron facilities at Fermilab, the Loma Linda University proton therapy machine, the Indiana University ISAT 30-200 Proton Cyclotron with fluxes from 10^2 to 10^{11} protons/cm²/sec, and the University of Massachusetts at Lowell irradiation facility which can produce between 100 rad to 2 Mrad/hour.

The LANSCE facility has a typical beam current of 83 nA or 5×10^{11} protons/pulse at a repetition rate of 1 Hz. A high-intensity pulse mode of up to 1 μA protons/pulse is also available with beam currents up to a microamp. The beam spot is quasi-Gaussian with diameters ranging from 1 to 8 cm.

8.6.3 Underground facilities

In the United States, there are three principal underground facilities, the 4,850 foot deep (1,478 meters) laboratory at the Sanford Underground Research Facility (SURF) in Lead, South Dakota; the Soudan Underground Laboratory at a depth of 710 meters in northern Minnesota; and the Waste Isolation Pilot Plant (WIPP) Underground Laboratory located in a low radioactivity salt deposit at a depth of 2,150 feet (655 meters) in Carlsbad, New Mexico. There is also an underground research facility at a depth of 1,700 feet (518 meters) at the Kimballton Underground Research Facility (KURF) in Virginia. We refer to Capabilities in Chapter 7 for a full overview of the world-wide facilities.

8.7 Universities and partnerships

8.7.1 University resources

Universities provide crucial expertise, infrastructure, and resources to the particle physics instrumentation program. They provide both depth and breadth of resources that cannot be supported at the national laboratories. Universities have unique connections to scientists at the forefront of materials science, nanotechnology, chemistry, and electrical engineering, any of which might provide vital insight into the next breakthrough technology. They also provide training for the next generation of scientists and instrument developers and provide the graduate students and postdoctoral fellows who do much of the work of developing and testing new technologies.

Despite continuing erosion of university R&D funding, there are still a large number of university-based centers of excellence that provide focused support for projects and R&D in particle physics. Some universities have machine shops capable of precise fabrication as well as fabrication of large objects, centers for design of integrated circuits, laboratories with expertise in bump bonding and circuit interconnection, laboratories for crystal development, and capabilities for silicon detector testing and radiation damage studies. University-based scientists and engineers develop integrated circuits for ATLAS and CMS, design and develop carbon fiber structures for pixel detectors at collider experiments, develop superconducting sensor technologies, provide the expertise to produce pixel detectors in U.S. university laboratories, and develop and test a variety of prototype detectors for use in large and small HEP experiments.

Partnerships with university nanotechnology and materials science departments have yielded radiation-hard 3-D silicon sensors now utilized in the ATLAS Insertable B Layer (IBL), techniques for thinning and annealing sensors as thin as 50 μm , transition edge sensors used in CDMS, and carbon fiber support structures used in the D0 and ATLAS detectors.

8.7.2 Partnerships

8.7.2.1 Research collaborations between industry, universities and laboratories

National laboratories and universities are key drivers for research and innovation by using their facilities and expert personnel to carry out fundamental and applied studies to advance basic knowledge. Industry growth is fueled by constant innovation to improve the performance and features of established products, as well as investing in disruptive technologies that ultimately create new products which can be cheaper, simpler, and/or smaller. Collaborations between national laboratories and universities, which develop the disruptive technologies, and industry, which can invest in these technologies to produce better products, can be mutually beneficial.

Because of the increased complexity of modern research experiments and the applications of many HEP detectors to other scientific fields, and to government, medical, and industrial applications, it is becoming increasingly advantageous for universities and laboratories to develop collaborations with industry for detector development, fabrication, and commercialization. The experience of industry with support, lifecycle, and supply chain management can help mitigate some of the challenges of supporting experiments throughout their lifetimes and bring down the total cost of ownership.

We have identified several ways to develop stronger ties between the national laboratories, universities, and industry that could provide a backbone for future industrial detector development work.

One of these is to enhance the communication between the HEP Detector R&D community and both small and large industries that have interests in HEP detector development. Another is to invite companies from various industries to future annual CPAD workshops to provide the industrial community with an overview of current and future detector development projects. It is crucial to involve industry at an early stage of an experimental program to help align development plans with experimental needs. Finally, forums at these workshops for industries to give presentations on their interests and capabilities would provide university and laboratory detector development personnel an overview of the capabilities and interests of the industrial community.

Universities that hope to use engineering, technical, and infrastructure resources at national laboratories are often deterred by not being able to charge tasks to an established project. A possible solution is for laboratories to set aside a small generic R&D fund dedicated to funding outside users wishing to use laboratory infrastructure as part of a detector R&D project. Use of these funds could be controlled by the laboratory point of contact or designee.

The DOE SBIR/STTR program provides grants to small industries, often in collaborations with universities and laboratories, to carry out work of mutual interest. Cooperative Research and Development Agreements (CRADAs) are another important tool to increase collaborations between national laboratories and universities and industry. These can help with confidentiality and commercialization of technologies and are open to larger companies. National laboratories and universities are currently working on reducing the administrative hurdles involved in establishing these agreements.

8.7.2.2 Instrumentation pool

At Fermilab there is a large Physics Research Equipment Pool (PREP) of commercial instrumentation. Fermilab has continued to loan this instrumentation to members of the HEP community who have research ties with Fermilab. Because of financial constraints, this program no longer has the necessary personnel to adequately maintain the existing instrumentation pool and to carry out the administrative overhead required to maintain an effective loan program. In order to use this instrumentation in a maximally efficient way in the U.S. HEP research community, Fermilab has proposed that the PREP loan program be expanded to the entire U.S. HEP research community. This would require a modest amount of DOE support. Furthermore, this pool might serve as a way for companies to expose their products to a large user community. It is not clear whether a large, national instrumentation pool is cost-effective for the U.S. HEP research community, but it might provide a way for universities and laboratories to collaborate effectively in the use of instrumentation resources.

8.7.2.3 Synergies between HEP and Nuclear Physics

Future experiments in nuclear and particle physics will require new instrumentation in order to carry out fundamental measurements of QCD, electroweak interactions, studies of fundamental symmetries, understanding the properties of neutrinos and the spin properties of nucleons, and a variety of other physics topics. Much of the instrumentation to accomplish this is common to both nuclear and particle physics and requires the development of new technologies. Both the nuclear and HEP communities have ongoing programs to develop new detector technologies. Both communities would benefit from identifying areas of

mutual interest and working together to achieve better performance and identify the most cost-effective detectors for future experiments, particularly in the development areas described below.

Mutually beneficial developments related to calorimetry include (1) dense, compact electromagnetic calorimeters, (2) new techniques for compensating hadronic calorimeters including dual readout calorimetry, (3) new crystal calorimeters, (4) new light collection schemes for fiber calorimeters, (5) new and improved silicon photomultipliers (SiPMs), particularly extending their dynamic range and improving their photon detection efficiency and radiation hardness, and (6) readout systems for large numbers of SiPMs.

Mutually beneficial developments related to particle tracking include (1) large area gas electron multiplier (GEM) detectors, (2) TPCs with GEM readout and short drift GEM detectors, (3) new readout structures for GEM detectors, (4) low-mass GEM detectors, (5) TPCs with combined particle ID capability, and (6) low-mass, monolithic, active pixel sensors (MAPS).

Mutually beneficial developments related to particle identification include (1) Cherenkov ring imaging detectors with GEM readout, (2) DIRC detectors, (3) large area, high-resolution (< 10 ps) time-of-flight detectors, (4) aerogel counters, and (5) techniques for muon detection.

Finally, mutually beneficial developments related to readout electronics and triggering include (1) ASIC development for micropattern detector readout, (2) ASIC development for silicon photomultiplier readout, (3) modular readout systems for micro-pattern gas detectors (MPGDs) and SiPMs, and (4) fast triggering and large scale readout systems.

8.8 Leadership in detector R&D

Leadership requires both vision and the ability to execute that vision. In instrumentation that vision must be both scientific and technical. We need scientists who not only understand the physics of Higgs boson decays, but also understand the physics of sensors, materials that can be used for supports, ways to interconnect the sensor arrays, the limitations of each, and the in-depth knowledge of the physics underlying the instrument to be able to improve its science reach. These scientists in turn require access to infrastructure, engineers, and technicians to turn visions into prototypes and working detectors. These resources might be at universities, national laboratories, or industry, but they must be made available to scientists with good ideas. A goal of the U.S. instrumentation program must be to enable the execution of visionary ideas as well as to support incremental development and system engineering.

Over the years, the U.S. has led many significant detector R&D efforts in HEP. Examples include the invention of the TPC, the development of transition edge sensors for phonon detection, the development of CCDs as detectors for cosmology, and the development of liquid argon calorimetry. A significant fraction of the overall HEP detector R&D activities is now being carried out in Europe, Japan, and China. The U.S. has no equivalent to the CERN RD programs or the EU Framework initiatives that can address significant technical challenges in a systematic way.

Another factor which contributes to the loss of detector R&D activity in the U.S. is the unwillingness of most U.S. graduate programs to grant a Ph.D. to HEP students who develop hardware instead of carrying out an analysis of experimental data. The multi-year design and construction time required for detector systems in large accelerator experiments makes it impossible for graduate students to take on major hardware roles in large experiments and graduate in a timely manner. In addition, new Ph.D.s with primarily instrumentation development backgrounds typically have difficulty getting U.S. university positions. To address these problems, incentives must be found to encourage younger physicists to become

more proficient in hardware development. Summer programs have been organized by national laboratories with HEP programs to give graduate students hands-on experience with instrumentation.

The U.S. maintains clear leadership roles in a number of detector R&D projects of importance to HEP. In recent years the U.S. has developed 3-D sensors, applications of 3-D circuits, water-based scintillators, large area photodetectors, cryogenic germanium and silicon detectors with phonon and charge readout, dual readout of calorimeters, and deep depletion CCDs. The U.S. also leads in the development of cryogenic superconducting detectors. These developments have been based on the strong interdisciplinary culture among universities and national laboratories with utilization of centers of excellence in each. In the longer term these resources can provide a continued focus for innovation and leadership.

8.9 Conclusions

Modern particle physics experiments and high-energy accelerators put increasingly extraordinary demands on the experimental tools employed. In addition, the development of state-of-the-art detectors is broadening beyond experiments at the highest energy accelerators to include lower-energy, high-intensity and ultra-low-background experiments, very large volume experiments to study very rare processes, and experiments that study fundamental properties of the cosmos.

The field of particle physics finds itself with a panoply of possible large-scale physics experiments, each of which has the potential to provide fundamental insight into the structure of space, time, and matter. These experiments are exceedingly difficult technically, and many of them currently deploy technologies that are expensive, limit physics capabilities, or both. Furthermore, multiple approaches are often required to explore and establish new scientific principles. New technologies must be developed to provide the answers to these fundamental science questions.

Focusing only on scaling up existing technologies to larger experiments, or carrying out detector R&D only when it is needed, is tempting in tight budgetary times, but this approach will not serve the field well in the long term. A development program of new approaches and innovation will be necessary to make future experiments feasible and economically viable. The demands of the future particle physics science program will require an investment in a detector R&D program over intermediate and long time frames to develop new tools and technologies that are both cost-effective and have an enhanced physics reach. Such an investment will enable the field to carry out flagship domestic experimental research and have leadership roles in off-shore experimental projects. New, transformative detection capabilities will ensure an affordable and healthy experimental particle physics research program in the future. The major challenge for the Instrumentation Frontier is, therefore, to design a program that will enable the U.S. to maintain scientific leadership in many key areas of a broad international experimental program in particle physics.

Ideally, detector development should take place at various levels of risk and maturation time scales. Detector R&D carried out within existing experiments is, by necessity, project-driven and provides low-risk, relatively incremental improvements to existing technologies. Detector R&D that is motivated by common needs among various experiments is generally longer-term, can be higher risk, and adds value to multiple experimental areas at the same time. This type of R&D can lead to incremental or significant improvements in cost reduction, scientific reach, or both. Finally, long-term detector R&D leading to transformative changes in cost reduction, increase in scientific reach or both, across a significant part of the experimental program is the kind of high-risk, high-reward detector R&D which has the potential to lead to scientific breakthroughs. Underpinning all these R&D efforts is the urgent need for training the next generation of instrumentation experts, without which there can be no long-term future.

Major technological advances based on a better understanding of the underlying science are also occurring in other scientific disciplines such as materials science, photonics and nanotechnology. Many of these advances have the potential to lead to transformational new technologies for particle physics detectors. Technology advances in other experimental scientific disciplines that could contribute to opportunities for innovation and developing transformative technologies for particle physics must be exploited.

A healthy national instrumentation program must, therefore, provide a balance between evolutionary and revolutionary detector development while training the next generation of experts. We recommend that this national program accomplish the following goals:

1. Dedicate an appropriate fraction of the HEP budget to supporting detector R&D with clearly identified areas of detector development based on the strengths of the HEP community.
2. Achieve an appropriate balance between evolutionary and revolutionary detector R&D, i.e., an appropriate “portfolio of risk”, and build expertise in new and innovative technologies that can be applied to the design and construction of novel, cost-effective particle physics detectors.
3. Develop a process for optimizing the use of existing university, national laboratory, and industry resources, for growing and retaining local technical expertise at universities and laboratories, and for identifying incentives and mechanisms for improving detector R&D collaborations and equipment sharing among universities, national laboratories and industry.
4. Create opportunities for attracting and providing careers for high-energy physicists with interest in, and outstanding capability for, innovative detector design and development to develop a community of experimental physicists with the background and skills needed to design and build future generations of high-energy physics experiments.
5. Provide mechanisms for identifying and transferring appropriate technologies developed in other scientific disciplines to high-energy physics and transfer applicable technologies developed in high-energy physics to other science disciplines, such as nuclear physics, basic energy sciences, and related branches of science, medicine, and national security.

The Snowmass process, as summarized in this report, provided broad input and guidance from the HEP community in identifying the crucial elements of a national technology and instrumentation program. This information will be used by CPAD, the Coordinating Panel for Advanced Detectors created by a previous DPF Task Force on Instrumentation, whose mission is to advocate this program, promote its merits, and provide venues for regular presentation of results. CPAD will also bring different scientific communities together to make HEP aware of developments elsewhere and provide information about instrumentation needs and activities to facilitate coordination between the funding agencies and the HEP instrumentation community.

An outstanding U.S. instrumentation program requires a variety of enablers, some of which already exist, and some of which need to be developed. The U.S. has a unique resource embodied in its national laboratories, particularly with regard to extensive technical infrastructure. Multidisciplinary laboratories have broad scientific breadth and the potential to provide cross-fertilization of technologies from nuclear physics, basic energy science, materials research, engineering, chemistry, and computer science. Many national laboratories have close links to top-ranked universities, which are also centers for multi-disciplinary innovation and ideas but which generally cannot maintain the level of technical infrastructure available at the laboratories. By combining the intellectual and manpower capabilities of universities with the resources of the national laboratories and the product development capabilities of industry, the U.S. can continue to confront and overcome many of the technological challenges future particle physics experiments will face.

Test beams, irradiation facilities, and centers of technical excellence are also program enablers. The U.S. has several test beams and irradiation facilities as well as centers of technical excellence and expertise, both at universities and laboratories. These facilities are a critical component of instrumentation work, and there are Snowmass white papers describing them [117]. However, since the Snowmass process did not complete the identification of all the available facilities in the U.S. which support instrumentation development, CPAD will need to complete this work. Adequate support for these facilities is essential for a healthy detector R&D program.

A crucial enabling element for an instrumentation program is the development of expert physicist manpower. Investing in younger physicists who work on instrumentation is critical for a long-term, viable particle physics program. Doctorate degrees are awarded in instrumentation in the U.S. only under exceptional circumstances, and early careers in instrumentation are strongly discouraged given the emphasis on analysis to obtain faculty positions. A major challenge for CPAD will be finding ways to develop and retain outstanding instrumentation physicists within the U.S. HEP community.

We should identify mutually beneficial collaborations between HEP, other scientific fields, and industry. The increased complexity of modern research experiments and the applications of many HEP detectors to other scientific fields and to government, medical, and industrial applications, makes it advantageous for universities and laboratories to develop collaborations with industry for detector development, fabrication, and commercialization. One mechanism to increase the communication between the particle physics community and both small and large industries is to invite these industries to future annual CPAD workshops to provide the industrial community with an overview of current and future detector development projects. Industrial forums at these workshops would provide the university and laboratory detector development community an overview of the capabilities and interests of industry. It is expected that this venue could also lead to a better utilization of the DOE SBIR/STTR and CRADA programs.

This report describes some of the overlaps in the instrumentation developed by particle physics and the instrumentation needs in other areas of science. This holds especially true for the instrumentation needs for nuclear physics. All science communities have ongoing detector development programs to develop these technologies, and each would benefit from identifying areas of mutual interest and working together to achieve better performance and the most cost effective detectors for future experiments. CPAD could play a leading role at facilitating this dialogue.

Detectors will need various combinations of finer granularity, higher speed, larger sensitive detection volumes, and large area readouts to carry out experiments at higher energies and luminosities, to make more precise and inclusive measurements, and to search for rare processes. From the overview given in this report the following strategic instrumentation areas can be identified: ASICs, calorimetry, high speed trigger and data acquisition, large volume detectors, photodetectors, pixelated sensors, and power and mechanics (see Table 8-1). These areas all focus on the major technological barriers that stand in the way of reaching our science goals. Some of them have the potential to deliver very cost-effective instrumentation methods and provide breakthrough technology. The choice of these areas is guided by their physics impact and existing strengths and capabilities in the U.S.. Consideration was given to the technology's usefulness to other branches of science. Although some of the research goals described in this report may seem very challenging, it is likely that many, if not all, can be realized with a dedicated instrumentation effort. This selection of strategic themes also illustrates the breadth of their applications in HEP.

Providing additional support beyond the existing directed and generic R&D funding to initiate a set of "grand challenges" to encourage long term, more speculative, but potentially higher-impact R&D in instrumentation would be another enabler for transformative innovation. Once identified, such grand challenges would be effective in focusing the creative power of the HEP community on problems that have the potential for large payoff. Areas where existing technologies do not provide sufficient physics performance or would be cost-

prohibitive for meeting the goals of future experiments are good candidates for new initiatives. These grand challenges should be issued nationally, and cross-disciplinary collaboration should be strongly encouraged. This would allow the program to take advantage of the tremendous advances that have been made in areas of science outside the field of particle physics that could prove very valuable for the development of future instrumentation. Because of the cross-disciplinary aspect, close collaboration between universities and national laboratories will be key. Industry should be involved where appropriate. Funding would be subject to proposal review, but should be at a substantial level for a period of at least three years. CPAD should be involved in identifying the set of grand challenges that would define the program.

Table 8-1. *Strategic themes in instrumentation with areas of applicability.*

Instrumentation area	Technology examples	Energy F.	Intensity F.	Cosmic F.	Nucl. Phys.	BES	Applied
Pixelization and tracking	MAPS, 3-D silicon	X	X	X	X	X	X
Mechanics and power	Carbon foams, thin silicon Power delivery	X	X	X	X	X	X
Calorimetry	Crystal EM and hadronic Compensating, PFA	X	X		X	X	
Trigger and DAQ	TCA, high-speed optical links	X	X	X	X	X	X
Photodetectors	LAPPD, SiPM	X	X	X	X	X	X
Large volume detectors	Neutron detectors, photodetectors Low background materials		X	X	X		X
ASICs, custom electronics	Cold, fast electronics, 3-D Waveform digitization	X	X	X	X	X	X

In particle physics we explore extreme realms of space, time, and energy. These explorations require unique instruments. Developing these instruments requires vision and support, both financial and scientific. The pace of innovation has not yet slowed, but the technical and fiscal challenges have increased with the scale of experiments. In this report we have reviewed the instrumentation needs in the Energy, Intensity and Cosmic Frontiers. We have identified significant opportunities and synergies for instrumentation development. We have argued for programs to support the development of critical and strategic technologies, and argued for renewed emphasis on the training and support of young scientists. We have reviewed technologies, and identified areas of particular importance and promise. We have also argued that a program to address strategic challenges would provide a missing component and better prepare the field for the future. We hope that CPAD can provide the future support to the community and advise the agencies, which will enable particle physics to realize its vision. A stably and adequately funded generic instrumentation program with a balanced portfolio of risk, opportunities for young physicists, and a dedicated portion of the HEP budget is essential. It will ensure that the field invests in its future, and establishes a foundation for a competitive, healthy program for the long term.

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Computing

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9.1 Introduction

Computing has become a major component of all particle physics experiments and many areas of theoretical particle physics. Progress in particle physics experiment and theory will require significantly more computing, software development, storage, and networking, with different projects stretching future capabilities in different ways. As a result of considerable work throughout the Snowmass process, we recommend improved training, more community planning, careful and continuing review of the topics outlined in more detail below, and expanded efforts to share our expertise with and learn from experts beyond our field.

The Computing subgroups covered user needs and infrastructure. Experimental user needs subgroups included those for the Cosmic, Energy and Intensity Frontiers. Theory subgroups covered accelerator science, astrophysics and cosmology, lattice field theory, and perturbative QCD. Four infrastructure groups predicted computing trends and how they will affect future costs and capabilities. These groups focused on distributed computing and facility infrastructures; networking; software development, personnel, and training; and data management and storage. The Computing subgroups engaged with the other frontiers to learn of their plans and to estimate their computing needs. The infrastructure groups engaged with vendors, computer users, providers, and technical experts.

Our study group considered the hardware and software needs of particle physics for the next ten to twenty years, and recommends, not a grand project or two, but a continuing process of monitoring and supporting the hardware and software needs of the field in order to optimize the contribution of computing to scientific output. One difference between computing and other enabling technologies is that there is a vibrant global computer industry that is constantly creating new products and improving current ones. Although some of the computing equipment that we deploy is customized, such as application-specific integrated circuits, the vast majority of it is widely available. We are not building a bespoke detector with a multi-decade lifetime. For the most part, we are purchasing commercially available computing equipment that will last less than ten years. However, we need to carefully examine our computing needs and the available technology to ensure that our systems are configured to cost-effectively meet those needs. In contrast to the short lifetime of the hardware, the software that is developed for both experiment and theory has a longer lifetime. However, the software is undergoing continual development and optimization.

Two different styles of computing are currently used in particle physics. The experimental program mainly relies on distributed high-throughput computing (HTC). The distributed computing model was pioneered by Energy Frontier experiments. It relies on distributed computing centers that are part of the Open Science Grid in the U.S., with additional centers across the globe. The theoretical computing and simulation

needs are more commonly addressed by high-performance computing (HPC) in which many tightly coupled central processing units (CPUs) are working together on a single problem. These resources are provided mostly through DOE and NSF supercomputing centers.

One important issue to consider is to what degree can or should data-intensive applications that have traditionally relied on HTC use national supercomputer centers, which have traditionally been designed for HPC usage? Work is proceeding to determine how well and how cost-effectively these HTC applications can run at HPC centers. Also, traditional HPC applications are developing more data-intensive science needs, which are currently not a good match to existing and next-generation HPC architectures. Computational resources will have to address the demands for greatly increasing data rates, and the increased needs for data-intensive computing tasks.

Another pressing issue facing both HTC and HPC communities is that processor speeds are no longer increasing, as they were for at least two decades. Instead, new chips provide multiple cores. Thus, we cannot rely on new hardware to run serial codes faster. We must therefore parallelize codes to increase application performance. Today's computer servers contain one or more multi-core chips. Currently, these chips have up to 10 (Intel) or 16 (AMD) cores. For additional performance the server may contain computational accelerators such as graphical processing units (GPUs) or many-core chips such as the Intel Xeon Phi that can have up to 61 cores. In the past, computing resource needs for Energy Frontier experiments scaled roughly with the rate that processor speeds increased, following Moore's law. In the future, this requires full use of multiple-core and many-thread architectures. Also, scaling of disk capacity and throughput is of significant concern, as per-unit capacities will no longer increase as rapidly as they have in the past.

These changes in chip technology and high-performance system architectures require us to develop parallel algorithms and codes, and to train personnel to develop, support and maintain them. Different subgroups are at different stages in their efforts to port to these new technologies. In the U.S., the effort to write lattice QCD codes, for example, started in 2008 and there has been code in production for some time; however, there are other parts of the code that are still only running on CPUs. Cosmological simulations have exploited GPUs since 2009, and some codes have fully incorporated GPUs in their production versions, running at full scale on hybrid supercomputers. Accelerator science is also actively writing codes for GPUs. Some of the solvers and particle-in-cell infrastructures have been ported and very significant speed-ups have been obtained. The perturbative QCD community has also started using GPUs.

These trends lead to vastly increasing code and system complexities. For example, only a limited number of people in the field can program GPUs. In this and other highly technical areas, developing and keeping expertise in new software technologies is a challenge, because well-trained personnel and key developers are leaving to take attractive positions in industry. Continued training is important. There are training materials from some of the national supercomputing centers. Summer schools are organized by the Virtual School of Computational Science and Engineering (www.vscse.org) and other groups. We must examine whether these provide the right training for our field and whether the delivery mechanisms are timely. On-line media, workbooks and wikis were suggested to enhance training. Another area of common concern is the career path of those who become experts in software development and computing. We should help young scientists learn computing and software skills that are marketable for non-academic jobs, but it is also important that there be career paths within particle physics, including tenure-track jobs, for those working at the forefront of computation.

Subsequent sections of this chapter summarize the finding of each of our subgroups. We start with the needs of the Energy, Intensity, and Cosmic Frontiers. We then turn to the needs of theoretical areas: accelerator science, lattice field theory, and perturbative QCD. Theoretical work in astrophysics and cosmology is included in the Cosmic Frontier section. Our last section briefly summarizes our conclusions. Additional details appear in individual sections and in the full subgroup reports.

9.2 Computing for the Energy Frontier

Computing for experiments at the Energy Frontier is now dominated by the huge data processing and analysis demands for the Large Hadron Collider (LHC). The scale of the LHC computing problem has required the creation of the global computing infrastructure of the worldwide LHC Computing Grid (WLCG), which has been hugely successful. In each of the LHC experiments, data samples at the 100 petabyte scale must be accessed by experimenters around the world and shared between different science groups within the overall data analysis organization. This has been accomplished by developments in networking based on a tiered system of computing centers at various scales. Both data storage and processing are distributed through this hierarchy of centers to maximize usability and throughput.

9.2.1 Current and future computing needs

Progress in distributed HTC, high-performance networks, distributed data management, remote data access, and work flow systems has helped experimental groups, production teams and scientists worldwide to effectively access and use their data. Collaboration, facilitated by groups such as the WLCG and national consortia, enables this progress. In the U.S. the Open Science Grid is bringing together the sites, experiments, infrastructure providers, and computing specialists, that are necessary sustain and further develop this distributed environment.

LHC computing today routinely uses 250,000 CPU processor cores and nearly 170 PB of disk storage in addition to large multi-hundred PB capacity tape libraries. The experiments generate over 1 PB per second of data at the detector device level. Triggering and real-time event filtering is used to reduce this by six orders of magnitude. The upcoming run of LHC experiments will have a final rate to persistent storage of around one gigabyte per second. The main requirement limiting the rate to storage is that of keeping the storage cost, and the cost of the computing to analyze the stored data, at a tolerable level.

Looking forward, the increased luminosity at the HL-LHC stands out as a significant challenge. The expected increases in trigger rate, pileup and detector complexity (number of channels) could increase the data rates by a about a factor of 10 or more. This order of magnitude increase in storage and CPU requirements presents a new challenge for the computing infrastructure, and the community will need time to prepare for it. The LHC community is beginning to review their computing models as they make plans for the next decade. It is anticipated that the general design will be an evolution from the current models, with the computing resources distributed at computing centers around the world. In contrast to the LHC and its future upgrade, science at Energy Frontier lepton colliders is unlikely to be constrained by computing issues.

The full report on Computing for the Energy Frontier [2] presents a prediction of the magnitude of changes that should be expected over the coming decade. It reviews the changes between the Tevatron and LHC over the past 10 years. We argue that the resources needed for LHC Run2, starting in 2015 and ending in 2021, can probably be accommodated with a roughly flat budget profile. However, the start of HL-LHC will be a large disruptive step, like the one going from the Tevatron to the LHC.

The increases in LHC computing and disk storage since its start are shown in Figure 9-1. CPU performance is measured in terms of a standard benchmark known as HEP-SPEC06 (HS06) [1]. The CPU increases at a rate of 363K HS06 per year and the disk at 34 PB a year on average. The rough linear increase in CPU/yr is the combination of three separate periods that average to linear. The period 2008 through 2010 covered the procurement ramp for LHC as the scale of the available system was tested and commissioned. The period

from 2010 to 2013 covered the first run, where the computing and storage needs increased at a rate defined by the volume of incoming data to be processed and analyzed.

The resources needed to accommodate the higher trigger rate and event complexity expected in the second run define the requirements for 2015. The three periods roughly average out to a linear increase in CPU power and disk capacity.

The growth curves below do not scale with total integrated luminosity but indicate that more hardware is needed per unit time as trigger rates and event complexity increase. It is not reasonable to expect that the techniques currently used to analyze data in the Energy Frontier will continue to scale indefinitely. The Energy Frontier will need to adopt new techniques and methods moving forward.

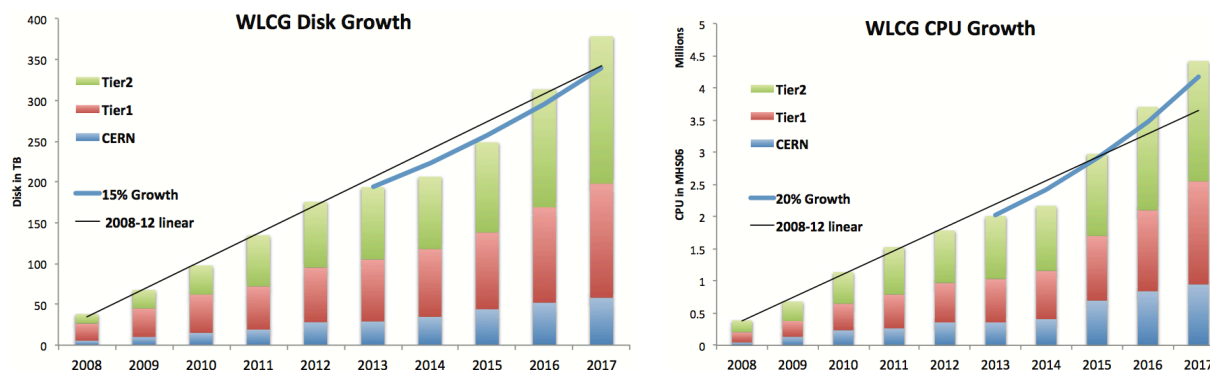


Figure 9-1. The CPU and disk growth through the first 6 years of the LHC program and projections to 2017.

Extrapolating the growth trend out 10 years, LHC computing would have roughly three times the computing expected in 2015, which is lower than that predicted by Moore’s law. LHC would reach nearly 800 PB of disk space by 2023, which again is roughly a factor of three greater than predicted for 2015. These increases could probably be achieved with close to flat budgets. There are potential efficiency improvements and new techniques that will be discussed below.

The luminosity and complexity increase dramatically going to the HL-LHC or other proposed hadron collider programs. If those plans are realized, computing would not be on the curve in Figure 9-1, but would require a significant shift. To estimate the increase in resources needed to move from the LHC to the HL-LHC, it is instructive to examine the transition from the Tevatron to the LHC. Data rates are about a factor of ten larger for the LHC at the end of 2013 than for the Tevatron in 2003. However, during that time the total computing capacity went up by a factor of thirty, while the disk capacity, the local data served, the wide area networking from the host lab, and the inter-site transfers all increased by a factor of 100 to accommodate this step. The step from LHC Run2 to the HL-LHC will similarly require very significant additional resources, and quite possibly a disruptive change in technologies and approaches. We identify two trends that will potentially help with this: the increased use of specialized hardware, and providing and using computing as a service.

9.2.2 Trends to specialized systems and computing as a service

The Energy Frontier will need to evolve to use alternative computing architectures and platforms such as GPUs and other co-processors, low-power “small” cores, etc., as the focus of industry development is moving

away from the classic server CPU. Using GPUs introduces significant diversity to the system, complicates the programming, and changes the approaches used in scientific calculations, but can increase performance by orders of magnitude for specific types of calculations. Co-processors have similar potential improvement gains, but also increase the diversity and complexity of the system, and pose additional programming challenges. Low-power mobile platforms are most interesting when they are combined into massively parallel, specialized system in which a single rack may have the same number of cores as a remote computing center does today. These systems would be used more like a supercomputer and less like a batch farm, which will require new expertise in dealing with these more highly interconnected computers.

Specialized hardware and architectures are likely to be deployed initially in extremely well controlled environments, like trigger farms and other dedicated centers, where the hardware can be controlled and specified. The next phase is likely to be schedulable, dedicated, specialized systems that permit large-scale calculations to achieve a goal similar to making a supercomputer center request. Large-scale clusters of specialized hardware owned by the experiment are likely to come last, and are only likely to come if they can completely replace a class of computing resources and perform a function at a reduced cost and higher efficiency.

The other trend impacting Energy Frontier computing is the move to computing as a service and other “cloud-based” solutions. Currently, commercial offerings, academic resources, and opportunistic resources are all being offered through cloud provisioning techniques. Opportunistic resources are computers with some amount of idle unused capacity that can be accessed by communities for free except for the effort required to make them useful. While commercial solutions are still more expensive than well-used dedicated resources, there is a steady decrease in the pricing.

Energy Frontier computing should expect a transition to more shared and opportunistic resources provided through a variety of interfaces, including cloud interfaces. Effort is needed to allow the community to make effective use of the diverse environments and to perform resource provisioning across dedicated, specialized, contributed, opportunistic, and purchased resources.

9.2.3 Becoming more selective

There are considerable concerns regarding the enormous increase in data produced by the next round of Energy Frontier colliders, to be processed by the offline computing systems. We observe that while the Energy Frontier processing capacity has increased largely as would be expected from Moore’s law and relatively flat budgets, the storage requirements have grown much faster. The larger number of sites and the need for local caches, the increase in trigger rates, and the larger event sizes drive the need for disk-based storage.

For Energy Frontier discovery physics and searches there is a case for storing all potentially interesting events, and then computationally applying various hypotheses to look for new physics. Some searches, and many measurements, may benefit from a new approach where much more of the processing and analysis is done with the initial data collection and only synthesized output is archived. This approach has the potential for preserving physics while reducing the offline processing and storage needs.

We expect a change of mentality, moving away from the approach that higher trigger rates are always better, and that all data from all triggered events need to be kept. As the Energy Frontier trigger rates go up by an order of magnitude, as expected for the LHC, and certainly for the HL-LHC, the experiments should expect to be more selective in what classes of events will be fully reconstructed, and instead develop an approach of on-demand reconstruction, calibration, and analysis.

Simulation and raw-data reconstruction produce derived data that can entirely be reproduced. At the LHC, already many of the intermediate steps are treated as transient data. More of the data analysis steps should be moved into the production chain, only storing the final output, with the understanding that it can be re-derived if required later.

9.2.4 Data management

With the expected increased diversity of computing resources, Energy Frontier computing needs to develop a data management system that can deal with all kinds of resources. In the next decade computing processing for the Energy Frontier will evolve to be less deterministic, with more emphasis on cloud-provisioned resources, opportunistic computing, local computing, and volunteer computing. A data management system is needed to handle the placement of the data and allow the operations team and analysis users to concentrate more on execution of work flows and less on placement and location of data.

Industry has put a focus on delivering content either through Content Delivery Networks (CDNs) or through peer-to-peer systems. The experiment data management systems need to evolve to be more flexible in terms of what computing resources can be used to solve each type of computing problem, in order to make efficient use of the diverse landscape of resources to which the experiment computing modes will have to adapt. The development of a data intensive content delivery network should not be unique to one experiment, and should even be applicable to several scientific domains, but this will require commitment and effort to develop.

Additional details may be found in the full subgroup report [2].

9.3 Computing for the Intensity Frontier

Computing at the Intensity Frontier has many significant challenges. The experiments, projects, and theory all require demanding computing capabilities and technologies. Though not as data-intensive as the LHC experiments, the Intensity Frontier experiments have significant computing requirements for simulation, theory and modeling, beam line and experiment design, triggers and DAQ, online monitoring, event reconstruction and processing, and physics analysis. It is critical for the success of the field that Intensity Frontier computing be up-to-date, with adequate capacity and support, and able to take advantage of the latest developments in computing hardware and software.

9.3.1 Scope

The Intensity Frontier encompasses a large spectrum of physics, including quark flavor physics, charged lepton processes, neutrinos, baryon number violation, new light weakly-coupled particles, and nucleons, nuclei, and atoms. The requirements and resources of quark flavor physics, as in Belle II and LHCb, are similar to those of the Energy Frontier. Intensity Frontier experiments are carried out at a number of laboratories around the world, including JLab, IHEP, KEK, J-PARC, and PSI. Our group looked most intensely at the complex of Intensity Frontier experiments planned for Fermilab and the issues in finding common computing solutions for these experiments. We hope that the insights we present here will also be relevant in a broader, global, context.

The Intensity Frontier has increasingly become a focus of the U.S.-based particle physics program. Many of the experiments are designed to use very intense particle beams to measure rare processes. There is a large number of experiments, a range of scales in data output and throughput, and a range in the number of experimenters. This situation can potentially lead to fragmentation, duplication of effort, lack of access to computing advances, and higher cost than necessary to support these experiments. Furthermore there is significant overlap of human resources among experiments, making any significant divergence of software, frameworks and tools between them particularly inefficient. A broad range of experiments leads to a broad range of needs in terms of number of experimenters and the sizes of the data sets. Experiments' computing requirements range from high-intensity, real-time processing with small data sets to stored large data sets that are themselves equivalent in size to the previous generation of collider experiments.

Over the last few years there has been a significant effort by the Intensity Frontier experiments at Fermilab to join forces in using a more homogeneous set of software packages, frameworks and tools to access infrastructure resources. This trend has reduced fragmentation and led to more efficient use of resources. We recommend expanding this trend to the broader Intensity Frontier community, adapted to the needs of each collaboration.

9.3.2 Survey

For this report a qualitative survey was conducted of the current and near-term future experiments in the Intensity Frontier in order to understand their computing needs and also the expected evolution of these needs. Computing liaisons and representatives for the LBNE, MicroBooNE, MINER ν A, MINOS+, Muon $g-2$, NO ν A, SeaQuest, Daya Bay, IceCube, SNO+, Super-Kamiokande and T2K collaborations all responded to the survey. This does not cover all experiments in all areas, but we consider it a representative survey of the Intensity Frontier field.

The responses and conclusions to the survey can be grouped into five categories as describe in detail below.

9.3.2.1 Support for software packages

There is significant benefit to encouraging collaborative efforts among experiments. An example is the LArSoft common simulation, reconstruction, and analysis toolkit, used by experiments developing simulations and reconstructions for liquid argon time projection chambers. LArSoft is a collaborative effort that makes better use of development and maintenance of resources, with maintenance support by Fermilab. In addition to software packages for simulation and reconstruction, there are several other computing tools that are widely used in the field that should be maintained. These tools provide infrastructure access for code management, data management, grid access, electronic log books, and document management.

9.3.2.2 Support for software frameworks

Efforts for common frameworks can have a significant impact, in terms of optimizing development and support, and in minimizing overhead on the training of experimenters. The Fermilab-based Intensity Frontier experiments (Muon $g-2$, Mu2e, NO ν A, ArgoNeuT, LArIAT, MicroBooNE, LBNE) have converged on ART as a framework for job control, I/O operations, and data provenance. Resources for the ART framework thus address needs that exist across the experiments, such as more accessible parallelization of each experiment's code. In fact, the primary limitation listed by users of ART was the inability to

parallelize jobs at the level of individual algorithms. The ability to do so will become more critical as the numbers of channels in Intensity Frontier experiments continue to increase and the separation of signal from background becomes more difficult due to the rare nature of the processes being examined. This ability will also allow Intensity Frontier experiments to take advantage of the design of modern computers containing multiple cores. Other experiments use LHC-derived frameworks such as Gaudi, or homegrown frameworks like MINOS+, IceTray, and RAT. The level of support for development and maintenance of such frameworks varies. These experiments would also benefit from more access to parallelization and professional computing support.

The survey identified the need for making consultants available to help with software development. All experiments indicated that they would like to put more computing professional effort toward parallelization of code, establishing batch submission to off-site computing, establishing best practices for writing software, software development, and optimizing use of Geant4. Such expertise is in high demand within the Intensity Frontier community. Existing expertise at Fermilab and elsewhere could fulfill this need of the wider Intensity Frontier community if this was promoted and properly funded.

9.3.2.3 Access to dedicated and shared resources

In general, demands for computing resources of Intensity Frontier experiments are modest compared to those of the Energy Frontier experiments. However, those needs are not insignificant, and all experiments require at least 1,000 dedicated batch slots to ensure timely physics results. NO ν A alone requires 4.8 million CPU hours per year for its simulation needs, LBNE expects to need several PB of storage space each year during operation, and even smaller-scale experiments like MINER ν A and MicroBooNE expect to need PB-sized storage. The full report shows a table of current and projected CPU needs. Each experiment has periods of peak demand that follow cycles which are strongly correlated with major conference cycles. It is important to take the peak demand per experiment into account when planning for resource needs. Those peaks typically are much larger than the planned steady state usage. To meet those demands for turnaround during peak usage, each experiment should have access to additional resources on which it may run opportunistically.

The survey showed that support of the Fermilab-based experiments in terms of storage and CPU is rated as excellent. There are still issues, mostly in efficient data handling and script optimization, that require additional professional support. Professional support is required to enable seamless use of resources through grid job submission, on- or off-site. For Fermilab-based experiments, university and other national lab resources are used in the production of Monte Carlo files. A common protocol to access these resources such as OSG is expected in the future.

Non-U.S. experiments with U.S. participation enjoy significantly lower levels of support. OSG provides some support of opportunistic use of grid resources. Without their own domestic computing resources these experimenters need to rely either on resources in other countries, with low priority, or on university-based resources that are shared among a broad pool of university users from multiple disciplines. In contrast, non-U.S. experimenters from T2K run intensively and very successfully on grid resources in Europe and Canada. In order to be competitive with analysis of data and simulation, the U.S. researchers must have access to dedicated resources that can be shared with other Intensity Frontier experiments. It was widely noted that the lack of dedicated U.S. resources has a detrimental impact on the science.

Networking requirements are determined by the demand that data move easily between storage systems, be accessible for data acquisition, reconstruction, simulation and analysis, as well as be able to take advantage of distributed computing, either as part of the grid or cloud. Networking must not be a barrier to making effective use of distributed computing. With Intensity Frontier experiments becoming larger and more international, network requirements will grow.

9.3.2.4 Access to data handling and storage

Fermilab-based experiments have their primary data copies stored at Fermilab. The infrastructure there handles active storage as well as archiving of data. The SAM system designed and maintained at Fermilab was noted as the preferred data distribution system for these experiments. Heavy I/O for analysis of large numbers of smaller-sized events is an issue for systems like BlueArc. Fermilab should continue to receive support from the DOE to ensure proper archiving of data. Other experiments indicated using grid protocols for data storage.

All respondents indicated the need for data handling systems that seamlessly integrate distribution of files across the network from multiple locations, to enable experiments to make optimal use of storage resources at national labs and universities. The need for such a system is acutely felt by experiments that are not based at Fermilab. One possible solution to this problem could resemble the tiered computing structure used by the LHC experiments, with all Intensity Frontier experiments making use of that structure.

9.3.2.5 Overall computing model and its evolution

The computing models used in various Intensity Frontier experiments have a lot in common, despite large differences in the type of data being analyzed, the scale of processing, or the specific workflows followed. The general model is that of a traditional event-driven analysis and Monte Carlo simulation using centralized data stores that are distributed to independent analysis jobs running in parallel on grid computing clusters. Currently, there is a remarkable overlap in the infrastructure used by experiments. For large computing facilities such as Fermilab, it would be useful to design a set of scalable solutions corresponding to each of these patterns, with associated toolkits that would allow access and monitoring. Providing resources for an experiment or changing a computing model would then correspond to adjusting the scales in the appropriate processing units.

Computing should be made transparent to the user, such that non-experts can perform any reasonable portion of the data handling and simulation. All experiments would like to see computing become more distributed across sites, but only in very large units where it can be efficiently maintained. Users without a home lab or large institution require equal access to dedicated resources. We need continuous improvements in reducing the barrier of entry for new users, to make the systems easier to use, and to add facilities that help prevent the users from making mistakes.

The evolution of the computing model follows several lines including taking advantage of new computing paradigms, like clouds; different cache schemes; and GPU and multicore processing. There is a concern that as the number of cores in CPUs increases, RAM capacity and memory bandwidth will not keep pace, causing the single-threaded batch processing model to be progressively less efficient on future systems unless special care is taken to design clusters with this use case in mind. There is currently no significant use of multi-threading, since the main bottlenecks are Geant4 (single-threaded) and file I/O. Geant4's multithreading addition might have a very significant impact across the field. There is also a possibility of parallelization at the level of the ART framework. Greater availability of multi-core/GPU hardware in grid nodes would provide motivation for upgrading code to use it. For example currently we can only run GPU-accelerated code on local, custom-built systems.

9.3.3 Summary

To summarize, the computing needs of the Intensity Frontier experiments should be viewed collectively. When combined, these experiments require the resources and support similar to a single Energy Frontier experiment. The support of these experiments directly impacts the quality of results and the efficiency with which those results can be obtained. There is significant support already for Intensity Frontier experiments that are based at Fermilab and the support requirements are expected to increase as the generation of experiments currently under construction begin to take data. The support of Intensity Frontier experiments that are not based at Fermilab but still have significant U.S. collaboration, such as T2K, needs to be improved. Specifically, there should be an investment in infrastructure and professional support to serve these experiments.

Transparent access to data and computing hardware resources is required for Intensity Frontier experiments. Users must have a simple interface with which to request data sets that then determines the stored location of those data and returns the data quickly to the user. Similarly, there should be a standardized grid submission tool that determines the optimal location for running jobs without the user having to specify those locations.

The Intensity Frontier benefits significantly from the ability to share common frameworks and tools, such as ART, GENIE, NuSoft and LArSoft. The support of these efforts must be continued and increased as new experiments come on line and more users are added to current experiments. Similarly, the common tools used across all frontiers, such as ROOT and Geant4, must be supported and continuously improved. Computing professionals are in demand as support for key software frameworks, software packages, scripting access to grid resources and data handling. Fermilab is a natural center for Intensity Frontier support in these areas given the existing expertise and large number of Intensity Frontier experiments already on site.

There are efforts and problems that are shared across frontiers. Thus, significant investments in ROOT and Geant4 optimizations, HPC for particle physics, transparent OSG access, and open data solutions would have a high payoff.

Additional details may be found in the full subgroup report [3].

9.4 Computing for the Cosmic Frontier

The Cosmic Frontier lies at the interface between particle physics, cosmology, and astrophysics. Experimental and observational activities in the Cosmic Frontier cover laboratory experiments as well as multi-band observations of the quiescent and transient sky. Direct dark matter search experiments, laboratory tests of gravity theories, and accelerator dark matter searches fall into the first class. Investigations of dark energy, indirect dark matter detection, and studies of primordial fluctuations fall into the second class; essentially the entire range of available frequency bands is exploited, from the radio to TeV energies. Relevant theoretical research also casts a very wide net — from quantum gravity to the astrophysics of galaxy formation.

9.4.1 Experimental facilities

The size and complexity of Cosmic Frontier experiments is also diverse, ranging from the tabletop to large cosmological surveys, simultaneously covering a number of precision measurements and discovery-oriented

searches. A defining characteristic of the Cosmic Frontier is a trend towards ever larger and more complex observational campaigns, with over a thousand researchers collaborating on sky surveys, making them roughly the size of a large Energy Frontier experiment. Cross-correlating different survey observations can extract more information, help to eliminate degeneracies, and reduce systematic errors. These factors are among the major drivers for the computational and data requirements that we consider below.

The dramatic increase in data from Cosmic Frontier experiments over the last decade has led to fundamental breakthroughs in our knowledge of the “Dark Universe” and physics at very high energies. Driven by technological advances, current experiments generate in excess of a petabyte of total data per year. The growth in data will be continued over the coming decade by large-format CCD cameras to measure the growth of structure from weak gravitational lensing, wide-field spectroscopic facilities to map the clustering of galaxies, increases in the size of direct dark matter detectors, massive radio surveys, and ground and space-based Cosmic Microwave Background (CMB) experiments. The mass of data will exceed 100 PB; in subsequent decades the development of radio experiments and energy resolving detectors will result in an increase in data streaming rates to greater than 15 GB/s.

9.4.2 Simulations

The intrinsically observational nature of much of Cosmic Frontier science implies a great reliance on simulation and modeling. Not only must simulations provide robust predictions for observations, they are also essential in planning and optimizing surveys, and in estimating errors, especially in the nonlinear domains of structure formation. Synthetic sky catalogs play important roles in testing and optimizing data management and analysis. The scale of the required simulations varies from medium-scale campaigns for determining covariance matrices to state-of-the-art simulations of large-volume surveys, or, at the opposite extreme, small-volume investigations of dark matter annihilation signals from dwarf galaxies.

For optical surveys, the chain begins with a large cosmological simulation into which galaxies and quasars (along with their individual properties) are placed using semi-analytic or halo-based models. A synthetic sky is then created by adding realistic object images and colors and by including the local solar and galactic environment. Propagation of this sky through the atmosphere, the telescope optics, detector electronics, and the data management and analysis systems constitutes an end-to-end simulation of the survey. A sufficiently detailed simulation of this type can serve a large number of purposes such as identifying possible sources of systematic errors and investigating strategies for correcting them, or for optimizing survey design (in area, depth, and cadence). The effects of systematic errors on the analysis of the data can also be investigated. Because of the very low level of statistical errors in current and next-generation precision cosmology experiments, and the precision with which deviations from Λ CDM are to be measured, this is an absolutely essential task.

Facilities for carrying out the required simulations include large-scale resources at DOE and NSF supercomputing centers, augmented by local clusters. Data-intensive computing platforms are also needed to deal with the enormous data streams generated by cosmological simulations. The data throughput can easily exceed that of observations; data storage, archiving, and analysis requirements (often in concert with observational data) are just as demanding as for observational data sets. Although there are significant challenges in fully exploiting future supercomputing hardware, available resources should satisfy performance requirements, currently at the scale of ~ 10 PFlops. These requirements are expected to cross into the exascale regime after 2020. The data-related issues are more serious and will need changes in the current large-scale computing model. Successful implementation of the recently suggested Virtual Data Facility (VDF) capability at computing centers would go a long way towards addressing these issues for Cosmic Frontier simulations.

Simulation requirements are projected to increase steeply. Current allocations are estimated to be of the order of 200M compute hours/year, with associated storage in the few PB range, and a shared data volume of the order of 100 TB. Data management standards and software infrastructure vary widely across research teams. The projected requirements for 2020 are an order of magnitude increase in data rates (to 10-100 GB/s), a similar increase in peak supercomputer performance (200 PFlops), and the ability to store and analyze data sets in the 100 PB class. It is difficult to make precise estimates for 2030, as hardware projections are hazy, but the science requirements based on having complete data sets from missions such as LSST, Euclid, and large radio surveys would argue for at least another order of magnitude increase across the board.

9.4.3 Computational resources and architectures

Today's architectures for data analysis and simulations include supercomputers, that are suitable for massively parallel computations where the number of cycles per byte of data is huge. These possess a large distributed memory but a relatively small amount of on-line storage. Database servers occupy the opposite range of the spectrum, with a very large amount of fast storage, but not much processing power on top of the data. For most scientific analyses, the required architecture lies somewhere in between these two: it must have a large sequential I/O speed to petabytes of data, and also perform very intense parallel computations.

The use of computational resources will need to grow to match the associated data rates for the processing and analysis of observational data and for simulated astrophysical and cosmological processes. Most of the data processing pipelines use linear time algorithms, where the amount of processing is roughly proportional to the amount of data collected by the instruments. Exceptions to this linear scaling arise, however, with many of the algorithms that are applied to the accumulated data including optimization and clustering methods whose computational requirements grow as a quadratic function of the data or greater.

Most pipelines can be characterized by the number of cycles needed to process a byte of data. Typical numbers in astrophysics today range from a few thousand to 100K cycles, so that processing a canonical 100 PB data set requires 10^{22} cycles, or about a billion CPU hours. One particular characteristic of this processing is that it will require a reasonable, but not excessive, sequential I/O rate to data storage disks, typically less than a GB/s per processing compute node.

Much of this processing is massively parallel, and thus will execute very well on SIMD (Single Instruction, Multiple Data) architectures. Emerging many-core platforms will therefore have a huge impact on the efficiency of data processing pipelines. While these platforms are harder to code for, pipeline codes will be based on well-designed core libraries, where it will be cost-efficient to spend resources to optimize their parallel execution, thus substantially decreasing the hardware investment.

The projected data volumes for archiving of observational data are not particularly large compared to commercial data sets (with the possible exception of the Square Kilometer Array). Given that the eventual data volumes will probably exceed a few exabytes, the analyses must be co-located with the data.

The most likely high-level architecture for scientific analyses will be a hierarchy of tiers, in some ways analogous to the LHC computing model, where the (top) Tier 0 data is a complete capture of all raw data. Derived and value-added data products are moved and analyzed further at lower tiers of the hierarchy, which are not necessarily co-located with the Tier 0 data centers.

The archives will have to be based upon intelligent services, where heavy indexing can be used to locate and filter subsets of the data. There is a huge growth in the diversity of such "Big Data Analytics" frameworks, ranging from petascale databases to an array of simpler solutions. Over the next five years a few clear

winners will emerge, allowing the Cosmic Frontier community to leverage the best solutions. A high-speed, reliable, and inexpensive networking infrastructure connecting the instruments and all the sites involved in the archiving will be crucial to the success of the entire enterprise.

Fast graph processing will become increasingly important to analyze large and complex simulations and track complex spatio-temporal connections among objects detected in multi-band time-domain surveys. To efficiently execute algorithms that require large matrices and graphs, it is likely that large (multiple TB) memory (RAM) will be melded with multiprocessors to minimize communication overhead. Also, new storage technologies with fast random access (SSD, memory bus flash, phase change memory, non-volatile RAM) will play a crucial role in the storage hierarchy.

9.4.4 Data access and analysis

Large-scale data sets, arising from both simulations and experiments, present different analysis tasks requiring a variety of data access patterns. These can be subdivided into three broad categories: localized data processing, global data processing, and rendering graphics.

Some of the individual data accesses will be very small and localized, for example, interrogating the properties of individual halos or galaxies, and recomputing their observational properties. These operations typically return data in small blocks, require a fast random access, a high I/O performance, and are greatly aided by good indexing. At the same time there will be substantial computation needed on top of the small data objects. These accesses can therefore benefit from a good underlying database system with enhanced computational capabilities. Going beyond the hardware requirements, this is an area where the clever use of data structures will have an enormous impact on the system performance, and related algorithmic techniques will be explored extensively. The challenge here is that the small data accesses will be executed billions of times, suggesting a parallel, sharded database cluster with a random access capability of tens of millions of IOPS and a sequential data speed of several hundred GB/s, with an unusually high computing capability inside the servers themselves.

At the other end of the spectrum are analyses that need to access a large fraction of all collected data, such as computing an FFT of a scalar field over the entire volume, or computing correlation functions of various orders, over different subclasses of objects. These require very fast streaming access to data, algorithms that can compute the necessary statistics over (possibly multiple) streams, and multiprocessors that can handle these highly parallelizable stream computations efficiently. Here the requirements would be a streaming data rate in excess of 500 GB/s between the data store and the processing, and a peak processing capability of several PFlops. These patterns map best onto traditional HPC systems, with the caveat of the extreme data streaming requirements.

The third type of access pattern is related to rendering computer graphics. These tasks will generate various maps and projections, touching a lot of data, and typically generating two-dimensional images. Such tasks include computing maps of dark matter annihilation in trillion-particle simulations, ray-tracing to compute gravitational lensing signatures over a large simulation, and generating ray-traced simulated images for future telescopes. These ray-traced images are based on simulations and detailed telescope and atmospheric models. As many of these tasks are closely related to computer graphics, mapping to GPU hardware will be very important, as this approach can yield performance gains of well over an order of magnitude.

Dealing with each of these access patterns demands substantial investments in hardware and software development. To build an efficient streaming engine, all hardware and software bottlenecks must be eliminated, since a single choke point can seriously degrade the performance of the whole system. In terms of algorithms,

Experimental Data	2013	2020	2030+
Storage	1 PB	6 PB	100–1500 PB
Cores	10^3	70K	300+K
CPU hours	3×10^6 hrs	2×10^8 hrs	$\sim 10^9$ hrs
Simulations	2013	2020	2030+
Storage	1–10 PB	10–100 PB	> 100PB – 1EB
Cores	0.1–1M	10–100M	> 1G
CPU hours	200M	>20G	> 100G

Table 9-1. *Computing requirements for Cosmic Frontier science over the next 10–20 years.*

many traditional RAM-resident algorithms must be recast into streaming versions. A rethink of statistical algorithm design is needed, and computations (and computability) should be explicitly included into the cost tradeoffs.

Table 9-1 summarizes current and future computational needs for the Cosmic Frontier.

9.4.5 Development and support of a computational community

The need for better programming models and better high-level abstractions is evident. In a complex, massively parallel system it will become increasingly difficult to write code explicitly instructing the hardware. Therefore, there is a need to explore and embrace new declarative programming models where the explicit execution of the code is transparent to the user. At a higher level, there is a pressing need for the development of a sustainable software effort that can provide a baseline of support to multiple experiments, with experiment-specific extensions being built on top of such a capability. This will require a community effort to develop and implement new algorithms, programming models, workflow tools, as well as standards for verification, validation, and code testing. A coherent plan for long-term support to maintain and further develop the resulting software base will have to be put in place.

Directly analogous to building a community-supported software base for Cosmic Frontier experiments, there is a related need for bringing together larger collaborations in the area of simulations. The lattice QCD community has shown what is possible in this direction by working together in a large national collaboration. Such efforts are now beginning within the Cosmic Frontier and will hopefully come to fruition in the near term.

While much of the science in the Cosmic Frontier is undertaken by small groups of physicists, the collaborations themselves have grown to hundreds and sometimes thousands of members. Many of the techniques utilized by these collaborations are common to multiple Cosmic Frontier experiments. Most experiments have, however, developed their analysis and processing software independently of other programs. This can lead to duplication of effort, software that is tailored only to meet a specific need, non-scalable approaches, and software that is difficult to sustain beyond the lifetime of an individual experiment. To make computing developments more robust, a sustainable software initiative is highly desirable. A substantial community must actively develop and deploy the tools created within such a program.

Additional details may be found in the full subgroup report [4].

9.5 Computing for accelerator science

Particle accelerators are critical to scientific discovery, both nationally and worldwide. The development and optimization of accelerators are essential for advancing our understanding of the fundamental properties of matter, energy, space, and time. Modeling of accelerator components and simulation of beam dynamics are necessary for understanding and optimizing the performance of existing accelerators, for optimizing the design and cost-effectiveness of future accelerators, and for discovering and developing new acceleration techniques and technologies. In addition, the combination of fast and sophisticated analytics with large-scale simulations will be very important to obtain the control-room feedback capabilities required by Intensity Frontier accelerators.

9.5.1 Simulations

The requirements for high-fidelity computer simulations of accelerator systems and accelerator components are driven by the need to develop and optimize new accelerator concepts and design machines based on these concepts, and maximize the performance of existing accelerators based on existing concepts and technologies. For Energy Frontier applications this means supporting the development of new techniques that will increase the accelerating gradients so future machines are more compact and less costly. The options considered in our study include acceleration in plasma structures, using either laser- or beam-driven wakefields, dielectric structures driven by lasers or RF (GHz), the development of new lepton collider designs such as muon colliders and two-beam acceleration, and optimization of existing technologies such as superconducting rf cavities. For Intensity Frontier applications, simulations are essential in developing and optimizing integrated designs in order to minimize beam losses. Such losses are caused by instabilities generated either by beam self-interactions, or by interactions of the beam with the accelerator structures or other media present in the beam pipe. The simulations considered in our study focused on designing mitigation techniques and determining optimal operational parameters. Hadron colliders at the Energy Frontier have similar requirements, although self-interactions are not important, while beam-beam interactions (which are similarly computationally intensive) have to be included. Simulations of accelerators for both the Energy and the Intensity frontiers are computationally demanding because they often involve a wide range of time and length scales and a wide spectrum of interoperating physics components. For example, simulations of high-intensity proton drivers which are a few km long and operate using EM wavelengths of 10-100 m, with machine components of the order of 1-10 m, must resolve particle bunches of the order of a few mm. Similarly, laser-plasma accelerators (LPA) of the order of 1 m in length must resolve laser wavelength and electron bunch size of the order of 1 μm .

Most software for accelerator science are already parallelized and scalable to more than ten thousand cores on high performance computers. Modeling physical fields using various approximations requires different numerical methods. For example, electrostatic models utilize multigrid, adaptive mesh refinement (AMR) multigrid or spectral methods. On the other hand, fully electrodynamic models use a variety of finite difference and finite element methods. Quasi-static models use spectral methods and particle-in-cell, among other methods. There are ongoing R&D efforts to port these numerical models to new architectures such as GPU-based machines.

Progress in accelerator science requires efficient use of HPC. Each simulation step requires communication among thousands to millions of processors, so a fast interconnect is essential. The major modeling applications from both Energy and Intensity Frontiers are shown in Table 9-2. The estimate of needs is based on the current performance of our codes on the Hopper supercomputer at the National Energy Research

Scientific Computing Center (NERSC). The Energy Frontier has much greater data storage and networking needs than accelerator science, so we do not detail our needs in this area, with one exception. We assume that we can make use of the systems required by the Energy Frontier.

Our user community (accelerator scientists operating machines or performing R&D) and our own community (computational accelerator physicists and theorists) identified the need for programmatic coordination and support of code development and computing R&D to create a sustainable computational accelerator science program as an essential requirement for the future. Porting of our algorithms and workflows to new computing architectures (light-weight CPU plus accelerator) and the R&D necessary to create and evaluate new algorithms is an important component of such coordinated program (including close interactions with HPC centers to utilize test-beds of new architectures). An example of such programmatic support of code development today is the SciDAC program, although it is desirable that in the future there is more focus on the specific physics solutions needed to further develop our tools. Another common theme is the need for supporting the development of community libraries and tools, including standardized user interfaces, geometry and data descriptions, I/O and analysis tools. Because our applications require true HPC capabilities, it is important to develop generic workflow tools that perform in an HPC environment as well as on local workstations and clusters. Also important is the development and integration to our toolkit of parameter optimization libraries, that will be available across all HPC platforms. The development of such an environment will enable experimentalists and machine operators to take advantage of these computational capabilities and will be essential in training students and young researchers to help develop the new accelerator concepts and technologies that will move the field of particle accelerators forward. In addition, it is essential for such a program to support and coordinate physics model validation and verification, ultimately with comparisons to experimental data of well controlled experiments in test facilities or operating accelerators.

9.5.2 Feedback and control systems

Intensity Frontier machines of the future require control room feedback capabilities because of the beam-loss implications. This capability is also important to Energy Frontier test facilities for guiding and interpreting experiments. The utilization of new computing technologies could make delivering such a fast turnaround possible. The challenge on both the performance of the computational tools and the availability of computing resources becomes even more daunting if we consider the need to analyze the simulated data in order to extract useful information. The analysis of the simulated data ($\sim$ TB) has to produce the same quantities observed by the beam diagnostic detectors. Note that this is a more general requirement, because it is necessary for accurate comparisons of simulated and observed data independently of the ability to do that in “almost real-time” in the control room. Analysis workflow and synthetic diagnostic tools similar to those used by Energy Frontier experiments have to be developed to properly model the detector response and maintain and correlate the information of the simulated physics variables to those “smeared” by modeling the beam diagnostics. Such analysis tools have to be HPC capable, to allow for the fast turnaround necessary for control room feedback, and they will also require new models and algorithms. Finally, this is probably the only application in accelerator modeling that data transfer speed and data availability, storage, and cataloging have similar requirements to those of a DAQ system for a particle physics experiment.

9.5.3 Multi-physics modeling

Different applications have different specific requirements for the development of new or more efficient physics or computational models, but all of them require integrated multi-scale, multi-physics modeling. Although the physics models implemented in today's simulation tools utilize sophisticated HPC infrastructure, because of the size of the computation, often "single physics" or "few physics" models are included in a run. The different physics effects are studied separately, as if they were independent. This is not the case in general, affecting our ability to find optimal design and operational parameters. More efforts are needed to integrate multiple physical effects for more accurate simulations, with the ability to utilize massive computing resources beyond the capabilities of today. In the Energy Frontier, where single components of the accelerator are simulated separately, end-to-end simulations and integration between components are needed. For example, plasma-based accelerator simulations must be advanced from modeling current experiments at the 10 GeV and 0.1 micron emittance level to future collider concepts involving hundreds of stages at the 0.01 micron emittance level. This also requires integration of additional physical models such as scattering and radiation. For high-intensity circular proton machines, a large number of macro-particles ($\sim 10^9$) must be used in the simulations in order to accurately represent percent-level losses. In addition, detailed models of important components relevant to all frontier applications are missing from our simulation toolkits because of prohibitive computational cost and complexity. (For example, target modeling, including gas dynamics, MHD, and heat loading/dissipation, must be integrated to our toolkit.) Deployment of such capabilities will enable end-to-end simulations to validate designs based on new concepts and end-to-end operational parameter optimization of accelerators about to be commissioned. It should be noted that in some cases end-to-end modeling also involves integration of physics and numerical models developed for different applications (for example, for a plasma-based accelerator consisting of many plasma stages, both plasma physics tools and conventional beam-dynamics tools have to be used in the model to produce an optimal solution).

9.5.4 Design optimization

Intensity Frontier accelerator needs are dominated by the need to control and mitigate beam losses. This demands both careful design of the accelerator structures and accurate modeling of beam-halo (and its creation mechanisms), the accelerator geometry (apertures), and the positions and field strengths of each accelerator element. This implies tracking many bunches of $\sim 10^9$ macroparticles per bunch for $\sim 10^5$ turns including self-fields, impedance effects, and bunch-to-bunch interactions. Finding the optimal parameters of operation will require end-to-end optimization runs, while developing mitigation techniques possibly requires the implementation of new physics in the HPC environment, to model the new components (for example, electron lenses for space-charge compensation).

Energy Frontier accelerator needs are dominated by the need to develop end-to-end simulations to characterize and optimize beam stability, emittance, and transport efficiency. New accelerator concepts have many specific new physics model capability needs. It will be necessary to develop electromagnetic plasma and beam methods capable of resolving 0.1 km-scale propagation of 10 nm scale emittance bunches and laser drivers, and the corresponding bunch conditioning and focusing. There are also needs common to the Energy and Intensity Frontiers: for example, radiation and scattering, which is relevant to muon collider, plasma and gamma-gamma options, and modeling of targets. Developing these new models demands R&D both on the physics and the numerical algorithms. Because of the physics requirements imposed by some of the new concepts considered, minimization of numerical noise is very important in these applications. This constraint has a direct impact on the choice of numerical techniques for different physics implementations. Plasma

Computation (Mhours)	15000
Typical cores for production runs	50000
Maximum cores for production runs	5M
Data read and written per run (TB)	1000
Minimum I/O bandwidth	100 GB/sec
Memory requirement per core	0.2 GB
Shared file-system space (on site)	6 PB
Shared file-system space (distributed, cataloged)	60 PB

Table 9-2. *Computing needs for accelerator science in 10 years.*

accelerators additionally require computation of these effects with accurate plasma and laser dynamics, often requiring unique algorithms.

Additional details may be found in the full subgroup report [5].

9.6 Computing for lattice field theory

One of the foremost goals of particle physics is to test the Standard Model and to search for indications of new physics beyond. In many cases, interpretation of the experimental measurements requires a quantitative understanding of the nonperturbative dynamics of the quarks and gluons in the underlying process. Lattice gauge theory provides the only known method for *ab initio* quantum chromodynamics (QCD) calculations with controlled uncertainties, by casting the fundamental equations of QCD into a form amenable to high-performance computing. Thus, facilities for numerical lattice gauge theory are an essential theoretical adjunct to the experimental particle physics program. Lattice QCD calculations now play an essential role in the search for new physics at the Intensity Frontier. They provide accurate results for many of the hadronic matrix elements needed to realize the potential of present experiments probing the physics of flavor. The methodology has been validated by comparison with a broad array of measured quantities, several of which had not been well measured by experiment at the time of the first precise lattice calculations. In the coming decades, lattice QCD will play an expanded role in the search for new physics at both the Energy and Intensity Frontiers.

The U.S. Lattice QCD Collaboration (USQCD), which consists of most theoretical physicists in the country involved in the numerical studies of QCD and beyond-the-Standard-Model theories, represents the lattice gauge community. Their efforts have been supported in an essential way by hardware and software funding provided by the High Energy and Nuclear Physics Program Offices of the Department of Energy. The USQCD Collaboration's current hardware project ends in FY2014, and the collaboration has applied for a five-year project extension, "LQCD-ext II." The Collaboration's ongoing software development and maintenance activities are supported by SciDAC-3 grants.

The report of the lattice field theory working group [6] summarizes the scientific goals of the U.S. lattice gauge theory community, presents the current and future computing needs and plans, and argues that continued support of the U.S. (and worldwide) lattice-QCD effort is essential to fully capitalize on the enormous investment in the particle physics experimental program.

9.6.1 Lattice field theory scientific motivation

Precision measurements at the Energy and Intensity Frontiers probe quantum-mechanical loop effects, and are therefore sensitive to physics at higher energy scales than those directly accessible at the LHC. Contributions from new heavy particles may be observable as deviations of the measurements from Standard Model expectations, provided both the experimental measurements and theoretical predictions are sufficiently precise. The scientific impact of many future experimental measurements therefore hinges on reliable Standard-Model predictions on the same time scale as the experiments and with commensurate uncertainties.

For many quantities, the comparison between the measurements and Standard-Model predictions are currently limited by theoretical uncertainties from nonperturbative hadronic matrix elements or fundamental QCD parameters that can only be computed numerically with lattice QCD. The USQCD Collaboration has laid out an ambitious vision for future lattice calculations matched to the experimental priorities of the planned experimental particle physics program over the next decade in the white papers “Lattice QCD at the Intensity Frontier” and “Lattice Gauge Theories at the Energy Frontier” [7, 8]. These detailed documents present a concrete five-year plan for both the collaboration’s foremost scientific goals and the theoretical, algorithmic, and computational strategies for achieving them. The highest scientific priorities include the following:

- Improving calculations of hadronic matrix elements involving quark-flavor-changing transitions which are needed to interpret rare kaon decay experiments
- Improving calculations of the quark masses m_c and m_b and the strong coupling α_s which contribute significant parametric uncertainties to Higgs branching fractions
- Calculating the nucleon axial form factor which is needed to improve determinations of neutrino-nucleon cross sections for experiments such as LBNE
- Calculating the nucleon light- and strange-quark contents which are needed to make model predictions for the $\mu \rightarrow e$ conversion rate at the Mu2e experiment and to interpret dark-matter detection experiments in which the dark-matter particle scatters off a nucleus
- Calculating the hadronic light-by-light contribution to muon $g - 2$, which is needed to solidify and improve the Standard-Model prediction and interpret the upcoming measurement as a search for new physics

Lattice field-theory calculations will also increasingly contribute to collider experiments at the LHC 14-TeV run by providing quantitative nonperturbative input for Higgs and other new-physics model building.

9.6.2 Lattice field theory computing resources

Substantial high-performance computing resources are needed to calculate hadron masses and interactions with sufficient precision to test the Standard Model against emerging experimental measurements. Lattice gauge theory simulations require parallel programming techniques, with the calculations running cooperatively across hundreds to many thousands of processors or processor cores. The simulations must be run on hardware suitable for massively parallel computations. Although the simulations are floating point intensive, on all current high-performance computing systems throughput is limited by the rate that operands can be

Year	ANL LCF (BG/P + BG/Q core-hours)	ORNL LCF (Cray core-hours)	Dedicated Capacity Hardware (core-hours)
2010	187M	53.6M	125M
2011	182M	49.8M	205M
2012	143M	77.9M	330M
2013	290M (allocated)	140M (allocated)	971M (planned)

Table 9-3. Utilized core-hours of leadership-class facility (LCF) and dedicated capacity hardware for lattice-QCD simulations. The conversion factors for lattice-QCD sustained Tflop/sec-years, assuming 8000 hours per year, is 1 Tflop/sec-year = 3.0M core-hour on BlueGene/Q hardware, and 1 Tflop/sec-year = 6.53M core-hour on BlueGene/P and Cray hardware. Only USQCD-Collaboration resources are shown. The drop in ANL LCF utilized capacity in 2012 occurred because fewer opportunistic core-hours (“zero-priority queues”) were available due to increased demand by other facility users.

supplied to the floating point execution units, either because of memory bandwidth limitations or by the latency and bandwidth of interprocessor communications. Interprocessor communications of data rely on message-passing algorithms, typically implemented using an MPI [9] library.

At present, lattice theorists in the United States run these codes on a variety of hardware. The first type is commodity clusters based on Intel or AMD x86 processors and Infiniband networks, which have hundreds of nodes and thousands of cores. A second type is accelerated commodity clusters, similar to the standard clusters but with general purpose graphics processing units (GPUs) or Intel Many Integrated Core (MIC) accelerators installed in each server; these clusters have fewer nodes but typically hundreds of accelerators. A third type is very-large-scale Cray supercomputers, consisting of thousands of AMD x86 processors with a proprietary network, with the newest models also containing thousands of GPUs. Finally, lattice theorists use very large scale IBM BlueGene supercomputers, consisting of hundreds of thousands of PowerPC cores interconnected on a proprietary network.

Access to high-performance computing at both supercomputer (*capability*) and cluster (*capacity*) scales is essential for the lattice field theory community. A typical lattice-QCD analysis campaign involves a mix of problem sizes. The largest-scale computations are the generation of ensembles of gauge fields, but at least as much integrated high-performance computing capacity is required for the small- to large-scale parallel computations (“analysis jobs”) to calculate different physical observables on these ensembles. In the U.S., the lattice community utilizes national leadership-class supercomputing centers for the ensemble generation and for the largest analysis jobs, as well as dedicated hardware purchased and operated by USQCD for the much larger volume of small-to medium-scale analysis jobs.

Table 9-3 lists the leadership-class facility capability and dedicated capacity resources utilized for lattice-QCD simulations since 2010 by the USQCD collaboration. The capability resources are broken out showing both the ANL and ORNL leadership class facilities; the capacity resources include all usage on the DOE HEP- and NP-funded hardware at Fermilab, Jefferson Lab, and BNL. Subgroups within USQCD also use the DOE’s National Energy Research Scientific Computing Center (NERSC), centers supported by the NSF’s Extreme Science and Engineering Discovery Environment (XSEDE) Program, and other facilities.

Because of the variety of processor types and parallel architectures, efficient utilization of the above computing resources requires flexible and effective software. Since 2004, DOE grants to USQCD during the three SciDAC [10] programs (2001–2006, 2006–2011, and 2011–2016) led to the development of the USQCD software stack [11]. This stack includes low-level communications and I/O application program interfaces (APIs) implemented via libraries ported to and optimized for each of the architectures. The stack includes

Year	Leadership Class (Tflop/sec-yrs)	Dedicated Capacity Hardware (Tflop/sec-yrs)
2015	430	325
2016	680	520
2017	1080	800
2018	1715	1275
2019	2720	1900

Table 9-4. Available resources for lattice-QCD simulations assumed for the planned program of physics calculations. The conversion factors for lattice-QCD sustained Tflop/sec-years, assuming 8000 hours per year, is 1 Tflop/sec-year = 3.0M core-hour on BlueGene/Q hardware, and 1 Tflop/sec-year = 6.53M core-hour on BlueGene/P and Cray hardware.

linear algebra libraries with routines that operate on single lattice sites, or across a full lattice with communications between neighboring sites. Lattice-QCD applications utilize the various libraries of the software stack to run efficiently on any of the available computing resources. The USQCD software stack is a publicly available resource supporting all of the main lattice gauge and fermion actions in current use. Further, it provides a general purpose framework that can be extended to other quantum field theories besides QCD.

The planned U.S. scientific program in lattice field theory over the next five years assumes the continued availability to USQCD of capability resources at the DOE leadership class facilities, as well as the availability of dedicated capacity resources at Fermilab, Jefferson Lab, and BNL, deployed and operated under the proposed “LQCD-ext II” project extension. Table 9-4 shows the anticipated sustained LQCD Tflop/sec-yrs provided by these resources by year. Completion of the planned physics calculations will require well over an order of magnitude of increased computing capacity beyond that used in prior years. Use of leadership-class facilities alone would provide insufficient computational resources and would be unsuitable for the full mix of lattice-field-theory job requirements. Cluster-class parallel computing hardware, including systems with GPU accelerators, delivers capacity with the highest cost effectiveness for jobs ranging in size from tens to thousands of cores.

Over an order of magnitude increase in storage utilization (disk and tape) from the current approximately 2 petabyte usage will also be needed to support the planned simulations. Further, the anticipated evolution of high-performance computing hardware will require the evolution of software and the introduction and refinement of new techniques and algorithms. Positions for postdocs and scientific staff to develop new lattice-gauge-theory code cannot be supported by grants to lab and university theory groups alone, but must be augmented through grant programs such as SciDAC.

9.6.3 Lattice field theory summary

Numerical lattice-QCD calculations are needed to interpret many upcoming experimental measurements at the Energy and Intensity Frontiers as tests of the Standard Model and new-physics searches. Nonperturbative hadronic matrix elements and fundamental QCD parameters enter the Standard Model predictions for many processes as diverse as rare kaon decays, Higgs branching fractions, and the muon anomalous magnetic moment. Thus, facilities for numerical lattice gauge theory are an essential theoretical complement to the experimental particle physics program.

The successful accomplishment of USQCD's scientific goals requires access to both capacity and capability machines, and hence support for both leadership-class facilities and dedicated computing clusters. The combined use of supercomputers to generate large suites of gauge fields and to perform the largest analysis jobs with these ensembles, and dedicated lattice capacity hardware to perform the much larger volume of small- to medium-scale analysis jobs, is the most cost-effective model for lattice-field theory calculations. The successful utilization of future computing resources requires software that runs efficiently on new computing architectures, and hence support for postdocs and scientific staff to develop lattice gauge theory code.

Support of USQCD through hardware and software grants, access to leadership-class computing facilities, and funding of lab and university theorists, is essential to fully capitalize on the enormous investments in the DOE's high-energy physics and nuclear-physics experimental programs.

Given continued support of the lattice gauge theory effort in the U.S. and worldwide, lattice calculations will play a key role in definitively establishing the presence of physics beyond the Standard Model and in determining its underlying structure.

9.7 Computing for perturbative QCD

9.7.1 Introduction

One of the main challenges facing the particle physics community to date is interpreting LHC measurements on the basis of accurate and robust theoretical predictions. The discovery of a Higgs-like particle in summer 2012 [12, 13] serves as a remarkable example of the level of detail and accuracy that must be achieved in order to enable a discovery [14, 15, 16]. Signals for the Higgs boson of the Standard Model (SM) are orders of magnitude smaller than their backgrounds at the LHC, and they are determined by quantum effects. Detailed calculations are therefore mandatory, and they will become even more necessary as we further explore the Terascale at the full LHC design energy.

Providing precise theoretical predictions has been a priority of the U.S. theoretical particle physics community for many years, and has seen an unprecedented boost of activity during the last ten years. With the aim of extracting evidence of new physics from the data, theorists have focused on reducing the systematic uncertainty of their predictions by including strong (QCD) and electroweak (EW) effects at higher orders in the perturbative expansion. This is particularly important as beyond-Standard-Model effects are expected at roughly the TeV scale. Typical decay chains of potential new particles would involve many decay products, several of which can be massive. The SM backgrounds are complex processes which call for highly sophisticated calculational tools in order to provide realistic predictions.

We have reached a time when no conceptual problems block us from being able to break next-to-leading order (NLO) perturbative QCD calculations into standard modular steps and automate them, making them available to the worldwide LHC community. It is implicit that such an effort will benefit greatly from a unified environment in which calculations can be performed and data can be exchanged freely between theorists and experimentalists, as well as from the availability of adequate computational means for extensive multiple analyses.

We see the frontier of perturbative calculations for collider phenomenology being in the development and optimization of next-to-next-to-leading order (NNLO) QCD calculations, sometimes combined with EW corrections, and in the study of more exclusive signatures that requires resummation of logarithmically enhanced higher-order corrections to all orders. It is also conceivable that techniques for matching NNLO

fixed-order calculations to parton-shower simulations will be constructed in the next five years. In all cases, the availability of extensive computational resources could be instrumental in boosting the exploration of new techniques as well as in obtaining very accurate theoretical predictions at a pace and in a format that is immediately useful to the experiments.

9.7.2 Results and recommendations

This planning exercise provided an incentive for implementing higher-order calculations in a standardized computing environment made available by DOE at NERSC. Resource requirements were determined for the calculation of important background and signal reactions at the LHC, including higher order QCD and EW effects. Prototypical results are listed in Table 9-5 and have been summarized in a white paper [17].

Different High Performance Computing (HPC) environments were tested during this workshop and their suitability for perturbative QCD calculations was assessed. We find that it would be beneficial to make the national HPC facilities ALCF, OLCF, and NERSC accessible to particle theorists and experimentalists so they can use existing calculational tools for experimental studies involving extensive multiple runs without depending on the computer power and manpower available to the code authors. Access to these facilities will also allow prototyping the next generation of parallel computer programs for QCD phenomenology and precision calculations.

The computation of NLO corrections in perturbative QCD has been entirely automated. Resource requirements for NLO calculations determined during this workshop can thus be seen as a baseline that enables phenomenology during the LHC era. NNLO calculations are still performed on a case-by-case basis, and their computing needs can only be projected with a large uncertainty. It seems clear, however, that cutting-edge calculations will require access to leadership class computing facilities.

The use of HPC in perturbative QCD applications is currently in an exploratory phase. We expect that the demand for access to HPC facilities will continue to grow as more researchers realize the potential of parallel computing in accelerating scientific progress. At the same time, we expect growing demand for educating young researchers in cutting-edge computing technology. It would be highly beneficial to provide a series of topical schools and workshops related to HPC in particle physics. They may be co-organized with experiments to foster the creation of a knowledge base.

Large-scale distributed computing in grid environments may become relevant for perturbative QCD applications in the near future. This development will be accelerated if computing grids can also provide access to HPC facilities and clusters where parallel computing is possible on a smaller scale. The Open Science Grid (OSG) has taken first steps in this direction, and we have successfully used their existing interface. The amount of training for new users could be minimized if the OSG were to act as a front-end to the national HPC facilities as well as conventional computing facilities.

Additional details may be found in the full subgroup report [18].

9.8 Distributed computing and facility infrastructures

Powerful distributed computing and robust facility infrastructures are essential for continued progress of particle physics across the Energy, Intensity, and Cosmic Frontiers. Experiment and theory require a combination of HTC and HPC systems. The LHC experiments are the dominant consumers of HTC.

Type of calculation	CPU hours per project	Projects per year
NLO parton level	50,000 - 600,000	10-12
NNLO parton level	50,000 - 1,000,000	5-6
Event generation	50,000 - 250,000	5-8
Matrix element method	$\sim 200,000$	3-5
Exclusive jet cross sections	$\sim 300,000$	1-2
Parton distributions	$\sim 50,000$	5-6

Table 9-5. *Summary of computing requirements for typical projects carried out by the U.S. community [17].*

They have been and will continue to be well served by it. Most Intensity Frontier experiments can also be supported by HTC. HPC is needed for applications such as lattice QCD, accelerator design and R&D, data analysis and synthetic maps, N-body and hydro-cosmology simulations, supernova modeling, and, more recently, perturbative QCD. Historically, national centers have focused primarily on HPC, but these centers have begun to address HTC, and are interested in attracting scientists who need HTC.

Energy Frontier experiments face a growth in data that will make it a challenge to meet their needs. Doing so is possible, but it requires near-constant funding of the Worldwide LHC Computing Grid (WLCG), greater efficiencies in resource usage, and the evolution of software to take advantage of multicore processor architectures. These experiments should also pursue and take advantage of opportunistic resources, be they in commercial clouds (which are not currently viable and cost effective as purchased resources), university and lab computing centers, or elsewhere. The experiments would also benefit from further engagement with national HPC centers. The centers could provide resources to particle physics experiments, and have support staff that could help port and integrate applications such as detector simulations that have not traditionally been used in HPC environments.

Intensity Frontier experiments have comparatively smaller computing needs. There are no technical reasons why they could not be met. Such experiments should use resources available through the Open Science Grid (OSG) or at national computing centers. They would benefit from a collective effort to gain access to resources and share software and training.

Cosmic Frontier experiments (and the simulations required to interpret them), lattice QCD, and accelerator design will need a large increase in HPC resources in the coming years. Demand for access to HPC across particle physics frontiers is expected to exceed the amount of available resources. HPC-based computations are needed to interpret results from a number of important particle physics experiments, and to realize the scientific returns from the substantial investments in those experiments. The NERSC report on particle physics computing needs [19] indicates a shortage of HPC resources by a factor of four by 2017. While funding and technology development needed to sustain traditional HPC growth rates are uncertain, they must be maintained to support particle physics. There are a number of applications within particle physics that would benefit from exascale computing and a cadre of scientists eager to support efforts to reach that scale.

Distributed computing infrastructures, based at labs and universities, have been critical to the success of the Energy Frontier experiments and should continue to be able to serve these and other applications even as experiments grow. There are no show-stoppers seen as scale increases, but various developments should be pursued to improve efficiency and ease of use. Keeping sufficient staff support at a reasonable cost is a continuing concern; finding operational efficiencies could help address this. Given that particle physics is the

largest user of distributed scientific computing, currently in the form of HTC on computing grids, members of the field must continue to take a leadership role in its development.

National centers play an important role in some aspects of computing, and particle physics might be able to take advantage of an expanded role. Experiments should explore the use of the HPC centers as part of their efforts to diversify their computing architectures. These centers do have access to large, state-of-the-art resources, operational support, and expertise in many areas of computing.

We expect that distributed computing and facility infrastructures will continue to play a vital role in enabling discovery science.

Additional details may be found in the full subgroup report [20].

9.9 Networking

Particle physics research in all areas depends on the availability of reliable, high-bandwidth, feature-rich computer networks for interconnecting instruments and computing centers globally. Most particle physics-related data is transported by National Research and Education Networks (NRENs), supplemented by infrastructures dedicated to specific projects. NRENs differ from commercial networks, because they are optimized for transporting massive data flows generated by large-scale scientific collaborations. In addition, NRENs offer advanced capabilities — such as multi-domain dedicated circuits — which commercial providers do not have an incentive to deploy.

For decades, network traffic generated by particle physics has been a primary driver of NREN growth, and particle physics requirements have motivated NREN architectures and research activities. In the next ten years and beyond, the productivity of particle physics collaborations will continue to depend on an ecosystem of innovative global NRENs.

Particle physics collaborations are now accustomed to viewing network transport as a reliable and predictable resource, so much so that data models for ATLAS and CMS have evolved rapidly in response to NREN capabilities, but this state of affairs is not inevitable. Other data-intensive communities have begun to generate large traffic flows and, following the example of particle physics, to incorporate high-performance networks into science workflows. As a result of this broad trend toward data intensity across many disciplines, NRENs around the world will be challenged to meet the requirements of large-scale research, and must be adequately provisioned in order to continue serving the critical role they have played in the past.

In support of our objectives through 2020, basic and applied networking research is necessary in a range of subjects. Critical questions include:

- What future architectures will maximize utilization and minimize cost in core and campus networks?
- How can emerging paradigms such as Software Defined Networking or Named Data Networking be harnessed most effectively to improve particle physics science outcomes?
- Can networks evolve into adaptive, self-organizing, programmable systems that quickly respond to requests of particle physics science applications?
- If well-tuned host systems (or ensembles of them) have the ability to saturate a single backbone channel, what techniques and architectures can NRENs adopt to maximize data mobility?

- How will the emerging complexity challenge arising from closer integration between networks and applications be managed, especially in the multi-domain, multi-national context?
- How can diverse networks cooperate automatically and securely to offer science-optimized capabilities on a worldwide basis?
- Can discovery or automation techniques reduce the need for fragile, manual configuration?
- How will networks respond to the operational challenge of deploying and managing dozens of wavelengths across large geographies under relatively flat funding prospects?
- Will post-TCP protocols become useful outside of highly-controlled demonstration projects?
- Would computer modeling of applications, networks, and data flows be useful in answering any of these questions?
- Will power consumption become a limiting economic or operational factor in this time period?

Recent investments in network research have been insufficient. Continued underfunding will compromise the ability of particle physics collaborations to maximize scientific productivity. Increased research funding, while necessary, is not sufficient; there also needs to be increased attention to the process of translating the results of network research into real-world architectures that NRENs can deploy and manage. Incentives and funding for such activities that enable deployment are urgently needed. Because network research has now begun to intersect with research in services and applications, cross-disciplinary funding opportunities should also be available.

A number of cultural and operational practices need to be overcome in order for NRENs (and global cyber-infrastructure more generally) to fully succeed. Expectations for network performance must be raised significantly, so that collaborations do not continue to design workflows around a historical impression of what is possible. The gap between peak and average transfer rates must be closed. Campuses must deploy secure science data enclaves — or Science DMZs [21] — engineered for the needs of particle physics and other data-intensive disciplines. Fortunately, each of these trends is currently underway, but momentum must be accelerated.

Ten years from now, the key applications on which particle physics depends will only be fully successful, efficient, or cost-effective if they are run on the networks that exist today. During the next decade, research networks need to evolve into programmable instruments — flexible resources that can be customized for particular needs, but that exist within a common, integrated, ubiquitous framework that is reliable, robust and trusted for its privacy and integrity. These are major challenges, but they are tractable if funding agencies invest in innovative research, and maintain support for the exponential growth of NREN traffic.

Additional details may be found in the full subgroup report [22].

9.10 Software development, staffing, and training

The success of particle physics will continue to critically depend on computing. Managing the human activities associated with computing (software development and management, training and staffing) is an important part of that. Based upon our own experiences, and from discussions with members of the particle physics community, we have identified the following main goals for the next decade in the area of software, staffing and training:

- Maximize the scientific productivity of our community in an era of reduced resources, by using software development strategies and staffing models that will result in products that are useful for the entire particle physics community.
- Respond to the evolving technology market, especially with respect to computer processors, by developing and evolving software that will perform with optimal efficiency in future computing systems.
- Insure that our developers and users will have the training needed to create, maintain, and use the increasingly complex software environments and computing systems that will be part of future particle physics projects.

Some specific recommendations we feel will help achieve these goals are detailed below.

- Software management, toolkits and reuse
 - Continue to support established toolkits (such as Geant4, ROOT)
 - Encourage the creation of new toolkits from existing successful common software (such as those for generators, tracking)
 - Allow flexible funding of software experts to facilitate transfer of software and sharing of technical expertise between projects
 - Facilitate code sharing through open-source licensing and use of publicly-readable repositories
 - Consolidate and standardize software management tools to ease migration of people from one project to another
- Software development for new hardware architectures
 - Invest in software needed to adapt to the evolution of computing processors, both as basic R&D into appropriate techniques and as re-engineering “upgrades”
 - Design new software and reengineer existing software to expose parallelism at multiple levels
 - Develop flexible software architectures that can efficiently exploit a variety of possible future hardware options
- Staffing
 - Recognize software efforts as sub-projects of the project
 - Integrate computing professionals as part of the project team, over the life of the project or collaboration
 - Integrate software professionals with scientist developers to insure software meets both the technical and scientific needs of the project
- Training
 - Use certification to document expertise and encourage the learning of new skills
 - Encourage training in software and computing as a continuing physics activity
 - Use mentors to spread scientific software development standards
 - Involve computing professionals in the training of scientific domain experts
 - Use online media to share training
 - Use workbooks and wikis as evolving, interactive software documentation

- Provide young scientists with opportunities to learn computing and software skills that are marketable for non-academic jobs

Additional details may be found in the full subgroup report [23].

9.11 Storage and data management

The largest Energy Frontier experiments have developed, and are improving, functional distributed data and workflow management systems that meet their needs. These systems are expensive to develop and operate and are thus rarely appropriate for smaller experiments.

Particle physics currently benefits from, but can also be constrained by, the highly successful ROOT features supporting reading and writing of persistent data. No other major scientific field uses ROOT or appears interested in it. Major developments in the technology for dealing with persistent data will be required to take advantage of storage hardware on the timescale of LHC Run 3.

Particle physics should maintain and promote a vision of the future in which fully functional and low-operational-cost distributed computing and persistency management is supported by software that is widely used in data-intensive science. To this end, developments in industry and the wider science community should be monitored actively. Particle physics should work with the wider science and computer science community to export and adapt particle physics technologies and vice-versa. In distributed computing, particle physics should organize itself to significantly reduce the number of diverse approaches and share the ideas and software developed in the largest experiments with other activities where they are needed.

Rotating disk storage will suffer a marked slowdown in the evolution of capacity/cost. This may be the largest perturbation of particle physics computing models that must attempt to optimize the roles of tape, rotating disk, solid-state storage, networking, and CPU.

Many of the components required to support virtual data already exist in the data and workflow management software of the largest experiments. The rigorous provenance recording required to support the virtual data concept would also benefit data preservation.

Computing model implementations should be flexible enough to adapt to a wide range of relative costs of the key elements of particle physics computing. In preparing for Run 3, the LHC program should seriously consider virtual data as a way to accommodate scenarios where storage for derived and simulated data becomes relatively very costly.

All experiments across all frontiers need infrastructure that will allow scientists to store, catalog, access, and reprocess data sets years after the original physics results are produced. The inherent similarity of the requirements across experiments and disciplines calls for a coordinated investment in common infrastructure to enable easy access and adoption of best practices in knowledge preservation. Solutions should be developed that meet the needs of the particle physics and astrophysics communities before widespread release of data to the public can be expected or mandated.

Additional details may be found in the full subgroup report [24].

9.12 Conclusions

For the **Energy Frontier**, computing limitations already reduce the amount of physics data that can be analyzed. The planned upgrades to the LHC energy and luminosity are expected to result in a ten-fold increase in the number of events and a ten-fold increase in event complexity. Efforts have begun to increase code efficiency and parallelism in reconstruction software and to explore the potential of computational accelerators such as GPUs and Xeon Phi. Saving more raw events to tape and only reconstructing them selectively is under consideration. The LHC produces about 15 petabytes (PB) of raw data per year now, but in 2021 the rate may rise to 130 PB. Attention needs to be paid to data management and wide-area networking, to assure that network connectivity does not become a bottleneck for distributed event analysis. It is important to monitor storage cost and throughputs. More than half of the computing cost is now for storage, and in the future it may become cost-effective to recalculate certain derived quantities rather than storing them.

Intensity Frontier experiments have combined computing requirements on the scale of a single Energy Frontier experiment, but they are a more diverse set than those of the Energy Frontier. We conducted a survey and found that there is significant commonality in different experiments' needs. Sharing resources across experiments, as in the Open Science Grid, is a first step in addressing peak computing needs. Continued coordination of software development among these experiments will increase efficiency of the development effort. Leveraging the data handling experience and expertise of the Energy Frontier experiments for the diverse Intensity Frontier experiments would significantly improve their ability to reconstruct and analyze data.

Cosmic Frontier experiments will greatly expand their data volumes needs with the start of new surveys and the development of new instruments. Current data sets are about 1 PB, and the total data set is expected to be about 50 PB in ten years. Beyond that, in 10–20 years data will be collected at the rate of 400 PB/yr. On the astrophysics and cosmology theory side, some of the most challenging simulations are being run on supercomputers. Current allocations for this effort are approximately 200M core-hours annually. Very large simulations will require increasing computing power. Comparing simulations with observations will play a crucial role in interpretation of experiments, and simulations are needed to help design new instruments. There are very significant challenges in dealing with new computers' architectures and very large data sets, as described above. Growing archival storage, visualization of simulations, and allowing public access to data are also issues that need attention.

Accelerator science is called on to simulate new accelerator designs and to provide near-real-time simulations feedback for accelerator operation. Research into new algorithms and designs has the potential to bring new ideas and capabilities to the field. It will be necessary to include additional physics in codes and to improve algorithms to achieve these goals. Production runs can use from 10K to 100K cores. Considerable effort is being expended to port to new architectures, in particular to address the real-time requirements.

Lattice field theory calculations rely on national supercomputer centers and hardware purchased for the USQCD Computing Project. Allocations at supercomputer centers have exceeded 500 M core-hrs this year, and resource requests will go up by a factor of 50 by the end of this decade. This program provides essential input for interpretation of a number of experiments, and increased precision will be required in the future. For example, the b quark mass and the strong coupling α_s will need to be known at the 0.25% level, a factor of two better than now, to compare precision Higgs measurements at future colliders with Standard Model predictions. Advances in the calculation of hadronic contributions to muon $g - 2$ will be needed for interpretation of the planned experimental measurement.

Perturbative QCD is essential for theoretical understanding of collider physics rates. Codes were ported to the HPC centers at NERSC and OLCF, and also run on the Open Science Grid. They have also been benchmarking GPU codes and finding impressive speed up with respect to a single core. A computer at CERN was used to benchmark the Intel Xeon Phi chip. A repository of codes has been established at NERSC. A long term goal is to make it easy for experimentalists to use these codes to compute Standard Model rates for the processes they need.

The **Distributed computing and facilities infrastructures** subgroup looked at the growth trends in distributed resources as provided by the Open Science Grid, and the national high performance computing (HPC) centers. Most of the computing by experiments is of the HTC type, but HPC centers could be used for specific work flows. Using existing computing centers could save smaller experiments from large investments in hardware and personnel. Distributed HTC has become important in a number of science areas outside particle physics, but particle physics is still the biggest user and must continue to drive the future computing development. HPC computing needs for theoretical physics will require an order of magnitude increase in capacity and capability at the HPC centers in the next five years, and two orders of magnitude in the next ten years.

The **Networking** subgroup considered the implications of distributed computing on network needs, required R&D and engagement with the National Research and Education Networks (which carries most of our traffic). A number of research questions were formulated that need to be answered before 2020. Expectations of network performance should be raised so that planning for network needs is on par with that for computing and storage. The gap between peak bandwidth and delivered bandwidth should be narrowed. It was not felt that wide-area network performance will be an insurmountable bottleneck in the next five to ten years as long as investments in higher performance links continue. However, there is uncertainty as to whether network costs will drop at the same rate as they have done in the past.

The **Software development, personnel, and training** subgroup has a number of recommendations to implement three main goals. The first goal is to use software development strategies and staffing models that result in software more widely useful to the particle physics community. The second goal is to develop and support software that will run with optimal efficiency on future computer architectures. The third goal is to insure that developers and users have the training necessary to deal with the increasingly complex software environments and computing systems that will be used in the future.

The **Storage and data management** subgroup found that storage continues to be a cost driver for many experiments. It is necessary to manage the cost to optimize the science output from the experiment. Tape storage continues to be relatively inexpensive and should be more utilized within the storage hierarchy. Disk storage is likely to increase in capacity/cost relatively slowly due to a shrinking consumer market and technology barriers. It can be costly for experiments to operate their own distributed data management systems, thus continued R&D in this area would benefit a number of experiments.

To summarize, the challenging resource needs for the planned and proposed physics programs require efficient and flexible use of all resources. Particle physics needs both distributed HTC and HPC. Emerging experimental programs might consider a mix to fulfill demands. Programs to fund these resources need to continue. It may also be possible to use shared computer resources and opportunistic sources of computing to meet some needs. Commercial cloud providers may also provide a useful resource, particularly if prices are reduced. There is increasing need for data-intensive computing in traditionally computation-intensive fields, including at HPC centers.

In order to satisfy our increasing computational demands, the field needs to make better use of advanced computing architectures. With the need for more parallelization, the complexity of software and systems continues to increase, impacting architectures for application frameworks, workload management systems,

and also the physics code. We must develop and maintain expertise across the field, and re-engineer frameworks, libraries, and physics codes. Unless corrective action is taken to enable us to take full advantage of the new hardware architectures, we could be frozen out of cost-effective computing solutions on a time scale of 10 years. There is a large code base that needs to be re-engineered, and we currently do not have enough people trained to do it.

The continuing huge growth in observational and simulation data drives the need for continued R&D investment in data management, data access methods, and networking. Continued evolution of the data management and storage systems will be needed in order to take advantage of new network capabilities, ensure efficiency and robustness of the global data federations, and contain the level of effort needed for operations. Significant challenges with data management and access remain, and research into these areas could continue to bring benefit across the Frontiers.

Network reliability is essential for data intensive distributed computing. Emerging network capabilities and data access technologies improve our ability to use resources independent of location. This will enable use of diverse computing resources including dedicated facilities, university computing centers, resources shared opportunistically between PIs, and potentially also commercial clouds. Leadership-class HPC centers may also become relevant for data-intensive computing. The computing models should treat networks as a resource that needs to be managed and planned for.

Computing will be essential for progress in theory and experiment over the next two decades. The advances in computer hardware that we have seen in the past may not continue at the same rate in the future. The issues identified in this report will require continuing attention from both the scientists who develop code and determine what resources best meet their needs, and from the funding agencies who will review plans and determine what shall be funded. Careful attention to the computational challenges in our field will increase efficiency and enable us to meet the experimental and theoretical physics goals identified through the Snowmass process.

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Communication, Education, and Outreach

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10.1 Introduction

The U.S. particle physics community recognizes the critical importance of consistent and coherent communication, education, and outreach (CE&O) activities. These efforts foster nationwide support for the field, develop the next generation of researchers, and help develop a scientifically literate citizenry.

Federally supported research in particle physics and related fields has led to an impressive list of Nobel Prize-winning discoveries: the first detailed study of the cosmic microwave background; the discovery of neutrino masses and mixing (and earlier work on solar neutrinos many years earlier); the discovery of the accelerating expansion of the universe; understanding the strong force; and the discovery that the Cabbibo-Kobayashi-Maskawa (CKM) matrix explains experiments that have observed an asymmetry in the behavior of particles and anti-particles. Most recently, the 2013 Nobel Prize was awarded for the discovery of the Higgs boson, an achievement made possible by scientific talent, technology and leadership from the United States.

The American public is fascinated by these discoveries, and by the full breadth of current and future particle physics projects. The saga of the Large Hadron Collider (LHC) and Higgs boson discovery reached audience levels unprecedented for a particle physics event. Public lectures and other events on particle physics topics draw crowds. Milestones, discoveries, and even proposals for projects in particle physics routinely make headlines.

Many individuals, groups and institutions in the U.S. particle physics community reach out to members of the public, decision makers, teachers, and students through a wide variety of activities. These activities may be effective in achieving their individual goals. However, there is room for improvement in the nationwide coordination of these activities, in the mobilization of the entire U.S. community to take part in the activities, and in efforts to use varied activities to convey consistent and compelling messages to stakeholders.

Translating this public excitement into greater support for the field requires a greater fraction of our community to engage in CE&O activities, and for the quality, coordination and number of existing activities to be increased. Our community must lower barriers to engagement in such activities, moving towards treating CE&O activities as another necessary component of research along with publishing and presenting scientific results. We must also increase our efforts to convey consistent, coherent, and compelling messages about the importance of particle physics research and its value to society. National coordination, training, and additional supporting resources will greatly help support an increased commitment to effective CE&O.

This Snowmass process was the first in which one working group was devoted to CE&O. Our task was threefold: to write a paper that summarizes current activities and provides recommendations for future activities; to carry out CE&O activities at the Minnesota meeting; and to educate and train physicists at Snowmass about CE&O. The Snowmass CE&O group was composed of five working groups organized by the main target audiences for U.S. CE&O activities: the general public; policy makers and opinion leaders; the science community; teachers in grades 5-16; and students in grades 5-16. As part of the Snowmass Process, the CE&O group held two pre-meetings associated with the March and April meetings of the American Physical Society, carried out formal and informal discussions with colleagues, surveyed the particle physics community regarding participation in and attitudes toward CE&O [1], and established a new website to update the 2005 particle physics education and outreach activities database. During *CSS 2013 Snowmass on the Mississippi*, we held lunchtime discussions with over 190 attendees and organized two panels—*Selling Long-Term Science in Washington* and *Fostering Interconnections and Common Cause with Scientists from Other Fields*. The particle physics magazine *symmetry* invited physicists to explain why particle physics matters in one-minute video talks and compiled a photo gallery of physicists holding whiteboards showing the big question in particle physics that drives their research.

Several common themes emerged from the CE&O study as necessary to take particle physics CE&O in the United States to a new level:

- Making a *coherent* case for particle physics—the compelling questions we address, the facilities we need for our research, and the value of particle physics to society.
- Recognizing—formally and informally—physicists, postdocs, and students who devote time to CE&O efforts.
- Developing and increasing access to resources, training activities, and opportunities that engage physicists with policy makers, opinion leaders, the general public, educators and students.
- Creating a national team dedicated to developing and providing communication and education strategy and resources, supporting and enhancing existing efforts, and to mobilizing a greater fraction of the U.S. community to participate in CE&O activities.

This summary report begins with a set of overarching goals for U.S. particle physics CE&O identified as a result of the Snowmass study, along with a set of implementation recommendations that support the goals for all target audiences. The balance of the report is then divided into four audience groups: policy makers and opinion leaders; the general public; the science community; and teachers and students in grades 5-16. Each of the four sections provides an overview of existing CE&O efforts targeted at that audience group, strategies to achieve the overarching CE&O goals with that audience, and recommendations for implementation of the identified strategies. More detailed information for all audience groups can be found in the full subgroup reports.

10.2 Goals

The CE&O group developed its goals, strategies and recommendations with input from particle physicists and education and outreach professionals obtained prior to and during the Minneapolis meeting. The recommendations support a proactive, coordinated CE&O effort from the entire U.S. particle physics community, reinforcing the view expressed in the 2013 European Strategy Report: “Outreach and communication in particle physics should receive adequate funding and be recognized as a central component of the scientific

activity” [2]. The following are the three overarching goals identified for all U.S. particle physics CE&O activities:

1. Ensure that the U.S. particle physics community has the resources necessary to conduct research and maintain a world leadership role.
2. Ensure that the U.S. public appreciates the value and excitement of particle physics.
3. Ensure that a talented and diverse group of students enter particle physics and other STEM careers, including science teaching.

The CE&O group identified the following four actions that could be taken over the next five years to achieve these goals for all target audiences:

1. Augment existing CE&O efforts with additional personnel and resources dedicated to nationwide coordination, training and support.
2. Develop a comprehensive central communication, education, and outreach resource for physicists, with initial content available before the end of the 2013/2014 P5 process.
3. Provide communication training to the U.S. particle physics community.
4. Work with the APS Division of Particles and Fields (DPF) and the High Energy Physics Advisory Panel (HEPAP) to develop a sustainable process for collecting statistics on workforce development and technology transfer and with APS to investigate a U.S. economic impact study for physics research that includes particle physics.

The following four sections discuss additional strategies to achieve the overarching goals for specific target audiences.

10.3 Building support among policy makers and opinion leaders

The U.S. particle physics community has recognized as part of the Snowmass process that it must embark on a coordinated effort that mobilizes a greater fraction of scientists and students to translate the public excitement about and interest in particle physics research into greater support among the policy makers that make decisions about research funding and the opinion leaders whose views they trust to guide them in their decisions.

The U.S. particle physics community has engaged for decades in a variety of efforts to educate and inform the public, including policy makers and opinion leaders, about the excitement and importance of particle physics research and its benefits to society. These public outreach efforts have met with success, as demonstrated by the public attention to events in particle physics over the last six years, including the saga of the Large Hadron Collider, experimental hints of faster-than-light neutrinos, and the transit of a huge electromagnet from New York to Chicago. Public lectures and other events on particle physics topics draw crowds that other fields of science envy. Discoveries, milestones and even project proposals in particle physics routinely make headlines, while successes in other fields of science fail to attract media attention.

Yet while particle physics research fascinates and excites the American public, their continuing support for the funds required to build new facilities is not guaranteed. In the current climate of fiscal austerity,

government-funded programs must make compelling cases for the societal benefit of their work. The bar is set high for scientific fields such as particle physics, which receives more than \$750 million each year in the budget of the DOE Office of High Energy Physics and has proposed to build new projects with total costs of more than \$1 billion.

A CE&O survey of more than 600 respondents conducted in the first half of 2013 revealed that approximately 60% of particle physicists are engaged in outreach activities to the general public; 50% are engaged in activities that reach K-12 teachers or students; 35% in activities that reach scientists in other fields; and only 30% in activities that reach policy makers and opinion leaders [1]. Building greater support among policy makers will require a larger fraction of the particle physics community to engage in these activities, and for these activities to be coordinated, consistent, and effective.

The summary for this subgroup of the CE&O frontier contains strategic recommendations for actions the U.S. particle physics community can take over the next few years to improve communication and outreach to policy makers and opinion leaders. Policy makers include elected and non-elected officials in federal, state and local government who influence particle physics funding. Opinion leaders are defined as notable figures whose views on scientific research and science funding influence policy makers and the public.

The full subgroup report [1] includes a definition of the two audiences, a more comprehensive overview of existing activities targeted to those audiences, and additional details about each of the recommended activities.

10.3.1 Overview of existing activities

The U.S. particle physics community engages in a number of efforts to build support for research among policy makers and opinion leaders, including:

1. Annual visits to Washington, D.C. by facility user groups
2. Email and letter-writing campaigns at key points in the budget cycle
3. Participation by particle physicists in Washington, D.C. events organized by AAAS, APS, the National User Facilities Organization and other organizations
4. Direct advocacy with legislators by scientific and industrial societies, national laboratories and individual scientists
5. Efforts to place stories of particle physics and particle physicists in influential media outlets.

10.3.2 Communication and outreach to policy makers and opinion leaders

We identified three strategies that will help achieve the first two overarching CE&O goals. These strategies are high-priority efforts that should be undertaken by the particle physics community over the next five years.

- Empower and enable members of the particle physics community to communicate and advocate coherently, consistently and effectively on behalf of their science.

- Develop an enduring process to track, update, and disseminate statistics on the impact of particle physics on society.
- Put informed third-party advocates to work raising the profile of and informing key stakeholders about the importance of particle physics, physics, and discovery science to the United States.

These strategies are targeted to the policy maker and opinion leader audiences, but also support the goals of outreach to the general public and scientists in other fields. They are in turn supported by strategies identified by other CE&O subgroups, including:

- Develop consensus in our field that we need to prioritize, buy into the mechanism of prioritization, and support the resulting plan.
- Make the public aware of direct and indirect applications of research, both historical and potential.
- Communicate the role and stories of U.S. physicists in particle physics, particularly in major discoveries and in the context of our international collaborations.
- Foster more dialog and understanding among subfields of physics and with other sciences. Identify areas of common cause and unite in support of them.

10.3.3 Implementing the strategies

The U.S. particle physics community has been engaged for decades in a variety of efforts to inform the public and policy makers about particle physics research, and to encourage support for that research. The activities recommended below do not replace existing efforts. Instead, they augment and enhance ongoing efforts by providing nationwide coordination and support, and by developing needed resources to make a compelling case for support of particle physics research.

Recommendation 1: Augment existing efforts with additional personnel and resources dedicated to nationwide coordination, training and support.

The U.S. particle physics community, as part of the 2013 Snowmass process, has strongly indicated that it wishes to greatly enhance its CE&O efforts. Taking things “to the next level” requires a nationwide effort that mobilizes a greater fraction of the community and is supported by dedicated personnel and resources. This effort will need to be a partnership between scientists and professionals in the areas of communication, government relations, and education. It will need to encompass existing efforts carried out at laboratories, universities and by experimental collaborations.

The greatest initial need is a team of a few people to coordinate and support existing activities and spearhead and organize new nationwide initiatives. Such a team would be most effective if coupled closely with existing nationwide efforts, such as the particle physics magazine *Symmetry*, QuarkNet, the outreach activities of the Contemporary Physics Education Project, and the advocacy efforts of users’ groups. Funding would also be required to establish and maintain technical infrastructure for the national effort, such as websites and databases.

Recommendation 2: Develop a central communication, outreach, and education resource for physicists, with initial content available before the end of the 2013/2014 P5 process.

The first activity for the team identified in Recommendation 1 would be the creation of a website to act as physicists’ central clearinghouse for resources related to CE&O. This website would be designed to fulfill the

needs of physicists who engage in CE&O activities, not the needs of the general public who wish to learn about particle physics. It would provide physicists with the tools, techniques, information, and resources they need to engage effectively with policy makers, opinion leaders, the general public, teachers, and students. The content would include:

- Tips, techniques, and training videos for communicating to various audiences.
- Fact sheets, brochures, and other handouts available for download.
- Talking points about the science of particle physics and its impact on society, with supporting examples and data.
- Links to existing websites and electronic materials that support communication, education, and outreach activities.
- Links to external databases or community-generated databases that track important statistics on workforce development, technology transfer, or economic impact.

Creating and maintaining such a website requires at least one FTE dedicated to nationwide particle physics communication, plus initial funding to create the website framework and continuing funding for website hosting and maintenance. The creation of particle physics-specific training videos and databases to track various types of statistics could require significantly more additional resources.

Recommendation 3: Organize and identify logistical support for year-round campaigns in which particle physicists strategically advocate for scientific research with policy makers.

Many attendees at the 2013 Community Summer Study meeting in Minneapolis expressed the desire for an ongoing effort that strategically leverages the widespread U.S. particle physics community to keep support high among policy makers. Existing efforts are mainly organized by users' groups and rely on scientist volunteers. Year-round strategic campaigns require additional logistical support dedicated to such a nationwide effort. A minimum of 0.5 FTE would be needed to:

- Provide logistical support that enables the annual facility users groups' D.C. trip to be expanded to the whole U.S. particle physics community.
- Provide logistical support to organize a nationwide effort for physicists to visit their Congressional representatives in their home districts.
- Liaise with professional lobbying firms/organizations to organize nationwide letter-writing campaigns at key times during the federal budget cycle.
- Organize a nationwide effort to encourage every recent particle physics Ph.D. graduate from a U.S. university to write a personal letter to his/her Congressional delegation thanking them (if applicable) for the support that made their training possible, providing a brief overview of their research and its value to society, and describing where that student hopes to use their training.
- Set up computers at key particle physics meetings and conferences at which scientists can send letters to their representatives.

Recommendation 4: Provide communication training to the U.S. particle physics community.

Communication training can greatly enhance scientists' ability to build support for their field, yet very few scientists have gone through such training. This subgroup recommends that the dedicated personnel identified in Recommendation 1:

- Identify existing training resources and make them available through the central communication resource identified in Recommendation 2.
- Investigate, together with the physics community, the most effective training format(s) and carry out pilot trainings.
- Organize at least one communication-related seminar or training at each major U.S. particle physics conference.
- Provide yearly assessments of this training and its impact on nationwide communication efforts.

Recommendation 5: Work with APS to investigate the feasibility of a U.S. economic impact study for physics research that includes particle physics as a key component.

Many policy makers and opinion leaders evaluate requests for funding based on an economic model of investment and return. The particle physics community recognizes this and seeks ways to compile such economic data for its research. Economic impact studies are relatively simple to conduct on behalf of an organizational entity such as a national laboratory or university, when the items studied are based on capital and operating costs such as federal funding, purchases of goods and services, payments of salaries to employees and expenditures by visitors. Studies carried out by CERN [3] and the Canadian Light Source [4] have gone one step further, including some aspects of the economic and social impact of academic research, and the auxiliary effect of research activities and companies doing businesses with the laboratories on the region surrounding the laboratories. Studies that attempt to rigorously quantify the long-term impact of an entire field of fundamental science on a national economy are very rare, but there is no such study for particle physics.

Due to the complexities and costs involved in such a study, this subgroup recommends that the first step for the particle physics community should be to partner with APS to investigate the feasibility of an economic impact study of physics research in the United States. This study should be balanced with respect to subfields of physics and identify connections of specified disciplines with particular industries. It should work toward an evaluation metric that includes weighted quantities in an attempt to better gauge the indirect economic impact of physics research. This study should also include the direct impact from the purchase of goods and services in particular regions as well as the impact of workforce development.

Recommendation 6: Work with DPF and HEPAP to develop a sustainable process for collecting statistics on workforce development and technology transfer.

Ongoing efforts exist to collect and analyze statistics on particle physics-related workforce development, in particular through the HEPAP Demography Committee and the HEPFolk database maintained at Lawrence Berkeley National Laboratory. We recommend that resources be allocated to extend the work of this committee to set in place long-term, sustainable procedures for collecting and maintaining data on workforce training (including undergraduate training) and the jobs taken by Ph.D. physicists who do not pursue academic careers.

We further recommend that resources be allocated to create and initially populate a database of design and engineering patents that are associated with particle physics experiments. A simple search in August 2013 of the current U.S. patent collection database for the words “particle physics” yields 497 hits [5]. Physicists will be able to use the database to generate reports for communication activities.

Recommendation 7: Generate letters and statements from third-party advocates in support of the impact of particle physics on society.

Third-party advocates such as CEOs, notable scientists in other fields, and opinion leaders can be very powerful voices for particle physics research funding. Biologist Harold Varmus advocated for particle physics by pointing out that research instrumentation for biological science research was not developed by biologists, but by physicists [6]. We recommend that the U.S. particle physics community, together with the APS Division of Particles and Fields, identify and solicit support from CEOs of major companies in various U.S. industries in the form of letters of support for particle physics research that can be forwarded to policy makers. Similar letters should also be written and signed by the presidents of the major U.S. particle physics research universities. In conjunction with these communications, testimony before appropriate Congressional committees by CEOs, university presidents, and notable scientists from other fields should be arranged in support of particle physics research, facilities, and training.

10.4 Interconnections and common cause with scientists from other fields

The field of particle physics has made fantastic advances over the past two decades. The discoveries of the top quark in 1995, tau neutrino in 2000 and a Standard Model Higgs-like boson in 2012 completed the Standard Model picture of particle physics. However, many other important developments such as the discovery of neutrino oscillations, discrepancies in some precision measurements of Standard Model processes, observation of matter-antimatter asymmetry, and the evidence for the existence of dark matter and dark energy all point to new physics beyond the Standard Model. The measured mass of the Higgs boson, while consistent with a SM particle, is also tantalizingly consistent with what could be expected from new physics such as supersymmetry. While much knowledge has been gained, there remain many intriguing questions to be addressed and a rich landscape of physics to be explored.

As the field has marched on to probe deeper into the nature of fundamental particles and forces, the particle physics facilities and experiments have become larger, more complex and ever more expensive. The field of particle physics has become international and global. The United States, which dominated the field for many decades with many world-class high-energy particle accelerators in quick succession, is now facing a crisis with regards to maintaining such world leadership. The cancellation of the Superconducting Supercollider (SSC) project in the U.S. in 1993, the Large Hadron Collider at CERN superseding the Fermilab Tevatron as the world's Energy Frontier machine for particle physics, and the lack of a grand vision for U.S.-based particle physics facilities beyond the Tevatron have considerably weakened the U.S. position in the field, even while the field is growing stronger world-wide.

If particle physics is to remain viable and strong in the U.S., we need support from the public, policy makers, and from the broader science community. The goal of the subgroup on science community outreach was to review previous outreach activities our field has undertaken aimed at scientists in other fields and develop strategies for improving communication and outreach in the future.

Our broad goals for the subgroup through the Snowmass process were to explore ideas on how to build rapport with scientists from other physics subfields and other sciences, to get them to stand with us in areas of common cause and support a healthy particle physics program in the United States. In order to be able to communicate our science to the broader science community, it is also critical that the particle physicists themselves have broader appreciation and respect for all areas of particle physics and support the community plan to move the field forward. We therefore made efforts on both “particle physics inreach” among the three established frontiers of particle physics (Energy, Intensity and Cosmic Frontiers) as well as “particle physics outreach to scientists from other fields.”

Here is a brief summary of our efforts and activities as a part of the Snowmass Community Summer Study over the past nine months.

10.4.1 Past and current outreach activities

We surveyed past and current outreach activities of the particle physics community targeted at colleagues in the broader science community. These include: (1) colloquia and seminars at university departments and labs, (2) plenary sessions organized at APS and AAAS meetings, (3) publication of particle physics results in journals such as *Science* and *Nature*, (4) pedagogical review articles in similar journals, (5) articles in *Scientific American*, *Popular Science*, and similar magazines, (6) online articles and science blogs, (7) popular science books, and (8) reports written for a general audience commissioned by labs and agencies, for example, *Quantum Universe* [7], and *Discovering the Quantum Universe* [8].

These activities have been useful in informing scientists in other fields about the latest work being done and results from particle physics, and these activities should be encouraged and continued. However, we conclude that more direct engagement with scientists in other fields is critical.

10.4.2 Fostering connections with other sciences

Particle physics research has had a significant impact on other areas of science. Examples include applications developed for health and medicine, photon detection devices, neutron scattering, muon spin relaxation, large-scale computing and a variety of other fields. Opportunities for improving connections in overlapping areas and for leading interdisciplinary efforts have to be embraced. To this end, the DOE Office of High Energy Physics has recently charged a “Science Connections” task force with highlighting the scientific areas where particle physics advances, informs and benefits from other DOE Office of Science programs. Such a report has not been attempted since the 1998 National Academy EPP Decadal Survey [9].

10.4.3 Survey results

In the spring 2013 CE&O survey two questions asked how particle physicists thought their colleagues in other fields perceived their field, and asked their opinions on the best way to broaden support for particle physics [1]. A large majority, 81%, thought that particle physics is well or highly regarded by colleagues in other fields while only 17% felt other scientists were indifferent or regarded particle physics negatively. A plurality, 46%, felt that exploring interconnections with other disciplines combined with engaging in stronger outreach to scientists in other fields are two of the best ways to broaden support.

10.4.4 Particle physics inreach/outreach discussions

Following discussions at the planning meeting held at Fermilab, October 10-12, 2012, we had several informal and formal discussions within the particle physics community and with non-particle physicists at meetings such as the AAAS meeting in Boston, February 2013, and the 2013 APS spring meetings in Baltimore and

Denver. We held discussion sessions at the April APS meeting in Denver with invited speakers and a meeting at Fermilab regarding particle physics inreach and outreach.

We organized three discussion sessions at the meeting in Minneapolis. The first was a panel session on “Fostering Interconnections and Common Causes with Scientists in other Fields.” This panel had a mix of particle physicists and condensed matter, astronomy, astrophysics and theoretical physics colleagues. At this session discussions led to ideas for future interactions between particle physics and scientists in other fields: (1) first and foremost there has to be mutual respect; (2) there are many opportunities for interconnections and interdisciplinary collaborations; (3) particle physics is often a technology incubator for other sciences; (4) particle physics has a lot to give and take provided that we exert the time and effort to break down barriers; (5) all fields in the United States are facing serious funding problems, and supporting each other will achieve better results for all sciences.

The second session was on “Particle Physics Inreach and Communication Across the Three Frontiers of Particle Physics.” The goal for this session was not only to discuss challenges and strategies for communication across frontiers within particle physics but to also facilitate a discussion on the need for all frontiers to weave a more unified story for particle physics.

Many within the community have voiced concern regarding the Venn diagram used for the visual representation of the three frontiers. While it is a tool for the funding agencies to promote and support a more modern view of the field, those concerned said that the resulting stovepiping threatens to effectively turn the field into three disjointed communities fighting for the same budget. The discussion on this topic elicited comments on the pros and cons of the frontier representation. In the end, there was a consensus that we need analogies such as these, maybe more of them rather than eliminating what we now have, and we need to be careful to use them in a positive way rather than to impose limits. The future success of our physics will require a program that is agile and maximizes the input from all the frontiers, even if in a temporally and spatially asynchronous way.

The third session was a community discussion on the HEPAP P5 prioritization process that is expected to produce a U.S. particle physics roadmap for the next one to two decades, with a report due before the summer of 2014. The particle physics community has been frequently told by representatives of the funding agencies that the community has to buy into the prioritization process and support the resulting plan. This community discussion provided an initial necessary opportunity for the particle physics community to express its views and provide input to the agency leaders and the HEPAP chair on the P5 panel and the process.

As a result of these many meetings, discussion sessions and deliberations, we developed the following strategies and implementation plans to help the particle physics community achieve the overarching CE&O goals.

10.4.5 Strategies for fostering connections and making common cause with other sciences

1. Foster more dialog and understanding among subfields of physics and with other sciences.
2. Identify areas of common cause and unite in support of them.
3. Develop recognition for the need to prioritize critical research directions for the U.S. particle physics program; support the process and the resulting plan.

10.4.6 Implementation ideas

1. Work with APS to create and foster new opportunities for dialog between leaders of physics subfields.
2. Hold combined open sessions at March/April APS meetings to disseminate the better understanding and the identified common causes.
3. Continue the community discussion about the prioritization process so that the members of the community will support the resulting plan irrespective of how their favorite activities fared.
4. With the help of APS, create and foster new opportunities for interaction with fields beyond physics.

10.5 Building public appreciation for particle physics

Modern physics, like ancient physics, is driven by curiosity about the nature of our universe and of the world around us. By engaging the broader community in our research, we share this curiosity beyond the bounds of active physicists. The word “engage” is central: we need to do more than inform the public: we need to engage them. It is critical that the public recognize the value of science in informed societal decision making.

The general public audience helps draw together the other CE&O audiences. Many of the concepts developed for the general public apply to other audiences and successful communication to the general public reinforces communication to other audiences. When we communicate successfully with the general public, they will share their knowledge and enthusiasm with students, teachers, policy makers, the news media, and others.

Successful engagement with the public must be tailored to the audience. We have identified representative audiences meant to cover the range of situations where different engagement strategies and tactics are appropriate. Our messages have been categorized as follows: scientific goals, direct applications, and spinoffs and technology transfer. Each of the categories has a role to play in spanning the range of messages from human curiosity to economic impact.

Within our community, many individuals and groups bring their enthusiasm for particle physics and its fundamental concepts to a wide range of audiences including the general public. They are aided by resources created and made available by their colleagues and communication professionals. These resources also impact other audiences including policy makers, other scientists, teachers, and students.

Among these resources are text materials (in books, brochures, websites), photos, images, animated and real-life videos, PowerPoint-type slides, artifacts and exhibits, recorded talks, hands-on activities, posters, smartphone apps, and art and science projects.

Communicating with the general public has great opportunities and some challenges. Much as we might wish to engage everyone in the general public, our reach is more generally limited to the 20-30% of the public who are generally interested in science. These individuals come to us with very mixed backgrounds ranging from limited science literacy (with knowledge similar to that of a 14-15 year-old) to others who have read significantly about science and have a real foundation. Many come with broad misconceptions about the facts of physics, the nature of our research and even the basic principles of science. Others come with some strong skepticism that challenges our communication skills.

We usually have access to our audiences for very limited times, not the semester of lectures we have in universities. There are therefore strong limits to how much physics we can teach. We can be good at communicating the excitement and the impact of our research. We need to communicate the collaborative

nature of modern research and the broad groups that come together to do experiments. The experiments frequently have collaborators from tens of countries who work together intensively and harmoniously with common goals. Some people are fascinated by the bottom-up nature of a 3000-person experiment, the opposite structure of corporations which also builds massive projects.

We need to portray the scientists who do these experiments as real and ordinary caring people. Communicating the impact of our research beyond particle physics is also essential. The technology transfer of modern physics has transformed the world and had an impact.

We have great stories to tell and should celebrate our successes and accomplishments. The discovery of the Higgs boson is a great story of three almost miraculous accomplishments, and U.S. physicists played vital roles in all three. The first was the development of the theory of the Higgs mechanism almost 50 years before its discovery. It is an inspirational example of the power of science to analyze complex problems. The second was the development of a 17-mile-long accelerator using revolutionary technologies. This accelerator then achieved a fantastic rate of collisions that was essential to make possible this discovery. And finally the enormous detectors achieved incredible levels of precision almost from the beginning, so that they could extract this discovery from the complex phenomena. The computing power required was also quite remarkable. In all aspects, U.S. physicists joined forces with physicists from dozens of countries in a wonderful example of global cooperation in a peaceful venture.

10.5.1 Strategies for building appreciation for the value and excitement of particle physics among the general public

The following strategies will achieve our overarching CE&O goals:

1. Engage the public in a wide range of outreach activities.
2. Make the public aware of direct and indirect applications of research, both historical and potential.
3. Communicate the role and stories of U.S. physicists in particle physics, particularly in major discoveries and in the context of our international collaborations.

10.5.2 Definitions of message categories

- Scientific goals: The quest for understanding the universe, including both the theoretical mysteries and the experimental challenges.
- Applications: What applications might these discoveries have? Historical examples of unexpected applications of fundamental physics discoveries (e.g., quantum mechanics leading to the transistor and laser and special relativity leading to nuclear power).
- Spinoffs and technology transfer: Broader impacts of investing in science: (e.g., the web, grid/cloud based-computing, accelerators, and high-tech workforce).

10.5.3 Audiences within the general public

The message and format we use to engage the general public depends on the individual and on the context of the interaction. Below we establish representative audiences meant to cover the range of situations where different engagement strategies and implementations are appropriate. We are especially able to reach the first four categories.

Popular science enthusiasts are generally already convinced that science is exciting and valuable. They thirst for more information about the science, potential applications and the interaction with those actually doing the science.

Everyday people are engaged through natural social interaction ranging from a dinner party to an airplane flight.

Parents may or may not be comfortable with or appreciative of science themselves, but they often support their child's interest in science and/or a scientific career path.

A geographic audience consists of the populations near our universities and labs to which we have special access and opportunities.

Science skeptics are often misinformed or have misconceptions about the fundamental scientific process. While many may be steadfast in their opinions, some have never been exposed to the counterarguments.

Critics of public funding of science are often indifferent to our scientific goals and seek specific examples of historical success and future prospects.

10.5.4 Survey results

In the spring 2013 CE&O survey 62% of respondents self-identified as being engaged in CE&O activities [1]. While we do not know what fraction of people in the field as a whole are actively involved in such activities, these respondents are considered to be disproportionately active in CE&O activities.

The survey revealed that physicists think that the public is most interested in the potential application of discoveries followed by our scientific goals. The respondents reported that the general public is moderately interested in spin-off applications and least interested in a high-tech workforce, both of which belong to the third message category.

In terms of justification for the cost of particle physics research, the respondents indicated that the general public is most interested in potential direct applications of discoveries, followed by spin-off applications.

The respondents are very comfortable communicating the overarching scientific goals of particle physics, but less comfortable in communicating the messages that are considered most interesting and which best justify the cost of particle physics research. Keeping in mind that the respondents are likely to be more involved in outreach than the field as a whole, this result informs which areas we as a field need to improve our communication.

10.5.5 Existing communication and outreach activities to the general public

The existing activities are extremely broad but with varying levels of support in terms of interest, participation and financial support. The single most common activity (perhaps because it requires limited financial support) is public talks. These occur in schools, clubs, university and lab events, scientific cafes, speaker bureaus and science theater-type events.

Another common activity is participation in open houses and related events such as science festivals, lab and department tours, physics day shows, alumni weekends and workshops for the public. These require further effort but for limited periods.

Other activities take more time and require more resources, but also can have much more impact. These include contributions to external publications and shows. Examples include writing magazine articles and op-ed pieces in newspapers, participating as consultants to radio and television programs and movies and working with the news media. Individuals and institutions have collaborated with museums on exhibitions. In some cases these have included opportunities for the public to try simulated analysis of data.

The production of printed products, web-based materials and multimedia products has been important for existing activities. Printed materials include books, brochures and posters. Websites can deliver news, background information, activities, images and video. The social media (blogs, Facebook, Twitter, YouTube) are an important aspect of a web presence. Labs, experiments and others have developed many videos, photo and image collections, smartphone apps, murals and even a 9,660-piece LEGO model. Physicists use these valuable resources in their individual efforts.

10.5.6 Implementation notes for various audiences

Popular science enthusiasts are generally already convinced that science is exciting and valuable. They thirst for more information about the science and potential applications and the interaction with those actually doing the science. Extending our current activities is the core of this effort. Additional efforts in developing plausible examples of future direct applications are in order.

Everyday people are engaged through the natural social interaction ranging from a dinner party to an airplane flight. Being prepared with a few talking points or one-minute physics explanations is very helpful, as are a handy brochure or similar item to share. The personal narrative and the broader scientific enterprise are effective approaches for engagement for this audience.

Parents may or may not be comfortable with or appreciative of science themselves, but they are often supporting their child's interest in science and/or evaluating a scientific career path.

- Parents who are comfortable with science may wish to be more informed or have a point clarified so that they can directly educate their children.
- Those who are less comfortable with science may wish to be informed so that they can better connect with a scientifically enthusiastic child (tactics: understanding the universe, famous/great scientists, fancy gadgets, etc).
- Others may not want to be engaged directly but may collect materials for their children.

Science skeptics are often misinformed or have misconceptions about the fundamental scientific process. While many may be steadfast in their opinions, some have never been exposed to the counter arguments. We need to meet these people where they are, in the movie theater, shopping mall or farmer's market. We need new ways to attract their attention, capture their interest, and engage them.

Critics of public funding of science are often often indifferent to our scientific goals and seek specific examples of historical success and future prospects. They often start by asking about direct applications, though one can pivot the conversation to spinoffs. Historical examples can be powerful in making the point of direct applications; however, additional efforts in developing plausible examples of future direct applications are in order.

10.6 Increasing the presence of particle physics everywhere students learn

An important resource for a healthy particle physics community is a talented and diverse workforce. Whether a student's career path leads them to graduate school and research or into other STEM-related fields, particle physics provides an excellent training ground.

Working with learners of all ages, from school children to interested seniors, increases their excitement and interest in science. This engagement leads to better appreciation of the societal value of basic research, gaining advocates for our field. It also helps develop knowledge and critical thinking to help people make informed decisions in their daily lives on technological issues that affect society as a whole.

While particle physics generates a large degree of public interest, federal support for the field is showing a discouraging trend. We feel a public that is more scientifically literate will be more willing to support the costs of this field. A June 2013 survey of American attitudes toward science [10] indicates that 74% of respondents answered "Strongly agree" (32%) or "Somewhat agree" (42%) to the question:

Even if it brings no immediate benefits, basic scientific research that advances the frontiers of knowledge is necessary and should be supported by the federal government.

The survey indicates broad support of the *idea* of federal support for basic research. The field must seek ways to turn this support of the idea of federal support into *increased* support.

The three overarching goals for the CE&O frontier articulate all of these elements. One way to realize these goals is an increased presence of particle physics concepts everywhere students learn: in school, at home, in the community and on the Web. Educators and students should learn directly from research scientists about their exciting work, using tools that are grounded in education research. There should be vertical integration of opportunities for students to be exposed to particle physics, in line with the vertical integration of the Next Generation Science Standards, from elementary school through high school [11]. These opportunities should continue through the undergraduate level, with particle physics "hooks" in introductory, education, and other non-major courses, and research opportunities not only for future physicists but for future and current teachers.

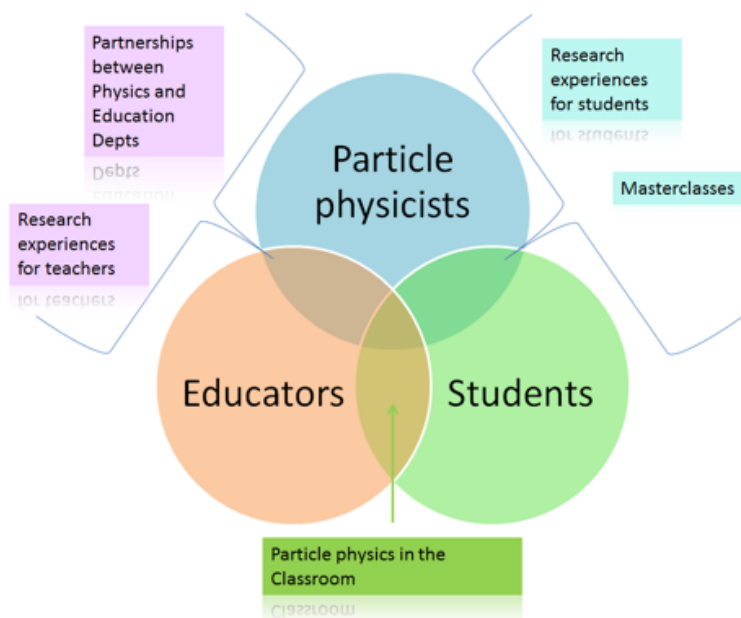


Figure 10-1. *A vision for inviting students and educators into the particle physics community.*

10.6.1 Achievements since Snowmass 2001

The particle physics community has played an important role in workforce development with considerable progress in the past ten years. Opportunities for students to learn about physics by doing physics have blossomed as student enrollment at all levels has been increasing. Particle physics-related programs such as QuarkNet have been at the forefront.

K-12 education

From 2001 to 2009, the number of students enrolled in physics in high school has grown from approximately 918,000 to approximately 1,350,000; while the percentage of girls has gone down slightly, the overall number of girls as well as boys has increased substantially [12]. AP Physics enrollment has also grown. At the same time, there is room for more growth, especially among women and minorities, which should start at the elementary level.

Particle physics offers tremendous opportunities for high school students. In QuarkNet, a long-term program embedded in the particle physics research community, students experience a research environment through summer research and cosmic-ray detector programs; they analyze particle physics data as physicists do in the International Masterclasses, e-Labs, and direct manipulation of 4-vectors in csv files.

The demand for physics high school teachers is enormous. Of all sciences taught at the secondary level, physics has the greatest shortage of qualified teachers: fewer than half of the 27,000 high school physics teachers in the U.S. have completed a major or minor in physics or physics education [13]. The particle physics community has made progress in creating meaningful professional development for teachers and career enhancement through research opportunities.

Undergraduate education

The number of physics bachelor's degrees awarded in the United States has been increasing steadily. According to an AIP report, the academic year 2010-11 produced new all-time highs for physics bachelor and Ph.D. degrees conferred in the United States [14].

There has been great progress in research on physics education for undergraduates. Universities have access to curricula based on this research and best practices. These, when combined with the many available electronic aids, can have a positive impact on retention and student success. Faculty members are aided by the peer-reviewed journal *Physical Review Special Topics - Physics Education Research*. Dissemination of successful programs and important findings also occurs through the American Association of Physics Teachers (AAPT) and APS.

Reform of undergraduate physics education can be successful with support from physics departments, both in terms of the leadership and the culture of the department. Particle physicists can and do play a role in such reforms through their own efforts and through their collaboration with teachers in education and outreach.

Successful efforts

The particle physics community in the United States and abroad has had success in increasing student interest and achievement in STEM fields, including particle physics. Examples include:

- The successful University of Illinois undergraduate physics education reform initiative was recognized for its achievement with the APS 2013 Excellence in Physics Education Award [15].
- With 53 centers at universities and labs across the country, QuarkNet has put cosmic-ray detectors with online analysis tools and substantial data from LHC into the hands of high school students and teachers. Independent evaluation data from the long-term teacher professional development program show it has changed how many teachers see science and education. They also indicate it has helped students develop aspects of science literacy such as an understanding of how scientists make discoveries and talk about their work. Students develop this understanding through research internships, participating in masterclasses or conducting scientific investigations using e-Labs.
- Netzwerk Teilchenwelt in Germany is one example where the QuarkNet model was adapted for students, teachers, and physicists in another country.
- The International Particle Physics Outreach Group (IPPOG) has successfully sponsored International Masterclasses for high school students since 2006. In 2013 there were 161 masterclasses in 37 countries, including 29 masterclasses from nine countries in the Fermilab-based portion of the program.
- In partnership with educators and physicists, the Fermilab Education Office has run successful education programs with particle physics and related physical science content for over 30 years.
- The Contemporary Physics Education Project has brought together physicists and educators to create wall charts, posters, websites (e.g., ParticleAdventure.org and UniverseAdventure.org), and activities. The first efforts were in particle physics, and more recently in fusion and nuclear physics and cosmology.

10.6.2 Issues and concerns

Funding

The direct support of outreach efforts by particle physicists has been threatened in the 2014 budget by recent federal language to move support for K-12, undergraduate, and informal education programs from the agencies into three federal entities with very broad agendas. If this decision stands, it will threaten QuarkNet and other programs with DOE funding that connect scientists directly with students and teachers.

Technology

The omnipresence of digital devices requires the particle physics community to explore the effective use of technology to deliver its messages. In addition to 1990's era webpages, the community must consider utilizing social media, mobile applications, data portals, video conferencing and other, not-yet realized tools as ways to carry out its outreach efforts. Particle physics was at the vanguard of the Web in the early 1990's; CE&O should explore effective uses of this rapidly-evolving technology.

Recognition for outreach efforts

Young faculty members face many pressures on the path to tenure. These pressures often crowd out the important work of outreach. Physicists at other stages of their careers face different pressures. The particle physics community, and especially the university community, must change its culture such that — both real and perceived — are lowered for engagement in outreach efforts.

Physics education

The number of students enrolled in a high school physics course tripled from 1990-2005. If enrollment trends continue, the nation will need more people teaching physics. A recent survey of high school physics teachers asks what “Resources [they] use to find answers about physics content.” The top three resources were: college physics textbooks, the World Wide Web, and high school physics textbooks. The item “Asking research scientist acquaintances” was down by an order of magnitude from these three.

Improving physics instruction starts with helping teachers comprehend more about the field. This help can come from workshops and programs that build a teacher's instructional capacity and allow the teachers to learn from each other and from physicists.

Diversity

In spite of numerous efforts, physics is still not a very diverse field. While women now comprise approximately 23% of younger particle physicists [16]), that number is still well below 50%, and overall the number of women in the field is still 14%.

While there are many more girls taking high school physics, there are numerous reasons that they choose not to pursue physics majors in college. Working with girls in middle school and high school to introduce them to the range of careers available with a physics degree, and to female role models in physics, may aid in increasing the numbers entering the field.

The number of students from underrepresented minorities in the field is still too low for meaningful statistics. Clearly, then, we need to attract more of these students into particle physics. In order to attract more students from underrepresented minorities, it is critical to work with the students in elementary school to strengthen their mathematics skills, and to interest students in pursuing mathematics and science courses and careers in STEM. We should then continue to provide opportunities for the students to engage science in an authentic, meaningful and exciting way.

10.6.3 Strategies and implementation

We encourage the direct engagement of physicists with students and educators at all levels from elementary through undergraduate. Particle physicists should invite educators and students into their unique community. Particle physicists should be involved in and support local, national and world-wide efforts that:

- Offer long-term professional development and training opportunities for educators (including pre-service educators), aligned with current and appropriate standards and enabling educators to explore best-practice teaching methods. Make an effort to collaborate with local schools of education whenever possible.
- Create learning opportunities for students of all ages, including classroom, out-of-school and online activities that allow students to explore particle physics to construct their own understanding and develop the skills and habits of mind necessary to perform research.

The above strategies address all three goals of the CE&O frontier through scientific literacy for all students and through inspiring and exciting the next generation of students entering particle physics and other STEM careers. These strategies might require commitments from physicists ranging from 15 minutes for an Ask-a-Scientist segment with a middle school student to a summer mentorship with a high school or college student to a multi-year relationship with a local teacher. All such commitments are equally valued and should be made as seamless as possible for the physicist.

In order to fully realize these strategies, we call for the following implementation elements:

Programs

Extend existing educator networks (e.g., QuarkNet, Modeling, PTRa) and create additional professional development opportunities to educators (including undergraduate students studying to be educators) who present science at all grade levels, especially those who teach physics. QuarkNet is an excellent example of a program that fosters the connections between particle physicists and physics teachers, and, through the teachers, with students. It is successful in large part because it provides an infrastructure and framework that (1) fosters local learning communities of scientists and teachers doing science together, and (2) conveys an early understanding of the international nature of contemporary physics research to teachers and students. New programs will be piloted to expand professional development opportunities for teachers and reach more students.

Create programs that use examples from particle physics and experimental data to allow educators and students to develop skills, concepts and scientific habits of mind. Examples of such programs are University of Maryland's modern physics summer course for high school girls, and International Particle Physics Outreach Group (IPPOG) masterclasses where students analyze LHC data. Research skills workshops and research experiences for teachers, high school students and undergraduates are also important.

Align and collaborate with national efforts to improve diversity in STEM fields in order to broaden their reach in physics. In order to have a healthy particle physics workforce in the future that more closely represents the diversity of the general public, students from groups traditionally underrepresented in physics need to be engaged and encouraged in elementary school. While direct content in particle physics may not be appropriate at this level, a general introduction to physics concepts can be. Several programs grounded in research on how students learn have gained national recognition in the past decade. These programs have not yet incorporated physics. We propose to partner with national and regional programs, for example SciGirls and GEAR-UP, to encourage them to include more physics content in their programs and materials. We

will make those resources and programs known and available to interested members in the particle physics community.

Material resources

Produce learning materials that support the programs above, and materials for scientists that assist them in reaching a variety of audiences. Groups such as the Contemporary Physics Education Project have produced particle physics posters and wall charts (including the latest on the Higgs discovery) and award-winning websites, such as the *Particle Adventure* [17], primarily through the effort of a few dedicated scientists and educators. Such efforts should be greatly extended to produce a variety of materials for a range of audiences, and these materials should be made available to scientists who wish to do outreach in their local communities. In addition, these materials should be made available to educators at both the K-12 and college levels to facilitate bringing best practices in physics education research to physics courses at all levels.

Training

Create workshops for scientists who want to engage educators and students. A variety of workshops should be made available to scientists who want to do outreach to schools or work with teachers, in order to keep them updated on education research and best practices, communication skills, etc. These could be coordinated in partnership with the APS and other professional organizations and made available to physicists in all fields.

Resources

Implementing these strategies requires a national infrastructure to provide an organizational structure and resources for scientists wishing to engage students and teachers as well as the general public. *Establish such an organization within the particle physics community.* A national outreach office with a small, few-person national office could serve as a liaison office and resource center for particle physicists wishing to reach a variety of audiences. With the help of IT specialists and graphic artists, either in-house or outsourced, the staff would, for example, develop material that scientists could use for public lectures or school visits, offer training to scientists and partner with other organizations to develop diversity. Staff would facilitate collaborations among universities, national laboratories, and professional and international organizations to apply for funding for longer-term programs such as professional development and student enrichment programs.

10.7 Conclusion

As a result of the Community Summer Study process, the community has recognized that more physicists must engage in CE&O activities, and that the quality and coordination of such activities must be improved, to increase public support for the field, develop the next generation of physicists, and ensure scientifically literate and engaged citizens.

A number of prominent voices reinforced the need for renewed commitment to CE&O at the Minneapolis meeting:

- We must educate our representatives in Congress, our fellow citizens, the business community and the scientific agencies. — D. Gross
- You are underselling yourselves... you are technology incubators for other fields of science. — R. Roser
- The media missed the substantial impact of the U.S. on the Higgs discovery. — J. Incandela

- You need to appeal to varied stakeholders to convince them that you do valuable science with a sensible plan. Illustrate the benefits of high-energy physics to society. — G. Blazey

The spring 2013 CE&O survey of 641 members of the U.S. particle physics community identified the greatest barriers to participation in CE&O activities as lack of time, little reward in terms of career advancement, and a lack of resources to communicate the broader societal impacts of particle physics research. Through collaboration with physicists, professionals in the areas of communication, government relations and education can expand the number of people engaged in education and public outreach by addressing these barriers.

Additional personnel and resources dedicated to nationwide coordination, training, resource development, evaluation, and support are required to achieve the goals and strategies outlined above. Such a team would enhance existing efforts and spearhead new initiatives such as: developing a comprehensive central CE&O resource for physicists; developing sustainable methods to collect statistics on workforce development and technology transfer; producing materials to inform the public about direct and indirect applications of particle physics; and creating professional development opportunities for educators and new learning opportunities for students of all ages.

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Glossary

$0\nu\beta\beta$: Neutrinoless double-beta decay, a nuclear decay process in which two electrons and zero neutrinos are emitted.

$2\nu\beta\beta$: Double-beta decay, a nuclear decay process in which two electrons and two neutrinos are emitted.

2HDM : Two-Higgs-Doublet Model, a model with two Higgs fields, usually with natural flavor conservation in Higgs boson couplings.

AAAS : American Association for the Advancement of Science.

ACTA : Augmented Cherenkov Telescope Array, the next-generation atmospheric Cherenkov telescope project for gamma ray detection.

ADMX : Axion Dark Matter Experiment, an experiment at the University of Washington searching for axions using a large electromagnetic cavity in a static magnetic field.

ADS : Accelerator-Driven Systems, a technology for driving nuclear reactors with particle beams.

AGILE : Astro-Rivelatore Gamma a Immagini Leggero, a space-based X-ray and gamma ray observatory.

AGN : Active Galactic Nucleus.

AIP : American Institute of Physics.

ALCF : Argonne Leadership Computer Facility.

ALICE : A Large Ion Collider Experiment, an experiment at the **LHC** focusing on heavy ion collisions and the quark-gluon plasma.

ALP : Axion-Like Particle.

ALPS, ALPS-II : Any Light Particle Search, a series of experiments at **DESY** searching for **axions** and similar particles using laser light conversion in a strong magnetic field.

AMANDA : Antarctic Muon and Neutrino Detector Array, the first-generation neutrino telescope experiment in Antarctica.

AMR : Adaptive Mesh Refinement.

AMS, AMS-02 : Alpha Magnetic Spectrometer, an antimatter detector located on the International Space Station.

ANITA : Antarctic Transient Antenna, a balloon experiment in Antarctica for detection of radio signals of ultra-high-energy neutrino events.

ANL : Argonne National Laboratory.

ANTARES : Astronomy with a Neutrino Telescope and Abyss environmental RESearch, a neutrino telescope experiment in the Mediterranean Sea.

APEX : A Prime EXperiment, an experiment at **JLab** searching for the production of dark photons using an intense electron beam.

APS : American Physical Society.

ARA : Askaryan Radio Array, a radiofrequency neutrino antenna experiment at the South Pole.

ArgoNeuT : Argon Neutrino Teststand, a small liquid argon **TPC** experiment at **Fermilab**.

ARIANNA : Antarctic Ross Ice-shelf ANtenna Neutrino Array, a radiofrequency neutrino antenna experiment in Antarctica.

ART : An Intensity Frontier software framework developed by the **Fermilab** Scientific Computing Division and used by **NOvA**, **mu2e**, **Muon $g - 2$** , **LArSoft**, **DarkSide**, **LArIAT**, and others.

ASIC : Application Specific Integrated Circuit, a silicon chip processor designed for a particular purpose.

ASTA : Advanced Superconducting Test Accelerator, an accelerator test facility at **Fermilab**.

ATCA : Advanced Telecommunications Computing Architecture.

ATF, ATF2 : Accelerator Test Facility, an accelerator test complex at **KEK** built to study beam dynamics issues for the *ILC*.

ATLAS : A Toroidal LHC ApparatuS, a large experiment at the **LHC** optimized for high transverse momentum particle production.

ATR : Advanced Test Reactor, a research reactor at the Idaho National Laboratory.

Auger : Pierre Auger Observatory, experiment in Argentina that detects ultra-high-energy cosmic rays through both fluorescence and surface water Cherekov detectors.

Axion : A hypothetical, very light particle, with couplings to quarks that reverse under **P** transformations. Such particles could make up the dark matter of the Universe.

BaBar : An experiment at **SLAC**, using e^+e^- annihilation at approximately 10 GeV to study rare heavy quark and lepton decays.

Baikal : A neutrino telescope in Lake Baikal in Siberia.

Baksan : An underground laboratory in the Caucasus mountains in Russia.

BAO : Baryon Acoustic Oscillations.

Baseline : In neutrino physics, the distance between the neutrino production point, near an accelerator, and the neutrino detector.

BELLA : Berkeley Lab Laser Accelerator, a facility at **LBNL** for the development of laser-driven accelerators.

Belle : An experiment at **KEK**, using e^+e^- annihilation at approximately 10 GeV to study rare heavy quark and lepton decays.

Belle-II : An experiment at **KEK**, using e^+e^- annihilation at approximately 10 GeV to study rare heavy quark and lepton decays with 50-100 times the data set produced by **BaBar** and **Belle**.

BESIII : An experiment at **IHEP**, using e^+e^- annihilation at approximately 3-4 GeV to study charm quark and tau lepton physics.

BEST : Baksan Experiment on Sterile Transitions, a proposed radioactive source experiment in Russia with a gallium detector.

BNP : Budker Institute for Nuclear Physics, a high-energy physics laboratory in Novosibirsk, Russia.

BNB : Booster Neutrino Beam , a neutrino beamline at **Fermilab** using the Booster.

BNL : Brookhaven National Laboratory.

Borexino : A solar neutrino experiment at **Gran Sasso**.

Boson : A type of elementary particle whose intrinsic spin is an integer (0, 1, 2, ...) multiple of $\hbar$. Such particles can make up a macroscopic force field such as the electromagnetic field.

BOSS : Baryon Oscillation Spectroscopic Survey, a galaxy survey aiming to measure baryon acoustic oscillations.

BR : Branching Ratio, the probability that an unstable particle decays to a particular final state.

BSM : Beyond the Standard Model, a reference to new physics associated with an extension of the **SM**.

C : Charge conjugation, the interchange of particles and antiparticles.

CALICE : CALorimeter for Linear Collider Experiment, an experimental collaboration aimed at improving the technology of hadron calorimeters, especially with the **PFA** method.

Canfranc Underground Laboratory : An underground scientific facility in a former railway tunnel in the Spanish Pyrenees under Monte Tobazo in Canfranc, Spain.

Capability : In computing, refers to high-speed, highly parallel computing that might require a large fraction of a supercomputer.

Capacity : In computing, refers to computing in which many moderately parallel jobs are run alongside one another.

CAPTAIN : Cryogenic Apparatus for Precision Tests of Argon INteractions, a liquid argon R&D detector.

CAST : CERN Axion Solar Telescope, an experiment at **CERN** searching for **axions** radiated from the Sun.

CC : Charged Current weak interactions.

CCD : Charge Coupled Device, a class of pixel silicon detectors.

CDF : Collider Detector at Fermilab, a large experiment at the **Fermilab** Tevatron optimized for high transverse momentum particle production.

CDM : Cold Dark Matter, a class of dark matter models in which the dark matter particles move at nonrelativistic speeds.

CDR : Conceptual Design Report.

Ce-LAND : A ^{144}Ce source to be placed in **KamLAND** to study the reactor neutrino anomaly.

CENNS : Coherent Elastic Neutrino-Nucleus Scattering, an **NC** neutrino process and also a proposed experiment to be sited at the **BNB**.

CE&O : Communication, Education, and Outreach, a topic of one of the “frontiers” in this study.

CERN : Conseil Européen pour la Recherche Nucleaire, the major European high energy physics laboratory, located in Geneva.

CESR : Cornell Electron Storage Ring, an e^+e^- colliding beam accelerator at Cornell University that operated in the energy range 3.5-12 GeV from 1979 to 2008. The accelerator is still in operation as a synchrotron light source (CHESS) and as an accelerator physics testbed.

CesrTA : CESR Test Accelerator, a configuration of **CESR** to study the design of electron damping rings, in particular, for **ILC**.

Chameleon : A new particle that has properties that depend on its environment.

CHIPS : Cherenkov detectors In mine PitS, a proposed experiment to use the **Fermilab** beams and massive Cherenkov detectors in flooded mine pits.

CHOOZ : A first-generation reactor neutrino experiment located in Chooz, France.

CKM : Cabibbo-Kobayashi-Maskawa matrix, the matrix relating the weak interaction and mass eigenstates of quarks.

CL : (Statistical) Confidence Level.

CLEAN : Cryogenic Low Energy Astrophysics with Noble gases, a cryogenic noble liquid experiment for dark matter and solar neutrinos.

CLEO : A general-purpose particle detector at **CESR** studying heavy quark decays and spectroscopy.

CLFV : Charged Lepton Flavor Violation.

CLIC : Compact Linear Collider, a concept for an e^+e^- linear collider, with center of mass energies up to 3 TeV, based on two-beam acceleration.

CM : Center of Mass, the system for viewing a particle collision or decay in which the overall system is at rest.

CMB : Cosmic Microwave Background, the approximately isotropic microwave radiation in the universe created in the original formation of atoms from electrons and ionized protons.

CMS : Compact Muon Spectrometer, a large experiment at the **LHC** optimized for high transverse momentum particle production.

CoGeNT : COherent GERmanium Neutrino Technology, a germanium detector for dark matter and other signals requiring low background.

COMET : An experiment at **J-PARC** searching for muon to electron conversion in the field of a heavy nucleus.

COUPP : Chicagoland Observatory for Underground Particle Physics, a dark matter detector using a bubble chamber, now located in the Sudbury Mine in Sudbury, Ontario.

CP : The combination of a **C** and a **P** transformation, converting particles to antiparticles, plus mirror (left-right) reflection. This is a very accurate, but not perfect, approximate symmetry of nature.

CPAD : Coordinating Panel for Advanced Detectors, an advisory panel on detector technology created by the **APS DPF**.

CPT : The combination of a **C**, and a **P**, and a **T** transformation, converting particles to antiparticles, plus mirror (left-right) reflection, plus reversal of the direction of time. Local quantum field theory predicts this to be a perfect symmetry of nature.

CPU : Central Processing Unit of a computer.

CRADA : Cooperative Research and Development Agreement, an agreement between a government laboratory and a private company to pursue R&D on a technology or project.

CSI : Coherent Scattering Investigations at the SNS, a proposed **CENNS** search experiment for the **SNS**.

CTA : Cherenkov Telescope Array, a planned large-area array of telescopes for high energy gamma rays.

CTF3 : CLIC Test Facility 3, the most recent in a series of test accelerators for **CLIC** at **CERN**.

CUDA : Compute Unified Device Architecture, which defines a parallel computing architecture for NVIDIA GPUs.

CUORE : Cryogenic Underground Observatory for Rare Events, an experiment searching for neutrinoless double beta decay, located at **Gran Sasso**, Italy.

DØ : D-zero, a large experiment at the **Fermilab** Tevatron optimized for high transverse momentum particle production.

DAEδALUS : Decay At rest Experiment for δ_{CP} studies at the Laboratory for Underground Science, a neutrino oscillation experiment based on beams created by cyclotrons.

DAMA : DArk MATter experiment, a scintillator-based dark matter search experiment at **Gran Sasso**.

DAMIC : DArk Matter In CCD experiment, an experiment at **Fermilab** searching for light dark matter particles.

DANSS : Detector of the reactor AntiNeutrino based on Solid-state plastic Scintillator, a reactor neutrino experiment in Russia.

Dark Light : An experiment at **JLab** searching for dark photons using an **FEL**.

DarkSide : A dark matter search experiment at **Gran Sasso** using a liquid argon detector.

Daya Bay : A reactor neutrino experiment located near Daya Bay, China.

DeepCore : A low-energy extension to the **IceCube** experiment with a high density of photodetectors in a central region of the cube.

DES : Dark Energy Survey.

DESI : Dark Energy Spectroscopic Instrument.

DESY : Deutsches Elektronen SYNchrotron, the major high energy physics laboratory in Germany, located in Hamburg.

DIRC : Detection of Internally Reflected Cherenkov light, a detector using quartz bars for tracking and particle identification.

DIS : Deep Inelastic Scattering, a process of lepton scattering from a nucleon or nucleus with large momentum transfer, especially when only the lepton recoil is observed.

DM : Dark Matter.

DOE : U.S. Department of Energy.

Double Chooz : A reactor neutrino experiment in Chooz, France, utilizing detectors at two distances from the source.

DPF : Division of Particles and Fields, a division of the American Physical Society.

DREAM : Dual REAdout Method, a method for hadron calorimetry that corrects the charge/neutral response ratio by separate measurement of Cherenkov and scintillation light.

Drive beam : A high energy particle beam used to create an electromagnetic field that can then accelerate another beam to high energy.

DSNB : Diffuse Supernova Neutrino Background.

dSphs : dwarf Spheroidal satellite galaxies of the Milky Way.

EAS : Extensive Air Shower, produced by high energy cosmic rays in the Earth's atmosphere.

EBL : Extra-galactic Background Light.

ECHo : Electron Capture ^{163}Ho experiment, a proposed neutrino mass microcalorimeter experiment.

EDM : Electric Dipole Moment.

EFT : Effective Field Theory, a method of quantum field theory in which the effects of new particles or interactions that might be present at high energies are expressed by the addition of more complex operators to the equations of motion of the **SM**.

ESA : European Space Agency.

ESS : European Spallation Source, a future facility in Lund, Sweden.

ESS ν SB : European Spallation Source Neutrino Super Beam, a proposal to use the **ESS** proton linac to generate a neutrino superbeam.

$\cancel{E}_T$: Missing Transverse Energy, unobserved momentum in a high-energy particle collision carried away by weakly interacting neutral particles.

Euclid : A dark energy space mission currently under development by the European Space Agency.

EVA : ExaVolt Antenna, a proposed balloon-based neutrino antenna experiment in Antarctica.

EW : ElectroWeak interaction, the unified description of the electromagnetic and weak interactions.

EWSB : ElectroWeak Symmetry Breaking.

Exascale : Of the order of 10^{18} , used in computing to refer to next-generation computation resources in memory or speed.

EXO : Enriched Xenon Observatory, an experiment searching for neutrinoless double beta decay of the isotope Xe^{136} .

FACA : Federal Advisory Committee Act, the legislation that governs questions of openness and transparency for certain Federal committees. The act generally covers grant review panels of the **NSF** but not those of the **DOE**.

FACET : Facility for advanced ACcelerator Experimental Tests, a user facility at **SLAC** for experiments on high-gradient electron accelerator technology.

FCNC : Flavor-Changing Neutral Current.

FEL : Free Electron Laser, an electron accelerator that produces high-intensity coherent synchrotron radiation.

Fermi : Femtometer = 10^{-13}cm .

Fermi-LAT : Fermi Large Area Telescope, a space-based gamma-ray detector.

Fermion : A type of elementary particle whose intrinsic spin is a half-integer ($\frac{1}{2}, \frac{3}{2}, \dots$) multiple of $\hbar$. Such particles can form rigid structures of matter such as atoms and atomic nuclei.

Fermilab : Fermi National Accelerator Laboratory, in Batavia, Illinois.

FFAG : Fixed-Field Alternating Gradient accelerator, an accelerator design concept with time-independent magnetic bending fields and strong focusing.

FPGA : Field Programmable Gate Array, a type of programmable integrated circuit.

FNAL : Fermi National Accelerator Laboratory, or **Fermilab**.

FRIB : Facility for Rare Isotope Beams, a linear accelerator at Michigan State University for isotope production and nuclear structure research.

FrPNC : Francium Parity Non-Conservation, at experiment at **TRIUMF** to measure **P**-violating atomic transitions in francium atoms.

FSR : Final State Radiation, radiation from a lepton, quark, or gluon produced in a high-energy scattering process.

GALLEX : GALLium EXperiment, a radiochemical solar neutrino experiment located at **Gran Sasso**.

GammeV-CHASE : An experiment at **Fermilab** to search for **axions** using a laser beam in a strong magnetic field.

GAPS : General Antiparticle Spectrometer, an experiment to detect anti-matter, especially deuterons, produced by annihilating dark matter particles.

GC : Galactic Center.

GDE : Global Design Effort, the collaboration responsible for the **ILC** Technical Design Report.

GEANT4 : A library of simulation programs describing the passage of high-energy particles through matter.

GEM : Gas Electron Multiplier, a type of **MPGD** ionization detector.

GEMMA : Germanium Experiment for measurement of the Magnetic Moment of Antineutrino, a neutrino magnetic moment experiment at the Kalinin nuclear power plant in Russia.

GERDA : Ge experiment searching for neutrinoless double beta decay.

GeV : Giga-electron Volt (10^9 eV), the energy scale of the proton mass and the subnuclear strong interactions.

GIM : Glashow-Iliopoulos-Maiani mechanism, a method for coupling quarks to the weak interactions that avoid flavor-changing **NC** processes, realized in the **SM**.

GLACIER : Giant Liquid Argon Charge Imaging Experiment, a proposed large liquid argon detector in Europe.

GNO : Gallium Neutrino Observatory, a radiochemical solar neutrino experiment at **Gran Sasso** (successor to **GALLEX**).

GPU : Graphics Processing Unit.

GR : General Relativity, Einstein's theory of gravity.

Gran Sasso : An Italian national laboratory under a mountain of the same name, about 120 km from Rome.

Grid : In computing, a network of distributed computer and storage resources. Typically, it refers to a network in which users need not know at which node the processor or data they are using resides.

GUT : Grand Unified Theory, a unified theory of all microscopic particle interactions.

GZK neutrinos : Greisen-Zatsepin-Kuzmin neutrinos, produced by ultra-high energy cosmic ray protons scattering off **CMB** photons.

HALO : Helium And Lead Observatory, lead-based supernova neutrino detector at **SNOLAB**.

HAWC : High-Altitude Water Cherenkov, a gamma-ray detector currently operating in Mexico.

HE-LHC : High-Energy LHC, a proposed stage of the **LHC** in which the bending magnets are replaced by higher-field magnetics, to create pp collisions at center-of-mass energies of 26–33 TeV.

H.E.S.S. : High Energy Stereoscopic System, a telescope for high-energy gamma rays seen as air showers in Cherenkov radiation, located in Namibia.

HEP : High-Energy Physics, the generic term for the areas of research described in this report.

HEPAP : High-Energy Physics Advisory Panel, a panel convened by the U.S. **DOE** and **NSF** to advise the federal government on high-energy physics research.

HERA : Hadron Elektron Ring Anlage, an electron-proton and positron-proton collider at **DESY** that operated from 1990 to 2007.

HETDEX : Hobby-Eberly Telescope Dark Energy Experiment.

HFIR : High Flux Isotope Reactor, reactor facility at Oak Ridge National Laboratory.

HHCAL : Homogeneous Hadron Calorimeter, a concept for hadron calorimetry based on total absorption of hadrons in large crystals equipped with dual readout (see **DREAM**).

- HIGS** : High Intensity Gamma-ray Source, a free electron laser at Duke University.
- HiRes** : High Resolution Fly’s Eye cosmic ray experiment in Utah.
- HLbL** : Hadronic Light-by-Light scattering contributions to the muon anomalous magnetic moment.
- HL-LHC** : High Luminosity LHC, the highest-luminosity phase of the **LHC** program, with plans to collect 3000 fb^{-1} of data between 2022 and 2030.
- HPC** : High Performance Computing, such as that done using supercomputers.
- HPS** : Heavy Photon Search, an experiment at **JLab** searching for the decay of dark photons to e^+e^- .
- HSC** : Hyper-Suprime Cam, wide-field camera for the Subaru telescope.
- HTC** : High Throughput Computing, i.e, data-intensive computing.
- HTS** : High-Temperature Superconductivity.
- Hyper-K** : Hyper-Kamiokande, a proposed large water Cherenkov detector at Kamioka in Japan.
- IACT** : imaging Atmospheric Cherenkov Telescope.
- IAEA** : International Atomic Energy Agency.
- IAXO** : International Axion Observatory.
- IBD** : Inverse Beta Decay, usually referring to the reaction $\bar{\nu}_e + p \rightarrow e^+ + n$.
- ICAL** : Iron CALorimeter atmospheric neutrino experiment at **INO**.
- ICARUS** : Imaging Cosmic And Rare Underground Signals, a liquid argon neutrino detector located at **Gran Sasso**.
- IceCube** : A neutrino telescope located at the Amundsen–Scott South Pole station in Antarctica.
- IceTray** : A software framework developed and used by the IceCube experiment for both online and offline processing.
- IDS** : International Design Study (for the Neutrino Factory).
- IHEP** : Institute of High Energy Physics of the Chinese Academy of Sciences, the major particle physics laboratory in China, located in Beijing.
- ILC** : International Linear Collider, an electron-positron linear collider with design **CM** energy 500 GeV.
- ILD** : International Linear collider Detector, a large detector proposed for **ILC** and **CLIC**.
- INO** : India-based Neutrino Observatory, a future underground laboratory in Tamil Nadu, India.
- IOPS** : Input/output Operations Per Second.
- IOTA** : Integrable Optics Test Accelerator, an electron storage ring to be constructed at **ASTA** to study nonlinear beam optics.
- IPPOG** : International Particle Physics Outreach Group, a network of science educators and communicators based at **CERN**.

ISIS : Research center at Rutherford Appleton Laboratory near Oxford.

IsoDAR : Isotope Decay At Rest experiment, a proposed cyclotron-based sterile neutrino experiment.

ISR : Initial State Radiation, radiation from a high-energy lepton, quark, or gluon emitted as it scatters from another high-energy particle.

JLab : Thomas Jefferson National Accelerator Facility, in Newport News, Virginia.

J-PARC : Japan Proton Accelerator Research Complex, the laboratory hosting the major Japanese proton accelerator, located in Tokai.

JEM-EUSO : Extreme Universe Space Observatory onboard the Japanese Experiment Module, proposed ultra-high energy cosmic ray detector on the International Space Station.

JUNO : Jiangmen Underground Neutrino Observatory, a proposed large scintillator experiment for reactor neutrino oscillations located in China.

K2K : KEK to Kamioka, the first-generation long-baseline oscillation experiment using beam from **KEK** to **Super-K**.

KamLAND : Kamioka Liquid scintillator ANTineutrino Detector, a reactor neutrino experiment at Kamioka in Japan.

KamLAND-Zen : Zero neutrino double beta decay search, a neutrinoless double beta decay experiment using a Xe-doped balloon deployed in **KamLAND**.

KATRIN : KArlsruhe TRItium Neutrino experiment, an experiment to measure neutrino mass from the endpoint of the tritium beta decay spectrum.

KEK : Ko-Enerugi Kenkyusho, the major high energy physics laboratory in Japan, located in Tsukuba.

KLOE, KLOE-2 : An experiment at **LNF** studying e^+e^- annihilation at energies near 1 GeV.

KM3NET : Multi-Km³ Neutrino Telescope, a future deep-sea neutrino telescope in the Mediterranean sea.

KOTO : An experiment at **J-PARC** to study the rare decay $K_L^0 \rightarrow \pi^0 \nu \bar{\nu}$.

kt : Kilotonne = 10⁶ kilograms.

Λ CDM : The standard model of cosmology in which the Universe consists of known particles, cold dark matter (CDM), and dark energy (Λ).

L/E : Length (**baseline**) / Energy, a figure of merit for neutrino oscillation experiments.

LAGUNA : Large Apparatus studying Grand Unification and Neutrino Astrophysics, a collaborative project to assess the possibilities for a deep underground neutrino observatory in Europe; includes the **GLACIER**, **MEMPHYS**, and **LENA** concepts.

LANL : Los Alamos National Laboratory.

LANSCE : Los Alamos Neutron Science Center.

LAPPD : Large Area Picosecond Photo-Detectors project, a collaboration working to develop large-area flat-panel photon detectors.

LAr : Liquid argon.

LAr1 : A proposal to add additional liquid argon TPCs to the **Fermilab** Booster neutrino beamline.

LAr1-ND : A proposal to add a liquid argon TPC near detector in the **Fermilab** Booster neutrino beamline.

LArIAT : Liquid Argon In A Testbeam, a liquid argon TPC test beam experiment at **Fermilab**.

LARP : LHC Accelerator Research Program, a collaboration of U.S. national laboratories to develop technology for the current and future stages of the **LHC**.

LBNE : Long Baseline Neutrino Experiment: An experiment to measure neutrino oscillations, with beams from **Fermilab** and a detector at **SURF** in South Dakota.

LBNL : Lawrence Berkeley National Laboratory (formerly LBL, Lawrence Berkeley Laboratory).

LBNO : Long-Baseline Neutrino Oscillation experiment, a proposed accelerator-based neutrino oscillation experiment in Europe.

LCF : Leadership Computing Facility, one of the **DOE**'s high-performance computing centers.

LENA : Low Energy Neutrino Astronomy, a proposed next-generation liquid scintillator detector.

LENS : Low Energy Neutrino Spectroscopy, a low-energy indium-based solar neutrino experiment.

LEP, LEP-2 : The Large Electron-Positron collider at **CERN**, which performed precision studies of the Z meson and searches for new phenomena from 1989 from 2000.

LHC : Large Hadron Collider, a large proton-proton collider at **CERN**, with design **CM** energy 14 TeV.

LHCb : LHCb, an experiment at the Large Hadron Collider specialized to measure the production in the forward direction of hadrons containing heavy quarks.

LHeC : Large Hadron-electron Collider, a proposal for a high-energy electron beam colliding with the proton beam of the **LHC** at **CERN**.

LNF : Laboratori Nazionali di Frascati, the leading particle physics laboratory in Italy, located in Frascati, near Rome.

LO : Leading Order, applied to the level of a quantum field theory calculation (see also **NLO**).

LPA : Laser Plasma Acceleration.

LSND : Liquid Scintillator Neutrino Detector, an experiment at Los Alamos National Laboratory searching for sterile neutrinos.

LSO : Lutetium Silicate (Lu_2SiO_5).

LSST : Large Synoptic Survey Telescope.

LUX : Large Underground Xenon detector, a large dark matter detector using liquid Xenon.

LVD : Large Volume Detector, a neutrino observatory at **Gran Sasso** studying low-energy neutrinos from gravitational stellar collapse.

LXe : Liquid Xenon.

LYSO : Lutetium Yttrium Oxyorthosilicate ($\text{Lu}_2(1-x)\text{Y}_2(x)\text{SiO}_5$).

LZ : LUX-Zeplin, a next-generation xenon TPC dark matter detector.

MAGIC : Major Atmospheric Gamma-ray Imaging Cherenkov telescope, a telescope for high-energy gamma rays seen as air showers in Cherenkov radiation, located in the Canary Islands.

MAJORANA : An experiment searching for neutrinoless double-beta decay in Ge, located at **SURF**.

MAMI : MAInzer MIcrotron, a low-energy electron accelerator at the University of Mainz.

Many-core : Refers to computer chips that contain many more cores than **multi-core CPUs**, i.e., more than 16 **CPUs**.

MAPS : Monolithic Active Pixel Sensor.

MCP : Micro-Channel Plate.

MEG : MuEGamma experiment, an experiment at **PSI** searching for the decay $\mu \rightarrow e\gamma$.

MEMPHYS : MEgaton Mass PHYSics, a large water Cherenkov detector proposed for **CERN** or **ESS**.

MESA : Mainz Energy-Recovering Superconducting Accelerator, a proposed electron accelerator at the University of Mainz.

MicroBooNE : Liquid argon TPC experiment in the Booster neutrino beamline at **Fermilab**.

MicroMegas : Micro-Mesh gas detector, a type of **MPGD** ionization detector.

Milagro : A water Cherenkov gamma-ray telescope located near **LANL**.

MINER ν A : Main Injector Experiment for ν -A, a neutrino scattering experiment in the NuMI beamline at **Fermilab**.

MiniBooNE : A short-baseline neutrino oscillation experiment using a mineral oil-based Cherenkov detector in the Booster neutrino beamline at **Fermilab**.

MIND : Magnetised Iron Neutrino Detector, a proposed neutrino factory detector.

MINOS : Main Injector Neutrino Oscillation Search, a neutrino oscillation experiment located at **Soudan** using the NuMI beamline at **Fermilab**.

MKID : Microwave Kinetic Inductance Detector.

MNS : Maki-Nakagawa-Sakata matrix (see **PMNS**).

MOLLER : An experiment at **JLab** to measure the **P**-violating asymmetry in electron-electron scattering.

MPGD : Micro-Pattern Gas Detector.

MPPC : Multi-Pixel Photon Counter.

$\overline{MS}$: Modified Minimal Subtraction, a prescription for removing divergences commonly used in high-precision quantum field theory calculations.

MS-DESI : Mid-Scale Dark Energy Spectroscopic Instrument.

MSSM : Minimal Supersymmetric Standard Model, the simplest (though, not very simple) model that extends the **SM** by the addition of **SUSY**.

MSW : Mikheyev-Smirnov-Wolfenstein effect, the modification of the neutrino oscillation probability as neutrinos pass through matter.

Multi-core : Refers to computer chips that contain up to about 16 **CPUs**.

Muon $g - 2$: An experiment at **Fermilab** to measure the anomalous magnetic moment of the muon.

mwe : Meters water-equivalent, a measure of the depth of an underground detector.

NA62 : An experiment proposed at **CERN** to measure the rare kaon decay $K^+ \rightarrow \pi^+ \nu \bar{\nu}$.

NC : Neutral Current weak interactions.

NERSC : National Energy Research Scientific Computing Center.

NESSiE : Neutrino Experiment with SpectrometerS in Europe, a proposed experiment to search for sterile neutrinos using the **CERN** SPS beam and the **ICARUS** detector.

NEXT : Neutrino Experiment with Xenon TPC, a neutrinoless double beta decay experiment at the **Canfranc Underground Laboratory**.

NF : Neutrino Factory.

NH : Normal Hierarchy.

NIST : National Institute of Standards and Technology.

NLCTA : Next Linear Collider Test Accelerator, an accelerator at **SLAC** now used for tests of advanced accelerator concepts.

NLO, NNLO : Next-to-Leading Order, Next-to-Next-to-Leading Order, terms designating a quantum field theory calculation with two and three terms, respectively, in the power series in the coupling constant.

NLWCP : New, Light, Weakly-Coupled Particles.

NNbarX : An experiment proposed at **Fermilab** to search for neutron-antineutron oscillations.

NOMAD : Neutrino Oscillation MAgnetic Detector, a neutrino oscillation experiment at **CERN**.

NO ν A : NuMI Off-Axis electron-neutrino Appearance experiment, a neutrino oscillation experiment in the **NuMI** beamline at **Fermilab**.

NREN : National Research and Education Network, a high performance network designed for large scale data movement.

NSF : U.S. National Science Foundation.

NUFO : National User Facility Organization, the umbrella group for U.S. national user facility users' organizations.

NuMAX : Neutrinos from Muon Accelerators at Project X, a proposed neutrino oscillation experiment using a muon-storage ring as a source of neutrinos.

NuMI : Neutrinos at the Main Injector, a neutrino beamline at **Fermilab** using the Main Injector, extending to **Soudan** and Ash River, Minnesota.

nuSTORM : Neutrinos from STORed Muons, a proposed short-baseline neutrino experiment to study sterile neutrinos using a muon storage ring as a source of neutrinos.

NVRAM : Non-Volatile Random Access Memory.

OHEP : Office of High Energy Physics of the U.S. **DOE**.

OLCF : Oak Ridge Leadership Computing Center.

OPERA : Oscillation Project with Emulsion-tRacking Apparatus, an emulsion- and tracker-based neutrino oscillation experiment at **Gran Sasso**.

ORCA : Oscillation Research with Cosmics in the Abyss, a proposed experiment to measure the neutrino mass hierarchy using the **KM3NeT** neutrino telescope.

ORKA : A proposed **Fermilab** experiment to measure the rate of the decay $K^+ \rightarrow \pi^+ \nu \bar{\nu}$.

ORNL : Oak Ridge National Laboratory.

OscSNS : Oscillations at the Spallation Neutrino Source, a proposed sterile neutrino search using the SNS facility.

P : Parity, the inversion of all spatial coordinates.

PAMELA : Payload for Antimatter Matter Exploration and Light-nuclei Astrophysics, space-based anti-matter detector.

PANDA : antiProton ANnihilation at Darmstadt, a proposed experiment at the GSI Helmholtzzentrum in Darmstadt, Germany, studying proton-antiproton annihilation at few-GeV energies.

PandaX : A liquid xenon dark matter experiment to be located in the Jin-Ping Underground Laboratory in Sichuan, China.

PB : PetaByte, equal to 10^{15} bytes of information.

PDF : Parton Distribution Function, a function that describes the internal structure of the proton by giving the momentum distribution of a particular constituent, for example, the up quark or gluon.

PEN : An experiment at **PSI** to measure the ratio of decay rates $\pi^+ \rightarrow e^+ \nu$ / $\pi^+ \rightarrow \mu^+ \nu$.

Persistency management : Management of persistent data on disk, tape, or other media. This includes reducing the risk of loss to an appropriate level.

PEP-II : An e^+e^- collider operated at **SLAC** from 1998 to 2008 in the center-of-mass energy region of 10 GeV.

PF : PetaFlop, 10^{15} floating point operations (usually, per second).

PFA : Particle Flow Analysis, a method for hadron calorimetry based on separate measurement of the components of a hadronic shower with charged particles, photons, and neutral hadrons.

PFS : Prime Focus Spectrograph, wide-field multi-object spectrograph for the Subaru telescope.

PIENU : An experiment at **TRIUMF** to measure the ratio of decay rates $\pi^+ \rightarrow e^+ \nu$ / $\pi^+ \rightarrow \mu^+ \nu$.

PINGU : Precision IceCube Next Generation Upgrade, a proposed low-energy extension to **IceCube**.

Pipeline computing : Data analysis that proceeds in stages.

pMSSM : phenomenological Minimal Supersymmetric Standard Model, a 19-parameter subspace of the full MSSM.

PMNS : Pontecorvo-Maki-Nakagawa-Sakata matrix, the matrix linking the mass and flavor eigenstates of neutrinos.

PMT : PhotoMultiplier Tube.

Port : To adjust and test a computer program to run on a new architecture, or the result of this process.

PQ : Peccei-Quinn symmetry, an approximate symmetry of quark-Higgs boson interactions, which must also be spontaneously broken, that allows the possible **CP**-violating term in **QCD** to be set to zero. A consequence of this symmetry is the existence of the **axion**.

PREM : Preliminary Reference Earth Model, a model for the Earth's density distribution.

PREP : Physics Research Equipment Pool.

Project 8 : A proposed tritium-based neutrino mass experiment.

Project X : A planned upgrade of the proton accelerator injector complex at **Fermilab**, with a superconducting proton linear accelerator capable of producing multi-megawatt beams.

PROSPECT : Precision Reactor Neutrino Oscillation and Spectrum Experiment, a U.S.-based reactor short-baseline oscillation search experiment.

PSI : Paul Scherrer Institute, a national laboratory and accelerator center in Switzerland.

PTOLEMY : Princeton Tritium Observatory for Light Early-universe Massive neutrino Yield, a proposed relic Big Bang neutrino background experiment.

PWFA : Plasma Wake Field Acceleration.

QCD : Quantum Chromodynamics, the well-established theory describing the strong subnuclear interactions.

QE : Quasi-Elastic scattering. In neutrino physics, a reaction such as $\nu n \rightarrow \mu^- p$ in which no mesons are produced. In other contexts, a quasi-elastic reaction on a nucleon can involve production of a low-lying nucleon resonance.

QED : Quantum ElectroDynamics.

Qweak : An experiment at **JLab** to measure Q_{weak} , the charge with which the proton couples to the Z boson at very low momentum transfer.

RADAR : R&D Argon Detector at Ash River, proposal to add a **LAr TPC** to the **NO ν A** far detector building in Ash River, Minnesota.

RAT : A simulation and analysis package for optical detectors developed for the Braidwood project and now used within **SNO+**, **MiniClean**, and **DEAP**.

REAPR : Resonantly Enhance Axion Photon Regeneration, an experiment at **Fermilab** to search for **axions** using a laser beam in a strong magnetic field, the successor to **GammeV-CHASE**.

RENO : Reactor Experiment for Neutrino Oscillations, a reactor neutrino experiment in South Korea.

RENO-50 : A proposed reactor-based experiment with **baseline** ~ 50 km to measure the neutrino mass hierarchy with a large scintillator detector.

RF : Radio Frequency.

RHIC : Relativistic Heavy Ion Collider, a colliding beam accelerator for protons and heavy ions at **BNL**.

RICE : Radio Ice Cherenkov Experiment, neutrino detector in Antarctica.

RICOCHET : A proposed bolometric sterile neutrino search using **CENNS**.

RICH : Ring Imaging CHerenkov detector.

RIKEN : Rikagaku Kenkyujo, a major Japanese research institute covering physics, chemistry, and engineering, operating multiple research groups in Japan and one in the U.S. at **BNL**.

ROI : Region Of Interest. In neutrino physics, the region of a measured energy spectrum where the signal (typically a peak or dip from an oscillation) lies. In collider physics, a region of a detector whose data is used in trigger calculations.

RPC : Resistive Plate Chamber, a type of particle tracking detector.

SAGE : Soviet American Gallium Experiment, a solar neutrino experiment in the **Baksan** Neutrino Observatory in Russia.

SBIR : Small Business Innovation Research, a grant category of the **DOE** for collaboration of small businesses with experimental projects.

SciDAC : Scientific Discovery through Advanced Computation, a program of the **DOE**.

Science DMZ : A portion of a computer network designed for high-performance scientific applications rather than general-purpose computing such as web browsing.

SciNO ν A : A proposed neutrino scattering experiment adding a fine-grained scintillator detector at the **NO ν A** near detector site.

SDSS : Sloan Digital Sky Survey, a survey of more than a quarter of the sky using the 2.5-meter telescope at Apache Point Observatory, New Mexico.

SiD : Silicon Detector, a detector with silicon tracking proposed for **ILC** and **CLIC**.

SIMD : Single Instruction Multiple Data style of parallel programming.

SiPM : Silicon PhotoMultiplier, a silicon-based photodetector operated in Geiger mode.

SKA : Square Kilometer Array radio telescope.

SLAC : Stanford Linear Accelerator Center, a U.S. national laboratory in Menlo Park, California, featuring a 3 km electron linear accelerator.

SLC : SLAC Linear Collider, an electron-positron collider operated at **SLAC** from 1989 to 1998 to study the Z boson and develop the technology of linear colliders.

SM : Standard Model of particle physics, which describes the strong, electromagnetic, and weak interactions as mediated by vector fields.

SNO : Sudbury Neutrino Observatory, a solar neutrino experiment located in Sudbury, Ontario, Canada.

SNO+ : A successor to the **SNO** experiment aimed at the measurement of neutrinoless double-beta decay of tellurium.

SNOLAB : Underground science laboratory in the Vale Creighton Mine located near Sudbury, Ontario.

SNS : Spallation Neutron Source, at Oak Ridge National Laboratory.

Soudan : An underground laboratory in northern Minnesota, housing **MINOS** and low-background experiments.

SOX : A chromium and/or cesium source used with the **Borexino** detector to study the reactor neutrino anomaly.

SRF : Superconducting Radio Frequency (**RF**) cavities and associated technology.

STTR : Small business Technology TRansfer, a **DOE** program for this purpose.

STAR : Solenoidal Tracker at RHIC, a relativistic heavy ion collider experiment at Brookhaven.

STEM : Science, Technology, Engineering, and Mathematics, typically describing a domain of education.

STEREO : Search for Sterile Neutrinos at ILL reactor, a reactor short-baseline oscillation search in France.

SPS : Super Proton Synchrotron at **CERN**.

Subaru : An optical/infrared telescope on Mauna Kea operated by the National Observatory of Japan.

Super-K : Super-Kamiokande experiment, water Cherenkov detector in the Kamiokande mine in Japan studying proton decay as well as solar, atmospheric, and accelerator-produced neutrinos.

SuperKEKB : A high-luminosity electron-positron collider, with **CM** energy about 10 GeV, at **KEK**.

Super-NEMO : Super Neutrino Ettore Majorana Observatory, a neutrinoless double beta decay experiment in the Fréjus underground laboratory in France.

SURF : Sanford Underground Research Facility: An underground laboratory in the former Homestake Mine in Lead, South Dakota.

SUSY : SuperSYmmetry, a symmetry that links together **fermions** and **bosons**. In any realistic theory, supersymmetry requires a new space-time structure extending and generalizing that of relativity.

T : Time reversal, the transformation of reversing the direction of time.

T2K : Tokai to Kamiokande experiment, a neutrino oscillation experiment in Japan, using the **J-PARC** neutrino beam and the **Super-K** detector.

TA : Telescope Array, ultra-high-energy cosmic ray detector in Utah.

TCA : Telecommunications Computing Architecture.

TeV : Tera-electron Volt (10^{12} eV), the order-of-magnitude energy scale of Higgs boson physics.

TDR : Technical Design Report.

TES : Transition Edge Sensor, a cryogenic sensor based on the very small amount of energy needed to destroy superconductivity in a thin film painted on the surface of a detector.

TLEP : Triple Large Electron-Positron collider, a proposed e^+e^- collider in a large circular tunnel, with CM energies from 90 to 350 GeV.

TPC : Time Projection Chamber, a type of particle detector in which ionization from a track flows to a wall of a detector and the arrival time and location are measured, producing a 3-dimensional image of the track.

TREK : Time Reversal Experiment with Kaons, an experiment at **J-PARC** to search for **T**-violating muon polarization in the decay $K^+ \rightarrow \pi^0 \mu^+ \nu$.

TRIUMF : TRI-University Meson Facility, the national accelerator laboratory of Canada, located in Vancouver. It is now operated by a consortium of 15 universities.

UHE : Ultra High Energy, typically applied to cosmic rays with energies $> 10^{18}$ eV.

VCSEL : Vertical Cavity Surface Emitting Laser.

VEPP : One of a series of electron-positron colliders at the Budker Institute of Nuclear Physics in Novosibirsk, Russia.

VERITAS : Very Energetic Radiation Imagine Telescope Array System, a telescope for high energy gamma rays seen as air showers in Cherenkov radiation, located in Arizona.

Vev : Vacuum expectation value, the value of a condensed field, such as the Higgs field, at every point in space.

VLHC : Very Large Hadron Collider, a concept for a proton-proton collider with center of mass energy approximately 100 TeV.

Volunteer computing : A distributed computing effort in which computer resources are donated by the owner, for example, the SETI At Home project.

Wakefield : The electromagnetic field trailing a bunch of high-energy particles in an accelerating structure.

WATCHMAN : WATer CHerenkov Monitoring of Anti-Neutrinos, a collaboration of U.S.-based universities and laboratories conducting a site search for an advanced water Cherenkov demonstration detector.

WFIRST-AFTA : Wide-Field InfraRed Survey Telescope-Astrophysics Focused Telescope Assets

WIMP : Weakly Interacting Massless Particle, a category of particle that might make up the dark matter of the universe.

WIPP : Waste Isolation Pilot Plant, an underground facility located in New Mexico.

YBCO : Yttrium Barium Copper Oxide, $\text{YBa}_2\text{Cu}_3\text{O}_{7-x}$, one of the first high-temperature superconductors.