Planning the Future of U.S. Particle Physics

Report of the 2013 Community Summer Study

Conveners: M. Bardeen, W. Barletta, L. Bauerdick, R. Brock, D. Cronin-Hennessy, M. Demarteau, M. Dine, J. L. Feng, M. Gilchriese, S. Gottlieb, J. L. Hewett, R. Lipton, H. Nicholson, M. E. Peskin, S. Ritz, H. Weerts

Division of Particles and Fields Officers in 2013: J. L. Rosner (chair and corresponding author), I. Shipsey (chair-elect), N. Hadley (vice-chair), P. Ramond (past chair)

Editorial Committee: R. H. Bernstein, N. Graf, P. McBride, M. E. Peskin, J. L. Rosner, N. Varelas, K. Yurkewicz

Foreword

Particle physics research in the United States is a vibrant field, with discoveries stretching back more than half a century. Over the past year, more than 1000 U.S. particle physicists engaged in an in-depth process to define the most important questions for our field and identify the most promising opportunities to address these questions.

The process had its roots in a series of meetings held at Snowmass, Colorado over the past thirty years to take stock of progress in particle physics and chart the field's future. The last such periodic meeting was held in 2001. In 2011 the Division of Particles and Fields of the American Physical Society decided that the time was ripe for a new community study. Preparatory meetings during 2012 and 2013 began with a Community Planning Meeting at Fermilab, October 11-13, 2012. A final plenary meeting was held at the University of Minnesota, July 29 - August 6, 2013.

The 2013 Community Summer Study (the "Snowmass study") was designed to enumerate the questions the particle physics community wishes to answer over the next two decades, and plan how to answer them. The study does not prioritize activities, but aims to ask and answer hard questions. A subsequent prioritization panel with broad community representation will place these questions and answers within realistic budgetary scenarios. The study has produced this resource book, charting aspirations of the U.S. particle physics community over the next ten to twenty years, for use by the panel in its deliberations. We also intend the report 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 Particle Physics Project Prioritization Panel ("P5"), whose last report was issued in 2008, the division of the field into such categories should not obscure the focus on fundamental questions of physics, which, by their nature, cross such frontiers. These inter-frontier discussions have been a major component of the Minnesota meeting.

This volume can be read at various levels. An executive summary precedes a more detailed 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.

We thank all the host institutions and organizers of the preparatory and Minnesota meetings for their efforts on behalf of this review of our field.

For the Conveners and the DPF Executive Committee,

Jonathan L. Rosner	DPF Chair
Ian Shipsey	DPF Chair-Elect
Nick Hadley	DPF Vice-Chair
Pierre Ramond	DPF Past Chair

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 Nb_3Sn 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.
- Increase our efforts to convey the excitement 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 technoloigies, 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 SM reactions to differ from often very precise predictions. 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, 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 unambigously 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 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 $p \to e^+\pi^0$ and $p \to \bar{\nu}K^+$, with several other modes also being possible. The current limits on the proton lifetime in these two channels are roughly 10^{34} years and 6×10^{33} years, respectively, which is 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 TPC (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 $\tau(p \to e^+\pi^0) < 10^{35}$ years and $\tau(p \to \bar{\nu}K^+) < 3 \times 10^{34}$ 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 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 measurments 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 $\mu \to e\gamma$, 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 $\mu \to 3e$ 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 $\mu N \to eN$ 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 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.

Important observables in τ leptons are CLFV decays, CP violation, the electric dipole moment, and the anomalous magnetic dipole moment. New physics effects usually scale as a function of the lepton mass, and hence τ observables can be very sensitive to new contributions. 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 $4sigma$ 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 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 $e^+e^- \to \gamma\ell^+\ell^-$ 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.

Summary: 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 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 particle 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 exploitating 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 high-energy 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 ΛCDM 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 extradimensional 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 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 $\langle \sigma_{\mathrm{an}} v \rangle \sim 3 \times 10^{-26}$ cm^{3}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 $\langle \sigma_{\mathrm{an}} v \rangle$ 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 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 theie 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.

Summary: 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 Nb_3Sn 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 Nb_3Sn technology and by the absence of engineering materials with high-field properties beyond those of Nb_3Sn. 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 cryomodules. 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 100s 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 very large (of order 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, $\gamma\gamma \to H$. 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 1 μs, 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 DAEδALUS collaboration proposes multiple sources of decay-at-rest anti-neutrinos for short-baseline oscillation experiments. This project has narrower experimental scope than Project X. DAEδALUS 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 DAEδALUS 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 exciting 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 8×10^{35} cm^{-2}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} cm^{-2}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 (Finland). 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 increasingly 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 increasingly 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 is 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 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, detector development should take place at various levels of risk and various maturation time scales. 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 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 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 of this program, promote its merits, and provide venues for regular presentation of results. It also brings 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 intrumentation 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 HEP 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 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 saveing 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 interpretating 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 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 2 better than now, to compare upcoming ILC Higgs observations with SM 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.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 HEP, but HEP 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 scientists in other fields: 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 author pedagogical review articles in journals such as *Science* and *Nature*. They write articles in *Scientific American*, *Popular Science* and similar magazines, write online articles and science blogs, publish popular science books, and author 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 scientists in other fields: (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.
- Increase our efforts to convey the excitement 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.