
The Future of U.S. Particle Theory

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5.1 Introduction

Theoretical physics has played a crucial role in particle physics since its earliest days. Interpretation and synthesis of a broad range of experimental results (phenomenology), progress in quantum field theory (“formal” theory)¹, recognition of the role of symmetries (model building), and invention of new calculational methods (perturbative techniques and lattice QCD) were all crucial in developing the Standard Model (SM). Theorists have been the drivers in asking the questions which lead beyond the SM, including: the origin of the hierarchy between the scales of the weak and gravitational interactions, the physics of flavor, the origin of neutrino masses, the particle nature of dark matter, inflationary cosmology, baryogenesis, resolving the tension between quantum mechanics and gravity, and identification and explanation of dark energy.

Since the Second World War, the U.S. has been a world leader in theoretical particle physics. This has remained the case in recent years, despite the move of the Energy Frontier to CERN. This leadership results from a combination of university-based and national lab-based research, supported principally by the Department of Energy and the National Science Foundation.

The DPF Theory Panel was formed with the goal of understanding both the scientific problems and opportunities of the next decade, as well as the challenges involved in sustaining a first-class program in the U.S. Specifically, the panel’s charge included:

1. Enumerate areas of opportunity in particle physics theory research in order to set forth a vision for theoretical high energy physics for the next several years.
2. Establish a range of funding needs for individual PI’s to sustain an effective program (students, postdocs, travel, summer salary, equipment needs).
3. Examine roles and relative funding levels of university and national lab theory groups. This includes examination of Comparative Review processes for these two categories of research groups.
4. Consider if suitable mechanisms are in place to assure funding of young researchers.

To address these questions, we solicited comments and suggestions from the community, held town meetings at the BNL and KITP pre-Minneapolis workshops leading up to the main meeting in Minneapolis, and held two parallel sessions and a plenary session in Minneapolis.

Our basic conclusions and recommendations can be simply summarized. The U.S. should maintain a vigorous research effort in theoretical particle physics, ranging from perturbative and non-perturbative QCD studies,

¹While the term “formal” is often used, we prefer to refer to such work as “research on foundational questions” or “field theory, quantum gravity, and string theory”, and will do so in the rest of this document.

to collider phenomenology, to model building, cosmology, and research in foundational areas. We will provide more detailed recommendations in section 5.7.

Theorists and theory straddle the Energy, Intensity and Cosmic Frontiers throughout the international particle physics program, and have an important role to play in the developing international particle physics program. Theorists continue to provide the overarching intellectual framework for these programs. They also more directly help to enunciate the physics cases for future experimental facilities, and to define many of the analyses to be performed at the LHC and beyond.

The SM is likely complete, in the sense that it is a consistent theory up to very high energies. It is now the background to all of our experimental explorations. There are many questions beyond the SM, and the field now confronts new and serious challenges. In going beyond the SM, we are entering an environment with both high potential rewards and high risks. Theorists will continue to propose possible new phenomena relevant to all three Frontiers — Energy, Intensity and Cosmic — which will help guide future experimental studies and will respond to them. They will extend the structures of quantum field theory and quantum gravity, providing clues as to the possible underlying laws of nature, and they will continue to propose explanations for the hierarchy of energy scales, the origin of dark matter and similar mysteries, with implications for existing and proposed experiments. As they have for decades, they will provide critical input to analysis of experimental data.

Trends in funding endanger this vital enterprise. The U.S. funding agencies anticipate significant future reductions in funding for theoretical physics, and these are likely to harm the depth, breadth, and world leadership of this program. In FY 2013, support for particle physics in the NSF was cut by 10-12%. The DOE is facing a declining budget and is increasing the fraction of its budget devoted to new experimental and accelerator projects at the expense of research funding. Unlike many elements of the experimental effort, theoretical research does not lend itself to “project” designation, so the impact of this shift on theory is more pronounced. The consequences of these cuts may be amplified, if applied uniformly to research groups. This is particularly true for postdocs, as hiring a research associate requires some minimum funding level, and many groups are likely to find their funds fall below this minimum. Additional cuts to graduate student support will lead to shrinking numbers of individuals admitted to study particle theory, as well as a longer time to Ph.D. for those who remain. This will have a significant negative impact on our field for years to come. Preserving postdoctoral support at the present level is essential for the health of the field, since postdocs are the drivers of many challenging problems, and also the future leaders of the field. The recent adoption of a comparative review process in the Department of Energy has the potential to allow more targeted cuts, allowing for some control of the numbers of postdocs and students, but even then, serious harm will occur if current budget trends continue.

5.2 Particle physics and theoretical particle physics today

The past two years have seen a major triumph of theoretical and experimental physics working hand in hand. A scalar particle has been discovered in the mass range expected for the Higgs boson from analyses of precision electroweak data. This is an extraordinary experimental accomplishment. But this success also rests on our exquisite understanding of QCD, and our ability to predict, for the simplest Higgs theory, the production rate and the decay branching ratios with great precision. Early indications are that this particle is in fact the Higgs predicted by the simplest version of the SM, where simple has a precise meaning: it is the minimal number of degrees of freedom consistent with the symmetries of the strong, weak, and electromagnetic interactions, and the principles of quantum mechanics, locality, and special relativity.

Theorists are playing an important role in upcoming tests of the SM interpretation of the Higgs boson, which will be central to the continuing LHC program and to a possible International Linear Collider (ILC). Not only are they providing the required SM calculations for production and decay rates, new ways to test the properties of the Higgs boson, and alternative models against which these measurements can be tested, but they have a framework in which to quantify any would-be discrepancies between the simplest Higgs theory and experiment (using the methods of “effective field theory”).

On a slightly longer time scale, we have seen, over the the past two decades, many other successes of the SM. Among the most striking in recent years has been the experimental verification that the CKM phase explains the observed CP violation in the K and B meson systems. This is a triumph for the SM and for experimental ingenuity. However, it also reflects the development, over the past three decades, of a spectrum of theoretical tools, including: the general framework for weak interaction phenomenology employing the operator product expansion; the recognition of the incisiveness of time-dependent CP asymmetries; novel methods for understanding heavy quark systems; and an extraordinary increase in the ability to compute real physical quantities from lattice gauge theory. Indeed, the progress in lattice gauge theory over the past decade has been astounding, including, for example, calculations of the hadron spectrum yielding precision measurements of light quark masses and computation of decay amplitudes necessary for the precise extraction of CKM parameters from data.

Another success for theory (together with experiment) has been the verification of SM predictions with amazing precision in collider experiments. Experiments at LEP and SLC confirmed the electroweak couplings with great accuracy and tested QCD in a clean environment. Theorists and experimentalists have combined data from HERA and the Tevatron to provide precise knowledge of the momentum distributions of quarks and gluons in the proton. This knowledge, together with improvements in the theory of hadron collider processes, has enabled SM tests and measurements at the Tevatron and the LHC with a precision that was unthinkable even a decade ago. The W mass measurement at the Tevatron, and the tests of the SM electroweak vector boson production rates and kinematic distributions, as well as the top quark pair-production cross sections at the Tevatron and the LHC, are just a few examples of this progress.

The past decade has also seen confirmation of the existence of neutrino masses and the measurement of neutrino oscillation parameters. Theorists have played an important role in every aspect of these developments. They first laid the theoretical foundation for understanding neutrino oscillations, in vacuum and including matter effects, and provided natural frameworks for explaining the extremely small values of the neutrino masses. Theorists also provided a detailed understanding of issues arising in the detection of these phenomena using neutrinos from the Sun, the atmosphere, nuclear reactors, and accelerators.

Our seemingly complete understanding of physics, down to distance scales of order 10^{-17} centimeters, has brought other questions about the laws of nature into sharp focus. Again, theory plays a crucial role in delineating the questions and in suggesting possible answers. Among the questions which theorists have helped to identify and sharpen are:

1. What is the origin of the great disparity in the energy scales associated with the weak and gravitational forces? This is the hierarchy problem. It has two pieces: 1) why is there such a large disparity 2) the problem of fine tuning: any new energy threshold much above the masses of the W and Z bosons, such as the Planck scale or unification scale, tends to destabilize the Higgs boson mass through quantum corrections.
2. Where do the parameters of the SM originate?
3. Do the strong and electroweak forces unify at some energy scale?

4. Why is the strong interaction CP conserving? Is this accounted for by an axion field, and does this axion constitute some or all of the dark matter?
5. The quarks and leptons present many mysteries. Why are there repetitive generations? What accounts for the hierarchical structure of the masses and mixings of the quarks and charged leptons?
6. The discovery of neutrino mass has raised new questions. What is the energy scale associated with the generation of neutrino mass? Are neutrinos their own anti-particles?
7. The observed CP violation in the SM is insufficient to account for the baryon asymmetry of the Universe. What phenomena might account for this? Might they be accessible to experiments at the Energy or Intensity Frontiers?
8. What is the identity of the dark matter which makes up 25% of the energy density of the Universe?
9. What is the origin of the dark energy which makes up 70% of the energy density? Why is it just becoming important at the present epoch of the Universe?
10. What caused the inflationary epoch, and how did the Universe end up in its current state?
11. What is the nature of the quantum theory of gravitation?
12. From what set of principles or structures do the laws of nature originate?

Theorists are vigorously considering all of these questions. Some of them point to particular energy scales and types of experiments. Others questions in the list are more speculative. These questions straddle — as do the interests of most theorists — the Energy, Intensity, and Cosmic Frontiers. Proposals for physics beyond the SM include:

- Supersymmetry, a possible new symmetry of nature relating fermions and bosons, to understand the hierarchy between the Planck scale and the weak scale. In many realizations that theorists have considered, one might have expected its discovery in the first run at the LHC. Still, it remains one of the more plausible explanations, and is the subject of continued experimental and theoretical study.
- Composite Higgs models, technicolor, and Randall-Sundrum models. These provide alternative possible explanations of the hierarchy problem, and are the subject of ongoing experimental searches.
- Dark matter candidates. Weakly interacting massive particles (WIMPs) are natural in supersymmetry and several other theoretical structures; axions were invented to understand the strong CP problem. These are both topics of ongoing theoretical work and extensive experimental searches.
- String theory and other ideas for a quantum theory of gravity. String theory in particular provides a promising model for the unification of gravity and the other forces in a consistent quantum mechanical framework. It has also provided new tools for addressing problems in quantum field theory and in disparate areas of physics including heavy ion physics and condensed matter physics. It has suggested new principles (holography) and inspired ideas for particle phenomenology and physics beyond the SM. It has also inspired the invention of powerful techniques for computing scattering amplitudes.
- Leptogenesis: This is an attractive paradigm for explaining the baryon asymmetry of the Universe, which has an intimate connection with the origin of neutrino masses. Plausible indirect evidence for this mechanism would be the discovery of CP violation in the neutrino sector, the subject of tests in forthcoming long-baseline experiments. Other ideas for baryogenesis have different potential consequences.

In addition to raising questions, theorists have developed powerful perturbative and non-perturbative techniques for performing calculations essential for understanding collider experiments. Theoretical precision that was previously viewed as impossible is now routine. This precision has been, and will remain, crucial in both understanding SM physics and uncovering evidence of physics beyond the SM. This includes understanding Tevatron and LHC data, as well as results from BES III and Belle-II.

Upcoming results from the LHC or from experiments at any of the frontiers in the next decade might provide answers to some of these questions. For example, the hierarchy (or “naturalness”) problem provides the principal argument that new physics should appear at the TeV-scale, and it has inspired a range of proposals for physics beyond the SM.

The results of the first 25 fb^{-1} of integrated luminosity collected at the LHC have placed many of these questions in a new perspective. The current data are consistent with the SM, with a single Higgs doublet and a corresponding particle with mass 126 GeV . Many ideas about the hierarchy problem predict the existence of new colored particles to cancel large contributions to the Higgs mass from the top quark; there are now strong limits on such particles with masses below a TeV. Many specific proposals for new physics at the TeV-scale have been severely constrained by LHC searches, and the paradigm of naturalness has come under increasing scrutiny. However, there remains significant space to explore.

Answering theoretical questions will require experiments across the Frontiers. Intensity Frontier experiments and precision measurements at the Energy Frontier may provide evidence for new physics at slightly higher energy scales. Cosmic Frontier experiments may yield a WIMP candidate or further constrain this paradigm. For other questions, experimental input is likely to be limited for a long while, and theorists will try to put together answers, at first tentative, that combine experimental knowledge with theoretical insight. Aspects of the frontier classification scheme, and its relevance to theory, will be discussed in Section 5.4 below.

5.3 Prospects for advances in theory

During the various Snowmass workshops, and especially in Minneapolis, we asked theorists with a broad range of research interests to outline recent developments and to enumerate areas of opportunity likely to witness significant progress over the next decade. We present some highlights of these discussions. As with most theoretical ideas, several topics discussed below have overlapping boundaries. This list does not encompass all active areas of theoretical particle physics, but does represent a significant fraction of current activity. It is important to note that theorists not only cross the frontiers, but also the areas of activity we enumerate below.

Here we consider five broad areas: phenomenology, field theory calculational methods, model building, astrophysics, and cosmology, and string theory, quantum gravity and foundational questions. These are broad topics; within phenomenology, for example, we consider collider phenomenology, electroweak physics, neutrino physics, heavy quark physics and additional topics; in field theory, we include lattice gauge theory as well as perturbative and semiclassical methods; model building includes models of flavor, supersymmetry, grand unified theories (GUTs), and large or warped dimensions.

5.3.1 Phenomenology

Particle phenomenology plays the crucial role of linking experiments with the various aspects of theory, including model building, perturbative and lattice QCD, and more foundational issues. Over the past decades

it has been central to the extraction of the parameters of the fundamental Lagrangian from experimental data. It played a vital role in the success of the Tevatron and LHC programs, leading to precision determination of the top quark properties and the discovery of the Higgs boson. The successful operation of the B factories led to the confirmation of the Cabibbo-Kobayashi-Maskawa paradigm for quark mixing and CP violation, and the ever more precise determination of the parameters of the flavor sector. The phenomenology community has proposed many new kinematic variables now used on a daily basis by researchers in the LHC collaborations. Collider phenomenologists have also catalyzed an important new area of study at hadron colliders: jet substructure and various other jet and event properties, such as N -jettiness and N -subjettiness, have led to new experimental strategies for searching for pure-jet decays of heavy particles and of the Higgs boson. Phenomenology has also played a crucial role in the direct and indirect dark matter detection experiments. In particular, theorists working on dark matter have proposed that the proper framework of analyzing direct detection experiments is in terms of the couplings of a low-energy effective theory, broadening the possible interpretations of these experiments. Phenomenology will remain critical to the interpretation of continuing and upcoming experiments and for developing plans for the future.

5.3.1.1 Flavor physics

The study of flavor physics was an integral part of the development of the SM. The existence of the charm quark was predicted based on the suppressed decay $K_L \rightarrow \mu^+ \mu^-$. The fact that ν_μ is a distinct state from ν_e was inferred from the absence of $\mu \rightarrow e \gamma$ decay, and the top quark was predicted to be heavy from the measurement of $B_d - \bar{B}_d$ mixing. In the quark sector, the Cabibbo-Kobayashi-Maskawa mixing and CP violation paradigm was confirmed with data from BaBar, Belle, and the Tevatron. These measurements are quite impressive, but experimental and theoretical uncertainties still leave some room for contributions to processes in the B system. Theory has played a crucial role here, beginning with the idea of three generations of quarks, to heavy quark effective field theory and lattice QCD, which provided important form factors with a few percent uncertainty.

Proposals for physics beyond the SM are strongly constrained by flavor physics, whether or not they address directly the big questions of the subject. The scale of new physics that contains generic flavor violations is constrained by present data on rare processes in the muon, kaon, B and D -meson systems to be greater than about 10^4 to 10^5 TeV. For example, the spectrum of supersymmetric particles must exhibit special features. For example, the squark masses might be nearly the same (degeneracy), or they might be approximately diagonal in a basis in which the fermion masses are (alignment), if they are within reach of currently conceivable experiments. In the near future, LHCb and Belle-II will test non-standard theories of quark flavor with higher precision, and theorists will be engaged with analysis and interpretation of the results. Electric dipole moment limits on atoms, nuclei, the neutron and the electron severely constrain new sources of TeV-scale CP violation. Theoretical ideas which attempt to explain the pattern of fermion masses and mixings also typically predict new sources of flavor violation. The scale of flavor dynamics could be low, comparable to the TeV scale, or much higher. If it is not too high, experiments can test these ideas. The theory of flavor is relatively clean in processes such as $\mu \rightarrow e \gamma$ decay and coherent $\mu \rightarrow e$ conversion in nuclei (once the source of flavor violation is specified), but not so clean at the required level of accuracy in muon $g - 2$ for which there is currently a 3.6σ discrepancy between theory and experiment. For the next round of the $g - 2$ experiment, lattice calculations may decrease the theoretical uncertainty so that experimental results can be better compared to the SM prediction.

5.3.1.2 Neutrino physics

Neutrino theory sits at the intersection of particle physics, nuclear physics, astrophysics and cosmology, and as such provides great opportunities and interesting challenges. These are especially relevant today, given that a significant part of the U.S. experimental program over the next decade is likely to be focused on measurement of neutrino properties.

The experimental neutrino physics program has had tremendous success over the past fifteen years, beginning with the discovery of neutrino oscillations in solar and atmospheric neutrinos. Theory has played an important role in each step of these developments, from the calculation of neutrino fluxes from the Sun and from the atmosphere, to the recognition of the importance of matter effects including MSW resonances in neutrino propagation, to the calculation of various neutrino cross sections, to elucidating mechanisms for generating small neutrino masses.

This has focussed the field on measurement and understanding of the neutrino masses and mixings (the PMNS matrix), as well as fundamental issues such as whether neutrinos are their own antiparticles (Majorana particles), the scales of neutrino mass generation, and the possible role of neutrinos in the creation of the asymmetry between matter and antimatter (leptogenesis). The recent measurement of one of the neutrino mixing parameters, θ_{13} , in reactor- and accelerator-based experiments is a major step in pinning down the PMNS matrix. Discovery of CP violation in neutrino oscillations, as anticipated in forthcoming long baseline neutrino experiments in the U.S. and abroad, could provide indirect hints for leptogenesis as a plausible mechanism for generating the baryon asymmetry of the Universe. To unravel answers to the fundamental questions, theorists must undertake the time-consuming task of combining different experimental results, each with its own uncertainties, nontrivial correlations, and parameter degeneracies. Often the needed calculations, such as neutrino-nucleus cross sections, require a strong background in nuclear physics. The three-flavor oscillation paradigm needs to be tested by over-constraining parameters, which in itself is a time-consuming endeavor, requiring years of dedicated efforts. Currently, the U.S.-based theory community working on such topics is relatively small. There is an ongoing effort to enhance this community, led by a Neutrino Theory Task Force. This Task Force proposes new initiatives to attract more researchers into this area, in collaboration with experimentalists and nuclear theorists.

5.3.2 Field theory calculational methods

5.3.2.1 Perturbative and effective field theory methods

In a variety of circumstances, it is possible to extract important results from experiment only through very precise theoretical predictions. For example, a more precise understanding of the physics of heavy hadrons has been enabled by several breakthroughs: first, the operator product expansion for weak transitions; and later, the successive development of heavy quark effective theory, nonrelativistic QCD, and most recently, soft collinear effective theory (SCET). These developments all represent the construction of effective field theories (EFTs) that arise from a separation of physical scales. They have had great practical payoff for the experimental study of B mesons, charmonia, and bottomonia.

Monte Carlo event generators, such as Isajet, Pythia, Herwig, Sherpa, Alpgen and Madgraph, were developed by the theory community and have become indispensable tools for experimentalists to simulate events at all high-energy colliders, but particularly hadron colliders. There has also been enormous progress in computing cross sections relevant to both backgrounds and signals at colliders, at higher order in the strong coupling constant, α_s , and even including electroweak corrections. Next-to-leading order (NLO) QCD predictions

for the LHC are now routine and automated in programs such as MCFM, Rocket, CutTools, GoSam and BlackHat, based in part on an improved theoretical understanding of loop amplitudes. The next order (NNLO QCD) is now in sight for a variety of LHC processes as well, where it will make even more precise experimental tests possible. This progress at fixed order in the coupling has been accompanied by a greatly improved understanding of how to resum large logarithmic corrections: either analytically using, for example, SCET; or within Monte Carlo simulations by merging fixed-order results with parton showers.

Precise parton distributions are essential for all LHC physics, but it was only possible to obtain these once NLO and especially NNLO computations were combined with high precision data from HERA and elsewhere. QCD has truly come into its own as a precise theory for hadronic collisions. One also cannot overstate the importance of precision electroweak theory. The triumph of precision electroweak measurements in predicting the Higgs mass would not have come about if the quantum corrections to the weak mixing angle and weak boson masses had not been known to two loop order. With only the one-loop terms, the inferred Higgs boson mass would have been about 500 GeV higher than where the Higgs was found. Even better theoretical precision will be needed to match the experimental capabilities of an ILC running at the Z pole. Despite all this progress in precision applications of continuum quantum field theory, there is still plenty of room for new theoretical developments, both those with practical applications to experiments, and those that enlarge our understanding of quantum field theory and of the structure of physical law.

5.3.2.2 Lattice QCD

Lattice QCD is our main tool for understanding non-perturbative aspects of QCD. Numerical evaluation of hadronic matrix elements is crucial to progress in many areas of particle physics. The last decade has witnessed an enormous increase in the power of lattice methods. Lattice QCD has enabled the computation of weak matrix elements at the percent level, along with precise determination of the QCD coupling α_s and the quark masses. Computations of the decay constants of the D and D_s mesons provide striking examples of recent progress. Other applications include: 1) studies of condensed matter physics 2) work by the Columbia and Edinburgh lattice QCD groups in collaboration with IBM that resulted in the Blue Gene series of supercomputers 3) the MILC collaboration code has been used as a benchmark for many computer purchases. Breakthroughs are expected over the coming years, including: determination of m_b and α_s to 0.25% or better (important for precise calculation of Higgs decay modes at the proposed ILC); further calculations of weak matrix elements and form factors needed for tests of the CKM paradigm; calculation of the hadronic contributions to the muon magnetic moment, important for the interpretation of the upcoming $g-2$ measurement; nucleon matrix elements needed for prediction of nucleon decay and $n-\bar{n}$ mixing; form factors for quasi-elastic neutrino-nucleon scattering; calculations of strongly-coupled models of physics beyond the SM; and lattice calculations of supersymmetric models. Calculations are increasingly done with up and down quark masses at the average of their physical values. Future calculations will likely include dynamical electromagnetism and isospin breaking. Lattice gauge theory needs increased support at universities as well as laboratories, particularly in order to ensure training of a future generation of students.

5.3.3 Model building

Model building connects theory to phenomenology. Model builders discover new mechanisms, new phenomena, and inspire experiments and new experimental analyses. Here we distinguish TeV-scale model building, and model building involving higher energy scales, such as GUTs, models for neutrino mass and flavor, and string model building. Much of TeV model building has been driven by efforts to solve the hierarchy problem, but an important motivation is enumerating possible signals for experiments in the complicated

environments of the Tevatron and LHC. This will continue to be an important driver of theoretical work in this area in the coming decades. Higher scale model building yields possible solutions to many of our outstanding questions. In some cases, it has led to model building of relevance to lower energy physics, and/or has driven experiments which have shed light in other areas.

5.3.3.1 TeV-scale model building

For TeV model building, the idea of supersymmetry, which can potentially resolve the hierarchy problem, has been an important driver of experiments. Supersymmetry predicts an array of new particles, often with properties which are a priori known, and this has led to a variety of experimental searches looking directly for these new particles. Many of the searches for dark matter via direct detection have focused on the lightest supersymmetric particle, which is a natural candidate for dark matter. The theory of large extra spatial dimensions stimulated short-distance tests of Newton's laws. The axion hypothesis led to microwave cavity experiments and is pointing towards new types of experiments. Warped extra-dimensional models led to searches for the Kaluza-Klein gluon, which in turn was one of the inspirations for new techniques related to jet substructure. New types of models for dark matter led to new classes of experiments searching for dark sectors. Models of leptogenesis provide a very strong motivation to map out the parameters of the neutrino sector in great detail, especially the CP properties.

In the LHC era, in which trigger bandwidth is critical and backgrounds are large, it is important to have models that cover as many signatures as possible. For example, R -parity-violating supersymmetric models inspire new searches for colored states decaying without missing energy. Split supersymmetry suggests quasi-stable gluinos and leads to searches for out-of-time decays. Other models lead to quasi-stable charged particles or displaced vertices. Having a large number of exotic models significantly expands the range of search strategies at the LHC experiments, reducing the likelihood of missing critical signals of new phenomena. This synergy between experiments, phenomenology and model-building can work in both directions. Hints of new physics suggested by experiments, for example the muon $g - 2$ anomaly, have greatly influenced model building as well as phenomenology.

5.3.3.2 Flavor model building

The origin of the quark, lepton, and in recent years the neutrino mass matrices are among the great mysteries in particle theories. Theorists have explored a variety of ideas for understanding the hierarchies observed in the quark and charged lepton mass matrices. These have included, but are hardly limited to, flavor symmetries with small breakings, warped and extra dimension models, and composite and technicolor models. The evolving determination of neutrino masses and mixings poses challenges for many of these ideas, but at the same time, neutrino model building ties closely to possible mechanisms for the origin of the matter-antimatter asymmetry.

5.3.3.3 Grand unified theories

GUTs are proposals to unify the strong, weak, and electromagnetic interactions into a single force. They also unify quarks, leptons, anti-quarks and anti-leptons of each family into common multiplets. Such an arrangement would explain the coexistence of quarks with leptons, their quantum numbers, and the quantization of electric charge. With the assumption of low energy supersymmetry, the three gauge couplings of the SM are found to unify rather precisely when extrapolated to high energies at a scale $M_X \approx 2 \times 10^{16}$ GeV, which may be argued to be a piece of indirect evidence for grand unification and for supersymmetry.

This unification is the principal direct evidence for GUTs. But the study of these theories has had other impacts. GUTs stimulated proton decay experiments, which helped solve the solar neutrino problem, and led to the discovery of atmospheric neutrino oscillations, and to the detection of neutrinos from a supernova.

Even without supersymmetry, the gauge couplings can unify if the GUT permits an intermediate symmetry, as happens in SO(10). SO(10) theories also predicted the existence of the right-handed neutrino, which plays an essential role in the generation of small neutrino masses via the seesaw mechanism. Unification of matter into common multiplets implies that baryon number is not conserved, and that the proton should decay. In the context of low energy supersymmetry, these theories predict that the decays $p \rightarrow \bar{\nu}K^+$ and $p \rightarrow e^+\pi^0$ should occur with rates that are likely to be within reach of the next-generation proton decay search experiments.

Proton decay searches probe distance scales of order 10^{-30} cm, something not possible by any other means. If discovered, proton decay would be a landmark discovery in science.

5.3.3.4 String theory model building

There are many challenges in connecting string theory to the real world, but consideration of string models has profoundly influenced ideas for particle physics models. Some of these ideas will be mentioned below in Section 5.3.5, but further examples include ideas for understanding light Higgs bosons, theorems demonstrating the absence of continuous global symmetries, the role of discrete symmetries, ideas for the origin of repetitive generations, a natural setting for the Peccei-Quinn solution of the strong CP problem, candidate fields and mechanisms for inflationary cosmology, and alternatives to the conventional hot Big Bang theory at early times. We anticipate further progress in this field, exploiting theoretical developments and responding to experimental discoveries and exclusions.

5.3.4 Astroparticle physics and cosmology

The combination of a cosmological constant with a distribution of cold dark matter particles (Λ CDM) after an initial period of inflationary expansion is now considered the SM of cosmology. This model has provided a simple explanation of diverse physical phenomena in the Universe. However, even if this simple picture continues to be borne out, there is much still to explain, including the reason the cosmological constant has the value it does, the origin of cosmological density perturbations, and the nature of dark matter. Theorists have proposed a range of dark matter candidates; indeed, the need for cold dark matter has become a criterion for model building and particle phenomenology. Astrophysical and cosmological theory also plays a key role in the interpretation of a wide variety of current and future measurements, including dark matter experiments, massive galaxy surveys, gravitational lensing observations, and cosmic microwave background data. The hypothesis of inflation came from particle theory, emerging from ideas about grand unification and monopoles. It is now a central part of the Cosmic Frontier and of the Theory program. Recent data from WMAP and Planck have further constrained possible inflationary models. Given the limited amount of data we are likely to have, theoretical ideas will be critical if there is ever to be a single, compelling model underpinning inflation. Such ideas might come, for example, from string theory, or from connecting the degrees of freedom of inflation with those accessible to experiments (e.g., excitations in large extra dimensions or associated with supersymmetry breaking).

5.3.5 String theory/quantum gravity/foundational questions

The SM developed, in significant part, from theorists delving into fundamental questions in field theory. These included the demands of renormalizability and unitarity and the related constraint of freedom from anomalies. Its development and full understanding required that theorists master previously unfamiliar topics in mathematics such as group theory, topology and the theory of fiber bundles. String theory grew out of efforts to understand the phenomenology of the strong interactions, and later blossomed into a unified approach to quantum mechanics and gravity. In particular, the resurgence of interest in string theory in 1984 followed the discovery of anomaly-free string theories that could incorporate the chiral gauge structure of the SM. Since then string theory has been a major source of new ideas in particle physics. It has provided insights into questions of unification, the strong CP problem, black hole physics, supersymmetry, the possibility of flat and warped extra dimensions, and much more. It has had an important indirect impact on particle physics by inspiring new computational approaches to ordinary perturbation theory.

One of the most important recent developments in string theory is the “AdS/CFT” correspondence, or gauge/string duality. This is the startling observation that a quantum gravity theory in Anti-deSitter space is equivalent to a conformal field theory at the boundary of the space. This idea has provided a fundamental new tool for the study of strongly interacting field theories. As such it has provided a new method of studying non-perturbative QCD, has motivated new computations in lattice gauge theory, has found important applications to heavy ion physics, where it was used to predict the viscosity to entropy ratio of the quark-gluon plasma, and is now being widely applied to problems in condensed matter physics.

As a theory of quantum gravity, string theory also has close ties to general relativity and there has been a fruitful interplay between string-theoretic methods and more conventional methods in the study of the properties of black holes. Supersymmetry, alone and in conjunction with techniques developed in string theory, continues to be a powerful tool for unraveling the dynamics of strongly-interacting gauge theories. String theory practitioners have also contributed to advances in our understanding of cosmology (e.g., the systematic study of non-gaussianity), models of flavor physics, and much more. String theory and supersymmetry have also had a broad impact in pure mathematics in areas ranging from algebraic geometry to number theory. It is likely that the ideas and techniques to which string theory has led will be critical to resolving many of the questions we have about nature at the deepest level.

5.4 Particle theory, the Frontier paradigm and cross-disciplinary research

It is worth stressing again that theoretical particle physics, and most theorists, transcend the frontiers of physics. QCD, the theory of the strong interactions, is an essential element in both the Energy and Intensity Frontiers. QCD computations are important for the Cosmic Frontier, for example in evaluating cross sections for the direct detection of dark matter. Perturbative and lattice QCD are essential for understanding experiments in both areas. Furthermore, from a theoretical point of view, the physics of heavy quarks straddles the Energy and Intensity Frontiers, and has received contributions from theorists with a broad range of interests. In building models to describe short-distance phenomena, theorists will consider the collider signatures of their theories, the constraints from and predictions for flavor changing processes, and possible electric dipole moments. They will almost invariably ask whether their models possess a dark matter candidate, and if so, what sorts of experiments might detect it. They will consider cosmological issues, including creation of the dark matter and the baryon asymmetry, mechanisms for inflation and possible distortions of the microwave background, and the like. String theorists have pointed out the possibility of

alternative cosmologies, in which the Universe was not hot in the past, and provided a variety of frameworks for inflation. They have also suggested that axion decay constants might be larger than conventionally supposed, inspiring new cosmologies and exploration of new search strategies. String theorists and others working on more foundational questions have provided tools for scientists working on heavy ion physics (and other areas outside of traditional particle physics), as well as increasing our knowledge of field theory in ways which have proven useful for perturbative and non-perturbative QCD, and for those building models which go beyond the SM.

In addition to overlapping several frontiers, research in particle theory influences, and is influenced by, other areas of physics. The DOE and NSF have historically supported a broad science program, and, as indicated above, this has had significant scientific rewards. The question has arisen whether the traditional breadth of scope of the U.S. program in elementary particle theory should be narrowed in fiscally challenging times. There has been much discussion, in particular, about subjects like AdS/CMT, the application of gauge/string duality to condensed matter systems, with some questioning why the DOE, in particular, should support this activity. We would respond that there is a long history of fruitful cross-pollination of ideas between condensed matter physics and particle theory. For example, the BCS theory of superconductivity, which we now recognize as the first example of spontaneous breaking of a gauge symmetry, provided direct inspiration for Nambu's Nobel Prize winning work on chiral symmetry breaking in strong interactions. The renormalization group and its revolutionary implications for our understanding of quantum field theory had its origins in particle theory. It was then combined with ideas of universality and scaling and applied to understand phase transitions in condensed matter physics; insights gained from these applications then filtered back into particle physics. In the modern era, insights gained from the study of topological objects like solitons and instantons have had an impact on both condensed matter and particle physics. The recent progress in the condensed matter physics of topological insulators owes much to the understanding of anomalies gained by particle theorists. Recently these connections have expanded through gauge/string duality to include both general relativity and string theory.

There has also been increasing interaction between particle theory and areas of pure mathematics, an area of research sometimes referred to as "physical mathematics." For example, there are burgeoning connections between number theory, geometry and the mathematical structure of scattering amplitudes. There has also been a resurgence of interest in the formal structure of supersymmetric gauge theories and their application to areas of mathematics, including knot theory and the structure of low-dimensional manifolds. Dualities in string theory have found a direct connection to elements of the Langlands correspondence, one of the main drivers of research in mathematics. These areas are further from DOE's traditional purview (NSF is able, in some cases, to share such efforts between its Math and Physics Divisions), but may also lead to new tools and ideas with applications to particle physics. They are being pursued by some of the most talented members of the younger generation.

We see many reasons that such activities should be supported. First, the questions which drive our field may require entirely new concepts and methods. As Dirac famously said in the paper in which he introduced the magnetic monopole, "The steady progress of physics requires for its theoretical formulation a mathematics that gets continually more advanced." Second, these subjects have many ideas and techniques in common that often find application in areas far from their original source. Finally, we must note that these connections are at the cutting edge of research in the more foundational aspects of the subject and attract many outstanding young theorists. We will lose these people to other areas or other countries if their research is not supported.

Theoretical physics is in many ways a unified field in which the U.S. has been the dominant force in the world. Our universities and advanced graduate programs still lead the world but are under severe threat from funding cuts. We currently attract many of the top students from abroad. These students not only receive a world-class education, but also become familiar with our culture of scientific excellence. Other

countries understand the power of educating the best minds in the world. The panel believes it would be a great loss if the U.S. gave up this role.

5.5 The roles of national laboratories and universities

A significant share of research in theoretical physics is performed in National Laboratories, with a roughly equal level of funding for labs and universities. The number of PI's in the labs is significantly smaller than in universities, and the funding balance results, in part, from the fact that lab PI's work throughout the year on research (and professional service), without obligations to teach and to university service.

The U.S. national labs are critical players in the national theory effort. They support excellent theoretical programs, which provide leadership in research on QCD and Higgs physics relevant to collider physics, and essential support for ongoing experimental analyses and planning for future facilities. Essentially all of the U.S. expertise in parton shower Monte Carlo resides at the labs, and they have a strong focus on lattice and perturbative QCD as well.

Concerns about declining funding have led some to suggest an examination of the balance of lab and university funding, and we have discussed this among ourselves and with the agencies. These individuals have pointed out that, while the national labs represent about 20% of PI's nationwide and a comparable fraction of total output (as measured by publications, impact factors and similar measures) they represent about 48% of the total expenditure of DOE theory funds. This results from the fact that universities, as knowledge producers as well as educational institutions, pay for much of the research time of their faculty. This balance has served physics education and the field of particle physics well for many decades.

Still, declining funding has serious implications for research in labs and universities, both for the present cadre of researchers and research activity, and for future hiring in the field. We encourage HEPAP to establish a subcommittee to look at the complex question of university/lab balance.

5.6 Sustainability: theory funding

The U.S. has sustained a vigorous program in theoretical particle physics for many decades. Support for this effort has come principally from the DOE and the NSF, with modest additional funding from private sources. We have had extensive conversations with representatives of the DOE and NSF, and have been gratified by their strong appreciation of the value of theory. This is, however, a challenging period for funding of all aspects of particle physics; we have focused on a number of issues particular to theory. We are especially concerned, as we will describe in greater detail in this Section, with the consequences of anticipated future cutbacks for postdoctoral fellows and graduate students in national laboratories and universities, and for hiring of new faculty in theoretical particle physics at U.S. universities.

DOE and NSF funding has supported the following activities central to the theory effort:

1. **Training of students.** U.S. universities still lead the world in attracting the strongest students in many subfields of theoretical particle physics. Besides direct support for graduate student research through grants to PI's, support has also been provided through the DOE graduate student fellowship and the Fermilab visiting student program, and the LHC-TI graduate student fellowship funded by the NSF. The Theoretical Advanced Study Institute (TASI) summer school (for many years held at UC Boulder) has provided, since 1984, a thorough training for advanced graduate students in modern

particle theory concepts and methods. It is an extremely valuable component of the U.S. particle theory funding portfolio, and is the most highly regarded and competitive summer school anywhere in the world. Many TASI students have gone on to become leading researchers in particle theory. The federally supported training of graduate students directly supports the future of particle theory in the U.S. However, the effects of training students are broader than simply their impact on particle physics. Many students eventually leave the field, typically put their training to use in the “knowledge economy” of the U.S., whether it is in other fields of research, in teaching at undergraduate institutions, Silicon Valley, Wall Street, or other technical endeavors.

2. **Salaries of postdoctoral fellows.** Postdocs enable much of the particle theory research performed in the U.S. and constitute the future leadership of the field. While a substantial fraction go on to positions in universities and national labs, others move out of particle physics and contribute to the broader technical work force in the U.S.
3. **Travel to conferences and workshops and for collaboration.** Even in the internet era, face-to-face communication between theorists is essential for propagating ideas to others and for developing collaborations.
4. **Summer salary for investigators.** Summer salary frees faculty from teaching responsibilities in summer months and facilitates concentrated research time. The case for hiring faculty in particle theory at U.S. universities is driven in part by the recognition that they play an important role in nationally supported research. In addition to taking on teaching in the summer to supplement their incomes, faculty lacking summer salary are often assigned increased teaching loads during the academic year.
5. **Theoretical physics in the national laboratories.** Traditionally, the DOE has supported strong research groups at its national laboratories that have performed theoretical research closely related to experiments in particle physics. These theory groups have performed original research and trained postdocs and students. There has been a strong focus on relevance to, and support for, experimental activities, particularly those related to on-site accelerator-based facilities, but also including more general planning of large national and international facilities. While there is at present only one single purpose national laboratory dedicated to particle physics (Fermilab), the other labs maintain research groups in experimental particle physics working on the LHC detectors, on dark matter experiments worldwide, on the LSST, and other experiments. These efforts benefit from the presence of laboratory theory groups.
6. **Institutes running extended workshops.** A very important component contributing to the vitality and success of U.S. particle theory is the set of workshops run by the Aspen Center for Physics (ACP) and the Kavli Institute for Theoretical Physics (KITP at UC Santa Barbara), both partially funded by the NSF. Their workshops focus on a particular topic of current interest. An Aspen workshop typically runs for 3–5 weeks, with around 30 participants per week, while a KITP workshop might run for a few weeks or a whole semester. These programs have become major venues for intense exchanges of ideas, and canalization of new projects. KITP programs provided the impetus for much of the early progress in AdS/CFT, for the development of new methods for perturbative computations, and for numerous other activities across the field. The ACP hosted the discovery of anomaly cancellation in string theory, a major impetus to the field, and was the place where the original idea for the e-print archive arXiv was formulated. Numerous projects in model building, neutrino physics and collider phenomenology had their genesis at the Aspen Center. These institutes have become so successful that many other countries (and subfields) have put similar institutions in place. Examples include the Galileo Galilei Institute (GGI) in Florence, Italy, the Munich Institute for Astro-and Particle Physics (MIAPP) in Munich, Germany, the Mainz Institute for Theoretical Physics (MITP) in Mainz, Germany, the Kavli

Institute for Theoretical Physics China (KITPC) in Beijing, China, as well as the Institute for Nuclear Theory (INT) in Seattle funded by the DOE Nuclear Physics program, and the Simons Center for Mathematical Physics at Stony Brook. In order to maintain the ACP's and the KITP's status as the world-leading institutes for extended workshops, it is very important to keep funding these institutes at appropriate levels, allowing future generations of theorists to take advantage of these excellent resources.

This formula for supporting particle theory in the U.S. has been extremely successful. The theory program in the U.S. has arguably been second to none for many decades. With a changing funding climate, this model is at risk. Funding for DOE-funded groups is expected to decline several percent per year for the next several years; the NSF has seen a severe decline, as we have mentioned, in 2013 alone. This has a number of implications:

1. **Decline in support for graduate students.** In most university groups, particle theory students are currently serving as teaching assistants (TAs) during significantly more than half of the academic terms in which they are enrolled. These TA duties lengthen the time required to complete a Ph.D. thesis, with detrimental effects on education and careers.
2. **Decline in support for postdoctoral fellows.** Postdoctoral fellows play a vital role in particle theory groups, and the postdoctoral period is a crucial one in a research career. To prove that he or she qualifies to become a permanent member of the research community, a postdoctoral researcher must be open to new directions, invent or develop new concepts or methods that push the state of the art in some area, and evolve a unique, personal outlook within the research enterprise. These experiences often guide the researcher throughout his or her whole career. Developing a distinct professional identity is a far greater challenge than that of completing a PhD project with one's adviser. Many of those who succeed at the first level cannot achieve this one; others emerge because of the brilliance of their independent work. With the current and anticipated future funding cuts there is a possibility that the number of postdoctoral positions might shrink drastically in a short period of time. Any appreciable decrease in the pool of postdocs would have a significant negative impact on the future of the field.
3. **Severe restrictions on travel.** Travel budgets at universities and the national labs are shrinking substantially. Senior researchers are often forced to choose between travel essential for their research and travel by postdocs and students. It should be noted that, in addition, the approval process at national labs has become increasingly cumbersome and often is now hindering healthy exchanges of scientific ideas.
4. **Elimination of Laboratory Visitor Programs, Fermilab and DOE student fellowships.** One of the roles of theory groups at national labs is to be the national centers of scientific interaction. However, the recent elimination of visitor programs at national labs makes this impossible. This is in stark contrast with CERN, where the theory group is truly the center of European particle theory, mainly due to its visitor program and focused theory institutes. The very successful Fermilab visiting student program ("Fermilab Fellowship for Theoretical Physics") faces an uncertain future beyond the 2013-2014 academic year. Similarly, the DOE graduate student fellowship program has been suspended.
5. **Cap on university summer salaries.** The DOE has instituted a \$15K/month cap per investigator for summer salaries, which was also implemented this year in the NSF theory program. This effects only more highly paid senior faculty, and is preferable to further cuts in postdoc and student support. Significant cuts beyond this level could have negative effects on more junior faculty and overall productivity.

6. **The move towards a higher fraction of projects within the DOE has led to disproportionately large cuts in the theory budget.** At the recommendation of its 2010 Committee of Visitors, DOE HEP is moving towards increasing the fraction of projects in its funding portfolio to a level close to 20%. This is clearly the correct general decision: a vibrant HEP experimental program has to have a large fraction of the budget committed to projects, rather than to general research. However, the implementation of this move has also included the theory budget, which has no projects. This has led to a significantly higher effective cut in support for theory than in some other areas.

Each of these items has significant detrimental implications for the future of the field. Students make significant contributions to the research enterprise, both through their own work and as a significant source of new ideas and stimulation for their mentors; at the same time, it is the training of students that insures the future health and vibrancy of the field. Decreasing the number of postdocs will have a significant negative effect on research productivity, while again shrinking the pool of future investigators. Postdocs, particularly, bring fresh ideas to the research enterprise, and typically bring skills and a willingness to learn new techniques and methods. We are concerned that even modest funding decreases will mean that many groups will drop below threshold to sustain postdoctoral positions, so that a 10% decrease in funding over one to two years could translate into a much larger decrease in the number of postdocs. In the case of graduate students, in the past, decreases in funding could be ameliorated with increased TA responsibilities. While this would slow student progress and decrease productivity, it would at least provide a means of support. However, particularly in public institutions, the number of TA positions has been decreasing in many cases, closing this option.

This past year, the DOE instituted caps on summer salaries, and the NSF is following suit. We agree that this is preferable to further cuts in student and postdoctoral support, but it should be noted that still lower caps will have implications for research productivity, particularly if they reach the level of junior faculty (assistant or associate professor salaries). Many researchers may have to supplement their income with further teaching or other responsibilities in the summers.

5.7 Recommendations

The DOE, in the context of the P5 process, has referred explicitly to the role of theory. In particular, P5 is to address “Fundamental questions for the field and how to inform/connect the Frontiers framework . . . Input from the theory community will be especially important in this area.” We have outlined above our reasoning in support of these statements, describing both the historical role of theory and what we view as its crucial importance to the field going forward. In addition to enumerating what we view as the important contributions of theorists for the national and international particle physics efforts, we have also listed some specific areas where we see the field as particularly vulnerable.

- Seemingly modest cuts in theory funding are likely to lead to much larger declines in the numbers of postdoctoral fellows and students funded. This has implications both for research productivity and for sustaining an outstanding theory program in the U.S. This includes impact on the historic ability of the U.S. to attract outstanding talent from abroad.
- Shrinking numbers and size of theory grants will have impact on university hiring and teaching loads in theoretical particle physics, as well as obvious impacts for national labs.

Our recommendations to deal with these issues can be summarized as follows:

1. It is important to maintain the vitality and international competitiveness of theory programs both at universities and national laboratories.
2. The move to extract funds from research for projects should treat theory differently from other areas, as the damage to the program is more severe. At the same time, a possible class of projects for theory is proposed below.
3. A project category for theory, within DOE, could be the existence of theory *networks*, modeled loosely on such networks in Europe. A proposal would come from multiple institutions in response to a DOE solicitation to establish such networks, open to all subfields. We envision the scale of such networks as including of order five postdoctoral fellows, funds for travel between institutions and resources particular to the project. The projects would have a duration of order five years. They would have well-defined deliverables. Proposals would be peer-reviewed, preferably within a panel structure. Actual topics would emerge from the community, but one might imagine subjects such as neutrino physics (in this context, a proposal might have a component aimed at DOE Nuclear Physics), computations related to collider physics, or topics in more foundational areas (the “String Vacuum Project” of a few years ago, funded by the NSF, is a possible model).
4. The breadth of the topics and research areas supported in particle theory should be maintained. The successful formula of funding the best and most interesting research should not be changed. While in the experimental program there is obviously a need for prioritizing based on an agency’s missions, this is not the case for particle theory. We advocate that programmatic considerations in funding decisions should be kept at a minimal level. It is important not to limit the scope of high-quality theoretical research that is being performed, even if it appears to cross traditional funding agency boundaries.
5. HEPAP should examine the question of the balance of resources between laboratory and university theory groups. Both have been vital, historically, to the theory effort in the U.S. Both are vulnerable in the likely long term funding environment.
6. A target level of average support for funded researchers in university groups should include 1/2 postdoc per PI and 1/2 student (equivalent to fully supported) per PI. It should include travel funds adequate to attend two major meetings per year per PI, as well as one trip for postdocs and attendance every other year by students at summer schools or appropriate conferences. It should include two months of PI summer salary. Levels of support will vary among groups based on assessments by referees, comparative review panels, and agency officials.
7. Support for graduate student research should be increased. Ideally, particle theory students would be supported for three months during the summer, and for 50% of the terms during the academic year.
8. PI summer salary caps should not be lowered significantly below their current levels except to protect postdoc and graduate student support, and only with attention to impacts on research productivity, particularly of more junior PI’s (assistant and associate professors).

In broad brush, our recommendations are similar to the conclusions of the recent update of the European Strategy for Particle Physics on the importance of theoretical physics: “Theory is a strong driver of particle physics and provides essential input to experiments, witness the major role played by theory in the recent discovery of the Higgs boson, from the foundations of the SM to detailed calculations guiding the experimental searches. Europe should support a diverse, vibrant theoretical physics programme, ranging from abstract to applied topics, in close collaboration with experiments and extending to neighbouring fields such as astroparticle physics and cosmology. Such support should extend also to high-performance computing and software development.”

Similar support for our recommendations can be found in the statement, issued by a large group of experimental particle physicists, principally from U.S. institutions: “We, the undersigned experimental high-energy physicists, believe that a strong experimental high-energy physics program requires a vibrant theoretical physics community in the U.S. Over the last 50 years and more, the theoretical and experimental high-energy physics communities have supported and inspired each other. Not only are predictive tools developed by the theoretical community essential for the experimental endeavor, but the combination of experimental discovery and theoretical interpretation has led to revolutions in our understanding of the nature of matter. An experimental program which probes today’s outstanding questions is strongest when stimulated by new ideas from the theory community. When faced with difficult budgeting choices, we urge policy makers to protect the strength of the theoretical high-energy physics community and the balance between the experimental and theoretical programs.” Signatures can be viewed at http://amanda.uci.edu/~daniel/theory_letter.php

In the following subsections, we have included some topics which would be appropriate for consideration by the DOE Committee of Visitors, as well as some further information on the Snowmass Theory Panel.

5.7.1 Suggestions for agency committees of visitors

There are several issues that have come to this panel’s attention, that are more appropriately addressed by the Committee of Visitors at the DOE and the NSF. These include:

1. NSF panels are FACA panels. The DOE Comparative Review panels are not. The DOE COV should examine whether this limits the role of the panels, and consider whether a change in procedures is needed.
2. The DOE Comparative Review process was established, in part, to assure that awards were based on the quality of proposals and not on historical factors, as sometimes was the case in the past. Because of the challenging funding climate, it appears that in some cases, decisions about awards were made based on historical levels of support. The COV should examine whether, in fact, awards are in line with the quality of proposals, or whether there are still distortions due to past funding levels.
3. The COV should examine policies regarding overlapping proposals. For example, it should be possible to propose the same research for a DOE Career award and for a regular DOE grant (clearly with the understanding that only one would be funded). We understand that the agencies have made efforts to allow such overlap, but in the past, in some cases, agency policies have led to weakened proposals, and a review would be appropriate.
4. It appears to have become standard practice in DOE not to fund proposals from first year assistant professors. The DOE COV could consider whether this serves the best interest of the field.

5.7.2 The Theory Panel: additional information

Theory Panel Web Site The theory panel web page can be viewed at:
<http://www.snowmass2013.org/tiki-index.php?page=Theory+Panel>

Theory Plenary Presentations During the Community Summer Study

1. QCD (Kirill Melnikov)

2. Lattice Gauge Theory (Steve Gottlieb)
3. Neutrino Physics (André de Gouvêa)
4. Phenomenology (Tim Tait)
5. Model Building (Ann Nelson)
6. Cosmology (Jonathan Feng)
7. Field Theory and String Theory (David Gross)
8. The view from the NSF (Keith Dienes)
9. The view from the DOE (Simona Rolli)

Slides from most of the talks can be viewed at:

<https://indico.fnal.gov/conferenceTimeTable.py?confId=6890#20130804.detailed>