HIGH ENERGY PHYSICS ADVISORY PANEL
to the
U.S. DEPARTMENT OF ENERGY and NATIONAL SCIENCE FOUNDATION

PUBLIC MEETING MINUTES

National Science Foundation
4201 Wilson Blvd., Stafford II, Room 555
Arlington, VA

September 5 - 6, 2013

HIGH ENERGY PHYSICS ADVISORY PANEL
SUMMARY OF MEETING

The U.S. Department of Energy (DOE) and National Science Foundation (NSF) High Energy Physics Advisory Panel (HEPAP) was convened at 10:00 a.m. EST on Thursday, September 5, 2013, at the National Science Foundation, Arlington, VA, by Panel Chair Andrew Lankford.

Panel members present:

Andrew Lankford, Chair	Peter Fisher	Zoltan Ligeti
Ursula Bassler	Cecilia Gerber	Lia Merminga
Ilan Ben-Zvi	Tao Han	Leslie Rosenberg
Mary Bishai	John Hobbs	Thomas Shutt
Karen Byrum	Georg Hoffstaetter	Paul Steinhardt
Mirjam Cvetic	Klaus Honscheid	Robert Tschirhart
Robin Erbacher	Hassan Jawahery	Hitoshi Yamamoto

HEPAP members joining by conference call:

Patricia McBride	Murdoch Gilchriese	Jonathan Rosner

HEPAP Designated Federal Officer:

Glen Crawford, U.S. Department of Energy (DOE) , Office of Science (SC), Office of High Energy Physics (HEP), Research Technology, Detector R&D, Director

Others present for all or part of the meeting:

David Asner, Pacific Northwest National Laboratory (PNNL)
Michael Barnett, Lawrence Berkeley National Laboratory (LBNL)
Brian Beaudoin, University of Maryland
Juerg Beringer, LBNL
Ed Blucher, University of Chicago
Greg Bock, Fermi National Accelerator Laboratory (Fermilab), Associate Laboratory Director for Particle Physics
David Boehnlein, DOE, SC, HEP, Energy Frontier, Physics Research
John Boger, DOE, SC, HEP, General Accelerator R&D
Tim Bolton, DOE
Raymond Brock, Michigan State University
Rich Brower, Boston University
Holly Brown, National Science Foundation (NSF)
Joel Butler, Fermilab
Denise Caldwell, NSF, Division of Physics, Division Director
Lali Chatterjee, DOE, SC, HEP, Computational High Energy Physics
Norman Christ, Columbia University
Michael Coske, American Association for Advancement of Science
Jean Cottam, NSF, PHY, Particle Astrophysics, Program Director
Sally Dawson, Brookhaven National Laboratory (BNL)
Marcel Demarteau, Argonne National Laboratory (ANL)
Dmitri Denisov, Fermilab

Santiago Dernal, University of Maryland
Robert Diebold, Diebold Consulting
Keith Dienes, NSF, Program Director
Milind Diwan, BNL
Jonathan Feng, University of California-Irvine
Saul Gonzalez, NSF, Division of Physics, Experimental Elementary Particle Physics
Al Goshaw, Duke University
Rajan Gupta, Los Alamos National Laboratory (LANL)
Nick Hadley, University of Maryland
Mike Harrison, BNL
Young Kee-Kim, University of Chicago
Bradley Keister, NSF
Peter Kim, DOE, SC, HEP, Detector R&D
John Kogut, DOE, SC, HEP, Facilities Operations
Ted Lavine, DOE SC
Kevin Leski, LBNL
Michael Lucibella, American Physical Society
Ken Marken, DOE, SC, HEP, General Accelerator R&D
Helmut Marsiske, DOE, SC
Carole McGuire, FRA
Virginia Meehan, Northwestern University
Willie Molzon, University of California
Scot Olivier, Lawrence Livermore National Laboratory
Ken Olsen, Superconducting Particle Accelerator Forum of the Americas
Ritchie Patterson, Cornell University, Director of CLASSE, Professor of Physics
Abid Patwa, DOE, SC, HEP, Energy Frontier, Physics Research
Michael Procario, DOE
Doug Ray, PNNL
Steve Ritz, University of California Santa Cruz
Simona Rolli, DOE, SC, HEP, Theoretical Physics
Randy Ruchti, NSF, PHY, Experimental Elementary Particle Physics, Program Director
Marc Sher, William and Mary University
James Siegrist, DOE, SC, HEP, Associate Director
Anthony Spadafora, LBNL, Physics Division, Deputy
Alan Stone, DOE
James Stone, DOE
David Sutter, University of Maryland
Kathleen Turner, DOE, SC, HEP, Cosmic Frontier
Michael Tuts, Columbia University, Experimental High-Energy Physics, Professor
Harry Weerts, ANL, High-Energy Physics Division, Director
Jim Whitmore, NSF
Rik Yoshida, ANL
Kathleen Yurkewicz, Fermilab, Communication Director

SEPTEMBER 5, 2013

OPENING REMARKS

The U.S. Department of Energy (DOE) and National Science Foundation (NSF) High Energy Physics Advisory Panel (HEPAP) was convened at 10:00 a.m. EST on Thursday, September 5, 2013, at NSF, Arlington, VA, by Panel Chair Andrew Lankford. The meeting was open to the public and conducted in accordance with Federal Advisory Committee Act requirements. Attendees can visit http://science.energy.gov/hep/hepap for more information about HEPAP.

REPORT ON SNOWMASS

Dr. Nick Hadley reported on the Snowmass meeting held at the University of Minnesota on July 29 – August 6, 2013. Snowmass was preceded by a year of meetings to discuss key issues, important questions, and opportunities, and to develop ideas that were shared at Snowmass.

Activities were identified and discussed at Snowmass but not prioritized as that will be done by the P5 panel. A summary report will be published by November 1st and will be a detailed resource for the P5. There are processes similar to Snowmass occurring in other countries.

Discussions were defined by eight frontiers even though the physics within each transcend the frontiers. Specific conveners led each frontier and working groups with some subgroups.

Questions asked at Snowmass recognized advances made in understanding the universe and the properties of neutrinos and elementary particles. The discovery of the Higgs boson has formed understanding of the Standard Model (SM) and yet many questions remain. A goal of Snowmass was to develop a framework for 11 key questions that will serve as a basis for advancing particle physics. Areas needing greater understanding are:

- The Higgs boson, associated principles, and its composition
- Masses and mixing for quarks and leptons, and CP violation
- Neutrino mass, antiparticles, and interactions
- The mechanism behind matter and anti-matter production, and particle and anti-particle interactions
- The composition and density of dark matter (DM), and connections to the SM
- The composition and principles of dark energy (DE), and bonding with the universe
- The origins and evolution of the universe, and new fields active in the early universe
- Additional and unknown forces, additional quantum numbers, unification of the four known forces at short distances, and unification principles
- The discovery of new particles at the TeV scale
- The discovery of new and light particles, and experiments for observing them
- The existence of extremely massive particles that can only be coupled to indirectly at currently accessible energies

Attendees discussed accelerators, instrumentation and things that underlie it. This is considered to be an underlying frontier with unique questions. Considerations include achieving higher data speeds, optimizing resource use to develop new detectors, and drawing from other fields to capitalize on what is learned. Collecting data flow and managing it are important, as is the scaling of current accelerators to larger sizes, costs and demands, and using new technologies to manage this and use of a new Livingston cost model to manage this.

The field needs greater support and its value explained to the public to raise their interest and to develop interest in related careers.

Snowmass attendees develop a non-prioritized list of goals.

- Probe the highest possible energies and smallest distance scales with the current and upgraded Large Hadron Collider (LHC), reach higher precision with a lepton collider, and study Higgs properties in full detail
- Develop technology to build multi-TeV lepton colliders and 100 TeV hadron colliders
- The U.S. will host precision testing of the neutrino sector using an underground detector
- Identify the composition of DM and properties of the dark sector
- Map the evolution of the universe, and understand DE and the cosmos
- Develop new instrumentation to meet these goals
- Develop advanced computing for experimentation and theory
- Carry on theoretical work and explore new unifying frameworks
- Invest in physics training
- Increase efforts to convey the excitement of this field to others

The descriptions of the frontiers presented today to HEPAP were organized by frontier yet this division should not obscure the focus on fundamental questions that cross the frontiers.

Discussion

Dr. Tschirhart asked about Snowmass' endorsement of the concept of frontier, if capabilities and instruction are things that the community should use as vehicles to communicate with other fields, and if the frontiers will continue as categories. Dr. Hadley noted that the Office of High-Energy Physics (OHEP) is DOE's steward of high energy science and the frontiers and elements of high-energy physics (HEP). Physics instrumentation is used by other fields such as medicine. Instrumentation is vital to the community especially in areas such as cosmic. Those things should continue along with the division. Balancing the divisions is up to the P5 panel.

Tschirhart asked if Snowmass endorsed the use of capability and instrumentation frontiers to communicate to other fields outside of HEP. Hadley shared that an endorsement was not requested but attendees felt that instrumentation needed a place at the table like that of accelerator science. Frontiers are seen as a restriction on working in single fields. People supported calling out instrumentation and accelerators in education and outreach.

Hadley confirmed for Dr. Bishai that the discovery of Higgs will complete the picture of the SM, but many mysteries remain. The Higgs has been found but questions remain.

PRESENTATION OF ENERGY FRONTIER DISCUSSIONS AT SNOWMASS

Dr. Chip Brock presented on behalf of his co-convener Dr. Michael Peskin. Work since the discovery of Higgs has led to understanding particle theory and a more complete SM. There are still inaccuracies around the meaning of the SM. Its essence is linked to the Gauge structure and the entire set of precisely measured quantities. Higgs' potential relative to efficiency theory is unclear and the dynamics are not fully clear. Establishing this understanding is job one.

Higgs is the lens for examining particle physics and discerning meaning practically and in the details. There are pieces that do not yet fit such as the spin 0 elementary particle and gaps are known. This is an experimental problem.

There are experimental anomalies that will dramatically influence the energy frontier. Higgs mass is small, v's flavor and mass and symmetry properties are not SM, DM needs quantum, primordial anti-matter needs explanation, and the $(g-2)\mu$ results need confirmation. The energy frontier concludes that the following three-pronged research program is needed:

- Understanding and measurement of Higgs properties is needed
- Measure the properties of t, M and Z – the heaviest SM particles and how they interact with themselves and with Higgs
- Search for TeV-scale particles. This is inspired by the notion of how to deal with understanding of Higgs mass.

Prior to Snowmass, 35 frontier leaders conveyed with hundreds of others in six working groups. Technical advisors gave insight on accelerators and tools. There was active international collaboration and with facilities that could advance research. Three broad goals were established.

One is to determine the scientific goals that motivate the High-Luminosity (HL) LHC running. The groups discussed upgrades and how this would advance understanding, how precisely Higgs could be measured, the need for a precision Higgs program, and the case for high energy experiments after the year 2030.

The group tested against specific accelerators and parameters. A simulation was built that used a generic "Snowmass detector" with the best performance parameters of other detectors and DELPHES 3 instruments. Background samples and sample tails of distribution were statistically potent to use rare processes at high potencies. The collider community used tools and common platforms to generate signals and backgrounds. Many results came from this detector and there will be 350 pages of technical detail published soon on the energy frontier wiki.

The work pointed out tension due to a lack of constraints in making decisions about what constituted a viable accelerator. The group tried to compare facilities, but not all are equal. LHC detectors are understood and projections on their future are fairly conservative.

There was also talk about exclusion. Parameter space was previously excluded but now there is a need to look at the discovery potential in other space.

The group identified 11 big questions to be answered to advance the energy frontier.

In Higgs boson, a precision program needs to be outlined. Other needs are identifying Higgs coupling accuracy and getting a consistent table of projections of coupling.

Fitting for Higgs coupling has spawned a new industry and early results fit expectations. Precision Higgs work has begun but understanding of couplings is needed for greater discovery. The benchmark for this is a few percentages to sub percentages, rather than the current levels of 10s of percents. Extrapolating LHC will require a scheme used in CMS. The conservative expectation is in agreement with ATLAS expectations and assumes that nothing new is learned. Another expectation for Higgs couplings assumes that the results will go down with luminosity and theory be go down by a factor of two. Self-coupling is related to potential and will be hard to measure no matter the facility but self-couplings will start with high-luminosity running.

Mass and width would be measured at both LHC and International Linear Collider (ILC). The width of Higgs will be measured to a few percentages or less at an ILC and within the limits available at LHC. Spin will be understood soon at LHC and models anticipate multiple Higgs. CP violation will also be big and be a part of LHC running and an ILC.

The message from Higgs is that direct measurement enables understanding electroweak symmetry breaking. The full exploitation of LHC is the path to better precision in coupling and 50 MeV mass determination, and full exploitation of a precision electron collider is the path to a model-independent measurement of the width and sub-percent measurement of couplings.

Precision measurement and the study of vector boson interactions are themes in the study of electroweak physics. Efforts have looked at the mass of *Mw* versus the mass of the top, and now the Higgs mass fits into this but not precisely. An LHC goal is to determine the precise mass of *Mw*. This is conceivable if 5 MeV can be reached. *Mw* at the lepton colliders is possible. Vectory boson scattering has been a problem, but this group collaborated with others to study VB scattering. The examination of scales that support improved luminosity and energy led to major improvements in finding pathways for physics and contact interactions. The electroweak messages are that the precision of Ws and Zs could indirectly probe for particles with TeV masses. The measurement of VB interactions can probe for new dynamics in the Higgs sector.

Themes in top quark understanding were identified. There is a need to deal with top quark mass and coupling. There is also a question of stability and asymmetry sensitivity. There is uncertainty about top quark mass that alludes to understanding the situation and this could be something. Measuring top quark mass to understand what it really means is difficult. The group strived to understand this as clearly as possible. Ultimately, 500 MeV understanding of top quark mass is doable and the lepton collider will do that well. Top partner searches to high energy are doable with 5σ searches. This bumps against pile-up based on the Snowmass study. Many techniques were studied to understand pile-up conditions and there are a variety of techniques possible. Top is tied to problems with symmetry breaking and flavor, precise and theoretically well-understood measurements of top quark masses are possible at LHC and e+e- colliders, and new top couplings and particles decaying to top are key in models of Higgs symmetry breaking.

Themes explored in Quantum Chromodynamics (QCD) looked at PDF uncertainties and αs, event structure at hadron colliders, and improving the art of perturbative QCD. Uncertainties must improve, and a range of mass search is doable in the next run. Uncertainties will dominate backgrounds. The measurement of full rapidity coverage in LHC will add to work that cannot be done by ATLAS, CMS and LHCb alone. Other focus areas are the importance of photon distribution function, the need to incorporate full EW resummation, and lattice contributions especially aS. There is also the landmark NNLO calculation of the top quark. Messages from QCD are an improvement, PDF uncertainty is necessary and achievable, advances in the alpha error is achievable, and advances in collider experiments are possible but will need advanced machinery.

New particle physics themes center on the need for new particles TeV mass and that there are candidate particles. There are also connections to DE and to flavor issues.

A lot of work has been done at LHC. The gain in energy and luminosity has led to important increases in reach and discovery. Most top reach is not a full factor of two and there are regions available but they are dwindling. The Snowmass detector showed what is possible with higher levels of luminosity.

Z' sensitivity and DM connections are additional themes.

The TeV scale is in sight and it can be expected that the reach of a variety of machines will pass the TeV scale. The message for new physics is that TeV mass is needed and is possible with LHC and future colliders. This is connected to searches for DM and rare processes, and any new discoveries will demonstrate the role of high energy colliders.

The working groups came up with cases for different types of machines. Developing successive machinery should not depend on past machines not finding results but rather taking prior results and defining them with greater precision. The groups made cases for the LHC upgrades, the linear ee collider, GeV, CLIC, the muon collider, the photo collider, the circular ee collider, and the pp collider.

Knowing more about neutral fermions and the gravitating universe in order to truly understand mass is one outcome of new machines. This is important for new physics, especially determining if very light neutrino physics is Dirac or Majorana. Understanding mass is the primary goal of all particle physics and not just an energy frontier issue.

Energy frontier work involves precision, mass reach, and new discoveries. The LHC has equipment that can enable this and motivate advances to even more precise electron colliders.

The Higgs discovery changes everything in practical and scientific ways. Confirming the SM is no longer the goal, and research is now exploring how success in particle physics should be evaluated. This exploration forms the origin of the word "frontier."

Discussion

Dr. Ligeti asked about ratios of coupling sensitivities. Dr. Brock noted that there are benchmarks but that the table on slide 29 was an attempt to show some consistency. The meaning from one column to the next may not be the same. Professor Han added that normally vacuum expiration would occur. New physics occurs at 1 TeV which is why most information on the slide is at one percent. The data shown is not totally arbitrary.

Ligeti suggested that the primary question is how to do things better at one facility versus another. Professor Hoffstaetter shared that the opportunities for 33 TeV running of LHC are less than lepton colliders or even LHC3000. He asked where the 33 TeV concept excelled. Brock noted that this only occurred in strict base reach. The group started by characterizing against Higgs factories a year ago, and the infraction with the 33 TeV became less as time went on. The group did not decide if it is wanted or unwanted.

Dr. Merminga asked about the case for an electron proton collider. Brock shared that there was little discussion. The PDF people thought about it but there was little effort to simulate this.

Professor Jawahery asked what drives the case for subpercent measurements. He pointed out that the LHC will measure Higgs coupling to a few percentages and will look at charged Higgs but if that does not exist by the time the LHC program is complete, then it may be known that SM Higgs has been found. Brock added that there may be a discovery of particle couplings of named models that could be hiding in the two or three percent coupling. Jawahery noted that this assumed that there is physics at the TeV range.

Dr. Bishai asked about probing the same physics with ratios to measure more accurately in hadron colliders. Brock shared that the focus is the ultimate mode of independence of the lepton collider direct coupling measurement.

Professor Yamamoto noted that the two independent measurements act the same way and that there is a separation of branching ratios. Brock commented that the tables shown in the presentation are to get everyone on the same footing.

PRESENTATION OF COSMIC FRONTIER DISCUSSIONS AT SNOWMASS

Dr. Jonathan Feng presented the cosmic frontier discussion on behalf of his co-convener Dr. Steve Ritz. Understanding of the universe has been transformed in recent years. There is now new evidence for new physics and powerful approaches to address key questions.

Cosmic frontier working groups focused on DM direct detection, indirect detection, non-WIMP DM, DM complementarity, DE and cosmic microwave background (CBM), and cosmic particles and fundamental physics.

Workshops and summaries at many levels will provide the P5 with varying depths of detail on the frontier and exciting opportunities in the coming decades.

DM has been discovered, yet research is now investigating DM identification. One way to probe this is direct detection. The status of the field can be shown in independent WIMP-nucleon cross-section. There are constraints from different experiments but some experiments also see signals showing areas for discovery. There is some agreement on these signal regions and the constraints.

Boundaries for work in the next 20 years are set by the neutrino background. Researchers want to look at what they may encounter later and then at theory prejudices where they may want to be at a mass scale with a certain range of Higg-mediated scattering. There is also asymmetric DM that may have a mass of 5 GeV to 10 GeV. And there are projections of what future experiments might do. There are many future technologies that can lower the mass window by several GeVs. Moore's Law for DM in the early days excluded Z-exchange models. Things are improving at an exponential rate and there will opportunities in the next year.

Indirect detection is an orthogonal way to look at DM. This could be area where DM particles annihilate and give off undetectable stuff. The annihilation cross sections are produced by having the right amount of DM in the first place. There are photons you can detect and experiments in space and on Earth. Some of the first science from ground-based telescopes is expected around 2016 and the Cerenkov Telescope Project will be completed in 2019. Current experiments show that work is done at low mass and in the future, indirect detection with photons will pass about 10^4 GeV. Indirect detection work is also being done with neutrinos and will lead to strong ways to look at spin.

Snowmass also looked at Non-WIMP DM. This field is growing with many types of DM. Possibilities in this area are not mutually exclusive and multi-component DM is possible. The frontier report will cover many areas. One is the use of axioms for fine-tuning to handle strong CP problems. Within the parameter space, there is a window where the axion would have the right level of DM. This has led to experiments with ADMX that will cover future work and an entire preferred window in the coming years.

The frontier process brought together a community that cannot meet often. It addressed DM complementarity and noted that before a signal, there are different approaches that are sensitive to DM candidates and give different types of information. Even after a signal, research seeks to identify a quarter of the universe and needs high standards to claim discoveries and follow-up to measure properties.

There are drawbacks. It is hard with direct detection to have a mass measure at 500 GeV. It is hard to get detailed particle properties with astrophysics probes, hence there is a need for activities to pin down DM. Hundreds of models can be investigated in many ways. Investigating the complementary has shown a need for different methods to cover the theory parameter space.

DE is the main topic in every frontier. This was started by the discovery of the accelerating universe and brought more questions. The ACDM explanation is that this should be bigger but is it actually small and there is no answer why. More detailed probes of its properties and the Higgs boson are needed to provide more data.

The DE program has been well-reviewed and draws from U.S. leadership. At Snowmass, discussion of DE probes focused on DE impact on geometry and the growth of structure. There are also impacts through structure formation. Snowmass focused on complementary probes through spectroscopic surveys to determine DE properties. Discussion touched on weak gravitational lensing and bending of light. There is movement toward precision DE and current experiments at DESI will differentiate this on percentage scale levels. With spectroscopic surveys, LSST will pin down data even better.

Snowmass emphasized not measuring parameters and also modified gravity. It is possible to compare distances and geometry effects with structure growth. Various theory curves and projections show how well DESI will do on growth rate determination. This can help disentangle the modification of gravity from just geometrical effects. This will be an amazing discovery.

Influctuation and CMB is not the leading theory of the primordial origin of structure. Research is entering an era where these need to be tested. Physics at 10^{-32} after the big bang is being measured and is a powerful and important capability.

Neutrino cosmology is being discussed. Neutrinos were once radiated but are now cold and can allow for small scale structures. The mass of neutrinos is contained. In the future, combining large sets of data from CMB and LSST experiments will have greatly improved sensitivities. The lightest neutrino masses can be found and future cosmology could measure neutron masses and find this information to be very complementary to seeing long baseline results.

Matter and anti-matter symmetry was discussed. This is a three frontier problem that shows that researchers need to work together.

Cosmic particles and fundamental physics have outstanding questions for ultra-high energy cosmic rays. There are about 200 events per year at 60 EeV. There is a proposal for space-based detection to identify events and build a database in the next 10 years.

The discovery of ultra-high GZK neutrinos is possible in the next 10 years. Cosmic rays with CBM will provide a neutrino beam. There is a guaranteed neutrino beam that needs to be found. ARA and ARIANNA will eventually give even better results.

With other frontiers, the cosmic frontier can provide clear events beyond the SM and produce questions for popular interest. Snowmass discussions provided a menu of cosmic frontier options and any options identified by the P5 panel will be fascinating.

Discussion

Professor Fisher noted that a CVMS paper on plot shows both exclusion region and the area on the plot and wondered about collaboration reports. Professor Rosenberg wondered about the nuclear physics domain and if there is a search region left for gathering. He also asked if the escape hatch for KATRIN are Planck results and others. Current cosmological results with KATRIN could see a neutrino mass. Rosenberg asked if there are escape hatches for things such as Planck results. Feng thinks that it is getting incompatible but does not exclude anything on KATRIN. It is only probing active neutrino flavors.

Dr. Ligeti wondered if the cosmology limits are close to each other.

Professor Tschirhart note that with the sterile neutrinos in the plot (slide 27) how those at MeV scale could evade the search. Feng share that this measures contributions to energy density and that this is ineffective. If sterile neutrinos have not neutralized, they will not commit fully to being ineffective.

PRESENTATION OF INTENSITY FRONTIER DISCUSSIONS AT SNOWMASS

Dr. Hendrik Weert reported on the Intensity Frontier. It is broad and diverse with many connected scientific opportunities. Six working groups have convened for several years, including a workshop in 2011 that resulted in the publication, "Fundamental Physics in the Intensity Frontier."

The frontier has dealt with many of the questions that came out of Snowmass. The search for new physics is a theme. This can be measured by exploring high mass scales via indirect effects. Looking for rare processes such as lepton decay and not seeing new processes shows that the frontier reach can be very high in experimental research.

Snowmass 2001 posed questions that led to a mixing matrix. Things are now measureable and indicate a high level of progress. The frontier can claim continued success leading to an era of precision neutrino physics. There has been recent confirmation of a fit with the three neutrino oscillation mix paradigm. It is also a completely different measure than the early Daya Bay experiment yet is very complementary.

There is an established program to measure CP violation, mass hierarchy and 0νββ. New detection techniques are needed for challenges, clarity and new discoveries associated with precision measurement in the neutrino sector, complementary baselines and sources.

U.S. leadership can grow from the LBNE and a future underground beam. This plan is phase one, followed by next-next generation experiments with a qualitatively better beam.

Long-baseline experiments give a guaranteed mass hierarchy determination with sufficient exposure. Snowmass also discussed PINGU to show the significance of three σ and the results of efficiency after several years. Other experiments include JUNO/RENO-50.

CP violation work at LBNE is reaching new levels of accuracy. It is clear that achieving very small precision requires protons and mass. LBNE and Project-X will generate a lot of protons.

Work on neutrino anomalies shows that the confirmation of these would change the course of neutrino research in ways that include the discovery of new neutrino states. In three to five years, more can be understood due to MicroBooNE, MINOS+, radioactive source measurement, and new reactor measurements.

Neutrino physics knowledge comes from knowing the cross-variances and how they interact.

A goal for neutrino 0νββ is that all running and approved experiments will reach 100 MeV.

Direct neutrino mass measurements have been occurring since the 1950s. The next experiment coming online is KATRIN. There are other R&D efforts to move beyond the precision that KATRIN will achieve.

There is a new physics flavor problem. If there are no new physics then research is constrained to scale. If there is new physics at the TeV scale, then the flavor sector is unnatural.

The status of the CKM fit and the level of agreement between measurements is often misinterpreted. More experimental precision and theoretical cleanliness can enable NP sensitivity.

There is new physics possible in $B_{d,s}$ mixing.

Discussions included the future sensitivity of Belle II. Compared to current measurements, errors will shrink by a factor of eight, and LHCb upgrade errors will shrink by a factor of five.

The charged Kaon mode and neutral Kaon mode need to be measured. There is an effort at CERN to measure the charged mode, and a proposal at Fermilab to have about 1,000 events. In the neutral mode, there is an experiment at JPARC called KOTO and projections that Project X could generate five percent precision. It may be possible to achieve higher sensitivity with Kaon and higher precision with experiments at Project X.

Work in charged lepton physics shows that there is Muon, LFC, Tau LFC and lepton flavor violating processes. There are neutrino oscillations. Charged leptons are easy to produce and detect, and precise measurements are possible. They provide direct probe couplings of new particles to leptons. The branching ratios desired are B(μ to 3 conversions in ^{27}AI. Lower limits are projected from 1940 out to 2030, and Project X can get down to 10^{-18}.

The groups looked at the analogous magnetic movement of the muon. There is a discrepancy between experiments and SM. An experiment at LBNL was moved to Fermilab and will run in 2016 or 2017. The experiment can be done well but other theories have to be considered when achieving this precision. The lattice community is trying to drive precision down to 15 percent.

There are electric dipole moment neutrons, the nucleus, and electrons. The field has a sense of theory prediction and its goal. Experiments are being done in nuclear but not particle physics. EDMs are excellent probes in new physics but also the most constraining in SUSY models.

Grand unified models allow for different couplings at large scales. The proton should be able to decay. The current status of proton decay is that proton lifetime expectations and work has been dominated by Super K. That is the only experiment running and it is increasing very slowly. The potential turn-on of Hyper K and liquid argon would begin around 2020. It would be about 10 years before proton decay would be observed.

There is a Project X white paper that describes neutrino-antineutron oscillations.

The frontier discussed new light weakly coupled particles. The dark sector consists of particles that do not interact with known forces. There can be connections between the dark sector and SM and possibilities of portals including Higgs. Searches of ultra-hidden sectors have looked at coupling to SM versus the mass, and a mass of less than 1eV and more than 1eV.

Axion and ALPS work is constrained by astrophysical and experimental measurements. This occurs because stars do not burn out and hot DM is unlikely. Laser, microwave cavity, and solar telescopes are a partial list of techniques that give experimental bounds.

There was a staging of Project X at Snowmass with three stages. One conclusion was that U.S. growth would require megawatt sources of protons. Project X enables that reach in the intensity frontier.

The field will enter the era of neutrino physics in the next two decades. There will be information from the cosmic frontier. Intensity will probe mass scales for new physics using multiple approaches. There will be a separate group of new, light, weakly-coupled particles. The intensity frontier's strong synergy with other frontiers will lead to a stronger HEP program.

Discussion

Dr. Weerts confirmed for Dr. Han that CP violation measurements will not require above ground work and LBNE is involved. The current detector design has a small enough background that only mass is needed.

Dr. Byrum commented that small, mid-size and large experiments with physics all along the way are needed. Weerts noted small experiments in charged leptons. Others could be argued as small, challenging and inexpensive. Baryon work includes smaller experiments, as does neutrino work that measures the cross-section. The frontier has looked at the physics it wants to do and not just the experiments, and made no effort to identify small experiments.

Professor Honscheid asked about discussions at Snowmass that dealt with rare processes and that connect the intensity, energy and cosmic frontiers. There are precision measurements with a TeV sensitivity that could compel people to look at the intensity frontier. He asked about needed emphasis for the P5 panel. Weerts described a plot that shows the connection but that is only one he knows of. There is also an effort to predict a loud face-base by LSC on the impact of electric dipole efforts. He felt that more discussion was needed at Snowmass.

Dr. Bassler asked about proton decays and no new physics at 10^{14}. Weerts commented that there are many other experiments and opportunities outside of this, and likely processes at LHC that go to higher scales. The frontier has work to do here as the physics is not necessarily visible.

Weerts explained for Dr. Merminga how this fits into an international context. Results in neutrino physics have come from other experiments in which the U.S. has been involved. LBNE is trying to lead this area. There are two experiments in quark and flavor physics that are not in the U.S. but in which the U.S. participates. In Japan, there is work in charged lepton flavor and programs do talk to one another. G-2 measures do too, but take a different approach. In proton decay, there is Super K and the U.S. is involved. There is work in the U.S. in electric dipole moment and new light and weakly-coupled particle work is mostly in the U.S.

Dr. Ligeti noted that there are symmetries for lepton and quark flavor work that bring down the scales that Weert described. There is excitement around charged leptons and strong connectivity, as described in research.

Dr. Bishai shared that Project X will enable much higher precision, especially Kaon experiments and in comparison to experiments in Japan and Europe that are underway. Higher precision is also possible with lepton flavor measurements.

Professor Rosenberg added that DESI has the ALPS project that will pursue laser production of nuclear interacting particles. There is no known participation by the U.S. There is a cast experiment in CERN with minor U.S. participation. Dr. Han added that the masses shown in Weert's presentation (slide 7) are proportional to Higgs. The heaviest scale possible is the only question. He urged that efforts rush forward. A direct connection may be possible but accessibility may be a question. Dr. Tschirhart noted that this was not discussed deeply at Snowmass. Dr. Han responded that it was discussed and a paper should be produced. The physics here is all about scale and if it is accessible or far away.

Professor Cvetic observed that the energy frontier presentation showed comparisons of experiments and reach. She wondered if the intensity frontier compared experiments and did global comparisons. Weerts confirmed that the report covers this. Professor Jawahery noted that the energy frontier looked at the same parameters, and Weerts added that this frontier's report looked at experiments that do similar things and offers comparisons.

PRESENTATION ON ACCELERATORS AND OTHER CAPABILITIES

Dr. Mark Palmer shared information from the Frontier Capabilities Working Group. The group contributed to understanding electron facilities and accelerator-based facilities.

Underground detector facilities were examined by two principle subgroups that looked at very large detectors and at facilities for DM experiments, respectively. There were two subgroups that looked at instrument development, and non-proliferation issues.

Underground facilities and inherent capabilities are essential to supporting a world-wide experimental program and a large range of experiments. U.S. participation consists of about 1,000 U.S. scientists with an estimated 30 to 40 percent growth. In terms of existing or planned facilities, there are no technical hurdles to prevent desired experiments and global work on these experiments will double by 2020.

DM G2 experiments can be met by existing and planned facilities. G3 experiments are large and depth requirements are being determined. No U.S. facility is capable of supporting this.

There are several $0\nu\beta\beta$ experiments underway. Next generation ton-scale experiments are planned and more planning could be done further out. The longer-term path to complete ton-scale experiments is unclear.

There are initial efforts proposed for CP violation in the area of long baseline vs, nucleon decay and atmospheric vs. Atmospheric neutrinos and large detection with a sensitivity to 1 currently has an unknown oscillation parameter. In the case of Hyper K and LBNE, that path is a scale that represents a lost opportunity if it not achieved with LBNE.

In low-energy versus large detectors, there are opportunities for physics and astrophysics from supernova ν burst. This would be an underground requirement along with future solar work.

Reactor neutrinos are another class of capabilities. The key is that the scales being discussed require hundreds of meters of water overburden. Future experiments are being proposed with overseas efforts looking at medium baselines. The U.S. may be able to leverage the potential for synergy in non-proliferation activities.

One issue is access to infrastructure capabilities. Reasons behind this include materials assay storage and production. The current U.S. infrastructure looks sufficient if it is maintained. It could leverage other resources to maintain infrastructure, but this capability could be lost. Another issue is access to capabilities. There is domestic competition and it is important to move to a more open access scheme. Achieving international balance is possible when each country or region supports a major facility capable of hosting experiments. It is unclear if support could be maintained if one country takes a major role in research without supporting a facility.

At the end of the decade there will be significant U.S. expansion of underground facilities and U.S. scientists should be supported throughout decade. One approach is to put LBNE underground and make it a world leader in underground capabilities. Planning and maintenance will be needed to house the range of experiments that can ensure consistent U.S. leadership.

The accelerator facilities working group addressed questions that were of a long-term nature and asked if facilities can be made cheaper. It started by looking at proton colliders, the reach of LHC, the future of the energy frontier, hadron colliders beyond the LHC, and how best to exploit the LHC. The group concluded that there needs to be support for high luminosity LHC construction and reengineering readiness including the Nb_3Sn magnets.

Beyond LHC, there was interest at Snowmass in reaching the 100 TeV option and hope for a design study in the near future and that the U.S. should join it. There is also a need to go beyond current magnets and for work on beam dynamics, machine protection and beam abort dumps. There are strong technology overlaps with muon and high intensity machines.

In the area of lepton and photon colliders, ILC and CLIC designs could be improved using new technologies. Japan has agreed to host this work and the ILC, and with the release of TDR, the technical decision is ready for a decision. The U.S. has agreed in many ways to contribute and play leadership roles. For the ILC to be acceptable, it should have technical upgrade pathways to higher energy and luminosity.

There are Higgs factory options that include an electron ring in a large tunnel, a muon collider and feasibility assessment, and a photon collider. The Higgs factory concept shows that these span a range of technical readiness. There may be parameters where the P5 has to look carefully to see the potential for any given set of parameters. If big gains on accelerator R&D can be made, then there is the potential for powerful physics. Considerations support an integrated U.S. R&D program toward demonstrating muon collider feasibility.

Key questions in the intensity frontier have to do with secondary beams for IF experiments. Priorities in this frontier are diverse. The Accelerators group tried to determine the needs and available facilities, and concluded that the next generation of intensity frontier experiments will require proton bean intensities and timing structures that are beyond current capabilities.

Project X is can provide these capabilities. The first stage of construction could start in the last half of the decade. DAEδDALUS gives a narrower experimental scope and a force on anti-neutrinos with short baseline vs oscillations. NuSTORM is another example. These are smaller than Project X and need better understanding.

There is a range of considerations for these machines. Losing the beam in high intensity machines will do damage. High-powered targeting is a challenge. We know that in high-powered targetry there is no ability to scale from past experiments. This requires structured R&D.

Another area of interest is high-intensity electron and photon beams, and things like heavy flavor factories. There are new physics opportunities but required accelerator R&D is unknown. The capabilities for heavy flavor factories are seen in Super B-factory and Tau-charm Factory beyond BEPC-II. Factory machines require high intensity and low-emittance beams. There are areas of overlap with LC damping ring and light source R&D efforts, along with a history of fruitful international collaborations and cooperation.

High intensity electron and photon beam opportunities are possible using FEL facilities via connections with existing capabilities. This could include exploiting synergy with light sources.

Accelerator technology test bed research is charged with identifying a range of existing or needed test capabilities. The group identified 35 facilities. A range of technological changes would be needed and coupling to existing and planned capabilities. Energy reach beyond the ILC will need to be tested to identify new testing capabilities. In intensity frontier accelerators, these include a new generation of IF machines with new test facilities to achieve desired outcomes.

The accelerator capabilities group reviewed a broad range of capabilities. The first goal should be the exploitation of LHC capabilities and a path for upgrades. The Japanese initiative to host a linear collider as a Higgs Factory is welcome. The next generation of intensity frontier experiments requires proton intensities and timing structures that are beyond current capabilities, and intensity frontier colliders and photon sources have strong synergies with other parts of the accelerator community. There is also work on lepton colliders that can reach the 100 TeV scale. Dedicated test facilities are needed to develop future HLC capabilities.

Discussion

Dr. Jawahery commented that a lot of attention in the area of Higgs factories was given to TLEP and readiness compared to the cost of ILC. Palmer noted that there were discussions of energy frontier lepton and photon colliders, and TLEP. This is a machine with an overall reach that is tied to rapidly falling luminosity as energy intensity increases. There are machines that could increase the threshold but compared with the ILC design, this has already been designed. The TLEP parameters are the desired parameters. Assumptions about the TLEP design will take about two to three years to confirm. A review would give enough detail for the community to decide if it wants this machine. CERN has approved a design study. There may be performance evaluations that give a direct comparison. The working group could not do a reliable comparison now. There is also a muon collider option.

Dr. Bishai noted that a 40 TeV machine was possible in the 1990s and wondered about the technical barriers. Palmer responded that all of the design studies for the VLHC are applicable. There is now better detail on how to build a 100 TeV proton collider. The group recommended participating in design studies if there is a clear path and recognition that this is a real possibility.

Dr. Tschirhart asked if international support for underground capabilities could be maintained if one country pushed for hosting, with LBNE as an example. If this is a full suite of capabilities, then there is a rich set of experiments being done, Palmer noted. If large facilities were developed in this area, then the community might be concerned about fragmentation.

PRESENTATION ON INSTRUMENTATION AND COMPUTING FRONTIER DISCUSSIONS AT SNOWMASS

Dr. Marcel Demarteau presented discussion findings on instrumentation and computing. HEP has a long history of inventing detectors, building computing infrastructure, and then moving technologies to a large scale. Instrumentation pervades all of physics and the group looked at current and future instrumentation needs in the field.

Challenges in instrumentation include growing demand on detectors, DAQ systems and engineering, often at a large scale. Experiments and approaches are broad requiring a breadth of instruments. HEP is not leveraging technical advances in other branches of science. U.S. investment levels lag behind competitors, instrumentation expertise is eroding, and current funding is insufficient.

A real instrumentation program would maintain U.S. leadership and development. Efforts should balance between structure and research priorities, and aim at innovation, with balanced funding between projects and R&D.

A balanced program would simultaneously conduct evolutionary and revolutionary detector R&D at multiple levels. The current program is missing transformative technology investments. A new detector development program and grand challenge is recommended outside of existing R&D funding for long-term investment in more challenging but potentially high impact research. Areas where existing challenges would be cost-prohibitive would be ideal candidates. The benefits are the development of transformational technologies, a highly-visible and challenging opportunity that would be impactful, strengthened university and national laboratory collaboration, a reconstitution of infrastructure at universities, and the ability to keep pace with private sector advances. A necessary component is training the next generation of R&D experts.

Snowmass discussions explored candidates for strategic investment and considered all of the frontiers and projects, working to distill projects that use technologies that have promise for the whole field. The group developed a list of seven areas that can impact most if not all frontiers.

The computing frontier organized itself in sub-groups looking at the computing needs in physics and technical capabilities, respectively. Challenges in computation include the flexible use of all resources including distributed high-throughput computing and high-performance computing, sharing and opportunistic use, and data intensive computing.

Data challenges are driven by the need for continued R&D investment in data management, data access methods, networking, and data intensive distributed computing enabled by networks. There is a continued need for the training of software engineers and others, and providing career pathways. Young scientists need to learn skills that are marketable for non-scientific careers.

The outlook for both frontiers is to push hard in both areas and develop approaches that are transformative. As a field, there is a need for corrective action to retain U.S. scientific leadership.

Discussion

Dr. Bishai remarked that budget constraints usually eliminate these areas of work. She asked what levels things are at now. Dr. Demarteau sees the need to reinvigorate efforts at universities and national laboratories. Grand challenges can provide targeted R&D projects for universities and laboratories, and serve as a means for distinguishing themselves as centers.

Professor Gerber thought that this might be hard to sell at most universities, asking if a different type of physicist does this work. She wondered if the group is advocating some separation. Demarteau explained that there are instrument builders and observers, and this model should be avoided. Younger generations should know about instrumentation, and the current model should be reconstituted and include instrumentation information.

Professor Erbacher commented that most university engineers have been taken away, and the group's discussion presents the idea that this content should be available for university training. This depends on what exists, Demarteau responded. ASICS engineering, as an example, is pervasive in this field and used in the frontiers. However, the instrumentation group felt that it would be counter-productive to place this expertise in just one laboratory. Projects may come up where one group could develop expertise in one area of ASICS design. There could be multiple centers where work is done, and an increase in projects and programs.

Tschirhart senses that outside of HEP there is excellence and expertise in instrumentation. In the OHEP there is an argument that the office will be a steward for a broad set of offices. He wondered what HEP has to sell to universities that are supported by other offices. Demarteau thinks that HEP has a lot to offer to other fields. One example is basic energy science (BES) and detectors for x-rays. HEP has been able to take a concept and evolve it to a real working detector on a large scale. That expertise and approach so far does not exist in the x-ray community for light sources. Tschirhart hopes that the grand challenge call would not be limited to just the HEP realm and would generate unique ideas from outside the group.

Professor Fisher commented that many places around MIT are doing sensor development. Some are very innovative and have large systems. Schools can purchase incremental engineering which may be more in the spirit of the university and its advantage.

Professor Rosenberg pointed out that in looking at the landscape of things that are crucial to HEP, the local background would be hard to find in some other environments, as an example.

Bishai asked about the involvement of graduate students and a COE, and if there must be some overlap with education, NSF funding for course development, and time at facilities to do basic graduate student research. Demarteau shared that instrumentation courses are hard to take, based on anecdotal findings. That and multiple aspects to training should be addressed.

It was noted that the USPS school studied a pilot project for detector training. The course was held over a few weeks, was very intensive, and occurs every six months. If the pilot is successful, it will expand to teach graduate students.

Mike Procario wondered if the community was ready for this training, sharing that those interested in instrumentation can have greater knowledge of science. He asked if people will want to work on projects that will spread through the community, create interest in hardware, and have the chance to discover something new before anyone else. He wondered if those with new tools will share these tools with others. Demarteau commented that the community spirit is that whatever one does is available to the community and everything should be science driven.

Erbacher felt that Demarteau seemed to emphasize a multi-disciplinary approach and reconstituted university infrastructure that should not be funded solely by OHEP and NSF. Certain areas could be part of a broad call for many experiments, Demarteau noted. The group did not ask how to structure this and include other offices in the DOE Office of Science (SC).

Professor Shutt noted the challenge in encouraging young scientists' interest in developing equipment for experiments that may be far off in the future. Demarteau commented that there should always be a scientific goal driving the development of technology.

Shutt asked about the possible size of the transformational R&D that was described. Demarteau sees this as one percent of the overall OHEP budget. That would meet community need. The community should run this exercise and look at various scenarios and funding levels.

Professor Hoffstaetter asked about interfaces with accelerator physics. Demarteau knows that one U.S. school focused on accelerators already has two classes given in a joint setting with the accelerator community.

Dr. Siegrist commented on detector R&D and stewardship, noting that OHEP's portfolio for accelerator R&D is larger than any other offices in the SC. Other parts of DOE make large investments in non-proliferation and have annual budgets of several 100 million dollars. OHEP does not have as dominant a position in detectors as it does in accelerators, and yet both are closely related and there is asymmetry between the R&D for both. The programs can be linked together. He believes the level of generic R&D in government needs to be considered. As an example, non-proliferation activities are related to non-proliferation versus a scientific mission. DOE needs to examine overlaps with other offices and investments.

PRESENTATION OF SUMMARY OF SNOWMASS REPORTS

Dr. Nick Hadley shared a conclusion from the Snowmass reports and what it would like to communicate to HEPAP. There is a tremendous amount of science to be done and questions to be answered. This is a united field that attracts public and media attention. It is an international field. All are eager to participate in the next phase of a global effort in particle physics. The U.S. brings talent, expertise, and contributions to the field, and plays a leadership role.

The Snowmass report is due on November 1st. Proponents have generated excitement about the pursuit of experiments and questions that need to be answered. Advances in theory emerged from Snowmass, as well as the value of instrumentation, computing, and education and outreach.

The report is just a portion of the outcomes. There are talks and papers online. There will be references to even more detailed information. The conveners of the frontiers are resources for the P5 process and will help the P5 members set priorities. The process that P5 is to use will be very important as much as the decisions that are made. The Particle Physics Federation would like to help with the P5 process, bring in stakeholders, and talk about how it can help. Snowmass also brought a renewed appreciation of the breadth of the field and united people across the science that all wish to do.

Discussion

Professor Hobbs asked if the report will link the frontiers, perhaps through case studies that show how instrumentation and capabilities could scale to support physics cases. Hadley has not seen anything this explicit but it could be in the 30-page version and in the 350-page summary.

Dr. Bishai shared that Snowmass participants conceded a dislike of the frontier labeling and possible stove-piping. She wondered if there will be a stronger statement about this. Hadley responded that there will be a statement that the science to be done spans the frontiers and that these are an organizational construct that enables communication in the government. He also sees that different frontiers address physics questions in different ways.

Professor Shutt noted an impact when distinguishing high energy and nuclear physics. This goes beyond $0\nu\beta\beta$ and predates the frontiers. Hadley has not seen that explicitly addressed but believes that it is implied. He felt that the response too often is that the work is being done by someone else and noted that Shutt's point is valid.

Professor Erbacher commented that the frontier categorizations constrain researchers from working in multiple areas. The budget and review process is complicated by these boxes.

Dr. Siegrist thanked the Division of Particles and Fields (DPF) for its work at Snowmass.

Siegrist commented on training, noting that some of the best scientists are recruited by industry, and he expressed that laboratories and universities should recognize this.

PRESENTATION OF THE P5 PLANNING PROCESS

Dr. Andrew Lankford described the initiation of the P5 process. The OHEP and NSF Directorate for Mathematical and Physical Sciences (MPS) charged Lankford to constitute the panel. The last P5 report was in 2008 and much has changed since then, including priorities under fiscal constraints, the need for new experiments, and facilities.

The P5 panel was assembled based on their board expertise and to reflect the demographics and expertise in the field. Its size is determined to be efficacious considering the issues that it must address. Dr. Lankford assembled its membership. He received nominations, and spoke with agencies, committee leads, and others to determine the membership. There was no unique solution to the membership as there are many talented people in the community.

P5 will build on the Snowmass process and outcomes. It will enact community interaction, strive for transparency, and present rationale for its decisions. It will look for an independent review of the draft P5 report by HEPAP.

The HEPAP discussed the need for an adequate timeframe and forum to consider and react to the report, and for P5 to review HEPAP feedback

Dr. Steve Ritz is the P5 Chair. He discussed the charge, listed the key deliverables, and described the panel process.

An overarching deliverable is "to develop an updated strategic plan for U.S high energy physics that can be executed over a 10-year timescale, in the context of a 20-year global vision for the field". Dr. Ritz read the additional wording contained in the charge.

A second notable deliverable is to "examine the need to maintain a healthy and flexible domestic infrastructure so that the U.S. high energy physics program can deliver science results regularly throughout the coming decade".

The charge requests detail the need for construction, maintenance, and upgrades to HEP facilities. The panel must describe the needed level of resources for HEP core research and advanced R&D technology. The panel should also give perspective the fit of international partnerships such as the LHC upgrades and Japanese-hosted ILC into the program.

The charged asked for an update on the scientific questions driving the field, an articulation of the value of basic research and the broader impacts of HEP on other sciences and society, and the training of particle and accelerator physicists. Ritz pointed out that there are two supporting reports described broader science impacts and broader technology impacts.

Preliminary comments are due from the P5 by March 1, 2014, and will be presented to the public. The final report is due by May 1, 2014.

The P5 will look at three 10-year budget profiles. The first proposes an FY2013 budget with sequestration, then holding flat for three years, then increasing by two percent per year. The second scenario uses the FY14 President's budget request of around $750M, then holding flat for three years, then increasing by three percent per year. It is understood that "budget scenarios should not drive the prioritization to the degree that projects are promoted solely for their ability to fit within an assumed profile". The third scenario proposes an unconstrained budget.

Dr. Ritz believes that the P5 is being asked to show the next steps for the field, how it fits together, and why. The report is an opportunity to impress readers. The panel will make tough choices but also dream big and suggest bold, new initiatives. He believes that agencies will seek opportunities to make these projects come to fruition.

Snowmass output is important to the P5 process, and public meetings around the U.S. will allow for extensive discussions with the community about the P5, the process, and the issues. Two websites (http://interactions.org/p5 and http://usparticlephysics.org/p5) are under construction and will give frequent updates, and solicit information from the visitors. Ritz suggested linking this to the HEPAP and DPF websites.

Engagement plans recognize that community buy-in is critical. Some may not agree with the choices made by the P5 based on their individual interests, but the P5 will strive to offer clear rationale to ensure agreement with the plan and report. Ritz encouraged HEPAP to tell others to share input with P5 and with him personally.

P5 meeting and phone call plans and schedules are being developed. The first face-to-face meeting will occur during the week of October 14th. There will also be internal communication tools set up to enable efficient communication among P5 panel members.

This is an exciting time in particle physics. Agencies want to support interests and have a way ahead, and there is opportunity to unite the community. Ritz added that panel members are not there to represent constituencies but to look at the field broadly.

Discussion

Professor Erbacher asked if the panel should look at the no budget limits scenario first to determine what should be done, and then consider constraints. Ritz sees this scenario as a way to look at important things and to serve as an opportunity. Those who will do the negotiating need the details. How this information is reported depends on the choices that are made.

Dr. Tschirhart noted that the prior P5 defined a project as being around $5M. Ritz responded that the P5 has been asked to look at mid- to large-scale projects, and that small projects should not take away their attention. He is not sure if drawing a budget line is critical. The charge is not overly prescriptive. The panel can look at the issues and the possible options.

Dr. Siegrist confirmed for Dr. Ligeti that budget scenarios A and B were set based on a DOE perspective. These seem achievable and conservative. Scenario A is achievable at least through 2015. Scenario B is based on Congressional requests. He confirmed for Ligeti that the Office of Management and Budget (OMB) will know about the scenarios. These reflect the current OHEP environment. Ritz added that discussions held with other offices confirm that agencies will not eliminate scenario C and not consider the P5's suggestions. Scenarios A and B are realistic.

Ritz agreed with Professor Cvetic that the panel should start with an unconstrained budget and that science should take first precedence.

Dr. Caldwell clarified for Professor Fisher that the OHEP and NSF budgets are different. DOE's budget is built by line items. The NSF budget has four lines. One is research and related activities. This covers the research funding for all of NSF. Within this is the Division of Physics (PHY). Allocations for the NSF budget are made by the Office of the Director. NSF cannot say how much it will have in its budget for a span of years. About a third of the PHY budget is in this area of physics. P5 should consider scenario C and then things that cannot be done with the other scenarios. It should say what is most important and then identify missed opportunities.

Dr. Siegrist noted that the P5 should describe the level of participation in LHC. DOE is in the process of negotiating its involvement in CERN and the P5 will set the appropriate level.

Caldwell added that the largest fraction of the budget is research and related activities. There are concerns about the MRM7C process. That is a separate line item and is a one-time only investment in construction. It cannot be separated from anything that has to be built.

Bishai noted that the triple panel that was done for physics described some losses with statements such as the "field dies." Ritz commented that the narrative should be based on what the panel decides.

Tschirhart asked if the community will be polled or laboratories will be asked about initiatives. Ritz commented that the year-long Snowmass process can give that information but is not the only basis for information. The P5 should also identify what is missing. Community meetings can ask that question.

PUBLIC COMMENT

It was noted that scenarios A,B and C are a strong focus, but that people are looking at an upper echelon of development and how the field helps the Nation. Ritz clarified that the P5 can examine the charge language and advocating for the field. Those who have issues with the charge should tell the panel. The importance of HEP to particle physics will be front and center, and seek to create excitement, but collective action is needed if that does not carry over

Dr. Siegrist commented on a question about a charge from former SC Director Dr. Bill Brinkman to prioritize facilities. He has not heard whether there will be open meetings following input from meetings in Spring 2013. Congress has shared that they do not want a report from the SC staff but rather from a political appointee who will secede Brinkman. The Secretary noted that he is an appointee and perhaps he can hand-in the report.

Ritz clarified for the audience that he is not aware of any prohibition on instrumentation being excluded from the P5 considerations. Dr. Lankford added that consideration of HEP core research and technology, and other wording in the charge gives the P5 latitude to address instrumentation. The full text of the charge will be on the HEPAP website. Ritz added that this wording is also on the websites that he noted earlier.

Ritz noted that the report will indicate that this is a dynamic field with things changing all of the time. Lankford added that this has been brought up several times. There are related aspects that affect how to conduct the program. If there are small and medium-sized projects, they might not all map out in the P5 report. This is not in the charge but is important and needs resolution.

Tschirhart asked if the report will be a roadmap with milestones and branch points, noting that things such as running at 14 TeV could happen and there could be a significant announcement like Japan hosting a facility that would be a branch point. Ritz responded that a vision and roadmap are distinct. There can be assumptions with alternative realities. He sees that a roadmap would position assumptions as things that will happen in a certain way. Basing the report on rigid assumptions might cause it to be disregarded if the assumptions are not fulfilled.

Dr. Jawahery commented that the charge is cleverly written but shared frustration from the field that there is a mismatch between resources and what is needed to keep the field alive. Scenario C will be important if one outcome is that C is needed to sustain the field. He wondered if leaders would be willing to take this up to others. Ritz noted that there are a range of ideas, time dependencies, and reception for the P5 to provide a vision. He is convinced that all can see the potential for new opportunities and what will happen if there is no vision. Scenario C should not be seen as unrealistic but the panel should not write whatever it wants and offer options for A and B that are undesirable. Those two could be the fiscal realities.

Dr. Siegrist reminded the HEPAP of his talk at Snowmass and the Secretary's interest in supporting HEP if there is a plan supported by the community. Ritz added that the plan will be dead on arrival if there is perceived bickering about it. The report can be perceived as offering a grand vision and balancing hard choices. Both have to be done and the report has to show this. Jawahery commented that all are aware that there are many other fields that are constrained and HEP is expected to do the right thing. If the field is always limited, then there could be a perception that the community is not doing the right thing.

Dr. Weerts asked if parts of a roadmap already exist or are already inherent. There are some experiments underway about which there is no choice. Ritz responded that this is not explicitly written in the charge. Previous P5 reports are required reading for this panel, but it is not impossible to change directions. BES just made a big turn. These types of changes are difficult, and if the science changes and renders current projects futile then the panel may note this. Rationale is needed to explain redirections, and there is nothing stating that projects are above reconsideration. Lankford added that the charge includes guidance about considering input from Snowmass and the HEPAP Facilities Subcommittee Report. Changes are possible but they have to be well considered and well justified.

Tschirhart asked if the panel will validate costs or ask the DOE for help. Ritz responded that several methods been discussed. The panel membership includes expertise on budgeting and costing. The panel will find a good way to get advice if it is needed, and it may estimate to a certain level of precision in order to get the information it needs.

Ritz wants HEPAP to have input but the P5 cannot tweet every discussion. He asked for patience and for guidance if the panel makes errors. Updates on the process will be on the P5 website in hopes of generating community interest and those outside of the HEP.

Lankford confirmed for Erbacher that there will be a status report at the next HEPAP meeting but doubts that there will be interim conclusions. Ritz assumes that there will be a status report provided that people share their input prior to the meeting. Lankford told Erbacher that HEPAP involvement in closed meetings would be inconsistent with the efficacy of the process. A session with the P5 can be arranged if HEPAP feels that it needs one.

Lankford explained to Bishai the apparent lack of younger representatives on the panel. He formed the panel. Community engagement will involve younger members. One reason for exclusion of those who are new in their careers and non-tenured is that issues may surface that could be damaging to their careers. He does want to make someone vulnerable, especially someone who has more of their career to look forward too compared with those who are later in their careers. The panel consists of some mid-career people, and the panel represents the right levels of knowledge, experience and contextual knowledge. Ritz added that Lankford was systematic in bringing in the members, and there are a large number of people could have been on the committee and many are at this HEPAP meeting.

Dr. Siegrist emphasized that HEPAP's role is important and provides an independent review. The DOE regards HEPAP's review and discussion as a way to further engage the community. A valid discussion about priorities is needed. HEPAP can create community awareness and understanding of the arguments, issues and rationale.

CLOSING REMARKS AND ADJOURNMENT

HEPAP Chair Lankford adjourned the meeting for day one at 6:10 p.m. EST.

SEPTEMBER 6, 2013

The High Energy Physics Advisory Panel (HEPAP) was convened at 8:35 a.m. EST on Friday, September 6, 2013, by Panel Chair Andrew Lankford.

PRESENTATION OF THE DOE OFFICE OF SCIENCE PERSPECTIVE

Dr. James Siegrist presented details on the OHEP FY14 budget request. There is an increase for research due to an addition of R&D funds that were project funds designated for new projects that could not be started. The impact is that new efforts have been delayed, U.S. leadership will be challenged, and workforce reductions at universities and laboratories could occur.

The FY14 request allows for progress on new projects via construction and research. DOE is waiting for Senate and House budget decisions, and a Continuing Resolution may occur.

The SC was unable to implement most new Major Item of Equipment (MIE) starts in the FY14 request. The Muon g-2 experiment was the only new start in HEP that was not requested in FY13 as it was conveyed as a recycling effort that then drove toward budget decision making. LSST-Camera and Belle-II did not receive approval in FY13 but are requested again in FY14.

SC's FY12 actual budget was $4.873B. The FY13 July plan was $4.621B, and the FY14 request is $5.152B. OHEP's FY13 actual was $770.5M, the FY13 July plan was $727.5M, and the FY14 request is $776.5. By activity, there is a reduction in research mostly due to ILC R&D decreases. Other changes include an increase in the cosmic frontier with LSST moving along. Construction is increased mostly due to Mu2e and no ramp-up for the LBNE.

One take-away message from these figures is that the program is following a plan laid out by prior P5 studies. Boundary conditions have changed but there is still an effort to implement the plan despite constraints. OHEP has to maintain progress with projects already on the books and attract international partnerships to extend its scientific impact. There is increased emphasis on broader impacts via the accelerator stewardship, and the Senate supports this program and finds it to be non-controversial as they are responsible for having created the stewardship program. OHEP's only hope is to out-innovate and exploit unique U.S. capabilities.

OHEP's goals for the Snowmass meeting are to identify the most compelling science questions to be addressed, the approaches to address those questions, and the hard questions that facilities need to answer. The P5 will use these outcomes and draft reports as a starting point.

Core deliverables for P5 report are to examine current plans and proposed research capabilities, assess their role and potential for advancement, consider their uniqueness and scientific impact in a global context, and think about the time and research needed. HEPAP should also look at the balance of small to large experiments and identify multiple and complementary pathways to address the science questions.

The P5 will prioritize projects over a 20-year timeframe within reasonable budget assumptions and ways to position the U.S. to be a leader in some areas of HEP. This will involve the consideration of technical feasibility, and the need of domestic HEP facilities to maintain a global leadership positions. The P5 will also look at international partnerships.

The P5 should not examine agency review processes. It will not look at the role, duties and funding of laboratories and universities. And it will not look at the funding of experimental HEP versus theory versus R&D. These latter items should be considered by HEPAP in the future and OHEP is working with Dr. Lankford to identify the key topics to review.

Working groups will address important supporting work. The P5 is tasked to update the Quantum Universe questions in parallel with discussions of scientific priorities. Dr. Siegrist has worked with associate directors in the SC to identify candidates for these working groups. There are also two non-P5 working groups that will highlight scientific areas where HEP advances can inform, support and benefit from other DOE SC programs. This will show the value of the ideas, research and work that is done in HEP, and results will be documented.

The first working group is entitled Science Connections and will look at the science connections with elementary particle physics, and between HEP and research in other areas. It will consider how to identify synergies in Q1 to advance DOE Basic Science, OHEP or SC. The group will see if model systems or techniques in other disciplines can help test other particle physics ideas, and if there are areas that are ripe for testing. And, it will determine if advances can be made by sharing math and computational techniques between disciplines.

A working group called HEP Impact will look at the impact of particle physics discoveries, tool and technologies driven by research, and impacts on other scientific fields and the Nation. It will look at the benefits of particle physics from technological exchanges with other sciences and industry, and the interactions of the particle physics workforce with society and industry.

Reports from both groups will help develop a potential list of messages for the U.S. HEP community to use in communicating the broad impact of HEP in technology, workforce development, and other societal benefits. Group reports will be available by the end of 2013 to provide timely input to the P5. Other groups and inputs that may support P5 will be considered.

HEPAP is supported by the FACA and gives advice on the U.S. program in HEP. This is the only mechanism that agencies use to get formal advice from the community. Questions about science opportunities and vision for the field come through FACAs.

The Astronomy and Astrophysics Advisory Committee is a FACA that interacts with OHEP but reports to Congress. HEPAP's work is done through limited term subpanels like the P5. HEPAP reviews and approves subpanel reports and sends them to agencies. Dr. Siegrist would prefer that HEPAP not be a rubber stamp that moves reports to OHEP but rather strives to have community-based discussions of reports. Agencies work to implement panel recommendations, and have separate committees provide for the external review of funding, decisions, process and other things. An example of this process at work is questions that emerged from Snowmass, and community input via HEPAP offering ways to address how these are answered. Actual implementation of this is the agencies' responsibility.

A Committee of Visitors (COV) will occur on October 9 – 11, 2013, in Germantown, MD. There will be seven subgroups consisting of community members to review processes in the office undertaken over FY10 – FY12. The standard COV charge gives guidance to the COV. The COV will look at a large and expanding list of items, and a list of more than 25 additional and detailed questions. Documentation will be shared via a secure website with COV members in advance of their review. Patricia Dehmer of SC is responsible for the COV.

OHEP released a comparative review Funding Opportunity Announcement on June 14, 2013.

IN FY12, HEP stated a process for comparative review of grants scheduled for renewal. Grants that were not reviewed in FY12 were not affected by this change. Prior to this, all HEP proposals responding to the SC call were individually peer-reviewed by independent experts. This effort seeks to improve the quality and efficacy of HEP research by identifying proposals with the potential for the highest scientific impact. This process and outcome for FY12 will be one of the main topics of the upcoming HEP COV.

The next round of the Early Career Program (ECP) will occur in FY14. This will be the fifth year for the program and some candidates will no longer be eligible due to a three strikes rule. Pre-applications are due on September 5th, and SC has received 94 pre-proposals. Full proposals are due on November 19, and more than three months is available to develop a plan, write a narrative, and submit an application. HEP selects from the ECP awardees its Presidential Early Career Award for Science and Engineering (PECASE) nominees. There have been 48 ECP awards since FY10 across the frontiers. The program has a 10 percent acceptance rate.

Discussion

Dr. Siegrist confirmed for Professor Fisher that there will be two chairs per P5 working group, and that they are recruiting panel members including non-HEP people to help described why HEP is great. All are volunteers.

Dr. Tschirhart asked if HEPAP could meet outside of Washington D.C. to get more community input, and if there are travel restrictions that prevent program managers from being out in the field. Siegrist commented that the DOE does not even have Skype. This topic will be addressed later.

Dr. Bishai shared concern about the comment that HEP has to out-innovate competitors, noting that research is publicly-funded and that the competition capitalized on opportunities faster. Scientists are not failing and there are innovations being made. Siegrist responded that programs need vision. He cited CERN's interest in DOE building magnets because it does not want to develop this technology on its own and working alone would be less effective. Siegrist hopes that OMB can take the action on this as DOE is in a good position to help as it made the right investments some time ago. Bishai added that innovation relies on technical expertise at laboratories and universities, but this is often cut when budget constraints are in place. People with decades of experience are let go and organizations cannot hire experts from technical firms.

Professor Erbacher asked if recommendations that come from COVs to the HEPAP are final or can be reviewed. Siegrist confirmed that COV reports to HEPAP are open for review and comment. He asked Dr. Glen Crawford if the questions that the COV will address are available online. Lankford will talk with Paul Grannis, the Chair of the next COV, to determine the level of discussion that is needed with all of HEPAP. The charge is on HEPAP's secure website. Siegrist noted that Lankford and Grannis came up with the questions that will be considered. Lankford hopes that many of the questions that are not addressed during the single COV review can be addressed in a follow-up. The COV is a unique chance to review OHEP's activities and that is the primary goal. Erbacher added that COV members are asking for input from the community, and that the list of questions already posed would be helpful.

Professor Jawahery pointed out that there are many applications for jobs at places like CERN or for faculty positions, but few openings. Soon there may be little opportunity to identify open positions and OHEP may need to look at the mismatch between the way that the field is evolving and how universities are hiring. In Europe, staff positions and other options offset this challenge, but the U.S. is more limited. Lankford shared that this topic has come up and can be addressed later. It is closely related to mentoring members of the field for careers outside of HEP. The lack of permanent positions will always be an aspect of HEP. Siegrist added that advice should be sought from HEPAP about pathways, noting that universities and physics departments seem to be shifting their focus to green energy. This is closely related to the P5's charge.

Jawahery is concerned that fewer people are retiring, and even if with more projects in five to 10 years, the right people may not be available. Erbacher added that the number of graduate students in the coming years in HEP could plummet as people in their 20s are looking at trends in the field and see a lack of opportunities.

Professor Cvetic suggested that the P5 highlight the impact of HEP on mathematics, along with other fields, as the impact is broad and tremendous. Siegrist shared that the group will talk to mathematicians.

Dr. Bassler asked how budget changes will delay and impact programs. Siegrist shared that DOE is trying to keep its technical progress on track. Mike Precario added that the regulations and rules on project starts vary with each agency, and they have different agreements with OMB and Congress. This can cause confusion. If there is a continuing resolution, then things will have to stop. DOE is trying to convince them to do something despite the potential for a continuing resolution. Siegrist added that OMB has a list of options that allow project starts despite a continuing resolution. In the meantime, technical progress is ongoing where there are project funds. He hopes that this year Congress realizes that continuing resolutions can cause problems.

Professor Hoffstaetter asked about shifting project funds to R&D funding, and if this changed relative to funding between universities and laboratories. Siegrist shared that that was not the case and institutions were carefully guided not to do this. He hopes that HEPAP will have input on this.

Tschirhart recalled a prior COV that discussed the right number of CD0s in the system and that OHEP should have more to take advantage of the system. He asked about an optimum number of CD0s. Siegrist commented that having CD0s in an area results in collecting project costs. The CD0 for large projects should start as late as possible, but R&D can be done and risks can be reduced before construction starts. With LBNE, DOE needed to get into CD1 to start discussions with international partners. CD2 is delayed in order to achieve a sense of their contributions before seeking DOE support. OHEP is holding on CD0s until it gets more information about a potential continuing resolution. There were five rather small CD0s in September 2012. For example, there is R&D work on LHC magnets but no CD0 for High Luminosity LHC as the P5 report will guide that pathway.

Professor Rosenberg commented that is appears that it is standard practice to not fund proposals from first year professors. Dr. Crawford acknowledged that this issue has come up before. DOE gives lower priority to proposals from first-year incoming assistant professors as they lack the track record for conducting research in the university context, and are moving into a role where they are teaching, doing research, and becoming full professors. DOE's approach is that is wants people who have track records. Erbacher added that this hurts when there is a three-strike rule, and wondered if junior professors should be excluded from the submission process when they are just getting started or be told that DOE will not take them seriously. Crawford confirmed that the three-strike rule is just for ECP. Program managers have emphasized to junior faculty that they need to consider this, that they have three-strikes, and that there is a lower chance of acceptance in their first year.

Dr. Caldwell added that NSF PHY does not make pre-decisions about who should and should not apply, but rather uses a proposal and review-based approach, and encourages young professors to submit a proposal as soon as possible. The ECP is an NSF program with a separate set of rules. In PHY, one can start with a regular award through a regular program. Submitters will get feedback from the community. NSF's COV looks at how is it promoting young researchers, and inspects how things are reviewed and how decisions are made to ensure contributions from junior and starting faculty.

Crawford commented that DOE encourages people to apply to the ECP and be aware of the three-strike policy. This is DOE's way to encourage people to write a proposal, think about its value, and get feedback. Several awardees in 2012 were junior and first-year faculty.

Dr. Merminga asked if under the P5 scenarios A and B, new projects will leverage components of the OHEP FY14 budget request and if that is the basis for the scenarios. Siegrist commented that the budget numbers that are being used are from the FY13 plan. Siegrist added that P5 will have to determine if it will work with the FY14 numbers.

PRESENTATION OF NEWS FROM NSF DIVISION OF PHYSICS

Fleming Crim, Assistant Director of the NSF Mathematical and Physical Sciences Directorate (MPS), reported on the structure of the NSF and enacted FY13 budgets for various part of NSF. MPS has a budget of $1.250B shared by five divisions and an Office of

Multidisciplinary Activities. Each division has a different character. Program directors in each division think differently about how each builds its programs as not all of them have large facilities components. Facilities are how the core community of researchers gets their research done and this attests to the importance of not separating facilities from research funding.

The Astronomy Division (AST) facilities include the Advanced Technology Solar Telescope and National Solar Observatory. Physics funds LHC, LIGO and NSCL, among others. Materials Research supports three facilities including the Cornell High Energy Synchrotron Source.

The MPS budget since 2000 has stayed between $700M and $1,000M in constant dollars. At the same time, the number of proposals grew from just over 4,000 in 2000 to around 8,000 in 2012. NSF funded more awards in 2012 but the growth is not comparable with the number of proposals. This creates a tough environment with budget constraints and the need to support young faculty. The brochure at www.nsf.gov/mps describes the breakout for each division.

The FY13 budget for MPS across its five divisions decreased by 4.5 percent compared with FY12, going from $1,309M to $1,250M. The Physics Division (PHY) took a larger hit than the other divisions but there is an attempt to recover this in the FY14 request that totals $1,386M. There was a commitment to not decrease the career awards and not hold back support for existing awards. NSF will also largely meet the obligations it has to initiatives.

NSF is looking at the new budget reality of a flat budget. History shows that there are increases and flat budgets. This leads to difficult decisions and community priorities are essential. The P5 process will give guidance as NSF looks at the future. However, there will be tough decisions ahead. Community advice is important to NSF and helps shape programs.

NSF's connection with the DOE is important, especially in MPS. Within Major Research Equipment and Facilities Construction (MREFC), NSF is committed to near-term projects but longer-term investments will require a compelling case from NSF, from outside, and in the context of science. New commitments require operational funds and that drives budgets. The P5 and HEPAP need to think about budget realities, and community input is important.

Discussion

Dr. Crim described MFERC commitments as near-term and clarified for Professor Fisher that this process is slow. It is hard to put a date on this but likely the next decade unless something compelling comes along. NSF is waiting for an MFERC budget from Congress and a start seven years from now would be considered good.

Professor Hoffstaetter asked if there is an ideal budget allotment by percentage for each division and how that looks now. Crim explained that the facilities are provided enough support to do what is needed and then the rest is for projects. The AST feels that a 57 percent level for facilities is tight. Physics uses 30 percent for facilities and that seems right.

Crim added that there is a hole in the portfolio for mid-scale projects, defined as projects around $4M to $10M. NSF is trying to move funding to start projects. Dr. Caldwell added that NSF would like to have distinct funding for facilities. The MFERC is one-time funding for construction. Otherwise, discussion of facilities has to do with operations costs. Those funds compete with support for principal investigators (PI), and decisions are made on supporting operations or research. When there are no budget constraints, physics would allot no more than 50 percent for facilities, operations support, and centers. Fifty percent will always support researchers. During budget shortfalls, NSF draws facility funds. Crim noted that the National Science Board has asked MPS to strive for a goal of 20 to 25 percent of funding for facilities and he thinks the level is now around 22 percent. Some directorates do not have facilities.

Crim shared details on the mid-scale projects with Professor Shutt. The Astronomy and PHY divisions both have identified small amounts of funding for this as there are many important projects at this level. An example is instruments on telescopes. MPS is lobbying for these examples and believes it could grow the program to $150M if it met all of the needs that exist. He added that these projects would be small and run like centers in NSF's very large grants. Caldwell added that there is a "Dear Colleague" letter that asks for funding for more than a normal grant would give. Proposals would come through the program and should have scientific merit and go through a science program to be evaluated in a panel. The scientific value would be judged in comparison to other proposals. Mid-scale funds would support construction of needed equipment. Proposals must note if they will provide operating funds from their own program.

Crim told Dr. Byrum that this will be an ongoing program. Modest $10M to $20M projects could be funded over a number of years. Even starting with modest amounts, something on this larger scale could get started.

PRESENTATION ON THE NSF MPS PHYSICS DIVISION

Dr. Jean Cottam of the NSF described the direction for the Physics Division (PHY). She noted that most of what HEPAP considers is funded through PHY but the AST plays a key role.

PHY is broken into Facilities, Experiment, Theory, and cross-cutting areas that include Physics at the Information Frontier, Education, Physics Frontier Centers, and Physics Instrumentation. Among personnel changes at MPS, Dr. Crim joined in January 2013. Dr. Caldwell is now the permanent Division Director. Dr. Jim Whitmore is the acting Deputy Division Director. Dr. Keith Denise was the Program Director for HEP Theory and Theoretical Astrophysics and Cosmology. NSF requires rotations and Keith will leave NSF, and be replaced by Dr. Marc Sher.

The overall NSF budget was reduced by 3.1 percent and MPS decreased by 4.5 percent from FY12 actual funds to FY13 enacted funds. Former NSF Director Subra Suresh released a letter about sequestration indicating a desire to protect specific areas and hence resulting in a decrease in new award funding. PHY was reduced by 9.6 percent with major impacts. A 12 percent reduction in research program funding led to a reduction of about 36 percent for new awards. There are also fewer awards in some programs, and less support for faculty, postdoctoral researchers and students. As an example, the Theory Program implemented a cap on summer salaries. Within Particle Physics, there was a shift from $98.4M in FY08 to an estimated budget of $88.6M for FY13. MPS also uses allied funding from other NSF divisions to identify mutually-beneficial programs. This will represent an estimated $20.8M in funding in FY13. The science questions are compelling and NSF cannot rest despite budget constraints.

The Accelerator Science Program is a new addition for FY14. It will enable fundamental discoveries and train students and postdoctoral students across disciplinary boundaries. Proposals are due on November 26th. DM Experiments are also new. This is a single solicitation and one-time funding, with proposals due on November 26th. This will be closely coordinated with DOE with a joint panel making selections. The Mid-Scale Instrumentation Fund is another new activity. It is not a separate program, but will operate through regular programs. Program directors will request funds for equipment from the divisions to support those proposals.

Professor Shutt asked if people will think about the process in the Mid-Scale Instrumentation Fund as they develop proposals. Dr. Cottam shared that proposers will not have to identify the source of funds -- PHY will work out that detail. Mid-scale starts at $4M and proposals will go to the MFERC Board when they reach $10M. Dr. Crim added that a desire to be science-driven is pushing for work beyond the $4M cap. The need for hardware to do the science is clear.

Cottam noted for Professor Erbacher that the one-time allotment of funds is a one-time commitment to build the equipment.

Shutt added that many activities have a mix of DOE and NSF support. Cottam noted that this will be similar and is additional support for programs to implement needed science.

Professor Gerber asked about cooperative agreements. Cottam sees that proposing will be done as usual. This gives NSF resources to make the proposal work. Saul Gonzalez added that these are internal funds to support projects of a certain scale. Cooperative agreements are a funding instrument that is a regular award with more cooperation. NSF wants to be responsive and responsible funding stewards. This arrangement is less onerous than a contract with more cooperation than a regular award. Caldwell added that NSF would like to maintain more contact with activities that are ongoing when the complexity of the activity is sufficiently large. The Physics Frontier Centers are funded through agreements and communication is important.

Caldwell noted confusion around the Major Research Instrumentation Program and explained that is a solicitation to which a proposal is directly submitted. That is for equipment and equipment construction. There is no guarantee from NSF that funding will support operations once equipment is built. This program will avoid difficulty associated with a lack of funding for the instrumentation. Proposals will describe the equipment that is needed.

Cottam added that proposals will be presented in the usual way. Great ideas that are more expensive can be proposed, and NSF will have more freedom than it would in a regular program.

Byrum noted that NSF has a broader science program than does the DOE. Some mid-scale programs will carry both agency requests. She asked how mid-scale projects would impact the alliance between agencies. Crim noted that both want to capitalize on opportunities and ideas.

Cottam summarized the target dates for the current program and dates for one-time solicitations. The mid-scale effort does not have a timeframe as proposers will go through the other programs.

Cottam noted that the P5 process is off to a great start. NSF has requested a perspective on scientific opportunities over the next 20 years. Science is the goal and projects enable this goal but are not the goal itself. NSF is most likely to fund small, medium and the well-defined science side of large experiments. Due to budget constraints, small and medium approaches are needed to achieve big science.

NSF can be flexible with what comes out of the community. It is mindful of academia as the P5 process begins and of achieving a healthy balance for laboratories and universities.

NSF responds to ideas and proposals, but the current budget environment does not fund every excellent proposal and has to make programmatic decisions based on the P5's conclusions.

Discussion

Erbacher noted that the selections for a DOE competition for DM Next Generation were presented to HEPAP in March 2013, and that some would be funded for a trial period to build a prototype. She noted similarities to the new DM initiative and commented that NSF and DOE will coordinate their selection. Dr. Siegrist responded that NSF and DOE are coordinating. Both are funding their processes and will come together at the joint review.

Gerber asked about a funding drop for MPS and PHY compared to other NSF Directorates. Crim responded that the discussions have to do with cross-cutting initiatives and what the organization is doing. The Engineering Directorate did well as they have a big role in genomes work and industry initiatives. Decisions were based on OMB priorities and other priorities designed to lock-up certain funding levels. It involves shifting $20M to $30M in response to several initiatives. Historically, MPS always gets a bigger cut and lower increases, partially because it is a larger directorate. The fractions are not always indicative of the numerical cuts. Timing is a facet as in some years NSF is forced to make a cut that it can fix in an out year.

Hoffstaetter noted that there is a division in MPS for cross-disciplinary activities and asked if there is something similar one level above MPS. Crim confirmed that there is another office and funds that can be drawn upon. There is a new approach that will let funds from the Office of Multidisciplinary Activities and use this to get other funds.

Dr. Bishai asked about the new accelerator program, adding that historically DOE was mandated to be the steward in this area. Gonzalez clarified that NSF works with academia and the community to identify enablers needed for fundamental science and for transformational developments in accelerator science. There is also a focus on students, postdoctoral students, faculty, travel to facilities, and the workforce. The program will grow based on the identification of challenges by the community, and it will fund accelerator science in the same way that NSF funds particle astrophysics. NSF will not build accelerators.

PRESENTATION ON ACCELERATOR STEWARDSHIP

Dr. Eric Colby provided the OHEP update on accelerator R&D stewardship. OHEP has supported accelerator technology and its long-term vision. A workshop on accelerators in 2009 posed how technological advances could be put to greater use. This led to a 2010 report "Accelerators for America's Future," and a Senate request in 2011 for a 10-year plan describing how issues raised at this workshop would be addressed.

Community input is needed and OHEP is leading the development of a strategic plan for accelerator stewardship to respond to national needs. An approach has been developed.

The 2012 Accelerator Task Force Report recommended the identification of research opportunities. Opportunities have been noted in the medical field and in national security. The report summarized the status of key research areas and identified technology areas, and possible impediments to successful R&D accelerator stewardship for an envisioned broad user base.

Accelerator R&D consists of eight core technological areas. These link to science drivers or "push," and identification areas of application or "pull." Now there is a need to identify ways to push these technologies into key areas.

In 2012, the mission for stewardship was carried out to facilitate industry and agency user access to facilities. In addition, attempts will be made in the next five to seven years to solve different problems identified by customers in various fields, and then to declare these done. These will serve as catalysts to broaden and strengthen the community.

Dr. Siegrist is the owner of the Stewardship Program. He is advised by the SC Policy Committee. When the program is formally funded by Congress, a stewardship manager will be picked along with a technical evaluation group. Stakeholder boards related to primary topics will address thrust areas and problems. Key constituencies will be selected. The vision is that funding opportunity announcements (FOA) will go out, and proposals will be peer-reviewed. Reviewers will represent a broad spectrum and collaborative accelerator research teams. An FOA will assess the quality of the partners combined to do work, and plans will be documented and part of particle accelerators plan.

In 2012, Siegrist sent a survey to laboratories to learn what technology is available to support these questions and customers. The response showed a wide cross-section of facilities and that laboratory infrastructure falls into seven areas. A follow-up survey in 2013 or 2014 will ask laboratories to conduct user surveys to understand activities.

Three focus areas have been identified for doing work. The areas are the improvement of particle beam delivery and control for cancer therapy facilities, laser development addressing the needs of the accelerator community, and topics in energy and environment.

Two workshops were held in early 2013. The first addressed ion beam therapy and the second looked at laser technology for accelerators. Both meetings were small and produced recommendations. Energy and environment is a third topic that evolved from the workshops and is part of a Presidential order to improve accelerator facilities.

The ion beam therapy workshop was conducted by the DOE and National Institutes of Health on January 9 – 11, 2013, and asked about the direction of the technology. The charged asked for the identification of clinical applications. The workshop produced a report with eight themes.

There are just a few of these facilities that operate in this area and all are overseas. There are a small number of patients treated to date and little data to document the clinical efficacy of this treatment. Further studies are needed. Machine R&D is needed to support this work, as well as future facilities for multiple ion species. The report urged that collaboration with global facilities is needed to leverage their experience.

The second workshop on laser technology for accelerators was held on January 23 – 25, 2013, and was organized by LBNL. A report was produced that identified primary laser accelerator applications, technical gaps, and an R&D program. It compared worldwide R&D efforts. This area is different in that there are customers inside and outside of OHEP. There are outside users in defense in BES who want high repetition rates and high peak power to use high scale radiation sources. The attendees determined that one high impact and underfunded area is ultrafast lasers (<1 ps) operating at high peak power (>10 TW) and high average power (>1 kW), and highest power efficiency (>20 percent).

The energy and environment thrust is unique. It was one of many areas shared at the Accelerators for America workshop. There is a Presidential mandate for a nearly 30 percent reduction in greenhouse gases by FY20. There is a broader class of questions to be addressed.

In the area of the environment, topics are not new. These include pilot plants and pollution reduction, waste treatment, and reducing pesticide and pharmaceutical levels in waste water. Improved accelerator efficiency and lower beam powers could be a benefit of this focus. Industry was asked if accelerators could deliver heat more efficiently.

OHEP wants to talk with industry to determine if there is a set of topics where a modest investment could catalyze work and generate a return. Expert input is needed.

The first of five criteria for good accelerator stewardship is that applications must evolve in accelerators and less obvious is that DOE prefers a research aspect over the development side. The process is not open to whatever industry wants. DOE needs to pick topics that programs can solve and that lead to facility improvements. Development can be handled by others.

The non-trivial intellectual involvement of laboratories is a second criterion. The interaction must create something and not just machine parts. The best outcome is to design, build and test an accelerator technology component. DOE's substantial involvement would be the change to learn something and connect with industries that have skill sets that differ than those in DOE.

The third is that activities must be consistent with the mission of laboratory.

Laboratories must also be the best provider of a capability or service, and the best outcome is that the laboratory develops a new core skill or capability. On the business side, the laboratory must determine if the physical labor is close to the industry that is being engaged.

The fifth is that customers would benefit regardless of the level of their role, and that DOE sees clear evidence that whatever was proposed is clearly what the customer wants. The best outcome is a research partnership with the customer sharing a significant part of the cost.

Success is defined by opening test facilities and addressing major themes. This requires co-investments, and standard measures such as patents and publications, to improve quality and utilization and form intellectual connections.

In ion beam work, a new set of components and magnets would be tested and commercialized, and beam capability and technology would improve.

Laser technology would advance and new applications would evolve that push toward GeV demonstration for potential HEP application to serve as drivers for a new facility and possible science centers.

In energy and efficient power systems, the SC accelerator would profit by reduced power consumption. The next result is a significant impact on greenhouse gas emissions across the SC.

Budget language exists for these goals. These three depend on a budget going forward with $20M in funding. The FOA will be stalled if this does not happen.

More broadly, this needs to move from government to industry. The National Network for Manufacturing Innovation (NNMI) was announced. The effort has independent funding from the Department of Commerce (DOC), and should be operating on separate funding within seven years. There is a clear model and scale for the government push in this area. One group is already established, three initiatives are proposed, and an RFP will be produced.

In a broader context, most accelerator R&D stewardship activities will carry R&D through to prototype testing. DOE will operate at a technology readiness level (TRL) of TRL1 through TRL6 based on a TRL chart produced by the NNMI. The DOC TRL range overlaps somewhat and includes manufacturing readiness levels (MRL) that are outside of the DOE scope. Work for others ranges from TRL7 through 9. Industry can approach laboratories and hire them for work. The goal with this broad view is to work in the same risk and research domain and try to carry new applications for technology to a point where industry or other agencies will pick this up.

Another important interface is working with Small Business Innovation Research / Small Business Technology Transfer (SBIR / STTF) and the Technology Transfer Office (TTO). Stewardship will build a thinking environment and not participate in the manufacturing side of the business. The type of work can be whatever the laboratories and industry agree to, and topic selections will come from different places. The DOE can take community input and then will also talk with agencies and see if they want to pursue suggested ideas.

The funding mechanism for accelerator stewardship is FOAs and peer-review.

There is a rich opportunity by making modest investments to push leadership in accelerator technology into a broader set of applications. This will lead to contact with other agencies and industry that will bring up questions that will require broad skill sets. Each relationship will need to preserve the value of HEP and honor the OHEP mission. This will not impact a long-term view of accelerator work in HEP. HEP is a field that has looked to agencies to serve as the guardian with that long view. There are others in the field who aim for long goals and when they fail, this bring down the field. Stewardship will strengthen interactions with new agencies and this is a way to enable long-term social contributions from our research.

Discussion

Dr. Bishai highlighted the next generation of high intensity proton machines and the need for more R&D. There were efforts to bring in non-U.S. groups yet that effort does not seem to be considered here. Colby explained that other U.S. agencies have fiduciary responsibility for that technology. In reviewing their roadmap, accelerators are not mentioned and that is a problem. The primary problem is not having a customer on the other end for the technology.

Dr. Tschirhart agreed that with accelerator technology there is no customer in nuclear energy, yet cited the accelerator presentation on day one of the HEPAP meeting that described targeting particular physics as critical. Colby shared that high-powered targeting and accelerators are a need. In SC, targeting is identified as critical and central for fusion energy science (FES). The best example is cross-disciplinary areas in SC and the hope that a list of customers can grow.

Bishai added that ITER is worrying about the impact of protons and ions on materials. This is related to material ion and proton beams yet there is no discussion of cross-disciplinary work there. Colby shared that SC FES has not raised this as a primary issue that they would like to address. Key to this is identifying a customer. Tschirhart added that OHEP has its own facilities planning and lists FES as essential.

Professor Hoffstaetter sees a strict focus on customer orientation in accelerator development as a problem. This view has worked in HEP and led to new technology, but in other fields there has not been this long-term request for things such as light sources. The accelerator side has driven the research more and prior to a request. Colby agreed that at times the investment should be in the best science, look at the long-term, and understanding other applications. That is where the core program has a role. More targeted efforts are needed when it comes to picking specific applications. Stewardship's goals are to identify a customer who will benefit, involve them from the start, and hand things off to them when the DOE is done. DOE wants the customer to have benefitted. Long-term investing is still needed.

Dr. Merminga commented that success should include technology transfer to industry more quickly. Facilities and test beds are opened to industry to do experiments but then there is co-development and synergy on projects. In the end, once something is developed, they can take it from there. Colby agreed up to the point that there are some metrics that he did not present that include intellectual property. If all agree that industry should be involved, then assurance is needed on how intellectual property is handled. DOE recognizes that necessity.

Dr. Ben-Zvi noted that support for long-term programs is a hallmark of the OHEP. The delivery of beams for medical applications includes long-term projects that industry may not support due to their conservative nature. A customer may not disclose that they want something to be in place in 10 years. He sees problems with the timescale as ions may have long-term issues. Colby noted that this raises a distinction between technology as a long-term issue and the program's desire to be a catalyst and jointly do research and transfer technologies from the OHEP out of HEP use. That is a distinct way to draw a timeline. Other agencies occasionally look at longer-term things in this area. In medicine, there are HEP applications as well such as laser acceleration of electrons, and within the core program that is part of the long-term. The stewardship goal is to work in five to seven year bites with a customer that is advocating for the results, sets goals, and solves problems.

Colby clarified for Professor Jawahery that this connects with accelerator activity in BES. It has a program that pursues long-term technologies to advance light source capability. Laser technology is one area where there is synergy. OHEP is looking at the need for laser-driven schemes. Entry into that technology will have broad application for BES. Colby is not aware that these efforts will merge but are coordinating.

Professor Erbacher asked what will happen if funding for this does not come through. Colby shared that OHEP has looked at the portions of work that would qualify, have broad applicability, and already meet the definition of stewardship. These areas have not been redirected. There is no new funding for this and no intent to make this an unfunded mandate. The argument against this is that if there is need to do this and it is worthwhile to industry. DOC has the fiduciary hurdle for doing technology transfer. OHEP would argue that work for the industrialization side is a responsibility that should be given to them. He has heard nothing to date that this would be an unfunded mandate.

Colby confirmed for Tschirhart that academia could be involved on the collaborative teams that are built. Technology providers and the customer are mandatory. A more diverse set of skills, when needed, should draw on academia. OHEP expects universities to apply for funding.

PRESENTATION ON THE FUTURE OF U.S. PARTICLE THEORY AND THE REPORT FROM THE DIVISION OF PARTICLE AND FIELDS PANEL

Dr. Sally Dawson of Brookhaven National Laboratory shared the report from the Division of Particle and Fields (DPF) panel. The presentation was a continuation of Snowmass reports heard on day one of the HEPAP meeting. The panel was chaired by Michael Dine. The panel was charged to understand the scientific questions and prospects for the next decade, and to understand challenges involved in sustaining a first class program in the U.S.

The panel concluded that the U.S. should maintain a vigorous research effort in theoretical particle physics, ranging from perturbative and non-perturbative QCD studies to collider phenomenology, to model building, cosmology, and research in foundational areas.

Input was provided by email, from DOE and NSF, Snowmass discussions, and town hall meetings. This helped recognize the past successes of the U.S. theory efforts, illustrated open questions for U.S. theory in the future, and fed into recommendations from the panel for DOE and NSF consideration and for the DOE and NSF COV.

Successes have included contributions to the intensity, energy and cosmic frontiers, and supports work in the SM model, CKM phase and CP violation, neutrino masses and oscillation, and AdS / CFT correspondence. The panel felt that full conclusions can be reached even after discovering the Higgs boson, as there are many why and how questions to be answered.

There are many questions to be answered by the field, but it is recognized that its work crosses the frontiers.

Challenges include reduced funding from NSF, a declining DOE budget and increases to the project fraction of the budget, and that the model being used right now has stressful impacts. The move to remove funds from research and projects should think of theory differently.

One recommendation is to maintain the vitality and international competitiveness of both the laboratory and university based theory programs. Recommendations four and five are the major conclusions. The breadth of topics supported in particle theory should be maintained and the formula of funding the best and most interesting research should not be changed. The panel advocated that programmatic considerations in funding decisions be kept at a minimal level, and 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. The panel also recommended that HEPAP consider how resources should be balanced to counter the vulnerability of laboratory and university theory groups that are vital to theory efforts in the U.S.

Nearly 100 U.S. experimental theorists signed a statement declaring that a strong experimental high-energy physics program requires a vibrant theoretical physics community in the U.S. There is widespread community support for a broad-based theory program in the U.S.

and the theory program faces serious challenges in the future. The panel encouraged further study of this issue by the HEPAP.

Discussion

Dr. Bishai noted earlier comments that LHC can do precision measurements of Higgs. There is a similar problem in neutrino physics, but that requires nuclear phenomenology. There are problems when they are not part of the HEP program. Dawson confirmed that this was discussed and the panel suggested small networks with a particular focus. This is the way that nuclear theory groups work.

Professor Han agreed that program damage should be minimal, that the work crosses frontiers, and that it should not be categorized as program and project-driven. He is concerned about young researchers, and can think of three examples of funding situations where proposals receive funding for one year. These are strongly established and influential in the community. He hopes that other solutions can come up. Dawson shared that the panel report notes the importance of these programs.

Han asked about recommendation three that establishes the existence of theory networks as a project category for theory in the DOE. If there is a goal to set up something that is not long-term, he hopes that funding will come from different sources in addition to rather than competing with others. Dawson acknowledged that the panel favors this if it is new money.

Dawson shared that the panel discussed a minimal level of funding support for the university groups that include a 50 percent postdoctoral student per PI and 50 percent student, as described in recommendation six. Han noted that the model seemed very socialistic wondering how this can be justified if there is a comparative review that looks for the best science and best people to do the science, and factoring in the best PIs. He expressed that these numbers look similar to the national average and could be a rough target. Dawson explained that not all would follow recommendation six. This is a notion that there is some level at which a group could be viable.

Dr. Siegrist noted recommendation eight that recommends increased graduate student funding and asked if there is a sense of how funding is given now. Dawson shared that two months' of funding is given and has heard that there were not enough teaching positions to support theorists so they were being frozen out.

Dr. Tschirhart noted that breadth resonated with the analysis and that theory is an important part. He asked if this trumps depth in a budget constrained environment. Dawson noted that there was unanimous agreement on recommendation four that efforts should support the best science.

Tschirhart observed that theorists did very well in the ECP acceptance and asked if there was a message that could be translated to all based on this high success rate. Simona Rolli shared that her view is that proposals from theorists are written in a way that presents information clearly. Experimental areas are different as it is harder to shine and present something unique. She commented that other groups need to be more original.

Professor Erbacher noted that university physics departments are feeling pressure as there could be a point where there are too many theory students in too many teaching positions. This could disallow participation of theory students who need teaching assistantship money. Experimentalists put students in research assistant positions as soon as possible taking spots away from theory students.

Dr. Ligeti commented that theorists have a higher number of ECPs and at one time this number was higher. An explanation is that experimentalists have bigger start-ups. Historically, the current rate of theorists in the ECP is down.

Dr. Crawford asked if recommendation two asks for theory to be protected if the overall budget goes down. He asked for HEPAP's feedback on this, and that in principle, this is something that could be done but not necessarily as it would mean larger cuts for experiments and technology R&D. He wondered if HEPAP would endorse that, and if it would like to review this before it comments.

Bishai asked if this refers to phenomenologists. Dawson noted that they are supported by the theory budget.

Professor Hoffstaetter commented that an answer cannot be formulated in response to Crawford's question. HEPAP could identify the data needed to answer the question in the future.

Professor Cvetic noted that the recommendation is biased as it is from theorists but that a plan is being formed to understand the impact on other areas. The core essence came from theory and continues to come from theory. Without strong support, the whole field will be weaker.

Siegrist noted a tension between recommendations three and four. Three says that theorists will define the problems. Four suggests keeping a breadth of topics and not using programmatic decisions. Dawson clarified that three pushes for small targeted things and not a big program. Recommendation three requires guidance and programs that point to a well-defined need.

Han shared that pushing for free thinking and programs will drive people to think and form teams. This is an example that separates three and four. If there is a program that drives theoretical work, the program can come from the group. Siegrist suggested that the community could form proposals, send them in, and a peer review could be done.

Dr. Byrum expressed that money cannot simply be moved from research funds to projects. In certain types of projects, the funds cannot move from scientists to projects. Dawson noted that in theory there is nothing that can be done.

PRESENTATION OF LATTICE QCD PROJECT REPORT

Dr. Paul Mackenzie of Fermilab described lattice field theory's role in high-energy physics. In the coming decade, high precision lattice calculations will be needed through the HEP experimental program. The U.S. Lattice Quantum Chromodynamics (USQCD) planned program is aligned with the HEP experimental program discussed at Snowmass. Almost all of the U.S. lattice gauge theorists are in the U.S. lattice community. Its purpose is to purchase and deploy hardware and develop the software infrastructure for the U.S. and the world. Funding pays for hardware and software, and funds for theory is given like any other funding.

Computing activities in hardware have jobs of all sizes on small and large numbers of nodes. Lattice calculations are done in two steps. The first is to create a thousand gauge configurations. Then each is generated from the previous one. The generation of configurations is done on one big computing calculation that takes 10s to 100s of core calculations. These are done on the largest supercomputer facilities such as Argonne and Oak Ridge. These are analyzed and done in parallel so they need the same number of cycles but each individual job is smaller than jobs run on supercomputers. This part of work can be more cheaply done on clusters of computers. There are jobs that are 16-core up to thousands of cores but these are generally not welcome at TITAN and other places since these jobs can be run on smaller computers. These are run on USQCD and it is the continuation of that effort that is important. The USQCD works to identify resources around the world to run these jobs. These jobs ran in the billions of hours this year.

USQCD is building software and funds are used to create the QCD software infrastructure to include community libraries, community codes, optimization and porting to new architectures, and implementation of up-to-the-minute algorithm advances.

The community joined Snowmass and contributed a chapter to the Project X book. Members frequently speak at experimental calculation meetings that have interest in these calculations.

Only in the last 10 years have all of the systematic errors been brought under control and they are now well validated in certain ways. Experientialists and theorists think about these differently. Experimentalists may think about work in the hadron spectrum and reduce percentages. Theorists work in the opposite direction, and there are checks by groups to check gauge invariance and other things.

In recent years, the understanding of bottom-quark mass has supported the Higgs decay channel and Higgs in total. In high procession Higgs factories, there is an aim to bring branching fractions to ~one percent. In the Snowmass QCD report, the group aimed to achieve needed accuracies. These accuracies are very high if desired. Lattice QCD will do this eventually.

The best way to determine heavy quark mass without using lattice QCD is from EE annihilation. This cross section can be related to the derivative of the vacuum polarization. These moments can also be calculated easily by lattice QCD so there is awareness that these can be calculated accurately without going through experiments. This is at about a one-half percent precision and these accuracies can be improved with increasing computing time. The same method can also give precise determinations of strong coupling constant. The determinations of alphas are very robust as you can remove two of the most precise lattice calculations, and get the same answer with somewhat increased error pbars. If all of the lattice calculations are thrown away, the same answer will still come up. There is still a need to increase accuracy in the future and the ability to do this is known.

Lattice QCD has also contributed support to the CJM matrix elements expected for DTC. Lattice QCD also contributes to the unitarity triangle. All of these use lattice calculations and except for B_{k2} mixing, the USQCD leads the world on all quantities.

The goals of the USQCD are being accomplished. The U.S. is leading the world in all of these quantities because the calculations are good and the U.S. flavor effort is focusing on experiments and phenomenology more than other countries. For many quantities and especially in physics, U.S. calculations are the best but in some cases are the only calculations.

The USQCD proposes that its dedicated hardware projects will extend five more years. The extension of this will allow for meeting goals in white papers and goals in Snowmass reports.

Muon g-2 aims to reduce experimental error to 14 ppm. Now there are theoretical errors that are larger, hence the community needs to figure out how to do these calculations. This calculation is requiring the development of new methods and there are promising new results.

In μ to *e* conversion and DM, observing charged lepton flavor-violation would be ambitious but lattice calculations could help to observe the underlying models. The matrix elements most needed are scale calculations for quarks. These are the same matrix elements needed for DM experiments. The calculations are of an order of about 25 percent accuracy and could be lower.

Neutrino cross sections will need lattice calculations as they become more accurate. Vectory matrix elements cannot be obtained from experts like the vector ones. Dipole experimentation is needed. These are accurate enough such that they do not fit the dipole model anymore. These could be due to contributions from nuclear physics and experimental errors. So the source of the uncertainty is unclear. Richard Hill and collaborators at the University of Chicago showed that lattice can calculate parameters from first principles and take out of the question of contributions to new physics. With the neutrino community, this is a problem with two parts. The first is to figure out the right answer and then give them information that they can use. Help is being provided one of Hill's students who was a neutrino experimentalist and is incorporating a power series expansion in GENIE Monet Carlo.

Proton decay like μ to e conversion will be evidence for new physics and lattice theory will be needed. The BNL and UK collaboration has shown flavor results for proton-decay matrix elements. Certainties are not good right but great accuracy is possible as computing improves.

In quark flavor physics, there will be a continued need for greater detail. Improved lattice will contribute to removing these uncertainties. Once that lattice is more mature, more challenging matrix elements are more achievable.

$K \rightarrow \pi\pi$ decays is sensitive to new physics due to the penguins it has. These are more challenging in that lattice calculations have only a single hadron in final state. The RBC–UKQCD collaboration has made progress in this calculation. They have provided a delta study.

In non-SM Higgs physics, there are big ideas proposed for theoretical models. A lot of these underlying models require lattice field and non-perturbative calculations. In composite Higgs, these are entirely non-perturbative. In other models, if the fermion presentation of the underlying model has funny proportions it can use Goldston bosons that are smaller instead of pseudo scalar. The goal now is a viable model with light scalar and S-parameter consistency with precision DW constraints, and to make predictions for the particle spectrum and modifications to W-W scattering. Likewise, if SUSY is found, then it must be broken. Prototype calculations are occurring where the goal is simply to find if broken SUSY can be calculated. The longer term goal is to simulate super-QCD and compute the soft parameters of the low-energy theory.

Latice gauge theory is becoming a tool in the theorist's tool box.

Lattice gauge theory has had a high impact on supercomputing. Academic efforts in building computers for lattice QCD were very influential in establishing the parallel programming model that has been adopted by industry throughout the supercomputer world. IBM Blue Gene was started by Al Gara and Norman Christ. Blue Gene and TITAN are DOD collaborations. The hiring of lattice gauge theorists by NVIDIA for GPU development was successful, and they are helping develop GPU supercomputers.

Another spinoff is in lattice gauge theory and computing, and Monte Carlo builders who make use of GPUs. Although there is better cost per dollar, it is very difficult for experts to find funding for experiments to investigate them. Intel's Phi chip has many small lower power processors and these run a conventional instruction set. The Fermilab lattice QCD is leading a task force to work with those in HEP to compute and work in off-line track reconstruction, on-line reconstruction and high-level triggers, GEANT parallelization, accelerator modeling, and DES data processing. Intel seems to be entering the supercomputer market.

Discussion

Professor Han asked about the questions around calculation and proton structure functions. Mackenzie noted that the low moments can be calculated but get more difficult as they become more interesting and necessary. The lattice calculations so far have been helpful in testing what is known phenomenologically. For high moments, quarks and other things, progress can be made but as the number of the moments gets higher, the calculations become very difficult quickly.

Han cited a paper that showed that the calculation of lattice calculations was off by several orders of magnitude. Mackenzie responded that the paper was wrong as it was based on assumptions about the behavior of QCD that is not obvious.

Professor Lankford commented that the near-term and future possibilities did not illustrate the status and feasibility of new projects. He asked about the non-SM Higgs regime as an example and what exploration has already been done. Mackenzie noted that these are very exploratory for now. There were experiments in the 1980s that were similar. There was a method but early predictions were unclear. For about 20 years, the community worked to improve calculations. Ten years ago, methods and computers were strong enough to include anti-quark and extrapolate calculations to mass. Experiments now run at physic quark masses. It took 20 years in QCD to go from a qualitative correct picture to something quantitative. BSM calculations could be just as difficult. These are prototype calculations with little budget support. They are critical, however, if non SM Higgs is discovered at some point. Effort will need to go into theory, especially if LHC discovers something two or three years from now. The goal now is simply to show dynamical breaking on a lattice.

It was noted that in composite Higgs mechanisms, perturbative scenarios are sort of like QCD but are scaled in different ways. For example, it is unclear that if there is a QCD theory then the same model has been abandoned in which the S parameter has been constrained. Just deciding on which scenarios exist will guide the development of models.

Dr. Ligeti asked about impacts, computing limitations, and resource needs, and the ratio for demand of resources now. Mackenzie referred to lattice-QCD constraints on the CKM matrix. The bars could be improved a little with a lot of funding over the next 10 years. Reducing the number of cycles over the next 10 years would slow down some projects. Error bars on projects will be larger. Ligeti asked if there will be a factor of 10 higher or lower computing power. Mackenzie noted that there is a need to better understand systematic errors.

Professor Jawahery expressed concern that the biggest impact is on flavor physics, and missing is clarity on the resources needed and improvements in parameters. While the computing resources needed seem reasonable, the precision desired needs validation. He felt that the program is small and he is worried that in about five years, the community will be the same and challenged in achieving uncertainties and validating them. He asked if this is a reflection of the community not predicting things to see results and gaining confidence in the results. Mackenzie responded that the community is looking for opportunities for prediction. The important predictions are ones where the answer is not known. To make predictions, there needs to be quantities that experiments have not accurately measured and these are uncommon. There is a desire to calculate something that was about to be discovered but there are not many opportunities. There are some predictions that have been really well done. At JLAB, one of the upgrade purposes is to explore the iridescent spectrum. This will take a double calculation over the next five to 10 years. The community is just scratching the surface of residence physics.

Mackenzie shared that there are about 150 people working together in nuclear and HEP. In the program, there are actually two dozen calculations and in the future program that he sketched out, there are six or eight areas of need that mostly have a single lattice calculation.

Mackenzie noted that the community has the capability to take on these projects and many are calculations that simply have not been done yet such as neutrino physics calculations. One area where there is little understanding of how it will take to succeed is Muon g-2.

Professor Rosenberg asked why this was being discussed and not being treated like a typical proposal. Lankford clarified that this will go through the CD process. At this stage, the agency will consider it at CD0 and a mission need. The agencies were asked if there is a mission need. The project scope will be addressed at the CD1 and CD2 stages. He asked if the HEPAP had questions as it relates to HEP. The question is how HEPAP feels about past contributions and if it is valuable to go forward.

Precario noted that this is a smaller project that will not go through the P5 process, and that DOE wants to hear from HEPAP if this is something it should do. Dr. Kogut added that the previous five-year project was presented by Professor Robert Sugar and endorsed.

Discussion of the value of the USQCD program and agency mission need

Professor Rosenberg and Professor Jawahery both agreed that the program fits with the HEP mission. Rosenberg noted that asking for an endorsement for the hardware, budget, project and other things is different. Jawahery added that it seems that the program is huge and needs resources to do calculations, and that the only way is through DOE support.

Dr. Crawford clarified that the DOE is asking if this program fits with its goals and that HEPAP not try to balance this with other things.

Professor Honscheid asked if there is a scope limit for the P5 and specific project sizes. Lankford shared that there is no definitive answer to that, adding that the timing of the decision on the USQCD cannot wait for the outcome of the P5 panel.

Professor Erbacher commented that this is needed to make progress on experimental areas, and important for the U.S.

Dr. Ligeti expressed that the scientific case is clear and not supporting it would be like abandoning this work. He was concerned that the connection with the CD process and this project and where this project comes from are unclear. Precario responded that this project is estimated at $18M over several years. Rosenberg commented that there are project costs and a lot of research that would be more than $15M if it includes research, hardware and other things.

Professor Fisher commented that if the HEPAP gives positive feedback, that there would then be a CD0 review, and asked if this should be in the P5 process as they do things with CD0. Crawford clarified that the P5 is a project prioritization panel and that the USQCD is on the small end of projects. It is also a special type of project as it is in computing. There is no cut-off for how low the P5 will go, but they may decide that this is below their threshold.

Fisher added that the P5 report may come back to HEPAP and the HEPAP may ask the P5 to discuss it. Crawford responded that the CD0 is on his desk now and requires a decision.

Fischer commented that if there is a statement that the decision could be deferred to the P5, then HEPAP should vote on the program now.

Professor Hoffstaetter asked if the CD0 decision is coming in a year when there is no funding for it. Crawford clarified that there is a very rough cost range in CD0 projects. There is a commitment that this is a project that the agency wants to do. Hoffstaetter added that CD1 and CD2 would be needed for funding allocation and the P5 could still comment on this. Crawford noted that the agency only needs HEPAP to weigh-in on a scientific side.

Professor Han supports the physics case.

Ligeti noted that it would be interesting if anyone says that the physics case it not above some threshold. He supported moving forward based on comments from the agencies on what goes into a CD0, and if this is something that Professor Lankford and Dr. Ritz are comfortable making a recommendation on without the P5.

Professor Fisher commented that the DOE simply wants a sanity check.

Fisher made a motion for a vote, and the motion was seconded by Professor Cvetic. A vote was not conducted by the HEPAP.

Precario explained that there will be a technical review, and Dr. Kogut will plan a scientific review preceding the CD process. He added that there is typically not a CD0 review and that the projects are initiatives based on recommendations from DOE's advisory panels.

Lankford shared that he heard comments about the value of the science but concern about preempting the P5 and the level of resources that would be used for this versus other priorities.

Erbacher is not concerned about the P5 as they have always dealt with larger projects and gone ahead with smaller recommendations.

Lankford read a drafted statement to express the HEPAP discussion to this point:

HEPAP heard a presentation and held a discussion on the status and plans of the LQCD project. The panel also received the LQCD II proposal prior to its meeting. The LQCD project provides dedicated computational hardware (and an associated operations budget) used by the USQCD collaboration for numerical studies of quantum chromodynamics and strongly coupled beyond the standard model (BSM) particle theories on the lattice. These dedicated computational resources complement those used by USQCD on leadership class computers through DOE's INCITE program and on other facilities, and utilize software funded by the SciDAC program.

Lankford asked for comments on this portion and received none.

Lankford read the following draft recommendation:

HEPAP recommends continued support of computational resources for Lattice QCD, within budget constraints.

Rosenberg expressed that he is not qualified to support this as the HEPAP may not be able to determine that it cannot prioritize this over other things and that he did not want people to feel as though this was evaluated over other things.

Fisher commented that the recommendation should read that the HEPAP finds this proposal to be in alignment with the DOE mission.

Lankford modified the statement to read: HEPAP recommends that LQCD enter the CD process.

Professor Jawahery commented that there seems to be inconsistency with this and the earlier statement that there is a mission need for this and that DOE should consider this for continuing support. This is needed but the only thing that could come out of the CD1 process is that the DOE lacks the money for this or needs to make it smaller.

Precario commented that indicating alignment of this with mission need does not mean it will go forward. There are mission needs that get left out. The question is whether the DOE should pursue building this or stop and not consider it at all.

DISCUSSION OF PROPOSED HEPAP TOPICS

Professor Lankford noted that HEPAP has presented several issues during the meeting with a common theme. An example is themes in the high energy theory talk that implicitly addressed levels of support for university and laboratory theory. Another was the disappearance of infrastructure from research programs.

The past HEPAP meeting touched on the need for a systematic discussion of the respective roles in theory and experimentation with university and laboratory groups, acknowledging that not all are of the same size and scope. Lankford expressed that the basic question is how to execute the most effective scientific program within the constraints of the agency's science mission, and how institutions can work together to accomplish that mission.

Lankford envisions that a subpanel could take on this thorny issue as it is beyond the scope of the regular HEPAP meetings. He suggested that HEPAP come up with a charge.

Lankford shared that Dr. Siegrist had invited HEPAP to add to the general description and other topics that address how the program is executed. An example is how execution is monitored.

Earlier, the HEPAP touched on workforce questions.

Lankford shared other issues that have come up, to include discussing the respective roles of the OHEP and Nuclear Physics (NP) programs, stove-piping, and in particular how NP comes up relative to how HEP interacts with neutrino double-beta decays.

Lankford proposed that HEPAP could form a chart that lists the topics that should be addressed and should be taken up in a subpanel context.

Dr. Bishai commented that the issue of stove-piping would need to include people from the NP program. Lankford shared that a joint subpanel could include people from both programs.

Dr. Byrum asked if the HEPAP would come up with solutions to address stove-piping, and Lankford suggested that this could be done in general terms.

Professor Erbacher added that the themes reflect a university versus laboratory theme but also sees how to fit stove-piping in other areas in a coherent way. Stove-pipes are present not just in neutrino double-beta decay issues in NP but in other issues, too. She suggested that another issue in OHEP is the use of frontiers and perceptions that this boxes in people. Lankford clarified that stove-piping refers to the OHEP and frontier classifications. He noted that looking at this is part of the charge for the COV.

Dr. Siegrist commented that nuclear physicists may be part of the task force and could give input on overlapping science. He added that the Nuclear Science Advisory Committee will produce a request for a subpanel to include participants from HEPAP to determine the content space for double beta-decay moving forward. That issue is getting addressed and there might be information in December 2013 that gives this a stronger basis.

Professor Honschied commented on the university versus laboratory theme, suggesting that a topic could be looking at how programs are organized across the laboratories in a way that identifies the strength of the laboratories and indicates that each does not have to be involved in everything. Lankford commented that the respective roles of universities and laboratories could be covered in this theme, especially considering current resource constraints.

It was suggested that a workforce theme could be addressed in a separate forum with panel members that include non-laboratory and non-academic scientists that also have training in physics. Professor Jawahery agreed that the advantage of doing this separately is allowing focus on this as a separate problem. Lankford added that this couples to the role of universities but is loose enough that a different forum and study that feeds input to the larger panel.

Erbacher noted links into the laboratory and university discussion especially with topics from day one of this meeting and especially with accelerators and if a laboratory or university should host something like this. The workforce discussion could be opened up and examine opportunities that are available.

Lankford expressed that workforce and career issues are not specific to laboratories and universities.

Lankford recruited Steinhardt, Ligeti and McBride to help develop the charge. The charge would ultimately come from agencies to do this work. Comments can also come to Lankford from HEPAP to drive the comments to agencies.

BOARD BUSINESS

The next HEPAP meeting will be arranged based on feedback from HEPAP meetings. HEPAP meetings can be held outside of Washington, D.C. Lankford asked for feedback about locations, and was told that meetings could be webcast. P5 will be held outside of Washington D.C. The top priority is to webcast P5 meetings, and then HEPAP can have other locations.

Lankford commented that there is a need to have the P5 slides available to the community and interest in seeing the slides.

CLOSING REMARKS AND ADJOURNMENT

Lankford adjourned the meeting at 3:10 p.m. EST.

The minutes of the High Energy Physics Advisory Panel meeting held at the National Science Foundation on September 5 and 6, 2013, are certified to be an accurate representation of what occurred.

Signed by Andrew Lankford, Chair of the High Energy Physics Advisory Panel on (date).

(Insert electronic signature)

Andrew Lankford