Minutes of the
High Energy Physics Advisory Panel Meeting
July 13-14, 2007
Palomar Hotel, Washington, D.C.

HEPAP members present:

Jonathan A. Bagger, Vice Chair
Charles Baltay
Alice Bean
Daniela Bortoletto
James E. Brau
Robert N. Cahn
William Carithers
Priscilla Cushman
Larry D. Gladney
Robert Kephart
William R. Molzon
Koichiro Nishikawa
Angela V. Olinto
Stephen L. Olsen
Satoshi Ozaki
Saul Perlmutter
Tor Raubenheimer
N.P. Samios
Melvyn J. Shochet, Chair
Sally Seidel
Maury Tigner
Guy Wormser

HEPAP members absent:

Sarah Eno
Joseph Lykken
Lisa Randall

Also participating:

Alan Bross, APC Muon Accelerator R&D Department, Fermi National Accelerator Laboratory
Joel Butler, Particle Physics Division, Fermi National Accelerator Laboratory
Joseph Dehmer, Director, Division of Physics, National Science Foundation
Lyn Evans, LHC Project-Leader, Conseil Européen pour la Recherche Nucléaire (CERN)
Marvin Goldberg, Program Director, Division of Physics, National Science Foundation
Michael Harrison, ILC Americas Regional Director, Brookhaven National Laboratory
Steven Kahn, Director of Particle and Particle Astrophysics, Stanford Linear Accelerator Center
Nicholas Kaiser, Institute for Astronomy, University of Hawaii
Young-Kee Kim, Deputy Director, Fermi National Accelerator Laboratory
John Kogut, HEPAP Executive Secretary, Office of High Energy Physics, Office of Science, Department of Energy
Jonathan Kotcher, Program Director, Division of Physics, National Science Foundation
Alfred Mann, Department of Physics, University of Pennsylvania
Marsha Marsden, Office of High Energy Physics, Office of Science, Department of Energy
Peter Meyers, Department of Physics, Princeton University
Homer Neal, Department of Physics, University of Michigan
Pier Oddone, Director, Fermi National Accelerator Laboratory
Frederick M. O'Hara, Jr., HEPAP Recording Secretary, Oak Ridge Institute for Science and Education
Ruth Pordes, Computing Division Office, Fermi National Accelerator Laboratory
Moishe Pripstein, Program Director, National Science Foundation
Randal Ruchti, Program Director, National Science Foundation
Wesley Smith, Department of Physics, University of Wisconsin
Henry Sobel, Department of Physics and Astronomy, University of California, Irvine

Robin Staffin, Associate Director, Office of High Energy Physics, Office of Science, Department of Energy
J. Anthony Tyson, LSST Director, Physics Department, University of California, Davis
Philip K. Williams, Research and Technology Division, Office of Science, Department of Energy
Andreene Witt, Oak Ridge Institute for Science and Education
Frank Würthwein, Physics Department, University of California, San Diego

About 60 others were also present in the course of the two-day meeting.

Friday, July 13, 2007
Morning Session

Chairman **Melvyn Shochet** called the meeting to order at 8:46 a.m. and introduced **Robin Staffin** to review the status of the Office of High Energy Physics (HEP). A congressional mark has been obtained for HEP; for FY08, the House approved $780.2 million (the presidential request), and the Senate added $7 million. The language is important. $60 million was requested for the International Linear Collider (ILC), and it was approved by both the House and the Senate. The movement of $15 million to the Joint Dark Energy Mission (JDEM) last year did not occur this year. Congress says that dark energy experiments add excitement to the field. They set a launch for JDEM at 2013. The House pointed out the need for DOE to go forward irrespectively of the National Aeronautics and Space Administration (NASA). The House committed up to an additional $20 million for the "dark energy mission" with that money coming from "lower-priority" projects. The Senate added $7 million outright for JDEM, setting a launch date of 2014.

The years get more stressful as the Tevatron and B Factory shut down; the ILC ramps up; and the NuMI Off-Axis ν_e Appearance experiment (NOvA), Daya Bay, etc. are all under construction. There is very little new money. This transition requires taking operating funds and putting them into capital projects, preparing for the next decade.

Raymond Orbach has stressed that the ILC has high priority because the terascale is where new physics will occur. DOE is not unfamiliar with large-scale international programs. They take time. The preparatory activities for the ILC are ongoing, and Particle Physics Project Prioritization Panel (P5) is reviewing the options. The budget language calls for the Department to "consult with our international partners about entering into formal negotiations to establish an international agreement to direct the engineering design phase of the proposed ILC, should the U.S. and partner governments choose to proceed to fully costed engineering-design activities (EDA) for the project. This agreement would be similar to the International Thermonuclear Experimental Reactor (ITER) EDA agreement. The information provided by the activities would inform future decisions on the construction and U.S. participation in the proposed ILC." The Funding Agencies for the Linear Collider (FALC) will be more involved in working with the Global Design Effort (GDE) and will agree on terms of reference on its role and representative authority.

DOE maintains its interest in the possibility of hosting the ILC at Fermilab, given internationally based affordability and scientific validation at the LHC.

DOE congratulates the GDE for the completion of its Reference Design Report (RDR), representing the hard work of a coordinated international network of dedicated scientists and engineers. The international cost review recognizes the high quality of the RDR; it forms a basis to move on to the engineering design phase. The congressional language also calls for site studies.

For dark energy, Congress would like to see a clear path forward. The FY07 funding levels were set at $6.6 million for the SuperNova Acceleration Probe (SNAP), $2.4 million for the Dark Energy Survey (DES), and $3 million for a dark energy solicitation. The total amount for FY08 is to be determined.

The Deep Underground Science and Engineering Laboratory (DUSEL) is being supported by HEP, which has put aside $320,000 for grants.

In regard to the Large Hadron Collider (LHC), CERN has invited the United States to participate in accelerator upgrades.

The DOE contribution to the Very Energetic Radiation Imaging Telescope Array System (VERITAS) is complete, and the telescope is operating.

Daya Bay has had a critical-decision 1 (CD-1) review in April. The final sign-off is expected in August, with groundbreaking in October.

NOvA's CD-1 was signed off in May; its CD-2 review is planned for September of 2007.

Other projects are progressing.

The scale of the ILC shows that particle physics is not out of reach in terms of cost. However, new methods of accelerating particles are needed, say some.

The Office's people are very good, but the Office is understaffed. The Committee of Visitors (COV) said that understaffing is the biggest problem of the Office. There is a tremendous amount of work to do for each science project proposed and funded. The field of high-energy physics depends on the Office's ability to push projects forward. Some positions have been filled, but some staff have also been lost. There is a new head of the Accelerator R&D Program. There is movement on obtaining personnel for planning. There is an Intergovernmental Personnel Act (IPA) opening in program management. Successfully arguing for budget and new projects inside the government is very closely tied to the ability of the Office to perform detailed planning, provide program and project oversight, and argue for the science. The help of the community is needed.

Shochet noted that there is a strong process for prioritizing projects and maintaining a balanced program. That process is the only way to get the best science done. That process must be used, and it cannot be allowed to be subverted. The President's budget originally did not look good for HEP; it looked like PEP-II operating funds would be transferred to Basic Energy Sciences (BES) rather than being retained within OHEP for new projects. That is a concern for the program. Staffin replied that one must look at the activities from one year to the next. The amount of funding for HEP has gone up 4%. The percentage of funding for the Office of Science (SC) represented by HEP went up by 6.8%. The overlaps between DOE and NSF work out well.

Wormser asked if there will be an engineering design study (EDS) report by FALC. Staffin answered that the FALC agreed to support the engineering design and will identify and oversee the engineering design during the transition to a formal agreement. The purpose is to provide support to the GDE.

Bagger asked if the political agreement needed to advance the ILC would proceed in parallel or in series with the engineering design. Staffin responded that there will be discussions on how to do this. Some countries feel that the United States is moving too quickly; others feel it is moving too slowly. Sometimes these are the same countries. It will not be in series.

Bortoletto asked how the United States would reply to the CERN invitation. Staffin said that the United States would consider possible partnerships and is discussing with CERN how to go forward. HEP would consider contributing to the upgrades in its 5-year plan. The United States will likely be involved in the LHC upgrade, just as it was involved in the LHC.

Joseph Dehmer was asked to report on the status of high-energy physics at the NSF. The selection of the Homestake site for DUSEL will be reviewed later at this meeting by Jonathan Kotcher. The NSF is now looking forward to this underground infrastructure of global utility and scope to extract as much science from an underground environment as possible. The site will be open to all agencies and countries. Collaboration will be open, and safety rules must be followed. It will be an international laboratory. NSF will need a new program officer to deal with underground-laboratory issues.

He thanked the subpanels that have been working during the past months. The Neutrino Science Advisory Group (NuSAG), Dark Matter Science Advisory Group (DMSAG), and others are making significant contributions.

The HEP-NSF-NS [Office of Nuclear Science] joint vision is working well. HEP is leading the ILC. NS is leading the effort for a facility for rare isotope beams (FRIB) [formerly the Rare Isotope Accelerator (RIA)]. NSF is supporting all of these efforts. The budgets are promising, but the process is not over. In the physical sciences, NSF is up 6%, on track to double in 10 years. In FY08, it would be up 8%. In FY07, it was up $4 million to $18 million for the LHC, computing, and management and operating (M&O). In FY08, it will hold an open competition for the Physics Frontier Centers. There will eventually be about 20 such centers, but annual competitions will ensure that top performers will be funded (up to $4 million).

The United States needs an accelerator physics program. Perhaps in FY09 that will be possible to fund; by 2030, that program could be $40 million per year.

In FY08, the advanced Laser Interferometer Gravitational Wave Observatory (LIGO) has been approved with a very stable plan.

There are a lot of challenges in this period, but there has also been a rise in non-accelerator particle physics. This is a remarkable time. VERITAS has begun operating and is the most sensitive gamma-ray telescope in the world. Auger is operating, although not yet complete. Borexino is now operating after a 3-year delay for environmental reasons. The Cryogenic Dark Matter Search (CDMS) is maintaining the frontier of cosmic-ray physics. And IceCube has about one-third of its strings installed.

Wormser asked about Large Space Telescope (LST) proposals. Dehmer replied that that is being managed by the Astronomy Office. The Division of Physics will support physicists who want to participate.

Samios asked if the budget were under veto stress. Dehmer said that the discretionary portion is.

Olinto noted that a European roadmapping has requested U.S. participation and asked if the NSF will participate. Dehmer replied affirmatively; that is an important means of accomplishing science in an economical way.

Shochet introduced **Young Kee Kim**, who is chairing a Fermilab planning effort on its accelerator-based physics program, the recommendations of which will be vetted by the Fermilab Physics Advisory Committee and P5 before coming to HEPAP.

Fermilab has witnessed extraordinary years for physics in 2006–2007 and expects more in the near future. It is planning further ahead for an accelerator-based HEP program; its highest priorities are the LHC and its upgrades and the ILC. The current expectation is an ILC decision in 2010 with many other decisions to be made before that. The Tevatron and the B Factory will be shut down, offering a great opportunity for high-energy physics. The GDE has presented an estimate of size and complexity of the ILC. There is now uncertainty about the start time for the ILC. The United States must be prepared to lead this effort when the decision to go ahead is made.

Fermilab Director Pier Oddone formed a Steering Group to develop a roadmap for Fermilab's accelerator-based HEP program. It will develop a strategic roadmap, based on the recommendations of Elementary Particle Physics 2010 (EPP2010) and P5. That roadmap should support the international R&D and engineering design for as early a start of the ILC as possible and the development of Fermilab as a potential host site for the ILC; it should develop options for an accelerator-based high-energy-physics program in the event the start of the ILC construction is slower than the technically limited schedule; and it should include the steps necessary to explore higher-energy colliders that might follow the ILC or be needed should the results from LHC point toward a higher energy than that planned for the ILC. The Steering Group's final report should be finished and delivered to the Fermilab Director by August 1, 2007. This deadline would allow for presentations to the DOE and its advisory bodies before the structuring of the FY10 budget. The Steering Group has five subgroups: neutrino physics, flavor physics, advice/oversight, accelerator physics, and high-energy colliders beyond the ILC.

The Steering Group is meeting weekly by telephone and includes the ILC GDE, HEPAP and P5 leadership, Fermilab and Stanford Linear Accelerator Center (SLAC) user groups, and subgroup members. The subgroups are also meeting weekly. The Steering Group has reached out to the high-energy physics community for ideas and input. Six letters and 20 proposals have been received from the community.

Both EPP2010 and P5 have stressed diversity and assumed a fast decision on ILC construction (2010) and a technically driven schedule for ILC construction. An emphasis is being placed on dark energy. The Fermilab program is very well a line with the P5 recommendations.

The subgroup is asking itself, will the physics be important when the experiment is done? Would it be better at Fermilab than at other labs [e.g., the Japan Proton Accelerator Research Complex (JPARC)]? Would Fermilab research be unique in its physics reach and will it answer questions not answered at the LHC? A major question is always, will this program advance the ILC? Physics opportunities are seen in neutrino physics and in precision measurements involving charged leptons and quarks.

The preliminary conclusions are that the proton intensity frontier offers a rich menu of neutrino, muon, and kaon physics in the near-to-mid term. The physics driver is the search for beyond-the-standard-model physics. The proton intensity frontier offers a window to high energy scales in some scenarios, and its physics reach is complementary to that of the energy frontier. The beta beam and the luminosity frontier are longer-term projects with no benefit to the ILC.

Fermilab's current design could be reconfigured to produce a 120-GeV proton beam with power of 1-MW, an 8-GeV beam with slow and fast spills, and a 120-GeV slow spill at modest cost, although there would be no synergy with the ILC. Another option (Project X: The Proton Intensity Frontier) is an ILC-style 8-GeV proton linac feeding the Recycler and Main Injector, with the beam initially sent to NOvA and later to DUSEL. This configuration would have three components:

- 0 to 0.12 GeV (this would be a modest increase from the existing 60-MeV R&D program)
- 0.12 to 2 GeV linac (this would have a strong possibility of collaboration with India and others)
- 2 to 8 GeV (this would be nearly identical to the ILC requirements and would make use of international collaboration and U.S. industry; it also allows using the Tevatron for testing the ILC damping rings)

The size of this project is not small and cannot be allowed to interfere with the ILC. Fermilab is looking into this possibility. It has a good connection to longer-term programs. It could provide a proton beam beyond 2 MW. A sample roadmap has been developed for this project to contribute to ILC industrialization and large-system tests. By August, the benefit of this project to the ILC should be understood. The report of this Steering Group will be finished in September and submitted for review.

Cahn asked what would happen if there were no ILC. Kim replied that the Steering Group is considering that option. Project X could still be pursued, and higher-energy-collider R&D will still be needed. The LHC should indicate the need (or lack thereof) for the ILC, but it is not known when the LHC will indicate that.

Wormser asked what the cost, personnel, and user community of Project X would be. Kim replied that the cost is about $500 million for the proton driver with strong international participation. Oddone added that there are branch points in the roadmap that allow one to know what R&D needs to be done, no matter what decisions are made in the future. These branch points are a way of dealing with the complexity. Shutting down the Tevatron opens opportunities for funding for physics or other disciplines.

Samios asked if the ILC management supported Project X. Oddone answered that one wants to take some steps but also wants to help the ILC. From the ILC viewpoint, the focus should be strictly on the ILC's fast track. However, one needs to get ready. Anything can happen. One has to recognize all possibilities. Fermilab has a very different set of responsibilities than does the ILC management.

Baltay asked how one reaches a branch point and makes a decision. Kim replied that that moment is fuzzy. One cannot act quickly. Rather, one needs flexibility. One also needs to figure out how not to lose time. Branch points are not sharply defined.

Dehmer said that this is DOE's portfolio and that he was just an adviser. As such, he has an interest in a long-baseline neutrino experiment. This project contributes to the robustness of the high-energy-accelerator program. One must decide how much risk one wants to take on.

A break was declared at 10:42 a.m. The meeting was called back into session at 11:00 a.m. **Peter Meyers** was asked to report on the response of the NuSAG to its latest charge, for which it had delivered an interim report to HEPAP and the Nuclear Science Advisory Committee (NSAC).

Neutrinos can be described with a three-by-three matrix; the phase angle δ can produce a CP violation. The oscillation frequency of the neutrinos can be influenced by the square of the mass difference of the neutrinos, and there are two possible mass hierarchies.

Neutrino scientists are trying to fill out the understanding of three-neutrino mixing and oscillations: What are the orderings and splittings of the neutrino mass states? What are the mixing angles? And is there CP violation in neutrino mixing? A worldwide effort has laid out an ambitious program that can do all of this, subject to the values of the unknown parameters. Finding this out will not be easy. Detectors need to be huge. Beam powers have to be very large. Costs will be in the hundreds of millions of dollars.

All of these measurements are of the same process: the conversion of muon neutrinos to electron neutrinos, which depends heavily on the mixing angle of $\sin^2 2\theta_{13}$. There are several ways to see this mixing angle: more measurements, multiple energies, multiple baselines, larger baselines, and dumb luck.

Experiments to conduct these measurements have been proposed and/or started at Double Chooz (reactor), Daya Bay (reactor), T2K (accelerator), NOvA (accelerator), and NOvA + T2K (accelerator). The next round of accelerator experiments to extend mass-hierarchy and CP violation sensitivity to $\sin^2 2\theta_{13}$ to about 0.01 seems to be about the maximum reach of conventional beams.

The latest charge letter asked, assuming a megawatt-class proton accelerator as a neutrino source, what would be the scientific potential, associated detector options, optimal timeline, other scientific inputs needed, and additional physics that could be addressed for a multiphase off-axis program and a very-long-baseline broad-band program?

Both T2K and NOvA use off-axis techniques. Another approach, developed at Brookhaven National Laboratory (BNL), uses a broad-band neutrino beam, and Fermilab and BNL have conducted a study of this field. These experiments look for "new" electron neutrinos in charge-current events. Quasi-elastic events are the cleanest and allow reconstruction of the neutrino energy.

The off-axis approach gives neutrinos of about the same energy, but with a lower flux, a decrease in NC π^0 background, and electron neutrinos from kaons at different energy. This approach would use an upgraded Neutrinos at the Main Injector (NuMI) beam, but there are no deep detector sites available, and so it cannot use a water Cherenkov detector.

With the wide-band-beam approach, energy dependence lifts degeneracies, the on-axis beam maximizes the flux for long baselines, and long baselines enhance the matter effect. However, the high-energy component brings π^0 background that requires a small off-axis angle for suppression.

The U.S. experimental scenarios using these approaches all start with the Fermilab Main Injector (with a maximum achieved beam power of 315 kW at 120 GeV, an initial upgrade plan to 700 kW, and a possible longer-term upgrade to 1.2 MW). The off-access experiments would use about 100 kton of liquid argon in a time-projection chamber (TPC), use the existing/upgraded NuMI beam, and be deployed entirely at the NOvA site or be split, with a remote second-maximum site. The wide-band beam would use a 300- to 500-kton water Cherenkov (or 100-kton liquid-argon TPC) detector, be located in DUSEL, and use a new neutrino beam.

Detector technologies include:

- Water Cherenkov, which is a known, successful technology for neutrino oscillations and proton decay. It must be underground (e.g., at DUSEL), requiring R&D on large caverns. Its photomultiplier tubes (PMTs) drive the cost and construction time. R&D is also needed for new light sensors. Its efficiency is about 15 to 20%. Two versions are under study in the United States, a monolithic version and a modular version.
- Liquid-argon TPC, which has the ability to reconstruct events in detail, leading to excellent π^0 rejection and ~80% efficiency. If built underground, it would be good for studying proton decay to a kaon plus a neutrino. Two 300-ton modules have been built. Aggressive R&D is needed to prove feasibility at the 50- to 100-kton scale, and costs must be drastically reduced. It is possible that it could work at the surface. R&D is needed, leading to the demonstration of a substantial detector in the NuMI beam. The liquid-argon detector is still in the conceptual-design stage.

Other physics that could be done with these detectors include nucleon decay and low-energy neutrino astrophysics, although these applications might increase detector costs.

The BNL/Fermilab study group selected four options:

1. Off axis, 100 kton of liquid argon at the NOvA site
2. Off axis, 50 kton of liquid argon at the NOvA site plus 50 kton of liquid argon at the second oscillation maximum
3. Wide-band, a 300-kton water Cherenkov at the Homestake Mine
4. Wide-band, 100 kton of liquid argon at the Homestake Mine

The rule of thumb was that water Cerenkov required about 3 times the mass of liquid argon for similar sensitivity. However, under the assumptions developed for the calculations, the factor is more like 4 or 5.

NuSAG established a set of criteria for evaluating such options:

- Establish that θ_{13} is not equal to 0.
- Determine the mass hierarchy.
- Find CP violation.

The cost, effort, and time required demand that the program's discovery potential be held to high standards, 5 standard deviations.

These levels would require 3 to 5 years of experimental running. The longer baseline puts one in the game, and the current measurements of $\sin^2 2\theta_{13}$ already rule out certain possibilities. The liquid argon approach has an edge over the water Cerenkov approach unless one boosts the water mass to 500 ktons. There is a very small event frequency. One could increase the beam power or relax the criteria to overcome this problem.

Elsewhere, Hyper Kamiokande would use two 270-kton detectors and increased beam power but would have a short baseline.

In Europe, researchers are focused mostly on new neutrino-source technologies (beta beams and neutrino factories) that would be needed if $\sin^2 2\theta_{13}$ is about 0.02 or below. These technologies are not usually considered competitive with Phase 2 but may have to be reconsidered.

Considering the same detector options, the cost guestimates are

- $500 million for monolithic water,
- $335 million for modular water,
- $100 to $299 million for wide-band beam, and

- no idea for liquid argon.

None of these include the costs for injector upgrades, the near detector, or the hall.

NuSAG advocates learning the size of $\sin^2 2\theta_{13}$ from Phase 1 experiments before proceeding with Phase 2.

- Double Chooz hopes to have a sensitivity for $\sin^2 2\theta$ (of 0.05) by late 2012,
- Daya Bay (0.02) by 2013,
- T2K (0.01) by 2012 (at the earliest), and
- NOvA (0.02/0.01) by 2014/2017.

All of these dates are optimistic. The approval process may take 3 to 4 years. The construction times might be

- Water Cerenkov: 7 to 10 years and
- Liquid argon TPC: 4 to 6 years.

If any of these are located in DUSEL, DUSEL has to be ready.

In summary, plausible extrapolations of existing technology will allow 5σ searches for CP violation in the neutrino sector and 5σ determinations of the mass hierarchy down to $\sin^2 2\theta_{13}$ about 0.03 and with substantial sensitivity down to about 0.01. The large detectors needed for such measurements can also extend the sensitivity of searches for proton decay and neutrinos from astrophysical sources. These numbers are optimistic; $\sin^2 2\theta_{13}$ is somewhere below the current limit.

A draft report was delivered to HEPAP and NSAC a month ago. It has responded to some feedback already. That report includes four recommendations:

- The United States should prepare to proceed with a long-baseline neutrino-oscillation program to extend sensitivity to $\sin^2 2\theta_{13}$, to determine the mass ordering of the neutrino spectrum, and to search for CP violation in the neutrino sector. Planning and R&D should be ready for a technology decision and a decision to proceed when the next round of results on $\sin^2 2\theta_{13}$ becomes available, which could be as early as 2012. A review of the international program in neutrino oscillations and the opportunities for international collaboration should be included in the decision to proceed.
- R&D toward an intense, conventional neutrino beam suitable for these experiments should be supported. This beam may be in the form of intensity upgrades to the existing NuMI beam as well as development of a new beam directed towards DUSEL, which would likely employ the wide-band beam approach.
- R&D required to build a large water-Cherenkov detector should be supported, particularly addressing questions of minimum required photocathode coverage, cost, and timescale.
- A phased R&D program with milestones and a technology suitable for a 50- to 100-kton detector is recommended for the liquid-argon-detector option. Upon completion of the existing R&D project to achieve purity sufficient for long drift times, to design low noise electronics, and to qualify materials, construction of a test module that could be exposed to a neutrino beam is recommended.

Carithers noted that the R&D and construction costs would be about $25 million a year starting in FY09. That is a lot. Meyers said that these are very monolithic detectors; they do not have separate R&D per kiloton. NuSAG is not envisioning a $100 million for R&D.

Wormser asked how many facilities were envisioned. Meyers replied that NuSAG did not consider the possibility of multiple facilities.

Mann stated that, if one delays experiments until one knows $\sin^2 2\theta_{13}$, the timescale is such that one cannot build and operate these experiments and one will not have any graduate students. Anything useful has come about by dumb luck, but "dumb luck" is a gratuitous insult that does not provide leadership and guidance. Meyers replied that some overlap is valuable, but a huge number of neutrino experiments are being started. However, the scale is so huge that the money is not available to do all of these experiments. Therefore, one needs to see some credibility before committing those funds.

Dehmer noted that there are ideas floating around for accelerator mechanisms. There are also multiple uses to underground laboratories. NuSAG's findings will be very valuable.

Samios commented that there are a number of high-risk components to this program. The timescale presented, though, could be more optimistic.

Shochet asked for approval of the new NuSAG report. It was approved unanimously.

Henry Sobel was asked to summarize the changes that have been made to the Dark Matter Science Advisory Group (DMSAG) report in response to the Astronomy and Astrophysics Advisory Committee (AAAC) and HEPAP comments. Suggestions/corrections came from individual members: send the report to outside reviewers, distribute the findings and recommendations throughout the report in appropriate locations, and provide more discussion of DUSEL

The report was sent to 11 outside reviewers; 7 responded with comments on typos, organization, a new executive summary, and a new glossary. The executive summary now says that the most promising techniques are the cryogenic techniques based on (1) solid state (phonons and ionization in germanium and silicon) and (2) noble liquids (in two-phase systems of both liquid xenon and liquid argon), which are presently leading the field and showing the greatest promise for coherent scattering of weakly interacting massive particles (WIMPs). Methods with single-phase liquid argon and warm liquids or gases are showing significant promise for the future. The optimum strategy is a near-term push to construct at least two experiments of differing target materials with a goal of improving sensitivity at least a factor of 10 over present limits. The technologies should be chosen from those presently with the most promise to carry them out in a timely and cost-effective manner. At the same time, aiming for the longer term and next level of sensitivity, R&D should be conducted on all techniques with potential for scalability to at least ton-scale detectors. In addition to the U.S.-led experiments, there are presently between 7 and 10 dark-matter direct-detection experiments, principally in Europe, Canada, and Japan. Those programs are also making significant progress and expect to field additional experiments. U.S. experiments are presently leading the field in sensitivity in two or more of the major techniques [e.g., the Axion Dark Matter Experiment (ADMX), CDMS, and XENON10].

The recent XENON10 results are better than the results of CDMS. The XENON10 team is working to eliminate backgrounds. CDMS is expecting to reach a sensitivity of 4×10^{-44} by this summer.

DMSAG's recommendations for CDMS did not support going forward with all seven supertowers, yet. The subpanel felt strongly that it was necessary to demonstrate background reduction in the first two supertowers before recommending the third to seventh supertowers. In addition, the funding plan implies initial funding after 2009, so the two-supertower approach is the fastest way to proceed. However, this uncertainty could lead to unfortunate delays and be too long. DMSAG added, “and if the collaboration demonstrates the necessary control of the backgrounds, we support the completion and operation of the SuperCDMS detector with 7 supertowers at SNOLAB” [Sudbury Neutrino Observatory Laboratory]. If the required background reduction is demonstrated, then the subpanel recommends that SuperCDMS go forward if funding allows. This positive statement allows the review process to start at the agencies immediately. If one put in 1½ years for DOE review and if the review is positive and funding were available, funding could start immediately after the 2009 review.

The report also has an expanded section on DUSEL because DUSEL is very important for dark-matter research. Also, coming along with an underground laboratory may be money for dark-matter research.

Recommendation 8 was modified (expanded) to include equal priorities between (A) continuing the ongoing CDMS and ADMX experiments and the initial construction of SuperCDMS in Soudan with two super-towers and (B) funding the expansion of the noble liquids with the following priorities:

1. The expansion of the liquid xenon experimental efforts to their next level,
2. The U.S. participation in the WIMP Argon Programme (WARP) detector development, and
3. The next stage of the Cryogenic Low-Energy Astrophysics with Neon (CLEAN) argon/neon detector development.

One does not know what technique will be the best, so one needs to keep up progress on each of them.

In addition, the ideas for superheated liquid detectors and detectors capable of determining WIMP direction have great promise but still have significant R&D questions remaining to be answered. Many of the questions associated with the longer-term direction of the experimental efforts will be resolved during the next few years, and a program review in or around 2009 will be necessary.

To realize this program on an optimal time scale, the DMSAG recommends that DOE and NSF increase funding for the direct detection of dark matter from the present $2 or 3 million to about $10 million annually as soon as possible. The prospect of detecting dark matter while the LHC is operating amply justifies this increase. Such a figure is also consistent with the recommendations of P5 and EPP2010.

Cahn commented that the recommendations seem to treat the solid-state detector as being on a par with the liquid xenon. Sobel responded that they *are* on a par, but it is difficult to determine how they would perform at a larger scale.

Shochet asked for acceptance of the report. The vote was unanimous to accept the report. A break for lunch was declared at 12:30 p.m.

The meeting was called back into session at 1:46 p.m., and **Homer Neal** was asked to present the report of the University Grants Program Subpanel.

The charge to the Subpanel was very broad. The Subpanel had groups on writing, the university model, program administration, data collection, and findings and recommendations. It conducted two major surveys with more than 1000 question-responses, five town meetings, multiple Division of Particles and Fields (DPF) meetings, interactions with individuals, and informal reviews.

The landscape is changing. This is a time of great opportunity in astrophysics, cosmology, and neutrino physics. However, with the LHC, much of this work has been offshored. These new realities present great challenges to this important field.

The Subpanel endorsed the priorities of EPP2010 and P5. The program requires an investment in national laboratories and universities.

The major findings are: There must be a partnership between universities and national laboratories. Universities make theoretic breakthroughs, prepare undergraduates, and nourish the overall technical strength of the nation. To make research groups productive, they must have challenging scientific questions, outstanding personnel, freedom to innovate, infrastructure, and a clear and timely review path.

The major recommendations are: The university program must be strengthened in personnel and infrastructure to meet the goals of EPP2010. The scale of this finding is 1% of the HEP budget. Group sizes should be sustained, long-term research scientists should be supported, support for graduate students in theoretical particle physics should be increased, technical infrastructure (electronic and mechanical) should be funded, midscale research projects should be funded, a University grants program committee should be formed to consult with university program managers on the issues facing the university program, scientific assessment groups (SAGs) should regularize their roles in reviewing projects to inform P5 for projects greater than $5 million but below the P5 threshold, the COV process should be continued and strengthened, funding should be merit-based and peer-reviewed, infrastructure (especially computing and networking) should be funded, and remote access to overseas sites should be provided.

In summary, a very different era in U.S. high energy physics research in our universities is being approached, one that is full of promise as well as potential risks. Actions are required to address funding needs, organizational issues, and pipeline issues. Continuing our role as a leader in high-energy physics should be stressed as a national priority. And all parties should recognize the critical role universities play in driving the field and in ensuring its future.

Cushman asked if the agencies are going to have a formal response to the recommendations. Goldberg said that this information will be folded into the agency's peer-review process and other processes. Cushman noted that more support is needed for outreach, theory students, infrastructure, ILC R&D, computing groups, and collaboration software and asked how the Subpanel arrived at the value of 1%. Smith said that it focused on the number of people needed to support the core program and what it would take to have sufficient people in future years. The increase came to about 1%. Cushman asked if there will be a specific attempt to respond to this report and its recommendations. Staffin replied that, if the universities appoint a representative and if that person comes forward, the Office will meet with him or her regularly. If the program is increased by $8 million, the Office could use some guidance as to where to find that $8 million. Dehmer noted that the role of HEPAP and its Subpanels is to give advice to the agencies. The agencies will receive the recommendations of the Subpanel, act on them, and be held accountable by a COV. Goldberg added that the spokesperson should be on the NSF's annual panel. Neal noted that there is no organization for universities to use to coalesce a representative group. Cahn added that setting up another structure is likely to create problems. It would pit one part of the community against another. If there are issues that are not being addressed, that is the problem of HEPAP. Neal pointed out that, at this point, there is no pathway for HEPAP to get information except to set up a Subpanel every 10 or 20 years. Baltay suggested that the DPF would be the best mechanism to do that. Neal said that that would be fine. Some type of mechanism is needed to collect data, conduct surveys, analyze the situation, and pass it on to HEPAP.

Mann noted that many postdocs are paid by the federal government, not by the institutions they are affiliated with. The universities should pay them. Neal pointed out that the Subpanel added a statement on startup funding by universities. However, one cannot ask a university to support someone who is spending several years overseas.

Shochet noted that the field had really changed. HEPAP has to think a lot about these suggestions and see how they can be implemented. Cahn suggested that there be some representation from DPF on HEPAP. Shochet said that that will be done.

Kim asked if this issue has to be looked at from a broader perspective, including U.S. facilities, the reduction of the number of U.S. laboratories, and the bringing of a large program to the United States to get a solution that helps all stakeholders. Shochet stated that the question raised by this report is whether there *is* a whole group of stakeholders. He asked for approval of the report; the vote was unanimous to approve the report.

Jonathan Kotcher was asked to review the recent events regarding DUSEL, a joint initiative within NSF among the divisions of Physics, Engineering, and Geosciences. It is a broad, evolutionary, and multidisciplinary scientific program that offers a new opportunity for growth, diversity, and interdisciplinary research. It is intrinsically a strong program for education and outreach that is well matched to the NSF mission. It addresses a worldwide need for extensive space at depth for all programs over multiple decades and will enable new, long-term partnerships among disciplines and organizations. DUSEL is the centerpiece of the increasing the NSF Physics Division's investment in the U.S. underground science program and the number-one priority for a new project start in the Physics Division. It has been the subject of a large number of community planning activities.

DUSEL will investigate life at depth: the aseptic environment; subsurface biosphere; isolated underground life forms; life at high temperature and pressure; microbial activity at low respiration rates; and associated genomic features. It will form a platform to drill deeper, to 12,000 ft (120° C). It will study rock formation at depth, large-scale rock mechanics and slippage mechanisms, scale/stress dependence of rock properties, seismic transmission with a closer approach to earthquake conditions. It will study fluid flow and transport at depth, and the knowledge gained will have applications to the stability of water supplies, hazardous waste disposal, and remediation of contaminated groundwater. It will study the rock/water interface underground at high pressure and temperature. And it will study mineral resources and environmental geochemistry.

It will provide a very-low-level counting facility and experiments with low background, underground physics, cosmogenics, and applications to homeland security. It will support science, technology, and engineering innovation with novel microorganisms, analytic techniques for geomicrobiology, drilling and excavation technology, environmental-remediation techniques, subsurface imaging, creation of pure crystals without cosmic-ray-induced "impurities," basic research in underground and mining safety, and techniques for excavation of very large stopes at depth.

It will allow the study of neutrinoless double beta decay, solar neutrinos, CP violation in long-baseline experiments, neutrino mixing angles, nuclear astrophysics with low-cross-section measurements, dark-matter searches, proton decay, and supernovae neutrino observations.

DUSEL will have a surface building, an intermediate campus, and a deep campus so people can study what they are interested in. It will be much larger than any other underground laboratory in the world.

A three-tier process was conducted. A site-independent study was conducted to determine the potential use and need for an underground laboratory. It found strong support for pursuit of deep underground science, a need for a cross-agency deep-science institute in the United States, and a need for a deep underground science and engineering laboratory. A second solicitation asked for a site-specific proposed design. There were eight responses, and two conceptual designs were funded and delivered in August 2006. The third solicitation was held (twice). Four proposals were received. The chosen site (the Homestake Mine in South Dakota) will receive $5 million per year for 3 years. A broad-based panel reviewed the proposals very carefully and thoroughly. A cost consultant was also used to review the cost, schedule, contingency, risk, cost and schedule methodology, time and performance, etc. as input to the selection panel.

The panel held meetings and site visits. It evaluated the intellectual merit, broader impacts, site suitability, facility design, and strength of proposing teams. In a secret ballot, the Homestake site was unanimously selected. This is not a commitment to construction but to prepare DUSEL for major research equipment and facilities construction (MREFC). Planning can now take on a focused approach, and formal agreements can be sought with sister agencies. A town meeting will be held, and MREFC will be sought for infrastructure and instrumentation.

Wormser asked when the call for development proposals will be issued. Kotcher replied, in the fall of 2007; planning will continue for the next 3 years. The scale will be the same as for the third solicitation ($5 million per year for three years).

A break was declared at 3:33 p.m. The meeting was called back into session at 3:49 p.m. to hear **William Carithers** present the response to the final charge to P5 about the continuation of the running of the Tevatron. P5 looked at whether the detectors can handle the luminosity, will the accelerator deliver an interesting level of integrated luminosity, and whether the collaborations have the people to operate the detectors and analyze the data.

For running beyond FY09, it looked at the present reach and prospects for new discoveries and whether a few more inverse femtobarns will matter. In terms of the present reach (running through FY09), the increase in the integrated luminosity is dominated by the antiproton stacking rate (how quickly one can make and store antiprotons). Currently, the facility is doing 20 mA/hr and will deliver 6 to 7 fb^{-1} by the end of FY09. The detectors are running well, but there is silicon radiation damage and high fiber tracker occupancy. The radiation damage will not disable the detectors before the end of FY09. The D0 fiber tracking occupancy is worrisome but will likely last through FY09. The CDF intermediate-silicon-layer cooling has developed a leakage problem.

The D0 personnel needs are for a minimum of 124 full-time equivalents (FTEs) for service work by the end of FY09. The personnel were surveyed to project future resources, and 184 FTEs will likely be available, so minimum operations will be covered, and about one third of the personnel time will be available for physics analysis. CDF will need 102 FTEs of service work and has 236 FTEs available. They have also analyzed the availability of personnel with specific expertise.

The Subpanel recommends that

- The Tevatron should run at least through the end of FY09.
- Fermilab should work with D0 to augment human resources.
- The visitors program is highly leveraged and cost-effective.
- Additional applications physicists may be needed.

Under what conditions the Tevatron should run beyond FY09 will be analyzed by P5 in September.

The Subpanel was concerned, in light of the high fiber tracker occupancy, that the D0 tracking-algorithm work will be greater than estimated.

Wormser asked if there is an agreement with the collaborating institutions to finish Tevatron experiments. Shochet answered, yes, there is.

Cahn stated that in considering running beyond FY09 the Subpanel will have to discuss what will have to be given up. Carithers agreed that that is true. The discussion will probably center on opportunity costs. Cahn asked if P5 considered extra running at the B Factory. Staffin pointed out that the charge did not include that question.

Williams asked if the LHC slippage would affect running at the Tevatron. Carithers said that that has been accounted for, and additional slippage is affecting the European Community more than the Tevatron community.

Ozaki asked if it is technically feasible to run the Tevatron beyond FY09. Carithers replied that each year adds a percentage reduction in data efficiency. Radiation degradation is gradual.

Shochet asked for approval of the report. The vote for approval was unanimous.

Michael Harrison was asked to review the ILC Engineering Design Phase that will cover the next couple of years.

The reference design described at the February meeting of HEPAP by Barry Barish has not changed. The value costs have been reviewed three times and found to be conservative. These costs and the associated technical reviews were presented to FALC. The project is moving into engineering design, and the GDE will be restructured, which is consistent with the roadmap. The engineering design phase will include

- Basic R&D
- R&D into alternative solutions
- An overall design
- Selections between high-tech options
- A comprehensive value-engineering exercise
- A complete value cost estimate for the machine
- A project-execution plan
- Designs for facilities shared between different "area systems" and for site-specific infrastructure
- All information necessary to evaluate project technical and financial risks

The highest priority is controlling costs through

- Fundamental containment of the current RDR value estimate (no cost creep),
- Potential cost-reduction via engineering, and
- A risk-mitigating/cost-reducing prioritized R&D program.

It was noted that the ILC costs were reduced by 25% during the reference design phase.

With 22 km of linac, there are a lot of linac units, requiring mass production, achieved on a worldwide basis.

The RDR allows one to identify the cost drivers and the technical risks and is critical in prioritizing both engineering and R&D. The primary cost drivers are the superconducting radiofrequency (SCRF) linac technology and the conventional facilities and siting (CFS), which constitute about 70% of the ILC value estimate. On the basis of technical risk mitigation, cost risk mitigation, cost reduction, and preparation, these costs are broken down into work packages. Work and funding are dispersed. The reference design will be examined, and the question will be asked: "Is this really what we want to build?" before continuing the design and technical R&D.

The engineering design objectives are to

- Complete the critical R&D,
- Monitor industrialization efforts,
- Identify a plan for consolidating design work, and
- Identify ways in which the maximum benefit can be obtained from the European XFEL [X-ray Free-Electron Laser] project.

CFS has been identified as an RDR cost driver. Site-specific issues will complicate the specific engineering. A shallow site was not considered. Critical information for specific site selection and development must be prepared. Performance-driven specifications must be defined for the accelerator components and infrastructure. And cost/performance trade-offs must be iterated.

A program management is being set up for the EDR phase devised along functional lines. Relationships between the project and institutions will be defined through a series of memoranda that define the work packages for each institution.

The project management must be aware of the global perspective. The role of the project managers is to lead the worldwide technical development effort, setting the technical direction and executing the project toward realization of the ILC. The role of the regional directors is to promote and seek funding and authorization for the international cooperative program.

A proposed organization has been developed that has three project managers for SCRF linac technical systems, CFS and global systems, and accelerator systems. Level-three system managers will oversee work packages at various institutions. Matching the work required to the capabilities will be challenging.

Memoranda of understanding are being used to seek resources indirectly. Resources available include SCRF centers of excellence in each region and ILC design and engineering expertise in each region.

During the next 12 months, there will be EDR kickoff meetings, a final GDE meeting, and GDE meetings in each region. Project managers were announced in May.

In conclusion, there is a transition from a reference design phase to the engineering design phase under way. The goal is to produce a design by 2010 that can be used for project approval, site selection, and updated cost estimates with a construction start about 3 years later. What is needed is strong, steady support from the funding agencies, institutional commitment to further develop collaborations, and a commitment to unify and strengthen the governance.

Kephart commented that it seems like direction comes from a lot of different sources. Harrison agreed. Kephart worried that the organization will be dysfunctional. Harrison said that the personnel were trying to shovel mountains and will need a lot of help. Kephart pointed out that there are regional competitions and that choices can be made to make sure that one region does not dominate. Harrison added that one can also allocate market value across regions. They have to coordinate with construction. An ILC Council is being considered as a single coordinator.

Wormser asked if there will be one EDR per site. Harrison replied that the engineering design will have to be customized to the selected site.

Guy Wormser was asked to give a summary of the discussions that he had held with some Europeans in high-level positions.

Raymond Orbach's statement that "even assuming a positive decision to build an ILC, the schedules will almost certainly be lengthier than the optimistic projections" created a stir among European science leaders despite the facts that:

- The United States is very supportive of the ILC program, which is the number-one priority in EPP2010 and the P5 roadmap.
- The FY08 budget holds a very strong increase in and large absolute value for ILC R&D.
- The American Competitiveness Initiative offers a favorable atmosphere for ILC participation.
- Orbach is genuinely concerned about the health of the U.S. physics program in the event of an ILC delay because of international negotiations.
- HEPAP and P5 have said that the U.S. HEP program needs the ILC but can cope with some delay.

It is up to all involved to make sure that this delay is made as short as possible by working out the issues beforehand. The LHC upgrade and ILC-preparatory-phase European Union projects are good vehicles to start this work. The Orbach statement should not be over-interpreted. Everyone shares his concerns, and he did not retreat from the ILC. There are no plans for an ILC delay, but the complexity of securing international cooperation should not be underestimated, either. Without hard work, these delays might well occur.

Conclusions drawn by some European scientific leaders were that

- The United States is not a reliable partner.
- It tends to make unilateral decisions and exhibits a lack of communication between partners.
- It tends to make sudden direction changes, while most Europeans are relying on the United States to push the ILC on the fastest possible track.
- This statement was considered to be a "cold shower" after the excitement of the RDR, where the cost was presented as affordable.
- The ILC *is* indeed delayed with no apparent attention being paid to the potential nature of the delay.
- CERN is back in the loop! For 2008–2011, 240 million Swiss francs were added to the budget for the LHC consolidation, LHC upgrades, CLIC R&D, and ILC participation. Plans include implementing an LHC upgrade in 2012–2015 and building a new big facility at CERN based on ILC or Compact Linear Collider

(CLIC) technology in 2016 onward. (CLIC R&D is now performed primarily at SLAC.)

The message is that an ILC delay is not simply getting to the physics a few years later. The whole concept about having or not having a balanced worldwide program is at stake. Many Europeans see the strong U.S. R&D involvement in the ILC but do not automatically conclude that the United States is still pushing hard for the ILC. The interpretation of the Fermilab roadmap process outcome will most likely be interpreted with this in mind.

Ways to transmit a more positive message could include (1) a clear and strong message from FALC to support the EDR phase; (2) paying good attention to international communication, even on *a priori* U.S. domestic issues; and (3) developing an ambitious and clear plan to work on political issues, such as the siting process, in parallel. If possible, the LHC-upgrade discussions should be used to form an ILC-like "political prototype."

Staffin asked how much these reactions represented the opinions of the working scientists and how much they represented the opinions of the governments. Wormser replied that CERN Council members represent their countries. Staffin observed that the countries at the FALC meeting seemed to agree with Orbach's assessment on how long it will take to organize the ILC effort. The FALC has been approved as the coordinator of the engineering design phase of the ILC. Wormser responded that the European Community seems content with the future plans of CERN, including the plan to build the next big facility at CERN.

Raubenheimer asked if Europe is divided between the ILC and CLIC. Wormser replied that there are institutions in Europe working in CLIC. Perhaps the ILC will be built in the United States, and CLIC in Europe. Shochet noted that these two facilities should be on different time scales. Raubenheimer noted that SLAC has only tested three structures for CLIC for about $50,000. That does not constitute a domination of CLIC R&D by SLAC.

Shochet asked how many years in advance the plans would be made to fill the wedge at CERN and whether that would occur somewhere around 2011 or 2012. Wormser replied that it will depend on the development of the technology. Pripstein said that the upgrade of the LHC might preclude the participation of Europe in ILC. Raubenheimer pointed out that the CLIC gradient is four times greater than that of the ILC.

Staffin said that the Europeans are expecting to commit 240 million Swiss francs over 4 years for other projects and nothing for the ILC. They would have nothing for ILC construction until 2016; Japan might contribute starting in 2012. There are also mixed comparisons of ILC and CLIC in whether the schedule is technically limited or not. Wormser said that CERN is concerned about what technology will be appropriate.

Frank Würthwein was asked to describe the Open Science Grid (OSG). OSG will provide distributed computing infrastructure for the LHC. It is a significant resource for the Tevatron Program and many others.

The LHC computing problem is sociological: tens of millions of dollars worth of computing is distributed across tens of countries. Making this work requires new technologies and organizational structures.

The OSG is for forming communities that include the science community, the computational science community, and the information technology (IT) shops at universities and national laboratories. In addition, local interests and needs have to be distributed over international infrastructures.

This initiative started with three projects [International Virtual Data Grid Laboratory (iVDLG), Grid Physics Network (GriPhyN), and Particle Physics Data Grid (PPDG)] that combined into Trillium that morphed into Grid 3 and then into the OSG. Anyone can join the OSG Consortium. OSG was funded for 5 years in fall 2006. The infrastructure is deployed predominantly in the United States. The OSG project effort was initiated by DOE and NSF. The users are predominantly laboratories like Fermilab that use the resources for the Compact Muon Spectrometer (CMS), A Toroidal LHC ApparatuS (ATLAS), and other experiments.

For example, D0 requested 1.5 to 2.0 thousand CPUs for reprocessing of data. Unused LHC resources were used to fulfill this need, and more than 50% of the reprocessing was done on OSG in the United States, the United Kingdom, France, and Canada at institutions that had nothing to do with D0.

During the LHC commissioning, up to 100 Tbytes per day will be transferred on the OSG.

Cybersecurity is needed and is provided by OSG. Tier 3 (as well as Tier 1 and 2) resources have been made available over OSG. OSG will transparently manage changes in technology. It will also adapt legacy systems to the Grid. Personnel at OSG provide bootstrap assistance to research groups.

OSG does not own any computing resources or middleware or fund any site. What it does is help sites join the OSG facility, help virtual organizations join the OSG, maintain and support an integrated software stack, reach out to non-HEP communities, and train users.

OSG can offer middleware, organizational support, technical support, and a bridge that forms an integrated national cyberinfrastructure by connecting desktops to campus cyberinfrastructure and to national and international facilities. The benefits of OSG to high-energy physics so far are

- For LHC: a middleware stack for the LHC distributed computing systems of ATLAS and CMS; a strong partner to negotiate technical and operational problems with Enabling Grids for E-Science in Europe (EGEE) and Nordugrid; and a framework for integrating Tier-3 resources.
- For Tevatron: Monte Carlo production for the Collider Detector at Fermilab (CDF); reprocessing for D0; and Fermilab campus grid services.
- Others (e.g., IceCube and ILC) are starting to show interest, as well.

Wormser said that, in Europe, there is a similar program and asked what the cost was to the field to have two programs operating in parallel. Würthwein replied that multiple structures are being allowed, making the system more likely to succeed outside particle physics. Pordes added that Europe and the United States have common, interoperating, and collaborative facilities and software development. There will be other grids in other parts of the world.

Joel Butler was asked to review ATLAS/CMS. Startups are expected in March or April 2008. Construction of ATLAS is essentially complete, and commissioning is under way. The schedule calls for the detector to be installed by October 2007 and the experimental hall to be ready for closure in March 2008.

Subsystems are being integrated. The control room has been exercised, and valuable lessons were learned. 27,000 muon monitor drift tubes (MDTs) were used to detect cosmic rays. Physics opportunities will occur early, even with moderate luminosity. One wants to be ready for these early findings. High data rates were sustained between Tier 0 and Tier 1 sites. The United States is providing more than its share of resources.

The CMS is a different type of detector. The solenoid was lowered into the pit, and the other components are being lowered and installed. The entire device is completed and has been operated above ground. CMS detector installation and commissioning is to be completed in March. The pixel detector barrel is 95% complete, and both halves of the first endcap have been delivered to CERN. The first half of the barrel electromagnetic calorimeter (ECAL) is installed. The final section is scheduled for installation in June 2008. The muon system is done and is starting to be installed. It is one of the last components to be installed. The U.S. computing sites are up and functioning.

Both experiments are driving themselves to be ready at the new energy frontier. The output of the LHC will be 10^5 to 10^6 times that of the Tevatron. The first look at this energy will be quite exciting.

In summary, there are no known significant open technical issues although both experiments face challenges in getting everything commissioned and integrated. Both experiments have established powerful computing capabilities based on a grid-enabled distributed, hierarchical model. Both experiments will have significant experience operating their detectors on cosmic rays during the early part of 2008 before the collisions start. Both experiments are developing fast commissioning, calibration, and alignment strategies. Both experiments have developed advanced offline analysis packages capable of analyzing early data very quickly. So, ATLAS and CMS will be ready to record, process, and analyze data and to extract early physics. Both experiments eagerly anticipate the first 7-TeV by 7-TeV collisions in the summer of 2008.

Samios asked if they were working out discovery procedures. Butler replied that, there are so many people, both experiments are already working on that.

The meeting was adjourned for the day at 6:32 p.m.

Saturday Morning
July 14, 2007

The meeting was called to order at 8:30 a.m., and **Alan Bross** was asked to review the neutrino factory and muon collider R&D.

Since 1995, the Neutrino Factory and Muon Collider Collaboration (NFMCC) has pursued an active R&D program that has focused on muon production, capture, and acceleration. Initially, the physics emphasis was on muon colliders (both a Higgs Factory and an energy frontier machine). By 2000 the focus of the collaboration had shifted to studying the feasibility of a neutrino factory. Recently new ideas in muon ionization cooling have reinvigorated the collaboration’s efforts on the investigation of energy frontier muon colliders. The collaboration has a charter to study and develop the theoretical tools and the software simulation tools needed and to carry out R&D on hardware that is unique to the design of neutrino factories and muon colliders. It has two oversight groups: the Muon Collider Oversight Group (MCOG) and the Muon Technical Advisory Committee (MUTAC). The collaboration includes universities and national laboratories from the United States and abroad as well as a corporate partner. The core program includes

- Targetry R&D: Mercury Intense Target (MERIT) Experiment
- Ionization cooling R&D: MuCool and the Muon Ionisation Cooling Experiment (MICE)
- Simulations and theory
- Muon collider task force

The question is, is muon production, capture, and acceleration R&D worth the investment? The program started with intense low-energy muon physics, a neutrino factory, and an energy-frontier muon collider. If one compares muon facilities, the Very Large Hadron Collider (VLHC) was proposed at 73 km in diameter, the ILC at 40 km in length, and the CLIC at 33 km in length. The Compact Lepton Machine (CLEM) is very compact in comparison, about 1.2 km in diameter.

Low-energy muon physics offer a sensitive test of lepton flavor violation (LFV) and require an intense low-energy muon beam.

A neutrino factory should have an intense beam and well-known systematics. The preliminary-design scheme collects pluses and minuses at the same time. The low-energy neutrino factory is lower in cost. One question is whether a neutrino factory is needed to fill in the muon-neutrino blanks. The best possible reach in θ_{13} for all performance indicators is a neutrino factory. There are indications that $\sin^2 \theta_{13}$ is quite small. It is still not known if a neutrino factory is the right machine to build, but a decision point looms at 2012. After NOvA and T2K, if θ_{13} is not seen or is seen at 3σ, one should consider major upgrades or a new facility. In order to make an informed decision about a new facility and whether the neutrino factory plays a role, one will need an RDR to define the R&D program.

Muon colliders will reach multi-TeV lepton-lepton collisions at high luminosity. Muon colliders may have a special role for precision measurements. The small-ΔE beam spread will allow precise energy scans, and the small footprint would allow it to fit on an existing laboratory site. When a muon collider is compared to the energy-frontier electron-positron collider, for many processes there will be similar cross-sections, there will be an advantage in s-channel scalar production, beam polarization is also possible, and there will be a more precise energy scan capability. Muon-decay backgrounds in a muon collider do have detector implications, however.

In regard to the probability of resolving a degenerate Higgs, for larger values of $\tan \beta$, there is a range of heavy Higgs boson masses for which discovery at the LHC or an electron-positron linear collider may not be possible because of the suppression of coupling to gauge bosons. Other machines may not be able to fill this hole.

The needs common to the neutrino factory and the muon collider include the proton driver; target, capture, and decay; phase rotation; cooling; acceleration; and storage ring. However, there are key differences: The muon collider has bunch merging, while the neutrino factory does not. The muon collider's cooling reduces the 6-D emittance, while the neutrino factory's reduces the transverse emittance. The muon collider accelerates to 1 to 2 TeV, while the neutrino factory only goes to 20 to 40 GeV. The muon collider's storage ring has intersecting beams while the neutrino factory's does not. The key R&D issues are high-power targetry, initial cooling, 200-MHz RF, intense 6-D cooling, bunch recombination, acceleration, storage ring(s), and theoretical studies.

Muon ionization cooling is quite simple and will be accomplished by transverse cooling or longitudinal-emittance exchange. Both techniques are needed in a muon collider.

The front ends of these machines are similar. Although a great deal of R&D has been done for a neutrino factory, all of which is applicable to a muon collider, the technological requirements for a muon collider are much more aggressive: bunch merging, much more cooling, acceleration to much higher energies, and storage rings are required.

The helical cooling channel, which cools in 6-D, consists of magnets filled with hydrogen gas and includes radiofrequency (rf) cavities. There are several extreme ideas for muon cooling: parametric-resonance ionization cooling and reverse emittance exchange.

MERIT is the Mercury Intense Target, which studies a mercury jet target in an intense magnetic field. It has been put in the tunnel at CERN with operation expected this fall.

In MuCool studies, a drop in the cavity gradient (by a factor of 2) was seen with increased magnetic field. This phenomenon will be investigated with a button test at 805 MHz.

The 201-MHz cavity is now operating; it was tested to the design gradient of about 16 MV/m at zero and at a few hundred gauss after being assembled at Jefferson Lab. It came up to maximum almost immediately. A 2.5-T coupling coil is needed for the MICE design to provide high-gradient rf operation in a magnetic field.

In China, a high-pressure-hydrogen test cell is being used to study breakdown properties of materials in hydrogen gas and operation in a magnetic field.

Two liquid-hydrogen absorbers are being studied. A liquid-hydrogen (solid) design is also being considered for the neutrino factory.

MICE is the cooling demonstration program, a single-particle-emittance experiment. There are six steps to constructing and testing the apparatus. It has tracking spectrometers, matching coils, liquid-hydrogen absorbers, coupling coils, rf cavities, focus coils, and a magnetic shield.

There are some key simulation studies to be performed: muon capture and bunch rotation, performance of an open-cell rf lattice, full optimization of the acceleration scheme for the neutrino factory, full simulation and performance evaluation of phase ionization cooling (PIC) and reverse emittance exchange (REMEX), a complete baseline cooling scheme for a muon collider, an acceleration scheme for a muon collider, and the design of low-beta collider ring.

The conclusions are that there is a compelling case for a precision neutrino program. With present assumptions, the neutrino factory out-performs other options. However, more is needed before concluding that this is the right path. The ongoing neutrino-physics program tells us that θ_{13} is crucial. The collaboration is making excellent progress on R&D on the major subsystems. There was strong participation in the recently completed International Scoping Study; the goal is to deliver an RDR by 2012. New concepts in muon cooling improve the prospects for a multi-TeV muon collider. The front-end is the same as or similar to that for a neutrino factory. The first end-to-end muon cooling scenario for a muon collider has been developed. There is much more to do: detailed simulation and analysis of cooling designs, acceleration, and the collider ring. The NFMCC will work closely with the Fermilab Muon Collider Task Force.

Carithers asked how the funding was distributed. Bross replied that $3.6 million was used plus a supplement of about $400,000. There are 40 FTEs. The UK program contributes to the neutrino factory, muon collider, and MICE to the order of £7 million per year plus funding for other portions of the program.

Samios asked if the Palmer scheme was going to be tested. Bross replied that it was being investigated. In the fall, a workshop will be held on muon cooling.

Cushman asked what the strategy would be if θ_{13} were large. Bross replied that, in that case, the neutrino factory would not be the way to go. That would make the decision about the muon collider more difficult.

Anthony Tyson was asked to present a primer on the LSST, the Large Synoptic Survey Telescope, which will study dark energy with an 8.4-m-diameter main-mirror telescope. It takes 10 square degrees in one snapshot with 3 billion pixels. It goes deep, wide, and high at the same time. One can cover the complete sky in a few nights.

P5 recommended for Stage 4 of dark energy investigation developing the LSST and SNAP as collaborative interagency projects. One metric is etendu, the rate of sky coverage. When one compares all facilities, the etendu of the LSST is shown to be twice as powerful as that of any other facility, even when all the other facilities are assumed to be operating 100% in one survey. The key question is how many galaxies one can measure.

Multiple science results would come out of this single LSST experiment. The bottom line is that dark energy is accelerating the expansion of the universe, which is parameterized by the Hubble factor. Cosmic distances are proportional to integrals of H(z) – 1 over redshift. One can constrain H(z) by measuring luminosity distances of standard candles (Type 1a supernovae) or angular-diameter distances of standard rulers (baryon acoustic oscillations). Another powerful approach involves measuring the growth of structure as a function of redshift. Stars, galaxies, and clusters of galaxies grow by gravitational instability as the universe cools. This provides a kind of cosmic clock, the redshift at which structures of a given mass start to form is very sensitive to the expansion history. There are several techniques for probing dark energy: cosmic shear, baryon wiggles, supernovae, cluster counts, and cosmic microwave background.

The LSST probes dark matter in multiple ways (measuring 4 billion galaxies) with a single data set. How good is the $w(a)$ of the Dark Energy Task Force (DETF)? It covers only one type of behavior, whereas several models of behavior are theoretically possible. It is not known what dark energy is. There must be observational experiments to inform theory. The upshot is that the DETF underestimates the impact of experiments and the relative value of Stage 4 vs Stage 3 (by a factor of 100).

In investigating cosmic shear, the galaxies can be categorized according to redshift extent, typically into five bins. Baryons influence each category of galaxies differently. The LSST can categorize galaxies with much higher resolution (i.e., into many more bins).

Baryon acoustic oscillations (BAOs) are fluctuations from decoupling. If one looks at enough galaxies, one can analyze these oscillations. Combining BAO with the cosmic-shear technique gives a lot of information.

The cosmic-shear signal on larger angular scales is at a very low level. To make this measurement, one must be confident that one understands and can remove spurious sources of shear. LSST is the first large telescope designed with weak lensing in mind. Nevertheless, it is essentially impossible to build a telescope with no asymmetries in the point spread function (PSF) at the level required. Fortunately, the sky has some natural calibrators to control for PSF systematics: There are three stars per square arc-min bright enough to measure the PSF in the image itself. Light from these stars passes through the same atmosphere and instrumentation but is not subject to cosmic weak lensing distortions. By interpolating the PSFs, one can deconvolve spurious shear from the true cosmic-shear signal. The key issue is how reliable this deconvolution is at very low shear levels. Therefore, a lot of exposures were taken on the Subaru camera, and a shear–shear correlation function was found. It showed that 300 exposures per sky patch will yield negligible PSF-induced shear systematics. The other systematic error is photometric redshifts. It has seen a huge advance; a new technique for calibrating by using the clustering properties of galaxies.

Combining techniques breaks degeneracies; this requires a wide-sky-area deep survey. Precision versus integrated luminosity shows that combining probes removes degeneracies.

The LSST, by looking at the gravitational lensing of 4 billion galaxies, can produce a detailed map of dark matter and measure total neutrino mass.

The LSST is a public/private partnership. It has been proposed as an NSF construction project with 22 institutions in the collaboration. A site in northern Chile has been chosen. The design is completed. Vendors are designing components. It would produce 30 Tbytes per night. The proposed timeline leads to the first light at the end of 2014.

The LSST addresses two of the most central questions in physics, dark energy and dark matter, with a single facility with multiple high-precision measurements. It has a detailed design and is in a high degree of readiness. The team combines complementary skills and experience.

Wormser asked if there is international collaboration. Tyson replied that they have been talking to France, Germany, and the United Kingdom about joining the collaboration.

Cahn noted that much more will be known about dark energy investigation in the coming months. HEPAP will have to confront the issue of dark energy in November.

Kahn noted that there are advantages to both space and ground observations. The benefits of ground-based observations are a rapid coverage of sky and the ability to take many measurements.

Lyn Evans at CERN joined the meeting by telephone and was asked to review the progress of the LHC. The cryodipole overview showed that virtually all of the dipoles have now been installed. All of the magnets have been cold tested.

The cryogenic system (which is massive) has been almost all commissioned; the last four units are being commissioned now. The next job is to interconnect the dipoles. Three of eight sections have been finished and are cold. Three others are closed and are being cooled. Two others are approaching the completion of interconnection.

Sector 7-8 is now being warmed up to connect the inner triplet and make other repairs. Before warmup, the distribution feed box (DFBX) was successfully cooled down.

Sector 4-5 is being cooled down. And in Sector 8-1, the repair of the inner triplets in both sides of the sector is complete. A successful pressure test was conducted the day before the meeting. Cooldown will be started at the end of August.

The heat exchanger tube at Point 5 failed at a pressure of 20 bar and has been repaired. An Invar-rod/aluminum-alloy-tube cartridge was designed to fix this problem, and such cartridges have been installed and tested at Q1. The repair was able to be completed in the tunnel area

Cooldown of a sector is accomplished first from room temperature to 80 K, precooling with liquid nitrogen. It took three weeks for the first sector.

Hydrodynamic cold compressors are used for 1.8 K refrigeration. These compressors required some modification but are working well now. A uniform temperature at 1.9 K was accomplished along the length of Sector 7-8.

The rf cavities are all installed and are being connected. The last production problem was with the collimators. The production level needed to finish production on time has now been accomplished.

Procurement problems with the remaining components are now settled, and installation and interconnection work is proceeding at a high pace in the tunnel. Numerous nonconformities were intercepted by the quality-assurance program, resulting in added work and time. Technical solutions were found for the inner-triplet problems, and repairs are well under way. Commissioning of the first sectors can proceed by isolating faulty triplets, but that commissioning will have to be redone with the repaired triplets (requiring additional warm-up/cooldown cycles). The first sector has been cooled down to nominal temperature and operated with superfluid helium; teething problems with the cold-compressor operation have now been fixed. The second sector is being cooled down. Power tests have started.

The General Schedule calls for an engineering run, which was originally expected at the end of 2007 but was delayed by installation and commissioning and has been canceled. 450-GeV operation is now part of the normal setup procedure for beam commissioning to high energy. The General Schedule is being reassessed, accounting for inner-triplet repairs and their impact on sector commissioning. All technical systems will be commissioned to 7-TeV operation, and the machine will be closed by April 2008. Beam commissioning starts May 2008, and the first collisions at 14 TeV c.m. are expected to occur in July 2008. Luminosity evolution will be dominated by the confidence in the machine-protection system and by the ability of the detectors to absorb the rates. No provision is made in the success-oriented schedule for major mishaps (e.g., the additional warmup/cooldown of a sector).

The LHC General Schedule showed the machine commissioning progressing.

Shochet stated that the progress is impressive.

Wormser asked what the lessons learned from the power tests of Sector 7-8 were. Evans replied that the main problems were procedural in nature. The staff was pleased with how things went.

Bortoletto asked what the foreseen schedule was for bunch crossings. Evans answered that there are two magic 25-and 75-ns bunchings that will light up all the detectors. The 75-ns bunching will be attempted first. Then the luminosity will be increased, and the 25-ns bunching will be attempted.

A break was declared at 10:26 a.m. The meeting was called back into session at 10:44 a.m.

Nick Kaiser was asked to give a primer on PanSTARRS, the Panoramic Survey Telescope and Rapid Response System. It would exploit the observing sites in Hawaii. It is less powerful than the LSST.

The goal is to determine the equation of state $w(z) = p/\rho c^2$. Dark energy influences expansion history, geometry, and growth of structure. Multiple, and complementary, methods need to be exploited to measure dark energy within a few percent, such as weak lensing, galaxy clustering, the Hubble diagram from Type 1A supernovae, and galaxy-cluster abundances. The requirements are to conduct static sky studies with precision photometry for photometric redshifts, precision shape measurement for weak lensing, and area/depth for cosmic variance. To conduct transient studies, one must do precision photometry, PSF modeling, and cadence, all with the same hardware.

PanSTARRS is much smaller than the LSST and has a smaller collecting area. Also, it has 41.8-m collectors. Its etendu is about 50. Factors that affect performance include cost, risk assumed, and scalability. The way PanSTARRS gets around systematic errors is to collect many images and to convert systematic errors to statistical errors. It has a seven-square-degree field of vision with 1.4-gigapixel cameras.

PanSTARRS hardware development was funded in September 2002; development, infrastructure, and testing were implemented in 2003 to 2005; and the PS1 (one-camera PanSTARRS) science-vision camera is to be installed in August 2007. PS4 development and construction is expected to occur from 2007 to 2010, with a 10-year mission starting in 2011.

Not a lot of risk was taken with the optics. The optics deliver a 3° field of view to meet the science goals. PSF sampling of 0.26 min is required to match Mauna Kea seeing. Therefore, each focal plane will have 1.4 billion pixels. Compared to existing cameras, the goal was to increase the pixel count by a factor of 10, increase the readout speed by a factor of 10, and decrease the cost per pixel by a factor of 0.1.

The detectors are orthogonal-transfer arrays, a new paradigm in large imagers. It partitions a conventional large-area charge-coupled device (CCD) imager into an array of independently addressable CCDs (cells). A massively parallel design allows rapid read-out and rapid sky coverage. Reading can be multiplexed, and failure of a few cells does not affect the resolution significantly. A new pixel design noiselessly removes image motion at high speed (~10 microseconds).

It was decided to pursue high quantum efficiency at high wavelengths.

The image-processing process will produce a point-spread image from the observation compared to a database of the static sky. One must take multiple images of the sky and add them up.

In addition to major system components, the PanSTARRS design features a number of advanced auxiliary systems to control, reduce, or ameliorate the effect of systematic errors. Implemented features include an advanced tunable laser system for total-system-throughput calibration, wave-front sensing systems for real-time control of optical alignment and collimation, and an imaging sky probe to monitor sky transparency. The response of the camera will be calibrated pixel by pixel daily. Wavefront sensing systems are used to detect and correct for aberrations in the image. A control box is being developed to control the reflectors to maintain the quality of the images.

Sky transparency is measured in real time with an imaging sky probe.

The next thing to be introduced is a spectroscopic sky probe to detect deleterious effects of the atmosphere. A spectroscopic sky probe can provide accurate calibration of the absorption of the atmosphere and subtraction of air-glow.

The limiting magnitude in a 32-sec exposure is 2 orders of magnitude less for PS1 than it is for the LSST. PanSTARRS would complement the LSST in that the LSST is in the southern hemisphere, and PanSTARRS covers more of the northern sky. PanSTARRS could be operational in 2010, but that date is more like 2013. No funds have been identified for operation; it needs $3 million per year for 10 years. $100 million has been invested in R&D for PS1.

Carithers asked how Haleakala would be for a backup if Mauna Kea does not work out. Kaiser responded that 2010 is well within the permitting path. A permit is expected one year from now. The competition is the 30-m telescope. The fallback of Haleakala means a slightly lower image quality.

Shochet commented that HEPAP needed to be aware of what is going on in other review boards and panels. He initiated a discussion about the points that might be covered in the letter to the agencies, summarizing this meeting:

- HEPAP is pleased with the DOE budget numbers.
- FALC is taking positive steps.
- The CERN Director General has proposed an LHC upgrade.
- The dark-energy R&D solicitation has been issued.
- A step has been taken to internationalize the ILC.
- A HEPAP presentation on the structure of the budget would be of interest.
- The integrity of the planning process should be respected.
- HEPAP is pleased with the increased funding of the NSF Physics Division and with the planned competition for the new Physics Frontier Centers.
- The progress on the DUSEL site selection is encouraging, and HEPAP supports opening the facility to the international community.
- HEPAP is pleased with the increase of major research instrumentation (MRI) upper-limit funding to $4 million and suggests establishing a program to fund projects outside the current limit.
- A Fermilab strategy group is working on a roadmap for supporting the ILC and a synergistic R&D path.
- NuSAG has recommended R&D to inform technological decisions about beams and detectors; its report was approved.
- The DMSAG report was approved after small changes.
- The University Grants Program should be strengthened, and an advisory structure should be implemented; the Subpanel’s report was approved.
- The DUSEL site was selected, and an initial suite of experiments should be funded through a solicitation.
- P5 enthusiastically recommended running the Tevatron through FY09; its report was approved.
- An ILC R&D and engineering plan is being developed, and a draft is expected in the next 9 months; the process is complicated by the lack of a specific site; the expectation is to maintain or reduce the project cost.

- The view of some in Europe is that the United States is not a reliable partner and that CERN is back in the ILC game and could possibly start a new machine in 2016.
- OSG has had impressive successes in its first year; it will be crucial to the operation of the LHC and will serve a number of scientific communities.
- The ATLAS and CMS detectors are near completion.
- A neutrino factory and muon collider are being designed for neutrino research.
- LSST and PanSTARRS are being designed for ground-based observation of the effects of dark energy.
- Installation and commissioning of the LHC components are proceeding on schedule.

The meeting was adjourned at 12:07 p.m.

Respectfully submitted,
Frederick M. O'Hara, Jr.
Recording Secretary
July 25, 2007

Corrected – M.J. Shochet, August 18, 2007

The minutes of the High Energy Physics Advisory Panel meeting held at The Hotel Palomar, Washington, D.C. on July 13-14, 2007 are certified to be an accurate representation of what occurred.

Signed by Melvyn Shochet, Chair of the High Energy Physics Advisory Panel on August 18, 2007.