Minutes of the
High Energy Physics Advisory Panel Meeting
October 12-13, 2006
Latham Hotel, Washington, D.C.

HEPAP members present:

Jonathan A. Bagger, Vice Chair
Charles Baltay
Daniela Bortoletto
James E. Brau
Robert N. Cahn
William Carithers
Alex J. Dragt
JoAnne L. Hewett
Joseph D. Lykken
Peter D. Meyers
William R. Molzon
Koichiro Nishikawa
Angela V. Olinto
Saul Perlmutter
Steve M. Ritz
Tor Raubenheimer
Nicholas P. Samios
Melvyn J. Shochet, Chair
Guy Wormser

HEPAP members absent:

Satoshi Ozaki

Also participating:

Aesook Byon-Wagner, Office of High Energy Physics, Office of Science, Department of Energy
Joseph Dehmer, Director, Division of Physics, National Science Foundation
Persis Drell, Stanford Linear Accelerator Center
Lyn Evans, LHC Project-Leader, Conseil Européen pour la Recherche Nucléaire
Tom Ferble, Office of High Energy Physics, Office of Science, Department of Energy
Paul Grannis, Office of High Energy Physics, Office of Science, Department of Energy
Giorgio Gratta, Department of Physics, Stanford University
Daniel Green, US/CMS Technical Director, Fermi National Accelerator Laboratory
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
Marsha Marsden, Office of High Energy Physics, Office of Science, Department of Energy
Hugh Montgomery, Associate Director for Research, Fermi National Accelerator Laboratory
Homer Neal, Department of Physics, University of Michigan
Frederick M. O'Hara, Jr., HEPAP Recording Secretary, Oak Ridge Institute for Science and Education
Robert Plunkett, MINOS Cospokesperson, Fermi National Accelerator Laboratory
Lee Roberts, Physics Department, Boston University
Randal Ruchti, Program Director, National Science Foundation
Bernard Sadoulet, Department of Physics, University of California at Berkeley
Michael Salamon, Space Mission Directorate, National Aeronautics and Space Administration
Abraham Seiden, Director, Santa Cruz Institute for Particle Physics, University of California at Santa Cruz
Henry Sobel, Department of Physics and Astronomy, University of California at Irvine
Robin Staffin, Associate Director, Office of High Energy Physics, Office of Science, Department of Energy
Michael Tuts, Department of Physics, Columbia University
Andreene Witt, Oak Ridge Institute for Science and Education

About 70 others were also present in the course of the two-day meeting.

Thursday, July 6, 2006
Morning Session

Chairman **Melvyn Shochet** called the meeting to order at 9:00 a.m. He recognized the Nobel awards to Smoot and Mather and summarized the agenda of the meeting. He asked **Robin Staffin** to present the news from DOE.

The FY07 budget is still in conference between the House and Senate, and there is no telling when the final budget will be approved. There is a continuing resolution allowing spending at the lower of the rates based on last year's month by month with some adjustments.

The interagency work is continuing with the National Aeronautics and Space Administration (NASA) and NSF, particularly the call for proposals for the international linear collider (ILC). This cooperation will be extended to the Deep Underground Science and Engineering Laboratory (DUSEL) with NSF. With NASA's Beyond Einstein proposals including the Joint Dark Energy Mission (JDEM), the National Academy of Sciences (NAS) has been asked to advise on the order of missions. They will report next September, affecting the FY09 budget.

The P5 Subcommittee is setting precedent in the detail of its work. What comes from HEPAP's deliberations will have a valuable effect on the budget-development process. The Subpanel is telling DOE how to think about future budget paths.

Things are moving very well at the various facilities: Tevatron, B Factory, Neutrinos at the Main Injector (NuMI), etc. What is being waited for is new physics. The Large Hadron Collider (LHC) seems to be moving ahead well.

A decision point is being approached on high energy physics in this country. EPP2010 was a broad committee review of the field in a national and international context. It recognized the ILC as a key component of future physics. It also recognized other efforts.

The parallel European group and the Japanese Government are also considering such an assessment. India has also expressed interest.

Cahn asked for additional information on the National Research Council (NRC) study on Beyond Einstein. Staffin replied that there will be a webpage; DOE and NASA representatives will meet with the NRC staff two weeks after this meeting. Cahn commented that the study seems to be directed toward NASA and asked how it would impact DOE policy. Staffin said that the study's results will affect DOE in what NASA can bring to the table for JDEM. It will also affect negotiations with other countries. NASA is a valued partner in dark-energy investigations.

Shochet asked **Joseph Dehmer** to present the NSF perspective. Tony Chan, an applied mathematician, has replaced Michael Turner as associate director. The Senate and House marks are very near the request, a 6.6% increase over FY06. This is the final step up in support for the LHC. DUSEL and other projects are also supported. Both DOE and NSF are in a positive budget environment.

On the underground laboratory, a draft of the site-independent report is in hand. It addresses a lot of hard science and fills in a lot of gaps. There is a joint vision to advance the prospect of underground science in the country. It has also fallen to the DOE OHEP to pursue construction of the ILC and to the DOE Office of Nuclear Science (NS) to pursue construction of the Rare Isotope Accelerator (RIA). These are the main budget items. NSF will jointly support the science. This joint vision is remarkable. Subpanels of HEPAP need to put the future health of particle physics at their focus. They should not attempt to play off one project against another. They need to focus on what the field should look like 10 years from now.

There is the feeling that NSF's support for DUSEL will hurt the prospects of the ILC. The White House will be the lead in deciding about the ILC; it will not worry about a minor portion of a small agency (DUSEL).

It is clear that particle physics has bottomed out and that the prospects are brighter than they have been.

Brau noted that NSF has a policy that R&R [research and related] will be at least 50%, but more will be needed for DUSEL. Dehmer agreed; if the budget remains at $250 million, that will be a problem. However, the model calls for 5 to 6% growth, to raise the amount dedicated to principal investigators (PIs). Brau asked how that decision was made. Dehmer replied that, in the past, the policy is that the requesting group will have to provide funding, but that does not lead to good multidisciplinary science. The comment on feasibility is based on a different way of looking at things.

Cahn asked what other projects are looking to major research equipment (MRE) for support and in what time frames. Dehmer replied that the time intervals vary widely. DOE is great at building facilities; NSF is not. The process of cooperation is still being defined. There is a statement about this process on the web. The LHC, Ice Cube, Advanced Laser Interferometer Gravitational Wave Observatory (LIGO), and DUSEL are the projects that are being supported along with some astronomy projects [e.g., the Large Synoptic Survey Telescope (LSST) and Giant Segmented Mirror Telescope (GSMT)]. One has to make a science case and make it through a large organization.

Bernard Sadoulet asked what would happen if full funding was not received. Dehmer replied that there is a Plan A (in which everything goes well with precise timing) and Plan B (in which some things are deferred).

S3, a solicitation for establishing the baseline, is out; the deadline is January 9, 2007.

A break was declared at 9:48 a.m. Shochet called the meeting back into session at 10:20 a.m. The Office of Management and Budget (OMB) wanted to introduce performance measurements across the government. Measures were drawn up to assess progress toward intermediate and long-term goals. A mid-term assessment is currently being conducted. Three areas will contribute to that assessment: the Tevatron at Fermilab, the B Factory at Stanford Linear Accelerator Laboratory (SLAC), and NuMI/MINOS [Main Injector Neutrino Oscillation Search].

Young-Kee Kim was asked to report on the status of the Tevatron. There are many measurements done at the Tevatron, many the world's best or first. About one paper per week is published. A recent one was on B_s oscillation. Experimentally, asymmetry is measured as a function of decay time, and the frequency of particle-antiparticle oscillation is determined. Preliminary results a year ago were based on 610 pb^{-1}. There is no oscillation up to a specific point, about $\Delta m_s = 12$. This year, there has been a significant improvement in resolution, and a signal is being seen. During the past five months, analytical methods have improved the certainty of signal detection to 5 sigma, and evidence has become observation.

However, B_s oscillation is just one analysis. Other measurements are also being made. Measurements in the B_s sector that can constrain new physics include Δm_s, $\Delta\Gamma_s$, $Br(B_s \rightarrow \mu\mu)$, and charge-parity (CP) violation. Movement is being made toward understanding the origin of mass. Measurements have been made of the top-quark mass. The goal of 2 fb^{-1} was surpassed. Comparison of observation with the Standard Model indicates that the Higgs is light. With about 15 CDF [Collider Detector at Fermilab] and D0 results combined, Standard Model sensitivity is within a factor of 5 to 10.

The machine was shut down from March to mid June to do upgrades of the Run IIb detector and accelerator improvements and maintenance. All went extremely well and produced a 30% improvement. The Tevatron is performing extremely well, setting a peak luminosity record of 2.3×10^{32} cm^{-2} s^{-1} and a weekly integrated luminosity record of 33 pb^{-1} per week per experiment. This will lead to a doubling time of about a year and 620 pb^{-1} in FY06.

The Tevatron accelerator and D0 and CDF experiments have been performing well. We are confident we will get 8 fb^{-1}. B_s oscillations have been observed. In top and W mass, the 2 fb^{-1}

goal was surpassed. Processes are now being measured with a few picobarns.

The primary goal is to reach Standard Model Higgs sensitivity. Improvements are being made with statistics, triggers, acceptance, efficiency, jet resolution, and analytical techniques. The task is complex and challenging with many final states, small cross-sections, and large backgrounds. Many other high-profile analyses will benefit from this experience.

Current concerns are losing detector and offline experts (university groups). Modest help from funding agencies could make a big difference. A survey showed that new students are interested in the Tevatron experiments. Tevatron is a great place for education and training new PhD students.

Wormser asked what the cause was of the differences between experiments. Kim replied that the different predictions did not use the same model.

Bortoletto asked what the response was of the academic community. Kim responded that some strong universities have decided to leave. Some students want to continue. There has been a lot of verbal support from DOE in easing the transition. Ferbel added that most national laboratories as well as selected universities are deserting Fermilab.

Persis Drell was asked to review the status of PEP-II [Positron-Electron Project] and BaBar at SLAC. The B factory has two campaigns, one of precision measurements to define the charged sector of the Standard Model and Cabibo-Kobayashi-Maskawa (CKM) parameters and the other a highly constrained and redundant set of precision tests of weak interactions in the Standard Model. There is a large discovery potential here; 114 papers were submitted to the Summer International Conference on High-Energy Physics covering the data taken to June 1, 2006. In defining CKM quark mixing, there has been a highly constrained and redundant set of precision measurements.

There is a long-term metric relevant to the definition of the Standard Model: measuring the matter-antimatter asymmetry in the primary modes to a precision of 4%. The relative error is 5.4% for BaBar alone; for the world, it is 3.8%.

The measurement of α is within 7 to 10°, and of γ within 18°. A determination of γ within 5 to 10° is expected in 2008. Evidence of new physics and new phases that can enter into B decay via loop diagrams are being sought. The long-term metric is to measure the time-integrated asymmetry in at least 15 additional modes to an absolute precision of <10%. This goal is expected to be reached with the 1 ab^{-1} of data. In looking at the penguin measurements, they are what would be expected. More channels are showing increased precision. The bottom line is, new sources of CP violation in $b \rightarrow d$ and $s \rightarrow d$ are strongly constrained. New-physics contributions to the $b \rightarrow s$ transitions are much less constrained and are, in fact, well motivated by models explaining large mixing angles in the neutrino sector. However, there is no definitive evidence yet.

The physics reach of the BaBar data extends to charm. Soon we will be honing in on D0 mixing, which is highly suppressed in the Standard Model.

Run 5b was conducted from November 14, 2005, to August 21, 2006. SLAC is now doing safety checks, major upgrades for PEP-II and BaBar, and the Linac Coherent Light Source (LCLS) installation. The machine will be turned back on in January 2007. It will be down from September to November 2007, and then run from December 2007 to September 2008. Run 5 commissioned a new RF system, allowing increased beam currents; dedicated work on an optical magnet lattice for both rings paid off with reduced beta errors around the ring; horizontal tunes closer to half integer; higher beam-beam tune shifts, high specific luminosity, and better stability at high beam current. New records were set in peak luminosity (four times the design) and integrated luminosity in a day (seven times the design). This is well past what the machine was designed to do. Even though it is running at less than peak current, luminosity is improved because of the optics upgrades. Two major vacuum problems developed in the month of December. The first problem was quickly identified and solved in late January. The second problem was thoroughly investigated, replacement parts were manufactured, and it was repaired in late March with the elapsed time of about three months. A series of very successful machine studies throughout the run contributed to improvements in performance and stability of the machine. High-bunch-charge beam-beam effects were studied. Overheating in a cable near the transfer feedback kicker resulted in a small cable fire.

The goal is to increase PEP-II luminosity by 60% by the end of Run 6 by increasing each beam current by 40%, lowering β_y*from 11 to 8.5 mm, increasing the beam-beam parameters by 10%, keeping detector backgrounds at the predicted levels, and improving accelerator reliability. Final machine upgrades going in now will take the peak luminosity from 12 to 20 $\times 10^{33}$ cm^{-2} s^{-1}. The instrumented flux return (IFR) upgrade will be completed, and the expected higher data rates will be prepared for. "Good" is 940 fb^{-1}.

There is an enormous amount of flavor physics still to come from BaBar in its 1 ab^{-1} phase. If no deviation is seen with this much data, the results will serve as major constraints on the flavor structure of the new physics to be seen at the LHC. The upgrade efforts at PEP-II and BaBar are proceeding well and are on schedule.

Robert Plunkett was asked to report on the NuMI/MINOS experimental program.

Neutrino physics with ν_μ beams have become a study of oscillations and the search for and utilization of $\nu_\mu \rightarrow \nu_e$ at the atmospheric Δm scale.

The MINOS Long-Baseline Experiment begins at Fermilab and travels underground to Soudan, Minn. It is used to study the $\nu_\mu \rightarrow \nu_\tau$ oscillation hypothesis, by measuring precisely $|\Delta m^2_{32}|$ and $\sin^2 2\theta_{23}$, searching for $\nu_\mu \rightarrow \nu_e$ oscillations, constraining contributions of exotic phenomena, comparing neutrino-antineutrino oscillations, and looking at atmospheric neutrino oscillations. In MINOS, 120-GeV protons strike the graphite target with a nominal intensity of 2.4×10^{13} protons per pulse with a cycle time of about 2 s and an initial intensity of about 2.5×10^{20} protons per year.

NuMI had an excellent turn on and commissioning and exceeded Fermilab expectations in FY05. The spare target is in position, and work is proceeding well, with a mean of 2.2×10^{13} protons per extraction and a mean beam power of 150 kW.

Both MINOS detectors are tracking calorimeters composed of interleaved planes of steel and scintillator. Their uptimes routinely exceed 95%. They have a 1.5-T toroidal magnetic field and multi-anode photomultiplier tubes.

Timing allows separating events to determine the large-event rate in the near detector. The beam arrives in 10-µs batches. Multiple events are separated by timing and topology. Relative timing greatly simplifies event identification at the far detector. From the near detector distributions, the acceptance is seen to be well reproduced by simulation. In the far detector, the expected rate of contained charge-current (CC) events is about three per day. The far detector triggers on spill time and also has activity triggers for backup. After selection of far-detector events, the spectrum is compared to a default Monte Carlo (MC) and a tuned MC. The near-detector data and beam kinematics are used to predict the far spectrum. Bin-to-bin ratios and direct fitting to simulation are also used; the results are stable with these changes. A best-fit spectrum is calculated from these data.

The allowed region comes out of the fitting procedure, subject to three systematic uncertainties: near/far normalization, hadronic E scale, and neutral-current contamination. The fit is constrained to the physical region $\sin^2(2\theta_{23}) \leq 1$. The allowed region is being decreased, which is consistent with the results from other groups.

Currently, NuMI operates with five booster batches as the main running mode. There is a "slip-stacking" scheme that will allow nine-batch running. The power is being increased to about 400 kW. This is within the booster capability. It is expected that main-injector collimators and some RF changes will be required for high intensities. The proton plan now has realistic shutdown, efficiency, and turn-on curves. Protons on target are expected to go as high as 1.60E+21, producing a significant increase in MINOS sensitivity.

The potential for electron identification is challenging because of detector granularity. The typical electron is eight planes long and four strips wide. The background is high, especially for misidentified neutral current (NC) events. Several algorithms are under study.

The exclusion and discovery potential are presented as a function of mass hierarchy and the CP-violating phase. The mass hierarchy can have a ±30% effect on rate. MINOS can improve significantly on Chooz limit. If nature provides a θ_{13} that is close to the Chooz value and there is a full program of running with the proton plan, MINOS could observe the effect.

In conclusion, the MINOS experiment and NuMI beam have had a successful first running period. Two publications have been accepted, and three are in the pipeline. Operational difficulties with the NuMI beam have been repaired (tritium-mitigation, horn, and target problems). An industrialization effort for spares is under way. MINOS has active short- and long-term physics programs. The short-term program includes improved CC analysis, first near-detector physics, and special topics. The Fermilab proton plan provides a well-defined path forward in proton intensity. The MINOS program has developed a robust scientific culture with strong involvement by all segments of the collaboration. With 32 postdocs and about 40 students, the Laboratory has a commitment to young people in positions of importance.

Nishikawa noted that ANN PID (artificial neural network particle identification) at the far detector is very close to Chooz with a lot of data.

William Molzon was introduced to review the OHEP long-term goal assessment. The long-term goals are to:

1. Measure the properties and interactions of the heaviest known particle (the top quark) in order to understand its particular role in the Standard Model.
2. Measure the matter-antimatter asymmetry in many particle decay modes with high precision.
3. Discover or rule out the Standard Model Higgs particle, thought to be responsible for generating the masses of elementary particles.
4. Determine the pattern of the neutrino masses and the details of their mixing parameters.
5. Confirm the existence of new supersymmetric (SUSY) particles or rule out the minimal-SUSY "Standard Model" of new physics.
6. Directly discover, or rule out, new particles which could explain the cosmological "dark matter."

Each of these goals has an operational definition of success and of minimal effectiveness. Some of the measures of success have been accomplished. There are also short-term milestones for 2008; most of them have been met.

Success for the first goal consists of measuring the top quark mass to ±3 GeV/c^2 and its couplings to other quarks with a precision of about 10% or better. The top-quark mass has been measured to 2.1 GeV/c^2, and the long-term goal has been met. V_{td} has been measured as 0.0074 ± 0.0008 from Δm_d, which is dominated by theory error; the long-term goal is nearly met. V_{ts} has been measured as 0.041 ± 0.003 with inclusive $B \rightarrow X_s\gamma$ mixing; the long-term goal has been met. V_{tb} can be directly measured with single top production; it will require 4 to 8 fb^{-1} at the Tevatron, based on CDF estimates. ATLAS [A Toroidal LHC ApparatuS] and CMS [Compact Muon Spectrometer] estimate a precision of 5% may be achieved, so the long-term goal will likely be met.

Success for the second goal consists of measuring the matter-antimatter asymmetry in the primary ($B \rightarrow J/\psi\ K$) modes to an overall relative precision of 4% and the time-integrated asymmetry in at least 15 additional modes to an absolute precision of <10%. All 2008 milestones have been reached. The combined BaBar/Belle result for sin (2ß) has a precision of 3.8%, meeting a long-term goal. BaBar has recorded 391 fb^{-1} of data and measured asymmetries in nine $b \rightarrow s$ hadronic penguin modes. With 1000 fb^{-1} collected by the end of 2008, these and other modes will be measured. CDF and D0 have measured the CP asymmetry in inclusive di-muon events in $B_s \rightarrow K\pi$, $\psi\varphi$, and $\Lambda_b \rightarrow p\pi$, pK, and the precision is expected to reach 10% in some of these modes by the end of Run 2. With that, the second long-term goal should be achieved.

Success for the third goal consists of measuring the mass of the Standard Model Higgs, if discovered, with a precision of a few percent or better and measuring other properties of the Higgs (e.g., couplings) with several final states. LHC is currently on schedule for first collisions at 0.9 TeV in 2007 and collisions at 14 TeV in 2008. If no problems arise, the 2008 goal will be met. Current estimates from D0 and CDF are that about 3 fb^{-1} are needed to discover a 115-GeV/c^2 Standard Model Higgs. This sensitivity requires the use of new analysis tools that have been developed by the collaborations. The performance of these tools is currently under study. Projections for ATLAS and CMS performance are that the Standard Model Higgs will be found if it exists in the full mass range up to 1 TeV and branching fractions to several final states will be measured.

Success for the fourth goal consists of confirming or refuting the present evidence for additional neutrino species; confirming or ruling out the current picture of atmospheric neutrino oscillations; measuring the atmospheric mass difference Δm^2 to 15% (full width at 90% CL), if confirmed; and measuring a nonzero value for the small neutrino mixing parameter $\sin^2 (2\theta_{13})$ or constraining it to be less than 0.06 (90% CL, ignoring CP and matter effects). NUMI delivered a 0.17 MW average (0.29 MW peak), achieving a 2008 milestone. MINOS reported atmospheric ΔM^2 as $(2.74 + 0.44 - 0.26) \times 10^{-3}$ eV^2, achieving a 2008 milestone. Atmospheric neutrino oscillations were confirmed in KEK-to-Kamioka (K2K) and MINOS, meeting a long-term goal. Projecting the first MINOS result to 7.5×10^{20} delivered protons (1.27×10^{20} in the first year) gives 90% confidence level (CL) full width ~15% statistical. Many systematic errors should decrease with increased statistics, and prospects for achieving the long term goal are good. MiniBooNE has shown a projected sensitivity of 3σ coverage of the putative Liquid Scintillator Neutrino Detector (LSND) signal region with the full neutrino data sample, with results expected late 2006 or early 2007. MiniBooNE is currently running with antineutrinos, and the approved running will give a significantly smaller antineutrino data sample. Meeting the 2008 goal is not assured. Reaching a sensitivity for 95% exclusion or 5σ detection at the LSND signal value would require at least a significant new commitment of running. The Double Chooz reactor neutrino experiment in France is expected to start operation in 2007 with one detector and in 1 or 2 years later with two detectors. It expects to reach a sensitivity to $\sin^2 (2\theta_{13})$ of 0.02 to 0.03 in 3 years of data taking. The long-term goal is expected to be reached. The Daya Bay reactor neutrino experiment could extend the $\sin^2 (2\theta_{13})$ range by a factor of 3 or so over the Double Chooz sensitivity. The NuMI Off-Axis ν_e Appearance (NOvA) experiment could also extend the $\sin^2 (2\theta_{13})$ range on a somewhat longer time scale.

Success for the fifth goal consists of extending supersymmetric quark and/or gluon searches to 2 TeV in a large class of SUSY models and, for masses below 1 TeV, measuring their decays into several channels and determining the masses of the SUSY particles produced in those decays. LHC is currently on schedule for first collisions at 0.9 TeV in 2007 and collisions at 14 TeV in 2008. If no problems arise, the 2008 milestone will be met. Tevatron experiments have extended supersymmetry searches by about 50 GeV/c^2 and expect to extend sensitivity by another 50 GeV/c^2 by the end of the run. The 2008 milestone has been met. Numerous LHC studies show that the long-term goals for supersymmetry searches should be met.

Success for the sixth goal consists of discovering at more than five standard deviations the particle responsible for dark matter or ruling out (at a 95% CL) many current candidates for particle dark matter (e.g., neutralinos) in many SUSY models. Accelerator searches for dark matter candidates (e.g., neutralinos) are addressed in the section of this report on supersymmetry. Discovering or ruling out candidate particles is likely to be achieved during early LHC running. Confirming a particle detected at the LHC as the dominant source of dark matter particles will require significant analysis and, in some scenarios, more detailed information (e.g., from the ILC). CDMS-II [Cryogenic Dark Matter Search] expects to reach a sensitivity of 1 to 5×10^{-44}

cm^2 (depending on mass) within 2 years using their full detector. The 2008 milestone is likely to be met. SUPERCDMS 25 kg could reach a sensitivity of about 10^{-45} in approximately 2012, which would cover the mass range of many current candidates for dark-matter particles and would meet the long term goal for weakly interacting massive particle (WIMP) dark-matter candidates. Either a larger version of SUPERCDMS or one of a number of promising techniques using liquid noble gasses could reach higher sensitivity. If R&D on some of the noble-gas techniques is successful, sensitivity approaching 10^{-46} could be reached on this timescale. The Axion Dark Matter Experiment [ADMX] expects to reach an interesting coupling sensitivity in 2 years over the full mass region in which axions could account for all dark matter. Technical improvements (lower temperature) could push the coupling sensitivity over this range by another factor of 2.

Olinto observed that many areas were not mentioned. Shochet replied that measurable assessment goals were selected in 2003, and those metrics are what must be used. A letter summarizing the assessment must be submitted to and accepted by HEPAP and forwarded to DOE by the end of the calendar year. Byon-Wagner noted that this is a measurement of the management of the facilities.

Wormser stated that, at some point, it should be mentioned that new tools will be needed. Molzon agreed that an adjustment will be needed for that purpose. Shochet added that these are quantitative goals that were set to be achievable by 2014 by the existing facilities.

Perlmutter asked what the context of this exercise was. Byon-Wagner answered that the OMB intended this to be a long-term process. She did not know if this would continue into the future. Some updates to the goals and measures may be necessary.

Ritz noted that these goals do not represent the total long-term plan. Shochet agreed. This set of measures was set up to respond to an OMB request. It shows if DOE is using its funds effectively and making progress. Samios hoped that the committee would not be embarrassed to find something that wasn't planned. Gratta stated that important, unexpected findings should not be lost in the reporting process.

Meyers asked if things were missed because the Committee did not see them or because they are not there. Such a case could be explained. Shochet said that the metrics were selected to avoid such a problem.

Wormser observed that this process only works backwards. There is no way to measure launching new initiatives. Ritz said that the agencies know how this instrument is used. One should write things simply.

A break for lunch was declared at 12:08 p.m.

Thursday, July 6, 2006
Afternoon Session

The meeting was called back into session at 1:15 p.m. with the introduction of **Michael Salamon** to speak about the NASA astrophysics program. The Science Mission directorate is made up of four divisions: Earth Science, Planetary Science, Astrophysics, and Heliophysics. The Astrophysics Division has programs in the Hubble Space Telescope, Navigator, James Webb Space Telescope, Stratospheric Observatory for Infrared Astronomy (SOPHIA), Gamma-Ray Large Space Telescope (GLAST), International Space Science Collaboration, Universe Research, and Beyond Einstein.

Mather and Smoot have won the Nobel Prize, and Bernardis and Lange won the Balza Prize.

In response to the presidential budget request, the House and Senate appropriations subcommittees governing NASA have prepared legislation with differing but nonconflicting earmarks. The Senate version contains an extra $1 billion as emergency funding to offset hurricane Katrina losses and also those following from the loss of the Columbia shuttle. The final conference bill must be passed; continuing resolutions will begin in October 2006. The major differences in the FY06 and FY07 budgets are predominately in the James Webb Space Telescope (plus $70.6 million), the Hubble Space Telescope (plus $118.5 million), the Gamma-Ray Large Space Telescope (GLAST, plus $18.7 million), Navigator (minus $118.5 million), and Beyond Einstein (minus $62.7 million). Beyond Einstein is significantly reduced in the out years, also. The Stratospheric Observatory for Infrared Astronomy (SOPHIA) was canceled and then reinstated.

The Beyond Einstein program calls for the Laser Interferometer Space Antenna (LISA), Constellation-X (Con-X), and other missions. Things are now up in the air when (and whether) these missions are going to fly. Some planning dates go out to 2021. The NRC is to assess the Beyond Einstein program. There was an original 2004 ordering of Beyond Einstein missions and that the two initial missions were delayed by a year by the FY05 presidential budget; the other probes were deferred beyond the budget horizon. The intense focus on dark energy has created programmatic pressure to consider placing JDEM at the top of the Beyond Einstein queue. Funding reductions in the FY07 budget request have placed LISA and Con-X on a low level of technology development only, with a funding wedge for one new Beyond Einstein start in 2009. NASA and DOE have requested the NRC and the Board of Physics and Astronomy to convene a panel to recommend which of the Beyond Einstein missions should fly first. The report is due in September 2007. The subsequent decadal survey in 2010 would prioritize the remaining Beyond Einstein missions, along with the entire astrophysics division mission suite.

The letter to the NRC requests that they assess the five proposed Beyond Einstein missions (Con-X, LISA, JDEM, Inflation Probe, and Black Hole Finder Probe) and recommend which of these five should be developed and launched first, using a funding wedge that is expected to begin in FY09. The criteria for these assessments include (1) the potential scientific impact and (2) the realism of preliminary technology and management plans and of cost estimates. The NRC was also asked to assess the Beyond Einstein missions sufficiently so that they can act as input for any future decisions by NASA or the next astronomy and astrophysics decadal survey on the ordering of the remaining missions. This latter task will assist NASA and its investment strategy for future technology development within the Beyond Einstein program prior to the results of the decadal survey. The language in the request covers the LISA and JDEM partnerships, as well. Cost realism is an issue. Con-X is now estimated to cost $2.5 billion, and the U.S. contribution for LISA is estimated at $1.5 billion. NASA and DOE are jointly funding this NRC assessment, and the first meeting of the committee will be in early November 2006. The final report is due September 8, 2007. The information the NRC will be working with will be less than that which it would have had were the due date later than 2007. Several town hall meetings will be held by the committee to solicit input from the community on the Beyond Einstein Mission priorities.

For JDEM, a call for concept studies has been issued, proposals were due in March 2006, and selections were made in August 2006. Each award provides about $2 million for a 2-year term for concept development in final report preparation. Three proposals were selected out of six submitted:

- SuperNovae/Acceleration Probe-Lensing by Saul Perlmutter,
- Advanced Dark Energy Physics Telescope by Charles Bennett, and
- Dark Energy Space Telescope by Tod Lauer.

Their reports are expected by 2009. Each mission meets the finding of the Dark Energy Task Force: “no single technique can answer the outstanding questions about dark energy; combinations are at least two of these techniques must be used to fully realize the promise of future observations.”

NASA initiated these concept studies to help develop multiple mission concepts and competitive collaborations and to learn what science can be returned for a JDEM cost cap of $600 million (in 2006 dollars plus contingencies but including the launch costs and foreign cooperation). JDEM started out as a Beyond Einstein probe. The concept study reports are due at the beginning of FY09, which is when the Beyond Einstein funding ramp begins. A JDEM opportunity announcement may be issued as early as FY08 with proposals due in FY09. DOE/HEP continues to support the SuperNova/Acceleration Probe (SNAP) and generic dark-energy research at the level of several million dollars.

Staffin noted that DOE does not subscribe to a $600 million cap. If more is needed, more is needed. Seiden asked if there will be time for selection among the techniques. Salamon replied that there would be at least six months for proposal preparation.

A photograph of two galaxies colliding showed evidence of the existence of dark matter. Also, computational modeling of black holes has provided the foundation to explore the universe in an entirely new way, through the detection of gravitational waves.

Measurements of very distant galaxies have shown a great deal of galaxy formation; dwarf galaxies may be re-ionization sources. The Hubble Space Telescope has shown 16 candidate planets at the center of the Milky Way Galaxy. Extrapolation to the entire galaxy indicates the existence of about 6 billion Jupiter-sized planets in the Milky Way. A modified Boeing 747 will enable infrared telescopy at the top of the atmosphere.

Bagger asked how much WMAP [the Wilkinson Microwave Anisotropy Probe] cost. Salamon replied, $670 million. Bagger was upset that dark-energy research had been dumped into an astrophysics program. Salamon explained that that was why the NRC study was requested, with particle physicists participating and town hall meetings being held at physics meetings. The overlap is so strong, NASA will be happy no matter how the priority selection process turns out.

Shochet asked what the significance was of the NRC committee selection being led by the Space Studies Board with advice from the Board on Physics and Astronomy. Salamon replied that, to the best of his knowledge, they will be co-equals.

Carithers asked if JDEM would be the average of the three concept studies. Salamon said that there is no answer to that. A decision has to be made even though JDEM is not yet well-defined. All three will likely be presented to the NRC.

Cahn noted that projects with costs of $2 billion were competing against those with $600 million caps and commented that that could make the $600 million projects unrealistic and at a disadvantage. Salamon responded that NASA was not concerned with absolute costs. If the NRC decides that a $1 billion mission is feasible, then it, too, will be considered to be realistic. Cahn followed up that no one is being asked to present a $1 billion version of JDEM. Salamon said that they can consider such a program. The $600 million cap is irrelevant.

Wormser asked to what extent DOE was bound by the scheduling process. Staffin said that he would turn it back to the HEPAP roadmap subpanel. Shochet pointed out that the NRC is only being asked what should be the first Beyond Einstein mission. Staffin added that the NRC's recommendations are not advisory to DOE about what the first particle physics experiment should be.

Bernard Sadoulet was asked to report on the site-independent (SI) study, one of several studies that have been set up to look at DUSEL. This study was to organize a dialog inside the community; discover whether there is a compelling scientific justification for such a laboratory; and, if so, specify the infrastructure requirements.

Three questions require a deep laboratory:

- What is the universe made of?
- How deeply in the Earth does life extend?
- How does rock-mass strength depend on length and time scales?

Physics needs neutron shielding and the rejection of cosmogenic activity. Biology needs an aseptic environment at depth, the study of microbes in situ, and a platform for deep drilling. Earth science/engineering needs to investigate the scale/stress dependence of rock properties, get closer to the conditions of earthquakes, and complement other facilities. For each of these pursuits, the best strategy is to go deep underground. There is an exciting potential for cross-disciplinary synergies in pushing the rock-mechanics envelope, neutrino tomography of the earth, sensors, low radioactivity, education, etc. These efforts are relevant to underground construction, water resources, environmental stewardship, risk prevention and safety, and national security, not to mention training the next generation of scientists and engineers.

There does seem to be a need for new underground facilities, as seen by the chronic oversubscription of current facilities, the increase in community size, life cycle of experiments, and the need for several parallel experiments. However, budgetary constraints do not equal the sum of all dreams. The largest underground laboratory in the world is at Gran Sasso at 3000 m. U.S. scientists and engineers have managed to play a pioneering role without a dedicated U.S. deep underground laboratory However, there is no substitute for a premier national facility with unique characteristics for the rapid exploration of new ideas and unexpected phenomena, full exploitation of existing national assets (such as accelerators), and the maximization of the program's impact on our society. The United States is one of the only G8 nations without a national facility.

Recommendations of the Study Group are

1. Strong support is needed for deep underground science.
2. A cross-agency Deep Science Initiative is needed to meet the multidisciplinary needs. Numerous agencies should start down this path together, and the initiative should be coordinated with other national initiatives and take full advantage of international collaboration opportunities.
3. The U.S. should complement the nation's existing assets with a flagship world-class underground laboratory providing access to very great depth (6000 MWe), and ample facilities at intermediate depths (3000 MWe) currently not available in the U.S. DUSEL should be designed to allow evolution and expansion over the next 30 to 50 years. Because of this long lifetime, the initial investment must be balanced with the operating costs.

The initial program should take place in four phases:

- Before the excavation
- During excavation
- First suite of experiments
- Design potential extensions in the first 10 years

A 3500-m-deep facility with 25,000 m^3 (expandable to an additional 7000 m^3) could do a wide variety of physics, biology, and engineering. This proposal is for more than just DUSEL. The Major Research Equipment and Facilities Construction (MREFC) line covers the facility and the NSF contribution to the first suite of experiments. The strategy is to involve geology, biology, and engineering to secure a place in the MRE queue, initially bringing new resources to the HEP-nuclear science community. One needs to be careful about the long-term costs. The cost of operation will eventually be borne in part by the physics community. It will have an impact on future projects. Although multidisciplinary, the MRE would be seen as impacting other NSF initiatives in physics, but on a different scale from the ILC.

The recommendations of the Study Group explicitly include the full use of existing facilities and the full use of international collaborations [e.g., SNOLAB (Sudbury Neutrino Observatory Laboratory), Gran Sasso, and Kamioka]. The simple expansion of SNOLAB would be limited by the need to cooperate with INCO, Ltd.; operate projects at depths beyond what was needed; not be suitable for a multidisciplinary enterprise; and strongly reduce the benefits to the United States. A shallow site plus SNOLAB plus a subsequent deepening would necessitate performing frontier experiments at lower depths with shielding, all of which would be risky and only a temporary stop-gap; that lack of space may inhibit the rapid exploration of new ideas; a subsequent extension is not well adapted to MREFC structure; and a sequential approach would delay a frontier facility.

The site-independent study (S1) is done. The site preselection (S2) is being done. The site selection (S3) announcement was on Sept. 30, 2006; and proposals (open to any site) are due Jan. 9, 2007. The selection of one site will be announced in early 2007, and a draft technical report feeding into the MREFC process will be prepared by December 2007. The MREFC process has been started with contacts with other directorates within NSF. The potential first decision could be made in December 2007 with the earliest start date in FY10. Other agencies are being involved through a common working group with DOE. There will be a common R&D initiative. Multidisciplinary discussions are yet to be started outside NSF.

The United States needs an increase in funding to become a worldwide leader in deep science. A cross-agency initiative should be instituted with appropriate coordination mechanisms, calling for optimal use of existing facilities and coordination with other national initiatives. DUSEL would bring new resources by tapping MREFC. It has a multidisciplinary perspective aligned with many of NSF's interests. MREFC costs would not initially be borne by the physics community, but that community should be aware of large operating costs.

Hewett asked about interference with the LSST. Kotcher said that the two programs would compete at some level, but how they would compete is uncertain.

Cahn asked if Gran Sasso was incapable of doing these experiments. Sadoulet responded that the people working in dark matter say that Gran Sasso is not deep enough. There are things to be learned from Gran Sasso. There is a feeling that doing everything at once is not proper.

Ritz asked who the external reviewers were. Sadoulet replied that they were physicists, astrophysicists, biologists, and geomechanicists. They are listed in the back of the report. Ritz asked what the controversial issues were. Sadoulet said that the main issue is that the NSF chose not to look at other sites. They were worried about costs and risks. One technical possibility was not studied.

Lykken asked about SNOLAB. Sadoulet replied that it depends on the size of the dark-matter experiment. If one pushes several technologies or directionalities, one needs a bigger facility than SNOLAB or will have to delay one's progress.

Cahn asked if this program was of significant-enough interest to other disciplines that they would contribute to the operating costs. Sadoulet answered, yes, eventually.

Wormser asked if a particular distance between this site and an accelerator was being considered. Sadoulet said that SNOLAB was too close to an accelerator. There is a broad maximum of 2000 km. Homestead and Henderson are both within this magic distance.

Carithers noted that scientists seem more interested in existing mines. Sadoulet replied that that is changing rapidly as people recognize what they can do. The biologists are becoming convinced that this is a good way to study evolution and other forms of biology. The geoscientists are beginning to understand that it is much better to stay in a place 10 to 20 years instead of being kicked out by the miners after 5 to 10 years.

A break was declared at 3:04 p.m. the meeting was reconvened at 3:29 p.m. to hear **Abraham Seiden** present the P5 Roadmap report.

P5 is charged to maintain the U.S. Particle Physics Roadmap for major projects for the next 5 to 10 years along with ongoing projects. It has used input from the EPP2010 report, the Neutrino Scientific Assessment Group (NuSAG) report, and the Dark Energy Task Force (DETF) report. It also used the Office of Science 5-year Budget Plan: FY2007-FY2011, which calls for expenditures in high-energy physics of $775, $785, $810, $890, $975 million in those fiscal years, respectively. In addition, the closing of PEP-II and the Tevatron should allow funds to be reprogrammed to exciting new projects. P5 assumed that budgets will grow by 3% per year after FY11. An alternative doubling of the HEP budget over 10 years would change the numbers to $775, $829, $877, $950, and $1016 million. These numbers are $25-50 million per year more than in the other budget and were used in a contingency plan.

The major science questions to be addressed are:

- The question of mass: How do elementary particles acquire their mass? How is the electroweak symmetry broken? Does the Higgs boson exist?
- The question of undiscovered principles of nature: Are there new quantum dimensions corresponding to Supersymmetry? Are there hidden additional dimensions of space and time? Are there new forces of nature?
- The question of the dark universe: What is the dark matter in the universe? What is the nature of dark energy?
- The question of unification: Can all known fundamental forces, including gravity, be derived from a universal interaction ?
- The question of flavor: Why are there three families of matter? Why are the neutrino masses so small? What is the origin of CP violation?

These questions would be addressed by

1. The energy frontier projects: LHC-ILC;
2. A program to understand the nature of dark matter, which has been manifest to date only through astrophysical measurements (e.g., strong lensing);
3. A program to understand the nature of dark energy, which accelerates the expansion of the universe;
4. Neutrino science investigations using neutrinoless double-beta decay, reactor and accelerator neutrino oscillation experiments, and neutrinos from sources in space; and
5. Precision measurements involving charged leptons or quarks.

During the past 50 years, particle physics has achieved a remarkable understanding of the constituents of matter and the underlying dynamics describing the interactions between particles. This effort has resulted in the Standard Model, which is based on symmetries that we know are broken as revealed by the different behavior of the weak and electromagnetic forces. To understand how the symmetries of the Standard Model are broken we have to explore energy regimes that are beyond our current experimental reach.

The simplest picture for the breaking of the Standard Model symmetries involves a number of scalar fields. In this picture, the lowest energy state of the universe has all of space-time filled by a field, called the Higgs field, which through interactions with the other particles generates their mass and mixings. Because particle properties (for example the electron mass) appear to be the same everywhere in the universe, this field must exist everywhere, which is what we think of as the vacuum. Extensions to the simplest picture, for example in the case of additional symmetries (e.g., supersymmetry), can result in a number of scalar fields contributing to the vacuum. Fortunately, in all of these pictures, the Higgs field gives rise to new scalar particles (called Higgs bosons or Higgs particles) that can be produced in the laboratory. These particles have properties that reflect the mechanism by which the vacuum is generated. Using indirect measurements to date, the simplest Higgs picture would have detectable Higgs bosons at a mass near 100 GeV, while theory predicts that it is not heavier than roughly 1 TeV. From general arguments, new physics associated with the Higgs mechanism, such as supersymmetry or extra dimensions of space, should set in at masses not larger than 1 TeV if they are part of this mechanism.

With 5 fb^{-1} of data, the LHC supersymmetry reach is likely to be > 1.5 TeV, and the Standard Model Higgs boson could be seen over the full mass range. The LHC is a proton-proton collider with a center-of-mass energy of 14 TeV. Such a large collision energy is required to reach energies in the TeV range for the collisions of the elementary building blocks within the proton. In parallel, high-energy physicists throughout the world have been constructing components for two large general-purpose detectors, ATLAS and CMS. Given the high collision energy, the LHC will be an exploratory machine into the TeV energy range. It will definitively answer the question of the existence of the Higgs particle and of TeV-scale supersymmetry.

The ILC is a proposed electron-positron linear collider, designed for physics in concert with the LHC. It would consist of two roughly 20-km linear accelerators that would collide electrons and positrons at their intersection with initially tunable collision energies up 0.5 TeV, upgradeable to 1.0 TeV. Because the electron is a fundamental particle, the full collision energy of the ILC would be available to study new phenomena, and the machine properties would produce a complete knowledge of the quantum state of the collision. The ILC would identify the new particles observed at the LHC and allow the discovery of the underlying theory that gives rise to them.

The ILC would be designed, funded, managed, and operated as a fully international scientific project. The baseline configuration for the particle collider has been agreed upon and an international reference design is being developed with sample sites and cost estimates for Europe, North America, and Asia. An ILC Reference Design Report is to be released in early 2007 and a Technical Design Report in 2009 to 2010. This time scale matches well the expected date for first major physics results from the LHC. The linear collider R&D program is supported regionally by the major high-energy physics laboratories throughout the world.

The physics questions that the ILC will address require detector capabilities that are beyond the performance of current detectors. To achieve these advances a well-orchestrated detector R&D program is needed. Such a program has been realized in Europe where it is addressing some of the R&D areas that need attention. In the U.S. such a coherent program, including universities and laboratories and centrally managed, is only partially in place. The U.S. efforts on ILC detector R&D are lagging both in terms of funding and manpower. Given that the U.S. wants to play a leading role in the ILC, this problem needs to be addressed and a well-defined U.S. ILC detector program with sufficient funding should be realized.

The nature and origin of dark matter is one of the most important questions of science today. A large number of astrophysical observations provide strong evidence that roughly 23% of the energy density of the universe consists of dark matter, whose presence is inferred only from its gravitational influences. Furthermore, observations of the small-scale structure of the universe demand that dark matter particles are nonrelativistic; this is referred to as “cold” dark matter.

The Standard Model provides no viable candidate for cold dark matter. Candidates for dark-matter particles are:

- Axions: these particles were postulated to solve the problem of the absence of CP-violation in the strong interactions. They would have very small interaction cross-sections for the strong and weak interactions. Their masses should be extremely small, in the range 10^{-6} to 10^{-3} eV.
- WIMPs: these “weakly-interacting massive particles” should have masses on the order of the electroweak scale and would interact weakly, similar to the interactions expected for a heavy neutrino. WIMP candidates arise in models of electroweak symmetry breaking.

There are three avenues for observing dark matter experimentally: direct detection in scattering or interaction experiments, indirect detection of the products of cosmic interactions, and high-energy colliders that produce these particles directly in the collisions of hadrons or electrons.

Experiments searching for axionic cold dark matter are important since they have no counterparts in accelerator-based experiments and are likely to be the only way axions will be observed (if they exist). The ADMX experiment offers essentially unique capabilities, and any signal would be a triumph not only for revealing the nature of dark matter but also for understanding the strong CP problem.

The CDMS detector installed in the Soudan Mine has produced limits on cross-sections for WIMP detection between about 10^{-42} cm^2/nucleon and 10^{-43} cm^2/nucleon, depending on the mass of the WIMP. The goal of the next phase of the experiment is a sensitivity increase of about a factor of 100. Newer approaches use detectors based on large volumes of liquefied noble gases. R&D for such detectors should be strongly supported. There is a reasonable chance of seeing dark matter in one of these experiments in the next 10 years. The LHC will start honing in on this problem. With the ILC, one really narrows in on dark matter and gets the precise mass of the particle.

During the past several years, observations of distant supernovae, galaxies and clusters of galaxies, and the cosmic microwave background have provided strong evidence that the cosmic expansion of our universe is accelerating. The data are consistent with a standard cosmological paradigm augmented by the postulate that 70% of the universe is composed of a mysterious "dark energy" that drives the acceleration.

The dark energy is described by an equation of state that is different from all the other components of the universe. The goals of a dark energy observational program may be reached through measurement of the expansion history of the universe and through measurement of the growth rate of structures in the universe. All of these measurements of dark energy properties can be expressed in terms of the equation of state at different redshifts. If the expansion is due instead to a failure of general relativity, this could be revealed by finding discrepancies between the equation of state inferred from different types of data.

The proposed observational program focuses on four techniques: baryon acoustic oscillations, galaxy cluster surveys, supernova surveys, and weak-lensing surveys.

The most incisive future measurements of dark energy will employ a number of these techniques whose varying strengths and sensitivities will provide the greatest opportunity to establish the history of dark energy and make the comparisons that will test for alternative explanations. The Dark Energy Task Force has broken down the techniques into four stages: Stage I represents projects completed; Stage II is ongoing projects; Stage III are near-term, medium-cost projects; and Stage IV includes projects that are more costly. Seiden's personal assessment of the expected errors for the different stages is 4% for Stage-III experiments and 1 to 2% for Stage-IV experiments. Staffin asked what was needed. Seiden replied: 1% everywhere if it can be gotten. Staffin asked if it was like proton decay. Seiden replied, no, this process exists. Salamon asked if spending $X million to improve the values by a factor of Y would be justified and why. Cahn said that one has to ask, "How important is this topic?" How far does one have to go, and how much of one's resources does it warrant? There is not a figure of merit. The figure of merit is a poor way to go. One needs to find more data. Seiden said that the linear function from the Dark Energy Task Force may be wrong. One has to question one's current assumptions. Shochet said that what is needed to be known is the probability that an experiment would see a 3-sigma discrepancy.

The U.S. particle physics community has played a leading role in three major dark-energy initiatives: the Dark Energy Survey (DES), SNAP, and LSST.

The DES project is land based and proposes to develop a new 520-megapixel wide-field camera. Photometric redshifts up to $z = 1.1$ should be obtained. The program plans to use all four observational techniques. The observations could start in 2009, and a 5-year observational program is being planned.

The SNAP program is a joint DOE-NASA effort and a proposed part of the NASA-DOE JDEM space-based initiative. It is a natural follow up to the pioneering Supernova Cosmology Project that provided one of the initial evidences for an accelerating universe. SNAP would focus on the redshifts and luminosities of supernovae and observations of weak gravitational lensing. If JDEM is funded, it could begin construction in FY09 with a launch as early as 2013.

LSST would be funded jointly by NSF and DOE with some additional private funds. LSST is a ground-based effort that would use a newly constructed 8.4 m telescope in Chile. It would study dark energy through baryon oscillations, supernovae, and weak lensing techniques. The expected first light is in 2013 with the first science observations in 2014.

Three types of experiments have been proposed to address the most pressing questions regarding neutrinos:

1. Reactor neutrino experiments seek to observe the disappearance of low-energy electron antineutrinos from a reactor on their way to detectors 1 km away. They are uniquely sensitive to $\sin^2 2\theta_{13}$.
2. Accelerator neutrino experiments use the oscillation signals over longer baselines. They are sensitive not only to the θ_{13} mixing angle, but also to the atmospheric mixing angle θ_{23}, to whether the neutrino mass spectrum is normal or inverted, and to whether neutrino oscillation violates CP.
3. Neutrinoless double-beta decay experiments would establish whether or not neutrinos are their own antiparticles.

Planned nuclear-reactor neutrino experiments are expected to be sensitive to the probability of disappearance down to about the 1% level. The sensitivity of reactor experiments is typically limited by systematic effects. The current limit is < 0.12, established by the Chooz experiment in France, which uses a single detector. All new planned experiments include two or more detectors, placed near and far from the reactors. By taking ratios of event counts in the near and far detectors, the systematic uncertainties are substantially reduced. The upgraded Chooz experiment (Double Chooz) will be the first new experiment to come on line. Double Chooz will reach a sensitivity of 0.02-0.03 with three years of running and both detectors.

The Daya Bay project is a collaboration of Chinese and U.S. physicists. The reactor complex consists of two reactors at the Daya Bay site and two or more at the nearby Ling Ao site. Daya Bay's goal is to reach a sensitivity of 0.01 in 3 years of running.

The NuMI Off-Axis ν_e Appearance (NOνA) Experiment is a long-baseline experiment whose primary science objective is the use of $\nu_\mu \rightarrow \nu_e$ oscillations to answer the neutrino mass hierarchy question: Is the neutrino mass spectrum normal (i.e., quark-like) or inverted? NOνA leverages the existing NuMI facility infrastructure at Fermilab. Its long baseline (810 km), large energy, and capability of running both neutrino and antineutrino beams give NOνA unique experimental access to matter effects and, hence, the mass hierarchy.

Neutrinoless double-beta decay is currently the only feasible way to determine whether neutrinos are Majorana particles (that is, they are their own antiparticles). The rate is proportional to the square of the effective neutrino mass, which involves the neutrino masses and mixing parameters. For Majorana neutrinos, an inverted hierarchy, and no light sterile neutrinos, m_{eff} is at least 0.01 eV. NuSAG has identified this value as a worthwhile, if challenging, goal and has selected the Cryogenic Underground Observatory for Rare Events (CUORE), Enriched Xenon Observatory (EXO), and Majorana as the highest funding priorities.

The NSF is considering the creation of DUSEL. Construction might start in 2010 and contain experiments for large-scale dark matter direct detection, a large-scale neutrinoless double-beta decay probe, and an experiment on solar neutrinos or on nuclear reaction rates under very low background conditions. It would also encompass R&D on a megaton-scale proton-decay and neutrino detector.

Physicists have had tremendous success in predicting values with the Standard Model. However, several measurements (g – 2 of the muon and B hadronic penguin decays) show discrepancies at the 1- to 2.5-σ level. These discrepancies may be caused by measurement or calculational uncertainties or by contributions from beyond the Standard Model.

P5 articulated a number of planning guidelines:

- The LHC program is the most important near-term project given its broad science agenda and potential for discovery.
- The highest priority for investments toward the future is the ILC based on its potential for breakthrough science.
- Investments in a phased program to study dark matter, dark energy, and neutrino interactions are essential for answering some of the most interesting science questions. A phased program will allow time to understand the evolving physics and to develop additional techniques for making the key measurements.

- New investments should be split about 60% toward the ILC and 40% toward the new projects in dark matter, dark energy, and neutrinos through 2012.
- Recommendations for construction starts on the longer-term elements of the Roadmap should be made toward the end of this decade by a new P5 panel.

The P5 recommendations for major construction, R&D, and reviews are

1. The highest priority group goes to the full range of activities for the LHC program and the R&D for the ILC.
2. The second-highest-priority group includes the near-term program in dark matter and dark energy, as well as measurement of the third neutrino mixing angle, including DES, the 25-kg CDMS experiment, and the Daya Bay reactor experiment. Also in this group is support for the LSST and SNAP over a 2- to 3-year time frame. DOE should work with NASA to ensure that a dark energy space mission be carried out and that the three potential approaches to the mission be properly evaluated. R&D funding should be provided for DUSEL, along with support by NSF and DOE for R&D for both a large dark-matter and neutrinoless double-beta decay experiments.
3. The next priority is the construction of the NOνA experiment at Fermilab along with a program of modest machine improvements.
4. The final priority is the construction of the Muon g – 2 Experiment at Brookhaven National Laboratory (BNL).

The first three priority groupings can be carried out in the base budget plan. However, the ILC R&D ramp-up profile and the NOνA construction schedule must be slowed with respect to the most aggressive proposals if the costs are to be matched to the assumed annual budgets.

The alternative budget that would double support over a decade would allow (1) added support for the Stage-IV dark-energy experiments so that preparatory work would be completed in a more timely way, (2) pursuit of other important areas in the first priority, and (3) more-vigorous pursuit of ILC R&D. In this scenario, the Muon g – 2 Experiment could be considered for construction.

A recommended review by P5 toward the end of this decade would allow a significant number of these to move forward to construction. The review should take into account new physics results (especially those from the LHC), results on R&D for new projects, budget and cost projections at the time, and the status of interagency agreements and MREFC plans. The review would examine

- The ILC;
- The LHC upgrades;
- DUSEL and the large experiments to search for dark matter and neutrinoless double-beta decay;
- The Stage-IV dark-energy experiments, a large survey telescope, and a dark energy space mission; and
- An evaluation of the status of flavor physics and the importance of further experiments across a number of possibilities, such as the muon g – 2, μ-to-e conversion, a very-high-luminosity B experiment, and rare K decays.

A separate review by P5 will be required to look at the best directions for further experiments in neutrino physics. That review might also consider an ambitious proton-decay experiment. Both of these are candidates as DUSEL experiments. The physics results over the next 5 to 10 years will determine the best date and best set of areas to look at in such a review.

The Roadmap was expressed as a graphic schedule of periods of R&D, construction, operation, decision points, and results for the various projects proposed to address the energy frontier, dark matter, dark energy, neutrinos, flavor physics, astrophysics, and DUSEL.

Baltay pointed out that the SNAP schedule is lagging a year behind NASA's plan. Seiden pointed out that it was a 2-year study and that some things may be ready to go before others. The

Subpanel wants to make sure that everything gets looked at. SNAP may start earlier than is stated here.

Bagger said that this is what P5 was set up to do. It is wonderful to have this roadmap.

Lykken asked if the ILC number goes up in FY07 and remains flat. Seiden replied, no, it will probably ramp up. Lykken asked about the detector R&D. Seiden answered that he would not be surprised if it were about $5 million and ramping up.

Cahn pointed out that some projects may compete for MREFC in a given year. Seiden replied, yes; both should be funded by 2010. The projects are not ranked. The MREFC is not an HEP competition but an all-science competition.

Samios said that the section on why budgets should be increased should be expanded.

Wormser asked what the construction funding for ILC was. Seiden replied that there is significant ILC money in the budget but not enough for building the ILC. It is not known where it will come from. That is a future question for P5. Shochet said that, if the ILC is constructed, everything will have to be looked at again. Seiden added that it is not known how JDEM will play out. It is hard to know the answers at this time. There is a two-year transition where construction money may be needed for several projects. The Subpanel tried not to say “go ahead” to anything beyond 2012. Maybe all of these things will happen; maybe none.

Staffin asked if the Roadmap tells how to deal with a $15 million cut. Seiden replied that the report gives priorities. Priority 3 may survive small cuts but not large cuts. If Priority 3 has to be slower, it has to be slower.

Roberts noted that, if there is a doubling of funding, there may be money for the g – 2 collaboration but not otherwise. Seiden agreed. Roberts went on: most of the present discrepancies are 3 sigma. It has been 2001 since data were taken. In 2004, the final report was presented. The collaboration requires special skills and components that will evaporate quickly. That needs to be understood. P5 and HEPAP must realize all the information that is possible before the ILC. To mortgage everything to ILC means that other short-term opportunities will not be realized. Seiden did not deny the interest in these experiments; g – 2 and KAON should have remained running. The Subpanel did the best job it could with the numbers it had. These things have to be reviewed again after the LHC experiment, but that does not help g – 2 in the short term.

Shochet called for a vote on accepting the P5 report, given a change in the section on budgets that was commented on. Brau suggested that it would be good to get feedback from the overall community after the report is issued; perhaps HEPAP could receive rather than accept the report. Shochet replied that, if the Panel does that, that response will be perceived as negative. P5 is a living subpanel, and its documents are living documents, also.

Cahn noted that, when this document was received a couple of weeks previously, the recipients were not allowed to share it with their colleagues and asked why. Shochet explained that it is not a public document until accepted. HEPAP has to have the opportunity to provide feedback to the Subpanel.

Shochet called for a vote. There were 18 for and none against; none abstained.

Samios suggested that a summary cover letter would be useful. Ritz added that feedback on why this roadmap is helpful and how it could be improved would be helpful, also.

Staffin said that the Office considers this exercise an experiment and he thanked P5 for its hard work.

Ruchti said that the NSF will take this information seriously, and he thanked P5, also.

Perlmutter asked if the document was releasable. Shochet said that the requisite changes will be made very soon and that the document would then be posted on the HEPAP website.

Homer Neal joined the meeting by conference call to speak on the status of the Subpanel on the University Grant Program. The Subpanel was to study what the goals, objectives, and scope of the University Grant Program should be, appraise the scientific and technical quality of the work being supported by the University Program, assess the impact of the Program, determine if it has the correct number and distribution of university researchers at all levels to meet program objectives, determine if it has the resources to carry out its scope, decide if the right model of university funding is being used, and comment on management of the program and its improvement and on broader impacts.

What needs to be done is to collect data, interpret those data, pose solutions, and write a draft report for HEPAP review by Jan. 22, 2007, and a final report a month later. “Four-pagers” were assigned to Subpanel members on several topics, even before the Subpanel met. These documents were posted to a website. They provided basic information for the first meeting and were a helpful activity. Specific Subpanel members were assigned to overseeing the writing group, the university model, program administration, data collection, and findings and recommendations. They are now collecting data by conducting surveys and other means.

The Subpanel met in September to review agency budget profiles, the human resource situation, LHC and neutrino physics, nonaccelerator physics, the ILC, computing, and the four-pagers. An October meeting had presentations and discussion on education and outreach, high-energy theory, proposal-evaluation processes, funding-decision statistics, updates on Subcommittee deliberations, and data collection. Community input will be gathered at an American Physical Society Division of Particles and Fields (DPF) town meeting, meetings at Fermilab and SLAC with town meeting components, a website, a letter distributed via DPF, and PI surveys.

In summary, the Subpanel has been launched, individual tasks have been assigned, basic presentations have been received, and a data-collection strategy has been developed. The challenge now is to receive broader community input, to digest the data, and to develop recommendations. The Subpanel is on target for study completion by mid-January.

Ritz asked what data they were collecting. Neal answered, on the issues of infrastructure at universities (machine shops, etc.), faculty-member plans for what they will be working on, and drying up of budgets for teachers of theory and for teaching assistantships in theory.

The meeting was adjourned for the day at 5:53 p.m.

Friday, July 7, 2006
Morning Session

Chairman Shochet called the meeting to order at 8:30 a.m. and introduced **Guy Wormser** to present the CERN Council Planning Group’s priorities. The CERN Strategy Group has published a webpage (cern.ch/council-strategygroup) as an interface with the community; held an open symposium in Orsay to collect the views of the community; held a workshop in Zeuthen/Berlin to draft a strategy document; and held a Council meeting in Lisbon to attain unanimous approval of the Draft Strategy Document, which is posted on the webpage. The employment figures in the ECFA (European Committee for Future Accelerators) survey were updated to 4022 full-time equivalent (FTE) Ph.D. researchers and 1807 FTE graduate students in experimental elementary particle physics.

The European Strategy adopted by the Council in Lisbon in July has a Preamble, presents general issues, and prioritizes scientific activities. The Preamble stresses that “European particle physics should thoroughly exploit its current exciting and diverse research programme. It should position itself to stand ready to address the challenges that will emerge from exploration of the new frontier, and it should participate fully in an increasingly global adventure.” A general issue addressed was that Europe should maintain and strengthen its central position in particle physics. A well-coordinated strategy in Europe should be defined and continually updated by the CERN Council.

The first three scientific priorities are to

- Fully exploit the physics potential of the LHC. Resources for completion of the initial program have to be secured so machine and experiments can operate optimally at their design performance. To this end, R&D for the machine and detectors has to be vigorously pursued now and centrally organized toward a luminosity upgrade by around 2015.
- Intensify a coordinated program to develop the Compact Linear Collider (CLIC) technology and high-performance magnets for future accelerators and to play a significant role in the study and development of a high-intensity neutrino facility. CERN needs to be in a position to have a project for the long term.
- Complement the results of the LHC with measurements at a linear collider. There should be a strong well-coordinated European activity, including CERN, through the Global Design Effort, for its design and technical preparation toward the construction decision to be ready for a new assessment by the Council around 2010. Europe should contribute to this program in a strong way. The new assessment referred to will be on physics.

Shochet asked if this was similar to what P5 called for. Wormser said that it was in the same spirit. Samios asked if the conflict between CLIC and the ILC was avoided by this. Wormser replied, yes and no. Staffin noted that, at the Funding Agencies for the Linear Collider (FALC) meeting, the 2010 assessment would look at the physics, assessing whether 500 GEV is sufficient.

Scientific Priority 4 calls for an assessment of neutrino research in 2012. EPP2010 called for a similar activity but placed a slightly lower priority on it.

Priority 5 calls for a detailed strategy in the area of overlap between particle and astroparticle physics; the Council will seek to work with the Astroparticle Physics European Coordination (ApPEC) to develop a coordinated strategy in these areas of mutual interest.

Priority 6 deals with flavor physics. Such research should be led by national or regional collaborations.

A summary of the European and U.S. strategies shows some parallels, some differences in priorities, and some differences in objectives. Both strategies highlight the pursuit of the LHC, ILC, neutrino physics, astrophysics and cosmology, and flavor physics. The European Strategy also includes accelerator R&D.

Priority 7 deals with the interface between particle and nuclear physics. Here the Council will seek to work with the Nuclear Physics European Collaboration Committee (NuPECC) in areas of mutual interest, and maintain the capability to perform fixed-target experiments at CERN.

Priority 8 stresses the need to support theoretical developments and theoretical calculations.

The Strategy had a few statements about organizational issues. There is a fundamental need for an ongoing process to define and update the European Strategy for particle physics; Council, under Article II-2(b) of the CERN Convention, will assume this responsibility, acting as a council for European particle physics, holding a special session at least once each year for this purpose. Council will define and update the Strategy based on proposals and observations from a dedicated scientific body that it will establish for this purpose. The mandate and composition will probably be set at a meeting during the week following the current meeting. The Council was called upon to prepare a framework for Europe to engage with the other regions of the world with the goal of optimizing particle physics output through the best shared use of resources while maintaining European capabilities. This means that the Council will be the interface between Europe and the rest of the world.

Baltay asked if there were a president. Wormser said, yes; the President is elected by the CERN Council members. The President represents Europe on FALC. Dragt asked if a decision by CERN to participate in the ILC would have to be approved through the CERN Council. Wormser stated that it will not be trivial to unify the European states. He did not know what the result would be, bilateral agreements or unified consent.

Another organizational issue was the relationship between particle physics and the European Union. The European Union establishes in a broad sense the European Research Area, with

European particle physics having its own established structures and organizations; there is a need to strengthen this relationship for communicating issues related to the Strategy.

A final organizational issue dealt with the participation by nonmember states. That issue was left to the Council for future discussion and decision.

Several other complementary issues were considered by the Council: the need for a European particle physics communication and education strategy, a technology transfer forum, and future engagement with industry.

This Strategy realizes the vision set out in the CERN Convention to promote and coordinate the research activities of the CERN Laboratory and of the national particle physics laboratories and institutes. With it, Europe stands better prepared to engage with the other regions in the world in the increasingly globalized research environment.

The next steps are to

- Prepare a budget request to achieve the priorities set by the Strategy,
- Create a permanent Strategy group,
- Hold a CERN Council meeting at the research-minister level in early 2007, and
- Receive the EU ESFRI (European Strategy Forum on Research Infrastructure) roadmap to be issued the week following this meeting.

Bortoletto asked if ESFRI speaks independently. Wormser replied that it has a representative on the Council and contributes to the Strategy. What CERN will put on the table in 2008 to 2010 is noted in its budget plan, reflecting the priorities of the Strategy. It has also laid out plans for 2011 to 2016.

The existence of a process in Europe to define a common strategy is a major step forward. Its contents are remarkably similar to those of EPP2010. The community needs to build on the similarities rather than dwell on the differences. It is strongly supportive of the ILC. The CERN Council will have to learn how to exercise well its dual role. The Council forms a powerful basis to strengthen international collaboration.

Ritz noted that the Council is proposing a method for international planning and asked if there was a timetable and how the United States could cooperate. Wormser answered that right now the Strategy has to be developed and that will take a few months. The United States could indicate its interest.

Henry Sobel was asked to summarize the activities of the Dark Matter Scientific Assessment Group (DMSAG), which was asked to review the dark-matter program and to find out

- The most promising experimental approaches;
- The relative advantages and disadvantages in terms of stage of development, time to implementation, ultimate sensitivity, scalability, and overburden requirements;
- The optimum sensitivity strategy.

It was also asked to look at the present state of the worldwide dark matter program, other approaches to understanding dark matter, implications of astronomical observations or theory, the complementarity of the proposed approaches and observation of new elementary particles, and combinations of these approaches.

A panel was set up and held two 2-day review meetings on the various experiments. Breakthroughs in cosmology have transformed our understanding of the universe. A wide variety of observations now support a unified picture in which the known particles make up only one-fifth of the matter in the universe, with the remaining four-fifths composed of dark matter. The evidence for dark matter is now overwhelming, and the required amount of dark matter is becoming precisely known.

Although it is known that there is dark matter, it is not known what that dark matter is. The bulk of dark matter cannot be any of the known particles. The existence of dark matter is, at present, the strongest evidence that the current theory of fundamental particles and forces is incomplete. Because dark matter is the dominant form of matter in the universe, an understanding of its properties is essential to attempts to determine how galaxies formed and how the universe evolved. Dark matter therefore plays a central role in both particle physics and cosmology, and the discovery of the identity of dark matter is among the most important goals in basic science today.

The theoretical study of dark matter is very well-developed and has led to many concrete and attractive possibilities. Two leading candidates for dark matter are axions and weakly-interacting massive particles (WIMPs). These candidates are well-motivated, not only because they resolve the dark-matter puzzle but also because they simultaneously solve longstanding problems associated with the Standard Model of particle physics.

Where does the science stand now? The U.S. projects CDMS and ADMX are leading the field in dark matter (WIMP/axion) sensitivity. Rapid advances in detector technology have reached interesting areas and can go further. There is a broad spectrum of technologies.

Axions were proposed initially to explain why CP-violating effects were not seen in the strong interaction. Axions resolve this problem by elegantly suppressing CP violation to experimentally allowed levels. Cosmology and astrophysics set the allowed axion-mass range from 1 μeV to 1 meV, where the lower limit follows from the requirement that axions not provide too much dark matter and the upper limit is set by other astrophysical constraints.

In a static magnetic field, there is a small probability for halo axions to be converted by virtual photons to a real microwave photon by the Primakoff effect. This occurrence would produce a faint monochromatic signal with a line width of $\Delta E/E$ of 10^{-6}. The experiment consists of a high-Q (Q = 200,000) microwave cavity tunable over GHz frequencies.

Phase-I construction of the ADMX is being completed. It will take 1 to 2 years to cover 10^{-6} to 10^{-5} eV down to the KSVZ (Kim, Shifman, Vainshtein, and Zakharov) model. Phase II is to cover same range down to the DFSZ (Dine, Fischler, Srednicki, and Zhitnitski) model. This second phase requires a dilution refrigerator to go from 1.7 to 0.2 K. Beyond Phase II, the researchers hope to develop cavities and superconducting quantum interference devices (SQUIDs) that make it possible to operate in the 10- to 100-GHz range, extending the mass range of the search.

WIMPs are particles that interact through the weak interactions of the Standard Model and have mass near the weak scale of about 100 GeV to 1 TeV. Such particles have strong motivations. WIMPs appear in supersymmetric theories and many other model frameworks independently motivated by attempts to understand electroweak symmetry breaking. These new particles are naturally produced by the Big Bang with the cosmological densities required for dark matter.

If one assumes the WIMP to be initially in thermal equilibrium, as the universe cools, there would be a decrease caused by the expansion of the universe and a change caused by annihilation and creation. If one integrates it over time, one gets the density now. It goes as one over the annihilation cross-section. Cosmology alone tells us that we should explore the weak scale.

How does one see these things? People usually assume a spherical distribution with a Maxwell-Boltzmann velocity distribution and look for elastic nuclear scattering. The overall expected rate is very small.

The WIMP "signal" is a low-energy (10- to 100-keV) nuclear recoil. One needs a large low-threshold detector that can discriminate against various backgrounds. One also needs to minimize internal radioactive contamination and external incoming radiation. The DUSEL would be especially important for dark-matter experiments

One needs to discriminate between nuclear and electronic recoil. One would need no multiple interactions, a recoil energy spectrum shape, consistency between targets of different nuclei, an annual flux modulation, and a diurnal direction modulation. The latter would produce a nice signature, but very short tracks would require a low-pressure gaseous target.

Some discrimination techniques are ionization, scintillation, and heat phonons. Different experiments used different combinations of these techniques. The Cryogenic Dark Matter Search

(CDMS) Collaboration has pioneered the use of low-temperature phonon-mediated germanium or silicon crystals to detect the rare scattering of WIMPs on nuclei and to distinguish them from backgrounds. With this powerful technology, operating deep underground in the Soudan Mine in Minnesota, the CMDS group has produced the most sensitive WIMP search in the world, and their reach is projected to grow by factor of 8 by the end of 2007.

CDMS has excellent event-by-event background rejection, experimentally measured gamma (99.995%) and beta (99.4%) suppression, and clean nuclear-recoil selection. No other technology has yet been demonstrated at the CDMS level of sensitivity. The comparison of silicon to germanium may confirm the origin of the signal. The experiment is sensitive to the spin-dependent cross-section, although the spin-independent cross-section is much larger.

Future plans include a run in Soudan through 2007 with the existing setup, a run in Soudan with two new supertowers through 2009, and a run at SNOLAB. Detectors must be maintained at 50 mK, a complex technology is needed, no fiducial volume free of surfaces can be defined, neutrons from internal activity need to be understood, and there is a relatively large cost-to-sensitive-mass ratio.

A lot of developments are occurring in the field. It has been energized by the emergence of noble liquid gasses (argon, xenon, and neon) in various detector configurations, as well as by new ideas for use of warm liquids and various gases under high or low pressure. These techniques offer an increased reach in sensitivity by at least three orders of magnitude for WIMPs, the possibility of recoil-particle-direction measurement, increased sensitivity to spin-dependent interactions, and detector sizes well beyond the ton scale. The complementarity of detector capabilities provides a range of target types suitable for establishing a WIMP signature and diverse background-control methods.

Starting with the noble liquids, they are relatively inexpensive, easy to obtain, and dense target materials. They are easily purified because contaminants freeze out at cryogenic temperatures. They have a very small electron-attachment probability; a large electron mobility; a high scintillation efficiency; and the possibility for large, homogenous detectors.

Single-phase techniques include the Dark Matter Experiment with Argon PSD (DEAP), Mini-CLEAN [Cryogenic Low-Energy Astrophysics with Neon], and XMASS [a xenon detector for weakly interacting massive particles]. Here, the pulse-shape discriminates electrons from nuclear recoils. The goal is a "simple and scalable" approach based solely on the use of scintillation and therefore free from the complications of high voltages and (almost) optical effects at phase boundaries. Both argon and neon have strong scintillation and singlet/triplet excimer lifetimes suitable to make pulse-shape discrimination (PSD) attractive and practical. Swapping neon for argon in the same detector gives a direct check of the total background level.

If a low background is achieved, the proposed 100-kg Mini-CLEAN could reach 10^{-45} cm^2 for a 100-GeV WIMP in 2007 to 2009.

With two-phase noble liquids and two scintillations, one gets a primary scintillation intensity, primary scintillation pulse shape, secondary scintillation intensity, S2/S1, multiple recoils, and fiducial volume. An example is the WIMP Argon Programme (WARP) experiment. After enlargement, it could get down to 10^{-45} with several years of running. XENON and ZEPLIN [ZonEd Proportional scintillation in LIquid Noble gases] are also noble-gas experiments. ZEPLIN II is operating with 32 kg of xenon and has already collected more than 1200 kg-days. XENON-10 is operating with 15 kg of xenon at Gran Sasso. Both ZEPLIN II and XENON-10 anticipate they will reach a dark-matter constraint of about 10^{-44} cm^2. Results are not yet available, but a dark-matter limit from this data set will provide a useful benchmark for the background levels and information on the electron-recoil-rejection capability as the devices get larger. Proposals are expected for larger versions.

Some of the challenges for noble-liquid experiments include the elimination or rejection of surface nuclear recoils, good knowledge of quenching factors, the quality of the γ and β rejection at low thresholds, fiducialization, neutron tagging, and freedom from ^{39}Ar and ^{85}Kr contamination.

It would be good to see and measure the track. One would need to achieve a full 3-D reconstruction for very short tracks (<2 mm) with the ability to distinguish the leading from the trailing end of the track. Ionization and scintillation signals are also available from gases at normal temperature and could provide reasonable size-competitive detectors at high pressure. Efforts are focusing on xenon at 5 to 10 atm and neon at 100 to 300 atm. The room-temperature requirement could simplify the design and operation [e.g., the DRIFT-II (Directional Recoil Identification From Tracks) experiment]. A time-projection chamber (TPC) filled with low-pressure electro-negative gas (CS_2) is observing recoil tracks that are a few millimeters long. Ion drift limits diffusion in all three dimensions. End planes allow the determination of range, orientation, and energy. The excellent discrimination is based on range and ionization density. Important R&D efforts by DRIFT groups and others include improvements in readout sufficient for achieving full directionality. In SIGN [Scintillation and Ionization in Gaseous Neon], very high pressure (100 to 300 bar) gaseous neon is contained in cylindrical modules. Discrimination is primarily based upon prompt and delayed scintillation pulse-height differences. Prompt scintillation is producing both a photomultiplier-tube (PMT) signal and photoelectrons produced and drifted from a cesium iodide surface lining the cylinder into a high-field region on the axis. Wavelength-shifting fibers along the axis carry light to a single PMT mounted on each end. Data suggest that some primary pulse-shape discrimination might be possible in addition to the PSD. The Chicagoland Observatory for Underground Particle Physics (COUPP) idea is based on a room-temperature bubble chamber of CF_3I. Other targets are possible or it. The fundamentally new idea is to operate the chamber with a threshold in specific ionization that is above the sensitivity needed to detect minimum ionizing particles so that it is triggered only by nuclear recoils. The goal is to produce a detector that has excellent sensitivity to both spin-dependent and spin-independent interactions of WIMPs and that can be scaled up to a 1-ton size at a reasonable cost. It has already reached stable operation with a 2-kg version at shallow depth and demonstrated excellent γ rejection. The principal background issue is decays of radon and its products in the vessel and in the bulk liquid; their combined decay rate determines the length of life time possible and thus must be significantly reduced. A well-planned R&D program has been started combining several avenues to control these sources.

In summary, past investments are now paying dividends as current experiments are beginning to be sensitive to the rates predicted in well-motivated models. CDMS and ADMX are leading the way. Recent advances in detector technology imply that these sensitivities may increase by 3 orders of magnitude in the coming few years. Such rapid progress will revolutionize the field and could lead to the discovery of dark matter for many of the most-well-motivated WIMP candidates. The pace of progress is such that physics discoveries based on these new detector developments could occur in the next 2 to 5 years. Most of these new experimental tools are U.S.-led or -inspired and, therefore, with appropriate investment in these technologies, the United States will be able to maintain its present leadership in direct-detection science. Direct-search experiments, in combination with colliders and indirect searches, may not only establish the identity of dark matter in the near future, but may also provide a wealth of additional cosmological information.

Ritz pointed out that it is important for the DMSAG to recognize that there should be just one neat experiment going forward and, with the successes of CDMS, where that type of experiment maxes out in scale. Sobel replied that those things are in the report. The group is now struggling with budgets.

Hewett asked how this group’s recommendations compared with P5’s recommendations. Sobel said that he did not see any disagreements.

Cahn noted that the possible discoveries at the LHC may focus funding in this area.

Lyn Evans joined the meeting by telephone to discuss the status of the LHC, which is at the end of a large industrial program of producing the dipole cold masses: more than 1200 of them have been delivered with just 22 to go. Almost all of this program has been conducted ahead of schedule. The magnets, which are coming from three manufacturers, are running a little behind schedule, but everything is going alright. Installation will be finished next February. This has been a great success. Installation of the cryomagnets is proceeding around the ring; 831 out of 1232 dipoles have been installed. Almost all of the equipment at the surface, in the shaft, and in the cavern has been commissioned; only the upgrading of the old LEP [Large Electron Positron Collider] refrigerators remains. Sector eight is being closed up and pumped. All this is an enormous step forward. Completion of the main tunnel junction is expected at the end of October. Only the last sector, through which all magnets were transported into the machine, remains.

The first of 16 distribution feed boxes has been installed in the tunnel. There is now a consolidated schedule. The rf modules for all of the LHC have been installed. There were a few problems with the rate of collimator production. All of the base-state collimators will be installed by hooking them up to electricity, water, and controls and precisely aligning them.

All of the important milestones (last magnet delivered, last magnet tested, last magnet installed, machine closed, and first collisions) will be met. The machine should be able to get up to 10^{13} under stable conditions before being shut down for the winter of 2008.

Sectors 7-8 and 8-1 will be fully commissioned up to 7 TeV in 2006 to 2007. The other sectors will be commissioned up to the field needed for degaussing. Initial operation will be at 900 GeV with a static machine to debug the machine and detectors. Full commissioning up to 7 TeV will be done during the winter 2008 shutdown; a high-energy run will be made in 2008. During the shutdown, the rest of the collimators will be installed.

In summary, the QRL [cryogenic ring line] and DFB [electrical feed box] problems are now resolved. There was some delay in collimator production, but sufficient collimators are available for 2007. The rest will be installed in 2008. Seven octants of the machine had been liberated for magnet installation, and interconnect work is proceeding in four octants in parallel. Magnet installation is now steady at 25 per week; installation will finish at the end of March 2007. The machine will be closed in August 2007. Every effort is being made to establish colliding beams before the end of 2007 at reduced energy. The full commissioning up to 7 TeV will be done during the 2008 winter shutdown, and a physics run will be made at full energy in 2008. A review committee meeting had just been held and it went well.

Shochet asked what the requirements were for the initial run. Evans answered that the first step is a circulating beam for 100 hours. Bringing it into collision is not a big deal.

Dragt asked about the need for manpower for commissioning. Evans replied that the commissioning of the beam is well covered. Hardware commissioning is now covered by 100 people from the member states.

Bortoletto asked if there was a possibility of damaging the detectors during startup. Evans responded that there would be no compromising of safety for personnel or equipment.

A break was declared at 10:37 a.m. the meeting was called back into session at 11:03 a.m. to hear **Michael Tuts** describes the status of ATLAS, the first of the two LHC detectors. ATLAS has magnets, a muon system, an inner detector, and a calorimeter. The collaboration is worldwide with 35 countries, 164 institutions, and 1830 scientific authors.

The agreed-upon plan calls for a luminosity of 5×10^{30} with a proton beam for 0.7×10^6 s in 2007; a luminosity of 0.5×10^{33} with a proton beam for 4×10^6 s and an ion beam for 0.2×10^6 s in 2008; a luminosity of 1×10^{33} with a proton beam for 6×10^6 s and an ion beam for 10^6 s in 2009; and a luminosity of 1×10^{34} with a proton beam for 10^7 s and an ion beam for 2×10^6 s in 2010.

The last barrel toroid was installed in August 2005. The toroids have been cooled down and are being ramped up in current. Full current will be achieved in December.

The end-cap toroids are still being assembled. There have been some mechanical problems, delaying progress about six months. Other parts are being worked on in parallel to catch up in time. By February, one end-cap toroid (ECT) will be installed and cooled down. The other will be installed in June 2007.

The barrel inner detector was commissioned above ground with cosmic rays. It is operating in excess of specifications; noise from the Transition radiation Tracker (TRT) is well below specifications.

One end cap for the inner detector has been integrated in October and will go into the pit in January 2007. The other will be ready for integration in mid-November.

The outer layer of the pixel barrel detector is done, all modules have been delivered, all problems have been resolved, and integration has gone about a month ahead of schedule. Both end caps have been accepted.

The liquid-argon calorimeter central solenoid has been commissioned. Some shorts are being tracked down. Electronics installation is well under way. Problems in delivery and quality control are being worked on.

The liquid-argon end caps will be cooled down in February 2007, and commissioning will be started in April for one side. The other side will be cooled and commissioned in November 2006 and February 2007, respectively. Commissioning with cosmic rays is under way. In the calorimeter commissioning, noise studies are pretty good.

The three tile calorimeters are installed in the pit. The scintillators have been installed. Cosmic-ray runs have been done.

In the muon system, the trigger elements and precision detectors are being installed; 500 out of 2000 are done. Commissioning with cosmic rays is under way.

In the muon end caps, 29 of 32 sectors are done on one, and 29 of 72 sectors are done on the other. One wheel has been installed. All cathode-strip chambers have been finished.

A schedule has been developed for integration of all these components. The beam pipe closes August 15, 2007. The remaining pieces are in the trigger and data acquisition. These are still being built.

Computing is being carried out on a tiered system. The top tier (T0) is located at CERN. The next tier down (T1) has 10 sites, of which one, BNL, is in the United States. T2 is comprised of 20 computer centers around the world, of which five are in the United States. T3 is at the desktops of the researchers. The tiers are connected through a worldwide network of computing grids. Service challenges have been held to test the grid and T1 sites. The service levels were met in these tests, which exercised all the software tools. The T2 centers are ramping up. The near-term commissioning plan calls for all the computing to be in place in November.

There has been tremendous progress in detector installation and commissioning. The computing grid is becoming a reality. Plans are being implemented in preparation for conducting physics. Surely, there will be bumps in the road, and the schedule is tight; but no showstoppers are in evidence. The program is on track for the first collisions in 2007 and 14-TeV physics in 2008.

Wormser asked if there is any danger of splitting physics among grids. Tuts answered that the grids are interoperable and should look like one grid to the users. The technical tasks will be distributed around this grid.

Daniel Green was asked to review the status of CMS. The tracker outer barrel has been finished, tested, and shipped to CERN. It will be installed in 2006. The new plan for pixels is to install a "slice" for the 2007 run to gain crucial operating experience and to advance the schedule for pixel commissioning with interactions.

The HF [hadron forward calorimeter] was moved into SX5 this summer and is waiting for infrastructure to be ready before it is lowered in October. The goals were to test the magnet to 4 T and to read out each CMS subsystem with cosmic-ray muons. The magnet is a 3-GJ object, the world's largest electromagnet. It worked like a champ. A field map is now being run. When this mapping is complete, the device will be lowered into place. Calibration and alignment data have been collected. All CMS subsystems logged in global data acquisition and were synchronized. This is a major milestone on the way to CMS data taking. Muons are used to cross-check the HB [central barrel] and HE [end caps] calibration and to align the muon chambers.

The tracker has been rigged into the magnet. The tracker clusters obtained signal to noise in the expected ranges: about 27 for the inner barrel and about 45 for the outer barrel. Track alignment and fitting are now in progress.

Differences in the hadron calorimeter (HCAL) timing were corrected in nanosecond steps by adjusting the pipeline delay in the HCAL trigger readouts (HTRs) to achieve synchronization. All the CMS subsystems are synchronized and taken globally through the 1/8 data acquisition slice.

The muon alignment experience helped the researchers understand the performance of the alignment system itself, the closing procedure, and the yoke behavior under magnetic forces. The goal is to achieve alignment within 200 μ.

The final trigger system was a major success, handling about 25 million events at a trigger rate of about 200 Hz.

Data-quality monitoring was conducted during the magnet test. The Detector Control System was in place during the magnet test and checked voltages, temperatures, etc. Data monitoring and analysis were successfully exercised at the Fermi National Accelerator Laboratory (FNAL) during the magnet test.

The H2 test beam was used. This was the first and last chance to exercise a "real" combined calorimetry system before the LHC beam. Responses were reconstructed. For the first time, a complete set of low-energy data was collected for pions, kaons, and (anti)protons for the combined calorimeter. These data are essential to correctly estimating the jet response of the CMS calorimeter system.

The U.S. CMS has 418 PhD physicists and 553 authors. The University of Tennessee has asked to join the collaboration. The U.S. CMS is now 36% of the CMS. This level of participation has been caused by shifts of faculty to CMS and by the arrival of many postdocs.

The grid computing service challenge was held. CMS has struggled to achieve the technical goals for running analysis and MC production jobs and for transferring data among the different tiers. The United States has been a major contributor. The computing and software challenge was launched October 2. The goal is to establish data handling at a level of 25% of 2008 operations. This is a test of new software tools. As of October 5, 8 million events were processed at T0, and data were exported to the seven T1 centers at 50 MB/s. T1-to-T2 transfers will also be exercised for six weeks. The T2 centers build on NSF-funded grid and network innovations spread around the country. The roles of the T2 centers in U.S. physics analysis need to be thought through. Productivity was achieved at the T1-to-T2 level.

The organization within the coordinator's purview will be discussed by the Executive Board as will the interconnects among functions. The CMS management structure is no longer a "federated" but a hierarchical "unitary" organization. It will provide technical coordination, commissioning and run coordination, computing coordination, off-line coordination, and physics coordination.

FNAL has agreed to form a research center. The details are to be worked out by the head of the center in full consultation with all the stakeholders. This will give the U.S. CMS much more visibility at FNAL.

The current thinking is to increase the LHC luminosity by a factor of 10 during a shutdown in 2013. This increase would require upgrading the detectors. Funding for R&D for these tasks needs to be started now.

In summary, the detector project is on track for completion in 2008. The only thing left is pixels. The magnet test has been a great success in learning how to operate the CMS. The data set will provide many important checks of the Monte Carlo modeling of CMS. CMS is being restructured to address data taking and analysis. The United States has leadership roles in the new

Executive Board management. Fermilab is restructuring to place more emphasis on CMS. Attention now shifts to final installation and the 2007 data taking and strategy. Simultaneously, planning has to start for the LHC upgrades with a ramp-up to major construction by 2009.

Bortoletto asked how the integration was going, given that the team is so thin. Green replied that the U.S. CMS is in contact with the Integration Coordinator and has offered technical help.

Shochet announced that former HEPAP chairman Peter Rosen was quite ill. Shochet will call him and thank him for all his contributions to the field.

The next meeting will be Feb. 22-23, 2007, with the DMSAG and the University Program Subpanel reporting.

The summary letter for the current meeting should include a mention of the winning of the Nobel Prize by Smoot and Mather with work supported by discretionary funding. This note should include a statement looking forward to a period when discretionary funding once again may lead to more Nobel prizes. Cahn suggested explicitly thanking DOE for allowing this work to be done at Lawrence Berkeley National Laboratory. Bagger noted that most scientific projects are funded through peer review; one does not want to give the impression that slush funds are being set up.

Shochet noted that not much will happen on the budget until after the election. Bagger suggested that operating costs at DUSEL should be spread across scientific interests. Bortoletto said that this long-term constraint should be stated at the beginning. Ruchti responded that this is an effort in progress, a creative exercise in which NSF would like to have some flexibility.

Shochet noted that the summary letter should state that the long-term assessment should include comments on the Tevatron performance. There is a concern about senior people moving on; this concern is heightened after 2008. Modest help from funding agencies would help this transition, but HEPAP does not have enough information to make a recommendation. The status report on BaBar and PEP-II pointed out the excellent performance there, as well, along with the physics productivity.

The first results from MINOS should be mentioned along with plans for increasing the number of protons per year. Fermilab handled the tritium issue very well, but HEPAP probably does not know enough about this volatile issue to comment on it.

Everything is in good shape at the Tevatron and MiniBooNE. The MiniBooNE results are being anxiously awaited.

The metrics used in the Office of High Energy Physics long-term-goal assessment work very well. A document from that Subpanel is needed and must be approved by HEPAP and forwarded to DOE by the end of the year. Caution signs should be raised for two programs about which not enough information is yet available.

A good and honest assessment of NASA's astrophysics program showed budgetary problems with Beyond Einstein. Priorities among JDEM and other projects will be set jointly by NASA and DOE according to a standard procedure. The NASA budget is already affecting the quality of the science being proposed.

Clearly, important science would be accomplished with DUSEL.

P5's major findings should be communicated, and HEPAP unanimously approved the report and was pleased with the process. There is a great breadth of physics that could be done. Within the report, there are a lot of milestones set after 2011; those milestones before 2011 should be emphasized. HEPAP should express appreciation for this roadmap; it is a wonderful thing for the field.

Staffin asked if there would be any communication among the regional strategy groups to discuss their visions. There is a mechanism to accomplish such communication in the CERN Council Strategy Group. Ritz asked if there are any other mechanisms to inform people about the P5 assessment, such as an InfoBlast from the DPF.

Shochet noted that the February deadline for the University Research Program Subpanel is daunting. There is no constraint requiring the February reporting rather than, say, a May reporting.

The Europeans and Americans are in sync in appraising the future programs and tools suitable for long-term funding in high-energy physics. Important are the science and techniques for dark-matter investigation; the field is expanding rapidly.

The LHC is in excellent shape for a 2007 turn-on and for a production run in 2008. The team there is to be commended for overcoming many hurdles.

The CMS and ATLAS detectors are in very good shape, both for the low-power run and the first physics run.

The floor was opened for public comment; there being none, the meeting was adjourned at 12:40 p.m.

Respectfully submitted,
F. M. O'Hara, Jr.
Recording Secretary
November 10, 2006

Corrected
M. Shochet
December 27, 2006

The minutes of the High Energy Physics Advisory Panel meeting held at The Latham Hotel, Washington, D.C. on October 12-13, 2006 are certified to be an accurate representation of what occurred.

Melvyn J Shochet