Minutes of the
High Energy Physics Advisory Panel Meeting
November 29-30, 2007
DoubleTree Hotel, Washington, D.C.

HEPAP members present:

Jonathan A. Bagger, Vice Chair
Charles Baltay
Alice Bean
Daniela Bortoletto
James E. Brau
Robert N. Cahn
William Carithers
Priscilla Cushman
Sarah Eno
Larry D. Gladney
Robert Kephart
Joseph Lykken
William R. Molzon
Koichiro Nishikawa, Thursday only
Angela V. Olinto
Saul Perlmutter
Lisa Randall, Thursday only
Tor Raubenheimer
N.P. Samios
Melvyn J. Shochet, Chair
Sally Seidel
Maury Tigner
Guy Wormser

HEPAP members absent:

Stephen L. Olsen
Satoshi Ozaki

Also participating:

David Asner, Department of Physics, Carleton University
Barry Barish, Director, Global Design Effort, International Linear Collider
Glen Crawford, Program Manager, Office of High Energy Physics, Office of Science, Department of Energy
Joseph Dehmer, Director, Division of Physics, National Science Foundation
Marvin Goldberg, Program Director, Division of Physics, National Science Foundation
Stavros Katsanevas, Institut de Physique Nucléaire de Lyon
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
Jacobo Konigsberg, Department of Physics, University of Florida
Jonathan Kotcher, Program Director, Division of Physics, National Science Foundation
Dennis Kovar, Acting Associate Director, Office of High Energy Physics, Office of Science, Department of Energy
Marsha Marsden, Office of High Energy Physics, Office of Science, Department of Energy
Akira Masaike, Director, Japan Society for the Promotion of Science, Washington Office
Salvatore Mele, Directorate Services Unit/Scientific Information Service, European Organization for Nuclear Research (CERN)
Hugh Montgomery, Associate Director for Research, Fermi National Accelerator Laboratory

Piermaria Oddone, Director, Fermi National Accelerator Laboratory
Frederick M. O'Hara, Jr., HEPAP Recording Secretary, Oak Ridge Institute for Science and Education
Joel Primack, Physics Department, University of California, Santa Cruz
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
Gene Sprouse, Editor in Chief, American Physical Society
Philip K. Williams, Research and Technology Division, Office of Science, Department of Energy
Andreene Witt, Oak Ridge Institute for Science and Education
W. John Womersley, Director, Particle Physics Department, Rutherford Appleton Laboratory

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

Thursday, December 29, 2007
Morning Session

Chairman **Melvyn Shochet** called the meeting to order at 9:00 a.m. He noted

- Wolfgang "Pief" Panofsky passed away in September after a highly distinguished career that included being the founding Director of the Stanford Linear Accelerator Center (SLAC).
- Personnel need to be recommended for Department of Energy (DOE) and National Science Foundation (NSF) openings in high-energy physics.
- Detailed recommendations on the response to the University Grant Program Subpanel by the agencies have been delayed.
- The International Linear Collider (ILC) will be discussed at length at the February 2008 meeting of HEPAP. The new Particle Physics Project Prioritization Panel (P5) will also report.
- Charles Baltay will chair the new P5.

Dennis Kovar introduced himself as the new Acting Associate Director of DOE's Office of High Energy Physics (HEP) and asked the members of the Committee to introduce themselves. Kovar has been the Acting Associate Director for six weeks and was asked review the status of the Office.

He stressed that strategic planning must be based on input from the scientific community. The mission of HEP is visionary and very ambitious: to understand the fundamental nature of time and space. This is an exciting program with the Tevatron and B Factory, the Large Hadron Collider (LHC) coming online, and the number of potential discoveries. However, the United States is poised to lose the leadership role at the energy frontier. Half of the U.S. high-energy-physics community will be at the LHC for the next decade. A backup plan is needed for Fermilab in case the ILC is delayed.

There are other compelling opportunities, and a strong U.S. high-energy-physics program is needed to pursue these opportunities in the context of available resources. In addition, a long-range strategic plan is needed. The FY08 budget is back on track to double the science and technology budget, but there is a continuing resolution instead of an approved budget. High-energy physics is not among the budget priorities. A compelling case must be made through a hard-nosed planning exercise.

The ILC will be pursued in a sustained manner, based on a roadmap that delineates the scientific outcomes that would justify the investment. While LHC results are awaited, R&D in support of the ILC will be performed. International arrangements will be needed to coordinate that R&D.

A coalition will be needed to identify the needed resources and to prepare for construction of the ILC so that it is compatible with the Global Design Effort (GDE) and the national planning. The earliest possible construction start is about 2015. Potential options should not be precluded, and the guidance of HEPAP will be important in keeping those options open.

Samios stated that the funding for ongoing projects should not be tapped and decreased to support the new R&D. Kovar responded that the FY08 budget will be a hard-nosed exercise that will set the tone for the next 10 years. HEPAP must judge what can and cannot be done with the projected budget. We have to make a convincing case about the science and the expected results.

Cahn asked when Kovar wanted this input by. Kovar replied that March would be good, but April 15 would be practical. Carithers asked if there are quidelines for the amount of R&D funds prior to CD0 [critical decision zero]. Kovar answered, no. The cost-of-living increase for the base program over 10 years is already a lot of money. If one tries to carve the ILC out of the rest of the program, it is very sobering in terms of lost science.

Joseph Dehmer was asked to present news about the NSF. He welcomed Dennis Kovar to HEPAP, commenting that Kovar has the full support of the NSF. Dehmer noted that, at Supercomputing 2007, the Open Science Grid achieved a data-flow milestone of more than 80 Gbps. The Physics Frontiers Physics Centers competition reviewed 58 pre-proposals, inviting 19 full proposals. The DUSEL town meeting in the District of Columbia (D.C.) involved the community and discussions of the initial suite of instruments; Sadoulet is presenting a report on this workshop later in this meeting. NSF and DOE are partnering to enable the Cornell Electron Storage Ring Test Accelerator (CESR TA) to perform critical-path R&D for the ILC. The United States and other regions are participating in the AStroParticle ERAnet (ASPERA), a European Union (EU) effort for international planning in astrophysics. There has been a noticeable blossoming of Physics of the Universe (POU)-style physics.

He observed that the opportunities for fundamental, transformative discoveries in particle physics have never been more numerous or compelling. As one makes discoveries, the opportunities ahead multiply. While the energy-frontier collider remains the tool of choice, additional approaches for major discovery have become indispensable portals of discovery. A balanced program will make the science more vigorous. The resources and time required for frontier facilities call for unprecedented preparation and planning, with a planning horizon extending well into the 21st century. A vigorous, world-class, globally engaged particle-physics community is important for science and society.

During the past few years, NSF and DOE have performed joint stewardship: For the ILC, DOE/HEP has taken the lead and NSF/PHY has played a supporting role. For DUSEL, NSF/PHY has taken the lead, and DOE/HEP-NP are playing supporting roles. For the Radioactive Ion Beam Facility (RIBF), DOE/NP is taking the lead, and NSF/PHY is playing a supporting role.

What is needed is a strategy for a sustained, world-class program of discovery well into the 21st century. In the short term, the programs at Fermilab, BaBar, and CESR need to be completed; the exploration of the TeV scale needs to be begun by the LHC; and plans need to be completed for the neutrino, astrophysics/cosmology, and rare-processes programs; R&D needs to be performed on all promising energy-frontier accelerator concepts; and the University Program and Theory Program need to be strengthened. In the long term, the United States needs to prepare to host the next-energy-frontier collider from the platform of a broad, vigorous, world-class program.

Real high-energy physics today is moving toward the Planck scale and is now performed with the Tevatron. The next steps will be done by the LHC. There will be a lepton machine and a hadron machine. The projects that will move on to the Planck scale are the Compact Linear Collider (CLIC), the muon collider, the Very Large Hadron Collider (VLHC), IceCube, etc.

The official start was *Quarks to the Cosmos* and was advanced by the *Physics of the Universe*. Priorities and relationships were sorted out. Investments were made in Laser Interferometer Gravitational Wave Observatory (LIGO, now at design sensitivity) and IceCube (which is going well with 22 strings and proceeding to 80). DUSEL, the Cryogenic Dark Matter Search (CDMS), and XENON are probing dark matter. The Pierre Auger Cosmic-Ray Observatory (Auger) and the High-Energy Cosmic Ray Experiment (HiRes) are operating at record levels. Borexino is working perfectly. The Advanced Compton Telescope (ACT) and South Pole Telescope (SPT) are looking at the structure of the universe. The B-mode polarization of the cosmic microwave background (CMB) is being investigated. And the origin of the elements is being studied by National Superconducting Cyclotron Laboratory (NSCL).

A concept drawing of DUSEL has been developed, and a preliminary design site has been identified. The Homestake Mine extends 8000 feet down and has 600 km of tunnels. Large caverns will need to be excavated. A town meeting was held at the NSF in March 2004. The first solicitation (S1) got one response. S2 was just awarded to develop conceptual designs for one or more sites. S3 was awarded to Homestake as the major research equipment and facilities construction (MREFC) candidate technical design. There will be a town meeting at NSF in November 2007, and S4 will set the technical design of the initial suite of instruments.

The NSF point of contact for DUSEL is Jonathan Kotcher. The community involvement is extensive. The science case is very well developed. The project has an interdisciplinary scope. The MREFC status is at the conceptual design phase and needs recommendations, a maintenance and operations plan, and a readiness memo. The project cannot start before 2011.

The total project cost is unknown but is about $500 million, half for infrastructure and half for the instrument suite, which will be the subject of a solicitation. There will be R&D support from NSF and DOE with international partnerships. The interim uses by state and private-laboratory operations may become integrated into DUSEL.

Carithers asked if there are other competitors for the DUSEL funding. Dehmer replied, yes. Some projects have been approved for budget inclusion. He could not speculate on the fate of any project. The best possible case will be made for DUSEL.

Cahn asked if there were an order. Dehmer said that there was not. Both are on the horizon list. DUSEL needs to take several formal steps to advance it to readiness. The Large Space Telescope (LST) is also progressing. Cahn asked, if the Advanced Technology Solar Telescope (ATST) gets delayed, whether that rules it out. Dehmer replied, no; it just has to compete at a different time.

Montgomery asked how the collaboration will happen. Dehmer said that all of the players will sit down and talk about the science and see where the process needs to be steered.

Bortoletto asked Dehmer if he could comment on the initial experiments. Dehmer replied that he could not say much except that the candidates will outstrip available funding. There will be a balance among the disciplines, although some will be ready to proceed before others.

Cushman asked if there were any plan for a Science and Technology Center request for proposals (RFP). Dehmer said that there was but that he did not know about it. There will be a solicitation in the coming months.

Wormser observed that there seemed to be a serious disagreement between Dehmer's short, intermediate, and long-term strategies and the EPP2010 report, reflecting a shift in policy. Dehmer said that the EPP2010 report is a very important document from the community. What he was giving in this presentation was his opinion of what *can* be done given the absence of a plan. He said that he did not make policy and that everyone needs to bear in mind the need for planning. Wormser said that the timeline for a 2015 beginning for the ILC seems too late. The United States has a window of opportunity to 2015. Dehmer said that starting something before CERN [Conseil Européen pour la Recherche Nucléaire (now European Organization for Nuclear Research)] does is not something that he worried about. The NSF is the younger brother here and does not have a great responsibility.

Shochet observed that the DOE budget was difficult for new HEPAP members to understand and asked **Glen Crawford** to expand upon and explain that budget request.

DOE follows an iterative bottom-up process in developing the budget request in which HEP's proposals go from the Office up to the Office of Science (SC), DOE, the Office of Management and Budget (OMB), and Congress. The community can interact at any of these levels.

Inside SC, the development of a new budget occurs from February to April, with each office within SC determining its program priorities within the constraints of the funding guidance provided by the Director of SC. Those priorities are presented to the Director of SC, and the program priorities of SC are determined within the constraints of the funding guidance provided by DOE.

Inside DOE, the new-budget development continues from April through July, when the Director of SC and the DOE Assistant Secretaries present their program priorities to DOE and DOE determines its overall agency priorities. With the feedback from the departmental decisions, each DOE office prepares its contribution to the President's Budget Request.

The DOE budget is submitted to OMB in August, and each DOE associate director defends his or her program budget at an OMB hearing in early September. OMB provides "passback" guidance to DOE in late November. Discussions between DOE and OMB refine the final budget numbers. In December, each DOE office redefines its contribution to the President's Budget Request, which is sent to Congress in February.

From March to September, the agencies present their budgets to Congress in formal hearings. In September, Congress is supposed to authorize 13 appropriations bills, using the President's Budget Request as a starting point.

A lot of staff time is spent preparing spreadsheet and document files for these interactions. This budget formulation is not simple. It is multidimensional (simultaneously meeting the needs of operations, construction, and R&D; of national laboratories and universities; and of other competing sectors of the budget), dynamic, strongly coupled, and constrained by significant boundary conditions.

The process has a few basic guidelines, treating project-like activities on planned profiles and treating facility operations and core research on a level-of-effort basis.

In recent years, more emphasis has been placed on budget integration, planning, and transparency, driven in part by focusing on good project management practices. Baselined construction projects are "protected" in budget planning, given that all significant (>$2 million) projects must be identified and approved internally at least 1 year before the funding is to flow. Planning requires a 1.5- to 2-year lead time. The process is not well-matched to basic research.

The publicly visible HEP product looks strange because it presents the information Congress needs or has requested. It is an overview that needs to be compelling, consistent, and reflective of research priorities.

Examples of how the support might play out are developed according to priorities (e.g., from EPP2010). The R&D needed is laid out to develop the budget. The 2008 HEP budget is broken down into Proton-Accelerator-Based Physics ($389.672 million requested), Electron-Accelerator-Based Physics ($79.763 million requested), Nonaccelerator Physics ($72.430 million requested), Theoretical Physics ($56.909 million requested), and Advanced Technology R&D ($183.464 million requested). These budget figures may change as they go through the budgetary process. The narrative format is set by the DOE Chief Financial Officer, OMB, and Congress. HEP Budget categories are a compromise between a "physics" basis and a "functional" basis. An additional complication is the mapping of budget functions onto the OHEP office structure, which is not necessarily intuitive. These categories do not map one to one to the HEP organization chart.

Once the budget is enacted, the Office of HEP has to write financial plans (for the national laboratories) and grants (for the universities and others), based on the appropriated (or expected) budget. The initial plan is usually based on the "worst case" of the House or Senate mark. The DOE CFO sets the overall funding level. In addition, a program may hold back funds for pending decisions, possible rescissions, and contingencies. Subsequent plans can rearrange the funding distribution or priorities. In the case of continuing resolutions, agencies can get stuck in a holding pattern, making execution difficult. This was the case in FY07. Under such circumstances, the Office tries to implement "big picture" priorities.

For FY08, the Office is both operating current facilities and preparing for the next decade's activities. Converting capital funds to operating funds is over. Re-converting operating to capital has begun and is not an easy step. New HEP construction projects will be ramping up, such as NOvA [Neutrinos at the Main Injector (NuMI) Off-Axis Neutrino Appearance Experiment], the Main INjector ExpeRiment for vA (MINERvA) neutrino cross-section measurements, the Daya Bay neutrino experiment with China, and the Dark Energy Survey (DES) with NSF. ILC R&D is also ramping up to a $60-million request for FY08, up from $42 million in FY07. The superconducting RF infrastructure initiative continues and is aligned with the ILC R&D. The Tevatron, B Factory, and NuMI are running full steam.

At the time of this meeting, the full House had passed the Energy and Water Bill in July. The Senate committee passed the Energy and Water Bill in June (with no full Senate vote). A continuing resolution was in force through December 14. The White House had stated it will veto the bill if it comes in at the Congressional spending level. The House and Senate were trying to work out a compromise in conference, which likely would split the difference between the President's Request and the Congressional markups. Whether this would become law was anyone's guess. The impact of the ultimate budget on the Office of Science was also uncertain.

Projects are managed according to approved baselines by the designated project manager. The extent of oversight is tailored to the total project cost (TPC). For smaller items, the decision process in the R&D phase is still ill-defined. New rules and guidelines for how to report costs, both pre-and post-baseline, have been issued. There is a complex dance between project and budget requirements and timelines.

The budget-project process is driven by the CFO and Dan Lehman's office. Larger projects automatically get higher visibility. All these approvals have to get in place before an item gets into the budget. Congress cares about the number of dollars, when something will happen, why it is needed, and what type of funding it is. Construction projects automatically get higher visibility.

There are nine major items of equipment (MIEs) started or about to start. All have to work within the overall budget.

In summary, HEP is a complex and multiply connected program. The current budget structure seems to work well with the Department's main customers (OMB and Congress). However, some important cross-cuts are not very transparent. There is a significant learning curve to understand the budget process in detail.

A break was declared at 10:55 a.m. The meeting was called back into session at 11:12 a.m. to hear **Bernard Sadoulet** report on the November 2 town meeting and November 3-4 workshop on DUSEL, which is entering a new phase with the publication of the S1 [Solicitation 1] report and the selection of the Homestake site. About 175 people attended the town meeting, and 195 participated in the workshop. A follow-up meeting was held at OSTP/OMB on November 5 at the request of Presidential Science Advisor Jack Marburger.

The town meeting gave a description of the DUSEL opportunities, its history, and international aspects. The S1 recommendations were presented, and the agencies spoke about what they were interested in. The partnership between the State of South Dakota and the federal government was described, and the S3 process was outlined.

The S1 report gave a series of findings:

- Underground science is an essential component of the research frontier in many disciplines, with strong benefits for society.
- There is a chronic need for underground space worldwide.
- The United States should strengthen its underground research and call for a cross-agency multidisciplinary initiative optimally using facilities both in the United States and around the world. As soon as possible, it should construct a DUSEL. The United States should complement the nation's existing assets with a flagship world-class underground laboratory providing access to very great depth (6000 meters water equivalent) and ample facilities at intermediate depths.

The S3 study calls for a variety of levels to be developed in the facility and three major campuses. An interim facility, the Sanford Underground Science and Engineering Laboratory, is being funded by the State of South Dakota and a private donor. It will keep the water in the former mine below 4850 ft until 2011.

The Nov. 2-3 workshop recommended that $500 million be split evenly between facilities and experiments. The workshop's goal was to focus on the next phase of the project, the science component of the MREFC (the first suite of experiments). Disciplinary and cross-cutting working groups are to produce white papers of about five pages on science, priorities, roadmap, R&D needs, costs, engineering and operations, and organization. In 2006, the Particle Physics Project Prioritization Panel (P5) constructed a Roadmap for Particle Physics. DUSEL was in the second priority group after the ILC. P5 reviewed the progress of DUSEL in September 2007 and was delighted to see that a potential location for the lab had been chosen. P5 reaffirmed the importance of the science program. In addition, DUSEL is an essential component of the Nuclear Science Advisory Committee (NSAC) long-range plan. Momentum is building for the excavation of a cavity for a 100-kT module as R&D for proton decay/neutrino oscillation. International collaboration is likely. There is some interest in n-nbar. White papers are coming in. The estimated cost of the superset of projects proposed by the working groups totals $650 million, a very rough estimate. NSF cannot do it alone; other agencies and international partners are needed. Some difficult choices lie ahead. At least there is clear evidence that there is a need for such a facility.

There was a lot of reflection on the time scale. However, even in the best scenario, the MREFC proposal must be ready by December 2008 for a March 2009 National Science Board (NSB) decision and funding in FY11. This would allow significant access to 4850 ft in 2013 and access to the 7400-ft level in 2015. If we insist on having the science at the same time with the facility, the science program has to be defined by December 2008, leading one to ask if the requirements of MREFC (a preliminary design report, which is equivalent to a CD2 Lehman-type review) can be fulfilled.

To fit into the MREFC framework, the project will need to define in detail a science program by December 2008. A number of difficulties are apparent: It is impossible to do this at the required level (a preliminary design report) with most experiments (there is more than $500 million of experiments to baseline). It does not make any sense to fix now what will be installed in 2013 and 2015. Advantage should be taken of an experiment construction time that is shorter than that of the facility to take into account input from previous experiments to maximize the scientific output. Compatibility must be maintained with the Science Advisory Group process. Time is also needed to raise the additional $300 to $400 million. One possible solution is to define an "initial scientific program" by determining as accurately as possible a scientific envelope costing for a representative set of initial experiments, making assumptions about other contributions, adding contingency, and making room for new ideas, and then living within this scientific envelope. Contingency should be shared by all experiments. Some flexibility could be injected by adapting the MREFC to this specific case.

The scientific program definition can be planned across a multiyear schedule in several waves, each requiring time for (1) review and (2) construction. There would be money available through S4 to bring the project to the next level.

The underground community is mobilizing to put forward a credible scientific program for an MREFC on a very short time scale. The emerging organization consists of working groups by subfields developing a scientific strategy. It is in the middle of self selection of overall coordinators to pull together the scientific component of the proposal. Cross-cutting working groups are looking at common functions and are eager to implement the synergies inherent to DUSEL.

There was a lot of discussion about maximizing DUSEL's potential. The ultimate goal is science, not building a facility. The frontier needs to be pushed as aggressively as possible at all existing facilities, including the Sudbury Neutrino Observatory Laboratory (SNOLAB) and Sanford Lab. The science obtained in the coming years is essential to inform the program. We must begin to realize the other promises of DUSEL as soon as possible, such as the multidisciplinary aspects and interagency and international cooperation by establishing a virtual laboratory, a Center for Deep Underground Science and Engineering.

In conclusion, the underground community is preparing a credible scientific program on a very short time scale. NSF should adapt the MREFC process to the DUSEL case. It is essential for DOE to get involved. DUSEL offers unique scientific opportunities that complement the accelerator and space frontiers. At the same time, the scientific frontier needs to be pushed at currently available underground sites.

Shochet asked why the timeline showed the earliest experiments occurring before the MREFC. Sadoulet said that these would be biology and other experiments that would not require MREFC.

Carithers stated that the experiments must be costed and scheduled. Sadoulet said that the costs would be for the experimental suite and that projects would be charged against that allotment. From the scientific view, that does not make sense.

Wormser observed that the initial suite of experiments does not seem to merge with the construction funding. Sadoulet replied that some experiments need to start right away with the facilities available.

Shochet said that there is real danger in setting a cap for an experiment's costs before undertaking the experiment.

Joel Primack was asked to report on the assessment of the Beyond Einstein Program. The National Aeronautics and Space Administration (NASA) has a roadmap for that program with five mission areas. The first step should be a Joint Dark-Energy Mission (JDEM) in 2009. The Laser Interferometer Space Antenna (LISA) should also be funded. The five mission areas were Constellation-X; LISA; and the three Einstein probes: inflation, dark energy, and black-hole.

In 2003, a National Science and Technology Council convened an Interagency Working Group on the Physics of the Universe, and Congress formed a three-agency

Astronomy and Astrophysics Advisory Committee (AAAC). The *Physics of the Universe* was published in March 2004, and formed a cross-agency implementation plan. Also in 2004, the NSF launched a community-based process for an underground laboratory and it funded the design and development for the LSST. In 2005, the Committee on Astronomy and Astrophysics (CAA) midterm report re-affirmed the *Astronomy and Astrophysics in the New Millennium* and *Quarks to the Cosmos* priorities. In April 2006, the report, *Revealing the Hidden Nature of Space and Time – Charting the Course for Elementary Particle Physics* embraced the *Quarks to the Cosmos* science as part of elementary-particle physics. In October 2006, the P5 roadmap included JDEM, the Large Synoptic Survey Telescope (LSST), dark matter, the underground laboratory, neutrinos, and more. In September, the Beyond Einstein Program Assessment Committee (BEPAC) identified the first Beyond Einstein mission.

The Committee charge was to

1. Assess the five proposed Beyond Einstein missions and recommend which of these five should be developed and launched first, using a funding wedge that is expected to begin in FY 2009. The criteria for these assessments includes (a) potential scientific impact within the context of other existing and planned space-based and ground-based missions and (b) the realism of preliminary technology and management plans, and cost estimates.
2. 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.

The second task will assist NASA in its investment strategy for future technology development within the Beyond Einstein Program prior to the results of the Decadal Survey.

The Black Hole Finder Probe would be a huge spacecraft and would be very expensive. This was not seen to be an optimized mission, and the cost/benefit was not seen to be attractive. The Inflation Probe would have to find an order-of-magnitude increase, and the probes, therefore, need additional research. The Cosmic Inflation Probe is ambitious but has good legacy information from other experiments and seems quite doable. It could narrow down the theory base tremendously. It comes close (but does not exceed) the scientific promise of JDEM and LISA.

Constellation-X would investigate motion near black holes, measure the evolution of dark energy using clusters of galaxies, determine where most of the atoms are located in the Warm Hot Intergalactic Medium (WHIM) and detect baryons, determine the relationship of supermassive black hole (SMBH) growth to formation of galactic spheroids, and determine whether dark matter emits energy via decay or annihilation.

JDEM would precisely measure the expansion history of the universe to determine whether the contribution of dark energy to the expansion rate varies with time. It has a good figure of merit. However, all the methods combined have to be extrapolated far beyond what has been seen so far, and there is no certainty that the systematics will allow that. But if it can be done, the results would be extremely significant. A lot of sky will need to be imaged. The scanning will go deeper in more colors; the increased data will change our understanding of the universe. The SuperNova Acceleration Probe (SNAP) and Destiny will give a wealth of scientific data. The Advanced Dark Energy Physics Telescope (ADEPT) is a spectroscopic analysis of 100 million galaxies; the discovery potential is enormous. The United States would welcome collaborations with the Europeans, who are considering a different type of mission. The broader science made JDEM the first choice.

LISA would determine how and when massive black holes form, investigate whether general relativity correctly describes gravity under extreme conditions, determine how black hole growth is related to galaxy evolution, determine if black holes are correctly described by general relativity, investigate whether there are gravitational waves from the early universe, and determine the distance scale of the universe. It would do all this with three satellites orbiting the sun. Proof masses measured constantly with two-way lasers would measure gravity waves. It faces difficult technical issues that have not been tested in space. Assuming all goes well, the United States with the European Space Agency should proceed rapidly. LISA could look for the formation of massive black holes and could test general relativity in the strong-field regime. More broadly, it would measure black-hole spacetimes, look for gravity waves from the early universe, and determine the distance scale of the universe. LISA and JDEM complement each other in such cosmography.

All of these experiments were evaluated in terms of (1) the potential for advancing the Beyond Einstein research goals (finding out what powered the Big Bang; observing how black holes manipulate space, time and matter; and identifying the mysterious dark energy pulling the universe apart); (2) broader science contributions; (3) the potential for revolutionary discovery; (4) science risk and readiness; and (5) the uniqueness of the mission candidate for addressing its scientific questions.

The findings were:

1. The Beyond Einstein scientific issues are so compelling that research in this area will be pursued for many years to come. All five mission areas in NASA's Beyond Einstein plan address key questions that take physics and astronomy beyond where the century of Einstein left them.
2. The Constellation-X mission will make the broadest and most diverse contributions to astronomy of any of the candidate Beyond Einstein missions. While it can make strong contributions to Beyond Einstein science, other Beyond Einstein missions address the measurement of dark energy parameters and tests of strong-field General Relativity in a more focused and definitive manner.
3. Two mission areas stand out for the directness with which they address Beyond Einstein goals and their potential for broader scientific impact: LISA and JDEM.
4. LISA is an extraordinarily original and technically bold mission concept. LISA will open up an entirely new way of observing the universe, with immense potential to enlarge our understanding of physics and astronomy in unforeseen ways. LISA, in the committee's view, should be the flagship mission of a long-term program addressing Beyond Einstein goals.
5. The European Space Agency–NASA LISA Pathfinder mission that is scheduled for launch in late 2009 will assess the operation of several critical LISA technologies in space. The committee believes it is more responsible technically and financially to propose a LISA new start after the Pathfinder results are taken into account. In addition, Pathfinder will not test all technologies critical to LISA. Thus, it would be prudent for NASA to invest further in LISA technology development and risk reduction, to help ensure that NASA is in a position to

proceed with ESA to a formal new start as soon as possible after the LISA Pathfinder results are understood.

6. A JDEM mission will set the standard in the precision of its determination of the distribution of dark energy in the distant universe. By clarifying the properties of 70 percent of the mass-energy in the universe, JDEM's potential for fundamental advancement of both astronomy and physics is substantial. A JDEM mission will also bring important benefits to general astronomy. In particular, JDEM will provide highly detailed information for understanding how galaxies form and acquire their mass.
7. The JDEM mission candidates identified thus far are based on instrument and spacecraft technologies that have either been flown in space or have been extensively developed in other programs. A JDEM mission selected in 2009 could proceed smoothly to a timely and successful launch.
8. The present NASA Beyond Einstein funding wedge alone is inadequate to develop any candidate Beyond Einstein mission on its nominal schedule. However, both JDEM and LISA could be carried out with the currently forecasted NASA contribution if DOE's contribution that benefits JDEM is taken into account and if LISA's development schedule is extended and funding from ESA is assumed.

The Subpanel recommended that

1. NASA and DOE should proceed immediately with a competition to select a JDEM for a 2009 new start. The broad mission goals in the RFP should be (1) to determine the properties of dark energy with high precision and (2) to enable a broad range of astronomical investigations. The Subpanel encourages the agencies to seek as wide a variety of mission concepts and partnerships as possible.
2. NASA should invest additional Beyond Einstein funds in LISA technology development and risk reduction, to help ensure that the Agency is in a position to proceed in partnership with ESA to a new start after the LISA Pathfinder results are understood.
3. NASA should move forward with appropriate measures to increase the readiness of the three remaining mission areas (Black Hole Finder Probe, Constellation-X, and Inflation Probe) for consideration by NASA and the National Research Council (NRC) Decadal Survey of Astronomy and Astrophysics.

The Subpanel report has 200 pages of technical comments. There are four bins of complexity, beginning with JDEM on the low end and culminating with the large observatories (LISA and Constellation-X) as the most complex. Approximate development cost (Phases B, C, and D) and schedule regimes are as follows for the Beyond Einstein mission areas:

- Large Observatories (LISA and Con-X) $2 billion over 8 years
- Black Hole Finder Probe [BHFP, including Hard X-Ray Black Hole Surveys in Space and Time (EXIST) and the Coded Aperture Survey Telescope for Energetic Radiation (CASTER)] $1.5 billion over 7 years
- JDEM (SNAP, ADEPT, Destiny) $1 billion over 6 years
- Inflation Probe (IP) [including Cosmic Inflation Probe (CIP), Cosmic Microwave Background Polarization (CMBPol), Experimental Probe of Inflationary

Cosmology (EPIC-F), and Einstein Polarization Interferometer for Cosmology (EPIC-I)] $1 billion.

These figures are all larger than expected. Independent cost estimates were similar to SAIC's [Science Applications International Corporation]. There are factors of 2 to 3 between what the projects were said to cost and their independent cost estimates. The program can be distorted if the costs are not estimated reliably.

HEPAP should think about DOE's opportunities and choices. Its role in JDEM will cost at least $400 million, comparable to a large accelerator experiment and an order of magnitude larger than the Gamma Ray Large Area Space Telescope (GLAST), DOE's previous large space astrophysics mission. With JDEM, astrophysics can no longer be supported by discretionary funds; it will become part of DOE's mission. The choice for DOE is whether to accept JDEM as a special case, or embrace astrophysics as a core DOE science mission. The advantages of embracing astrophysics include close connections with particle physics and physicists, DOE - HEP gets more credit for achievements and a larger budget, and the United States preserves its international astrophysics leadership.

P5 is strongly enthusiastic about dark energy science and supports an aggressive experimental program. At the same time, AAAC appreciates the growing interest in astrophysics within DOE HEP and recommends that HEP continue to enhance their support of programs at the interface of astronomy and particle physics.

NASA Space Science 2003 supported a robust astrophysics program with a balanced mix of Research and Analysis (R&A); flagship, midsized, and small missions, including the Hubble Space Telescope (HST), Chandra, Spitzer, the Wilkinson Microwave Anisotropy Probe (WMAP); and other explorers and future missions, including James Webb Space Telescope (JWST), the Stratospheric Observatory for Infrared Astronomy (SOFIA), GLAST, Kepler, the Nuclear Spectroscopic Telescope Array (NuSTAR), and the Wide-Field Infrared Survey Explorer (WISE). For Beyond Einstein, it supported JDEM, Inflation Probe, Black Hole Probe, Constellation-X, and LISA. For Navigator, it supported the Terrestrial Planet Finder and the SIM PlanetQuest (formerly called Space Interferometry Mission/SIM). This array would constitute a diverse solar system exploration program and an ambitious Earth observation program, despite a costly and wasteful International Space Station program sold as "science."

President Bush had promised new money that never appeared. Instead, $3.8 billion was taken out of the astrophysics budget. Recent developments include SOFIA being refunded, the Beyond Einstein NRC study is to choose the first mission for a launch sometime after 2015, Science Mission Directorate Associate Administrators Alan Stern and John Mather were appointed, and the NuSTAR X-ray Explorer mission was restarted. The National Academy of Sciences said that Earth science is in dreadful shape with no funding for any recommended mission.

Three nightmares for U.S. space astrophysics are that Moon-Mars eats up all available funds, the demise of Earth observation from space becomes an issue in the 2008 presidential campaign and the next administration cuts space astrophysics to fund Earth observation, and the next administration repudiates the Bush Moon-Mars initiative and drastically cuts the NASA budget.

Bagger noted that the EPP2010 and other reports were well thought out, and none of them say that 25% of the HEP budget should go to astrophysics.

Carithers asked what had occurred since HEPAP made its recommendations. Kovar responded that, at the moment, DOE and NASA are talking about what a joint mission means. A memorandum of understanding (MOU) is expected in early 2008. Carithers expressed concern about the process and its transparency at the scientific level. Secrecy has crept into the program. All scientific issues need to be aired. Primack agreed. One of the proposed missions is proprietary. The firm proposing it will have to be more forthcoming. DOE and NASA could say that explicitly. Salamon said that the solicitation will come out in mid-2008 with a launch in 2015 or 2016.

Samios stated that NASA is not a reliable partner. That may `be changed by the next administration. Primack admitted that a joint ESA/NASA project was dropped by NASA. Long-term stability is needed. NASA is seen as being oriented toward human spaceflight, but that program has been a great embarrassment to NASA. Unmanned missions have resonated better with the citizenry and produce far more science. The intellectual opportunities are greater than they ever have been.

A break for lunch was declared at 12:44 p.m. The meeting was called back into session at 2:02 p.m.

Jonathan Bagger was asked to describe the Astrophysics Decadal Survey, which has a process that engages hundreds of scientific researchers in formulating a decadal scientific agenda. From that process comes a report that contains a rank-ordered list of missions/facilities to carry the science forward into the decade. This survey has proven to be an effective and powerful roadmap for the community and its sponsors.

There have been five Decadal Surveys, starting in 1964. From 1964 to 2000, it has been the "gold standard" of decadal surveys, with more than 80% of the recommended projects being completed by NASA and NSF. With the exception of Gravity Probe-B, every major astronomy/astrophysics mission undertaken by NASA has been recommended in one of the surveys. As HEP overlaps with astrophysics, it needs to participate in this survey. NASA, NSF, OMB, the Office of Science and Technology Policy (OSTP), and even Congress rely on the survey to provide strategic guidance.

There is an overall Survey Committee Chair or Co-Chairs and 10 to 20 members. It supports 8 to 15 topical panels with independent chairs and vice-chairs, bringing participation to a total of 100 to 300 members. There are also cross-panel working groups. Each panel reports on priorities within an area; synthesis and prioritization is performed by the Survey Committee. It has been successful because the community participates in it and adheres to its recommendations.

The current status of Astrophysics 2010 is that the internal NRC proposal has received approval. In April, a planning meeting was held under auspices of the Board on Physics and Astronomy (BPA) and the Space Science Board (SSB). It developed guiding principles for the survey. The resulting proposal was submitted to NASA, NSF, and DOE. The survey will not start (and the committee members will not be selected) until the proposal is funded by the agencies. It is hoped that the committee will start in early 2008.

Many of the projects recommended in 2000 never happened for one reason or another. JDEM was not on that list because it had not emerged.

Issues facing Astrophysics 2010 include:

- Extensive community involvement with town meetings at the American Astronomical Society (AAS) and American Physical Society (APS)

- Panel organization, perhaps a matrix of science versus wavelength
- Costs were understated in 2000; a BEPAC-like model of cost estimation will be employed
- Unrealized projects will be on the table again
- International representation will be sought because some projects are too large for one country to go it alone
- Physics and astronomy are converging with uncertain boundaries
- Robustness to changing circumstances (such as new discoveries and cost growth) will be achieved through "decision rules"
- The addition to the committee of other experts in science, policy, etc. could help with critique and credibility

Nominations can be sent to astro2010@nas.edu.

Montgomery asked if the year 2000 was a glitch or a fundamental change. Bagger replied that he believed that it reflected a shift towards larger projects. Samios said that there was always a distinction made between big and little projects and asked if that practice would be continued. Bagger answered that he expected so.

Kovar asked what Bagger meant by "insider." Bagger responded that being an insider means that it does not make any difference if one is an astronomer or a physicist. Kovar asked whether Bagger, as an insider, was going to influence priorities in astronomy and astrophysics. Bagger said that he did not know. His gut told him that it is important to be at the table. Important changes occur at the borders of a culture.

John Womersley was asked to report on the committee of visitors (COV) to the DOE Office of High Energy Physics (OHEP), which was conducted on June 18-19, 2007. The COV looked at how the Office operated during FY04, FY05, and FY06.

The COV started by hearing presentations from staff members and then divided into four subgroups for interactive sessions with OHEP program officers. It read a selected sample of proposal folders; looked at the large HEP investments in the national laboratories, the accelerators, and the major detector facilities; and evaluated the methods used by OHEP for monitoring, reviewing, and prioritizing these programs.

The first conclusion was that proposals were handled equitably and that national-laboratory management was validated. The COV found the overall functioning of the OHEP office to be very professional and responsible in soliciting and evaluating proposals, making grants, and monitoring the funded programs. However, the COV did find some areas of concern, leading to 18 recommendations to improve the functioning of the Office:

1. OHEP is very seriously understaffed, worse now than in 2004, an unsustainable situation. An urgent effort should be made to fill all the vacant staff positions in the Office and to add additional Intergovernmental Personnel Act (IPA) positions. A search committee of community members should be established to identify and recruit potential candidates. The profile and awareness of OHEP staff's role should be raised. IPA posts should be used wherever appropriate. The Office should help applicants through hurdles created by the hiring process
2. Documentation and access to program data should be improved, with data being put into electronic format.
3. The planned program balance correctly reflects the priorities expressed by the field through HEPAP, P5, and the EPP2010 advisory panels. However, program

management is challenging and important. The Office should continue to work with P5 and HEPAP in evolving the medium-term program, ensure that program planning is adequately staffed and supported, and ensure effective and ongoing engagement with all stakeholders in the ILC.

4. OHEP decisions and the rationale behind them should be effectively communicated to the community.
5. The Office should develop a process to globally optimize and comparatively review the balance of support for HEP research at Fermilab, the universities, and the other laboratories in light of the evolving program.
6. Review committees need to be given sufficient time to explore issues in detail; to be able to meet privately in executive session; and to produce meaningful closeouts and timely feedback. The Office should understand and communicate best practices for reviews and ensure that they are followed. It should also consider whether the consultancy model used in laboratory reviews is optimal.
7. The Outstanding Junior Investigator (OJI) Program continues to be very successful in launching the research careers of some of the most talented junior faculty. The number of Outstanding Junior Investigator awards should be increased by devoting more funds to this program.
8. The proposals on file were found to be too long; 10 pages per senior investigator should be sufficient.
9. On the review process, outside visiting consultants should continue to be used for 3-year renewals of large grants, and the Office should consider eliminating site visits in continuation years unless some unusual circumstance warrants such a visit.
10. OHEP should consider providing a template to reviewers to provide guidance and greater uniformity of reviews. The Office should also ensure there are sufficient reviewers for the theory component of multitask grants.
11. Grant jackets should contain a brief summary sheet of funding levels by task, current funding, and personnel supported by category. This summary sheet should be available online.
12. The Office should establish a formal advisory mechanism to best optimize the split between ILC accelerator and ILC detector R&D funds.
13. OHEP should work with the community and the laboratories to formulate a plan for stewardship of accelerator science in the United States during the coming transition to a period without an energy-frontier machine. This plan should recognize the centrality of maintaining and developing high-energy accelerator science and technology in the United States and of training the next generation through collaborative activities overseas and participation in other SC projects. Recently, DOE OHEP and NSF created a HEPAP subpanel to review and advise on this topic, and several of the subpanel's recommendations have already been adopted.
14. The peer-review process should be expanded to cover midterm accelerator research to provide comparative evaluation of the merit of different research efforts.
15. The project-initiation and -management process in OHEP should continue to be closely aligned with the HEPAP/P5 prioritization process and the strategic goals of SC. Interactions with the appropriate advisory bodies should increase in frequency. Detailed budget and schedule planning for major projects need to be more proactive.

16. The Office's oversight is effective. However, some recent efforts were terminated after significant investments of resources and effort were made. In these cases, the decision process was perceived to be less transparent than desirable. To the greatest extent possible, only those major projects for which the physics goals are well matched to the priorities in the field and whose overall scope, cost estimate, and funding requirements are consistent should be advanced to construction status. A funding cap should not be established prior to establishment of a realistic baseline because it introduces risk that a project can not be completed within budget or that its scientific scope will not be delivered.
17. The Office should recruit an individual (or an IPA position) at a high level within the Office to proactively pursue opportunities to support projects in collaboration with other agencies, both domestic and international.
18. The Office should add staff to the Facilities Division to provide sufficient project management oversight for upcoming major projects.

In conclusion, the COV validated (1) the integrity and efficacy of the processes for treating proposals and for making funding actions and (2) the OHEP program management of the national laboratories and large facilities. The strategic direction taken by the Office reflects the priorities of the field but also reflects the tensions presented by the ILC. OHEP relies on a highly dedicated and very overworked staff to function; the current understaffing situation is not sustainable.

Shochet commented that this was an ongoing activity. He asked if the Committee was in favor of accepting the report. The Committee unanimously approved the report.

The final P5 report was presented by **Abraham Seiden**. This report was meant to comply with the Division of Particles and Fields (DPF) recommendation that P5 present a yearly progress report to HEPAP and was based on a two-day meeting at Fermilab on September 24 and 25.

The 2006 Roadmap addressed science questions on mass, the undiscovered principles of nature, the dark universe, unification, and flavor. It grouped the major science opportunities into five categories: (1) the energy frontier, (2) dark matter, (3) dark energy, (4) neutrino science, and (5) precision measurements involving charged leptons or quarks.

P5 had to make assumptions about budgets. For DOE, the base budget plan was: FY07, $775 million; FY08, $785 million; FY09, $810 million; FY10, $890 million; and FY11, $975 million. It also assumed the completion of PEP-II [Positron Electron Project at Stanford] running in FY08 and of the Tevatron running in FY09. An alternative budget assumed a 7% annual increase, resulting in a doubling of the HEP budget over 10 years. The annual funding in such a plan was $775 million, $829 million, $877 million, $950 million, and $1016 million, or about $50 million per year larger than the base budget plan.

Within the base budget plan, the priority recommended construction or R&D was determined. The highest priority group involves the investigations at the energy frontier, the full range of activities for the LHC program, and the R&D for the ILC. The second 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; Daya Bay; and support for the LSST, DUSEL (dark matter and neutrinoless double-beta decay), and SNAP. P5 recommends that the DOE work with NASA to ensure that a dark energy space mission can be carried out and that the three potential approaches to the mission have been properly evaluated. The third group includes the construction of the NOνA experiment at Fermilab along with a program of modest machine improvements.

P5 recommends a review by P5 toward the end of this decade to look at projects that could start construction early in the next decade. The base budget plan 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. Some of the areas to be examined are 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, flavor physics, and further experiments (such as the muon $g - 2$, μ to e conversion, a very-high-luminosity B experiment, and rare kaon decays).

A separate review by P5 will be required to look at the best directions for neutrino physics. A second area that might be included in this review would be an ambitious proton decay experiment. These two projects could be the major second phase of experiments for DUSEL.

P5 produced a roadmap that should be updated. This roadmap covered only large projects. Of the projects recommended for construction, the DES, Daya Bay, and NOνA experiments are all moving ahead. Work on developing the experimental equipment is progressing, as are the various international aspects. All three projects had their CD-1 reviews in 2007. NOνA has successfully completed the CD-2 process, and the other two experiments expect to have their CD-2 reviews within the next few months. Significant data collection for these experiments can be expected to start in FY11 or FY12.

Significant progress is being made on a broad dark matter search program that simultaneously explores many techniques. The CDMS experiment continues to collect data in the Soudan mine. The 25-kg SuperCDMS, an upgrade of the present experiment, would consist of seven supertowers of cryogenic detectors. There is now an agreement to move ahead with two of the seven supertowers needed for the experiment. Noble-gas experiments are building larger detectors and also presenting interesting results from prototype detectors. These experiments are now exploring a very interesting regime of cross-sections sensitive to models of Supersymmetry. The direct search for axions is continuing, and the GLAST satellite will soon be launched to search for signals of dark-matter annihilation.

In the neutrinoless double-beta decay experiments, the Enriched Xenon Observatory (EXO) installed a 200-kg chamber, associated clean rooms, and cryogenics underground in Waste Isolation Pilot Plant (WIPP) this past summer. Still to be installed is the time-projection-chamber (TPC) detector and other elements of the detection system. It is important that EXO be funded to complete the detector and start a physics run for the next few years, initiating this promising approach. The science team will simultaneously continue its R&D on barium tagging, which is required to make more-sensitive measurements with a larger volume of xenon.

Among the longer-term projects, the ILC engineering design remains on track with an expected completion date of 2010. A Research Director for the ILC detector program, S. Yamada, has recently been chosen. A call for letters of intent by groups wishing to prepare detailed designs for detectors by 2010/2011 has been issued.

R&D for the LHC luminosity upgrade is moving ahead internationally. A construction plan is likely in 2011 to 2012. A detailed plan for the U.S. contributions is needed.

Planning for DUSEL is moving ahead on schedule with the Homestake Mine chosen as the site. A possible start date is 2011, but this is dependent on a number of factors, among them the NSF MREFC process. An early small-scale experimental program can be launched in FY09, given special funding from the State of South Dakota and private sources.

JDEM has been recommended for NASA priority by an NRC committee, which will hopefully clear the way for rapid progress on this project. It could start construction in the 2009 or 2010.

LSST has just completed its NSF Conceptual Design Review and has been recommended to proceed to a Preliminary Design Review. LSST is likely to be recommended by the NSF to be advanced to the Readiness Stage by spring 2008. Considerable work has been done by the project to find additional resources.

Since the 2006 Roadmap, P5 has provided recommendations on three new topics: Tevatron running beyond FY09, JDEM, and the recommendations of the HEPAP University Grants Program Subpanel.

The Tevatron continues to be the leading accelerator collecting data at the energy frontier. Impressive improvements have been made, which motivated P5's recommendation that the Tevatron continue running in 2009. At its meeting at Fermilab in September, P5 discussed the criteria for extending the Tevatron running beyond 2009. In this case, an additional element in the discussion was the impact of additional data when a large amount of data had already been collected at the Tevatron. The experiments indicated that the researchers had made significant improvements in triggering and analysis and that this trend would continue, giving significant added value to additional data collected. The added data could be expected to allow further progress on the search for the Higgs boson, extending information over a larger mass range in the case of the simplest Higgs scenario, and providing further limits (or discovery) in the case of other models, such as Supersymmetry. Many of the improvements to the new-physics searches would be best demonstrated in the fall of 2008 after additional analysis of a larger data set. P5 therefore recommends that the option of continued running past 2009 be held open as a possibility and that the Fermilab management work with DOE on the implications of additional running. However, P5 recommends that funding for any additional running not come at the expense of the two highest-priority areas on the Roadmap and that Fermilab carefully evaluate both the physics potential and manpower situation in about a year.

The NASA-DOE JDEM is an important part of the P5 Roadmap. JDEM will be a very large step in scale for DOE investment at the intersection of particle physics, astrophysics, and cosmology because the cost could be comparable to that of a major accelerator and detector construction project. The science is very compelling, and P5 recommends that the DOE try to secure the larger of the budget projections that were looked at while formulating the Roadmap and to work intensively with NASA toward an early realization of JDEM. This project would be a good justification of doubling the HEP budget over a decade. There will be a great deal of work figuring out how this can be done.

The University Grants Program Subpanel has provided a comprehensive evaluation of the status of the University Grants Program and its critical role for the future of the field. In addition, the field's contributions to the training of scientific personnel for the long-term economic health of our country lie foremost with the universities and are based on participation in the major science opportunities available. The Experimental Particle Physics (EPP) and Particle and Nuclear Astrophysics (PNA) portfolios of the NSF continue to be impressively diverse and cover most of the key science areas of the field. Most of these projects are carried out in a successful collaboration with the DOE. The NSF is continuing on a trajectory to double funding over a 10-year period. However, despite annual increases in the HEP budget, the DOE University Grants Program remains essentially flat. As a result, immediate needs related to the LHC (especially student travel) as well as the longer-term goals outlined for the field are under unreasonable pressure. Given the evolution of the field and its science agenda, P5 recommends that the DOE work toward an increase in base grants by an amount of at least 9%, as is consistent with the University Grants Subpanel recommendations. This should be among the highest priorities for the field.

In the future, P5 should look at a possible phasing and make specific recommendations early next year regarding an increase in base grant support. It should also evaluate new ideas from Fermilab regarding a high-intensity-proton machine (called Project X) that integrates with the ILC and also a plan for future activities being generated at SLAC. Updated profiles for the new construction projects DES, Daya Bay, and NOvA should be available next year, as well as further plans for JDEM. These should all go into a new evaluation given funding projections.

Dehmer asked whether the grants program was within the scope of P5; one of the Ps in P5 is Projects. Seiden replied that the scope covers anything over $50 million. If the projects are to suceed, one must worry about how that is to happen. Also, if P5 is to do a roadmap, items that cost $8 or 9 million a year have to be included.

In regard to the Tevatron running after 2010, Eno noted that improvements in the analysis could be applied to the data already taken.

Cushman asked how many more small experiments could be axed. Seiden replied that NOvA could be delayed and funding for Project X could be delayed. Fermilab would have to come up with resources by redirection. ILC R&D would be untouchable for funding additional Tevatron running. Shochet noted that this is P5's assessment of the physics reach.

Samios noted that there is $50 million per year more in the optimistic scenario. That funding would be eaten up by JDEM. Seiden said that he believed that that funding would allow JDEM to happen sooner.

Cahn found it hard to evaluate the P5 report in the absence of budgetary numbers. The next P5's baseline will be $100 million less than the current baseline. He asked what the trade-offs were. Kovar responded that one can look at the long-range plan. The question is, is there going to be a new roadmap each year? If so, that is not much of a roadmap. HEPAP needs to develop a plan for the next 10 years. The P5 roadmap should be able to show how all the parts fit together over the next 10 years. It needs to tell what the scientific priorities are, not worrying about $2 million here and $2 million there. Seiden stated that the scientific priorities are unchanged. All the budgets were sent to Glen Crawford and not made public. Year-by-year dollars are not cited. A major challenge is what to do about JDEM, which may or may not come to fruition. Kovar replied that P5 would have to seek guidance and make adjustments. Cahn pointed out that the charge requested an annual reassessment, demonstrating that reassessments are necessary to adapt to changing conditions.

Shochet asked if the Committee was in favor of accepting the report. The Committee unanimously approved the report. Shochet commented that Seiden had done an incredible service to the community during the past 5 years.

A break was declared at 3:55 p.m. The meeting was called back into session at 4:30 p.m. **Young-Kee Kim** was asked to review the Fermilab strategy, the steering group for which was commissioned in late March 2007. The final report came out in September and was presented to P5.

The LHC program is Fermilab's most important near-term project, given its broad science agenda and potential for discovery. It is essential to support the physics analysis, computing, and accelerator and detector upgrades.

The particle physics community's highest priority for investment toward the future is the ILC, based on our present understanding of its potential for breakthrough science. Fermilab will continue to participate vigorously in the international R&D program for the ILC and to be one of the leaders in the global ILC effort. The Laboratory will strive to make the ILC at Fermilab a reality by accomplishing the preparatory work required for the United States to bid to host the ILC.

There is a need for a physics program in case the timeline for ILC is stretched out. The program should provide great discovery potential, support ILC R&D and industrialization, as well as conduct R&D on future accelerators beyond the ILC and the LHC. It should strengthen ties with the university community and with other laboratories. The plan must be robust and flexible.

Fermilab will continue a phased program of particle astrophysics, including work on dark matter and dark energy. These nonaccelerator-based efforts are outside the Steering Group's charge, and are not included in this plan.

The big questions in physics were summed up by HEPAP in 2004 in *The Quantum Universe*. These questions will be probed at the energy frontier, at the intensity frontier, and with nonaccelerator tools (e.g., telescopes and underground experiments). The energy frontier looks at proton-antiproton, proton–proton, electron–positron, and muon–antimuon interactions. The intensity frontier focuses on intense neutrino, muon, kaon, etc. beams and in B-meson and tau-charm factories.

At the Tevatron, a paper is being published every 1.5 weeks. The work is very productive. The Laboratory is involved in accelerator and detector work for the LHC and supports the U.S. Compact Muon Spectrometer (CMS) community. It has a remote operations center, a Tier-1 Center, a physics center, and about 30 offices in Wilson Hall for U.S. CMS universities. It also has large ILC R&D efforts. It provides beams for several experiments now and it plans more in the future (e.g., NOvA).

In the Standard Model, flavor deals with the fermion sector of elementary particle physics. Beyond the Standard Model, flavor phenomena cover a wide landscape: flavor-changing neutral currents, new flavors, and CP violation in gauge/Higgs couplings.

Flavor physics complements new-physics searches at the LHC. New terascale LHC discovery will raise flavor and unification questions most likely not accessible or only crudely accessible at the LHC. Flavor programs could measure systematically the new flavor-violation- (FV) and charge-parity-violation (CPV) couplings (i.e., the flavor structure of the new physics) and distinguish supersymmetry-breaking mechanisms. Flavor physics *is* unification physics. Flavor physics goes far beyond the LHC. New physics at scales beyond the LHC could give measurable flavor effects, and flavor programs would offer a unique opportunity to explore up to ~1000 TeV.

Electroweak symmetry breaking (EWSB) is intimately related to flavor. If there were no EWSB, fermions would degenerate, and there would be no visible flavor effect. In most EWSB models, flavor plays a key role. At the energy frontier, the gauge sector would include the Higgs boson and electroweak symmetry breaking. At the intensity frontier, the flavor sector would include mixings, masses, charge-parity violation, flavor-changing neutral currents, lepton flavor violation, and electric dipole moment, inter alia. Flavor addresses (1) the big questions that are driven by experimental facts and involve proven shortcomings of the Standard Model; (2) the big questions driven by theoretical curiosity that will evolve with new data; and (3) some big questions that still lack a solid, calculable, theoretical framework for their formulation.

Empirical proof that the Standard Model is incomplete is offered by neutrino masses, dark matter, and the baryon asymmetry of the universe. At least two of these are directly related to flavor.

Investigations of neutrino masses have produced much excitement. They have produced the only new physics seen so far in the laboratory, and they provide direct access to new physics. For unification, the existence of neutrino masses and mixings implies the breaking of a symmetry (neutrino flavor) and points toward new symmetries (unification) and new breaking of symmetries [charged lepton flavor violation (CLFV) and lepton CP violation]. Supersymmetry plus the neutrino see-saw mechanism implies CLFV. Supersymmetry plus neutrino see-saw plus CLFV would reveal key aspects of the unified origins of matter. For cosmology, neutrino masses produce an extra CP violation in the neutrino sector.

In the baryon asymmetry of the universe, the possible scenarios include electroweak baryogenesis (which will be tested at the LHC and ILC) and leptogenesis (lepton-driven baryon asymmetry, which is strongly suggested by the same ideas that link neutrinos to unification).

This intensity frontier relates to six of the big questions. It can be aligned with the ILC. Several facilities are being used in ILC R&D, specifically the superconducting radio frequency (SRF) linac. The Fermilab facilities also can be used in DUSEL and NOvA.

Project X has an 8-GeV proton linac with ILC beam parameters. The front end of the linac has no alignment with the ILC, but most of the linac would have ILC-identical components and would be a vehicle for national and international collaboration.

Regarding proton-beam power, currently the Main Injector Oscillation Search (MINOS) gets 200 kW. Upgrades will increase that to 700 kW and 1200 kW for NOvA and SuperNuMI (SNuMI). Project X would increase the beam power to 2.3 MW and possibly much higher with a main-injector upgrade.

A few examples of Project X programs are related to neutrinos, muons, and neutral and charged kaons. It would address the value of $\sin^2 2\theta_{13}$, whether mass ordering is normal or inverted, and CP violation. NOvA will be competitive with the T2K experiment. The ability of NOvA to determine the neutrino mass hierarchy is unique.

Project X is being compared to the Japan Proton Accelerator Research Complex (JPARC) upgrades and would have competitive sensitivities capable of observing $\sin^2 2\theta_{13} \neq 0$ at a significance of 3σ. The JPARC upgrades would produce a sensitivity of 2 to 3σ. Quite apart from their relative sensitivities, the Japanese and U.S. programs would operate under different physical conditions. In the U.S. program, there could be a higher beam energy, a wide-band beam, and a single large detector (possibly using liquid-argon technology) 1300 km away. In the Japanese program, there could be a lower beam energy, a narrower-band beam, and either a single large water-Cerenkov detector 300 km away or a split version of this detector with part of it 300 km away and the rest in Korea, about 1000 km away.

With muons for charged leptons, flavor violation could be probed with the $\mu \rightarrow e\gamma$ transition and the $\mu \rightarrow e$ conversion. The Project X reach in $\mu \rightarrow e$ conversion would be greater.

In kaons, there are rare decays, and there are several models. Project X would enhance the measurements of rare decays, measuring small deviations from the Standard Model, which are of great importance. Project X is directly complementary to the central physics program at the LHC. The experimental focus would be theoretically and experimentally clean with small errors at about 1000 kaon events.

The planned roadmap is based on Fermilab's highest priority: discovering the physics of the terascale by participating in the LHC, being one of the leaders in the ILC's GDE, and striving to make the ILC at Fermilab a reality. Fermilab will continue its neutrino program with NOvA as a flagship experiment through the middle of the next decade.

If the ILC is built in the United States and if the ILC remains near the timeline proposed by the GDE, Fermilab will focus on the above programs. If the ILC departs from the GDE-proposed timeline, Fermilab should also pursue neutrino-science and precision-physics opportunities by upgrading the proton accelerator complex. If the ILC start must wait for a couple of years, the Laboratory should undertake the SNuMI (an upgrade of NuMI) project. If the ILC postponement would accommodate an interim major project, the Laboratory should undertake Project X for its science capability and ILC alignment.

If the ILC is constructed offshore, Fermilab should pursue neutrino-science and precision-physics opportunities by upgrading current proton facilities while supporting the ILC as the highest priority. The Laboratory should undertake SNuMI at a minimum. Alternatively, the laboratory should undertake Project X if resources are available and ILC timing permits.

In all scenarios, R&D support for Project X should be started now, with emphasis on expediting R&D and "U.S." industrialization of ILC cavities and cryomodules as well as overall design of Project X. R&D for future accelerator options concentrating on a neutrino factory and a muon collider should be increased. The laboratory should support detector R&D and test-beam efforts for effective use of future facilities.

A roadmap was not put in the report, but during the next few years, R&D would be conducted for Project X. Project X would then be built to probe the magnitude of θ_{13}. Different experiments would be needed, depending on that magnitude.

In conclusion, the Steering Group plan gives the highest priority to energy-frontier physics with the LHC and the ILC. If the ILC is delayed, the Steering Group's plan keeps Fermilab and U.S. particle physics on the pathway to discovery in the domain of the physics of flavor (neutrinos and precision physics), while advancing the technology of the ILC. Project X would provide unique experiments to address these profound questions, would serve many scientific users, and would prepare future generations of U.S. particle physicists to exploit the potential of accelerator-based scientific opportunities in the United States and worldwide.

Many proposals have been issued, and the Steering Group is communicating with the community. Workshops are being held on

1. accelerator physics and technology to discuss issues related to Project X and to explore possible areas of overlap and interest between various particle accelerator laboratories and universities and
2. physics to discuss the big questions addressed by the intensity frontier and the energy frontier–intensity frontier connection.

Another physics workshop will be held in January. Fermilab is now asking for support for Project X R&D.

Randall asked what the costs were for R&D and Project X. Kim replied that deriving those costs was the purpose of the workshops. R&D is expected to cost about $50 million.

Seidel asked what the main-injector upgrade would entail. Kim reviewed the issues involved with an extremely high beam current.

Wormser asked what would happen if the ILC were delayed. Kim replied that that is not clear. The concern is understood, and how the two programs can be integrated is being looked into. Shochet pointed out that the Fermilab purpose is to have the ILC built there. Kim added that Fermilab has been putting 170 FTEs [full-time-equivalent employees] on infrastructure, pioneering the future. That is a positive sign.

Wormser asked if the cryomodules could be purchased. Kim responded that the United States wants to host the ILC and therefore needs to have core expertises. Having the cryomodules built in the United States is primary to that expertise. U.S. industries need to be engaged in this effort. Not everything can be done by one country, though.

Masaiki asked what international collaboration means for Project X. Kim replied that Fermilab is using the ILC collaboration. It views Project X as a U.S. project but would welcome collaboration from other countries. The front end would have nuclear physics as well as high-energy physics interest. Barish noted that the details of cooperation are not yet worked out. There may be competition for funding and human resources between Project X and the ILC. Whether or not there will be such competition is, as yet, unknown.

Carithers said that there would be two requirements, an R&D plan and a more formal statement of the physics case, detailing how the physics would be delivered and what physics would be delivered. Kim said that that will be done. Shochet said that there also needs to be an assessment of the available resources at Fermilab and whether those resources can support ILC *and* Project X. Kim agreed.

Dehmer said that it is true that Project X competes with ILC for resources, but it also provides an asset for ILC, making it more robust against failure modes. As long as the science is first class, Project X would be a worthwhile asset.

The Committee was asked to review the new P5 charge. Cushman asked what the third bullet point [funding above the previous level] meant and asked whether the Subcommittee gets to define a funding increase. Kovar said that the Subcommittee could bring in several projects that could be supported for Subcommittee-specified funding increases.

Cahn stated that this is actually a charge to HEPAP. Kovar agreed. Cahn said that the P5 report has to have more detail than has been provided in the past, including detail on the science. Shochet said that this process will be conducted over three months but should have more quantitative information and references in the scientific case.

Eno asked how the P5 membership is being selected. Shochet said that subpanel membership is decided by the funding agencies, the HEPAP Chair, and the subpanel chair, taking balance into account. The membership of the new P5 should be made public in about a week. The due date for the P5 report has been changed from March 1 to March 15. This is a compressed time scale to inform the FY10 budget request.

Wormser stated that the language is vague. For example, he could not understand what was meant by internal and external factors. Shochet stated that the language is not firm but is more explicit than previous versions had been. Kovar said that the way that this charge is written gives the Subcommittee flexibility. For example, the Subcommittee could put in a scenario in which the ILC starts late, and it could also look at benefits of an earlier startup. The Subcommittee will need to give the agencies some elements to work with. An all-or-nothing answer is not wanted. The funding profiles are not known. A good story is needed to position the agencies to do great science, a story that can compete with climate change, the energy crisis, and other interests and needs.

Dehmer noted that this charge is not to schedule the startup of the ILC but to provide the intermediate part that is missing from the business plan. This is a tool to reinvigorate high-energy physics funding to vigorous growth.

Raubenheimer said that the national core competencies need to be addressed, not just the Fermilab competencies. Kim added that one needs to talk about the science to be delivered and the capabilities needed to deliver that science. The products need to be highlighted. The reason Fermilab is highlighted is that the country needs to decide if it wants an accelerator program or not. Fermilab is the only accelerator laboratory left. Raubenheimer asked if all accelerator capability was to be focused in Fermilab. Kovar answered that, after 2009, Fermilab will be the only high energy physics user facility in the United States, although there are other accelerators. A case needs to be made for what is needed.

Randall asked if there would be any overlap in the membership of the old and new P5 subcommittees. Shochet said that there would be; about one-third of the new P5 would be members of the old P5. This was being done to make sure that there was institutional memory.

Eno asked if P5 would stop after April. Shochet answered that, in the past, P5 lasted 2 years and then responded to additional questions. The charge does not specify a term. Dehmer added that this is a very important charge. The Committee should focus on it. There may be more charges.

Cahn pointed out that the next HEPAP meeting occurs before the work of P5 will be completed. Shochet said that an intermediate report would be presented at the next meeting. Given the Committee’s schedule, it may have to approve the final report by mail. The next meeting is in February, and the following one will be in late May. Kovar pointed out that HEPAP should have an opportunity to discuss the P5 report. The interim report on March 15 will probably be very informative and give the lay of the land.

Lykken asked how the agencies wanted HEPAP to deal with the large cost uncertainties. Kovar replied, the best way that it can, explaining the uncertainties and assumptions.

Kephart said that it seems that there has to be an evaluation of human resources, which is not in the charge. Kovar replied, if it is important, put it in. Shochet stated that this story will have to be woven together under a number of scenarios. There will be a full discussion at the February HEPAP meeting. Samios offered to host the meeting at Brookhaven National Laboratory. Shochet said that the three P5 meetings will be held at Fermilab, SLAC, and Brookhaven.

Carithers commented that it will be a watershed period around 2010 with the first LHC results. Before that, the community is just groping. This report should describe what should be done until we get a view of the landscape at 2010. Kovar said that a 5-year plan needs to be provided to Congress along with the FY09 budget request. The community needs a strategic plan and a budget analysis. The scientific opportunities that will be missed without investments need to be pointed out. Shochet said that the new P5 plan will start with the old P5 plan and build in branch points for when new information becomes available. Carithers commented that the plan must have a lot of ifs and whens. Shochet said that several big-ticket projects will come in during the 10-year plan, and it must have a lot of branch points to adjust to how the science comes in. Kovar stated that Under Secretary Orbach wants a guide to great science for the next three to four decades. This 10-year horizon is much easier.

The meeting was adjourned a day at 6:26 p.m.

Friday, November 30, 2007

The meeting was called to order by Chairman Shochet at 8:33 a.m. An open phone meeting of HEPAP in March was discussed so it could have input to the P5 deliberations. However Kovar said that approval of the P5 report at the May HEPAP meeting would be OK.

Stavros Katsanevas was asked to describe the European Strategy for Astroparticle Physics.

The Astroparticle Physics European Coordination (ApPEC) was created in 2001 by the national funding agencies of France, Germany, Italy, the Netherlands, and the United Kingdom. Since then, Spain, Belgium, Portugal, Greece, Switzerland, and Poland have joined. ApPEC aims to

1. Promote and facilitate co-operation within the European Particle Astrophysics (PA) community;
2. Develop and promulgate long-term strategies for European PA, offering advice to national funding agencies and the EU;
3. Assist in improving links and co-ordination between European PA and the scientific programs of organizations, such as CERN, ESA, and the European Organisation for Astronomical Research in the Southern Hemisphere (ESO); and
4. Express their collective views on PA in appropriate international fora, such as the Organisation for Economic Cooperation and Development (OECD), United Nations Educational, Scientific and Cultural Organization (UNESCO), etc.

ApPEC has a Steering Committee with the executives from national agencies and a Peer-Review Committee (PRC).

Since 2001, ApPEC has been able to raise the profile of astroparticle physics in Europe, as witnessed by the increasing numbers of national funding agencies that have joined. Since 2003, the PRC has reviewed and issued recommendations on all major fields of PA, creating a climate of convergence among competing projects. Since 2004, it has obtained EU funds (7.5 million €) for networking and joint activities in the context of ILIAS (Integrating Large Infrastructures for Astroparticle Physics). In 2005, the successful km^3-scale neutrino telescope (KM3NeT) Design Study [funded by the EU at 9 million €] came out of this process. In 2005, the PRC was charged to draft a roadmap proposition to the community and the agencies that would link to the CERN European Strategy document. ApPEC obtained EU funds for further interagency coordination for the European Research Area Network (ERAnet) program ASPERA, a European network of national government agencies responsible for coordinating and funding research in astroparticle physics. Funding is at the level of 2.5 million € over 3 years, starting in July 2006. It has the objectives of studying funding and evaluating astroparticle physics in Europe. Members will study each others' systems through informal visiting committees; identify formal and legal barriers to inter-European coordination; define a roadmap on infrastructures and R&D; explore further linking of existing astroparticle infrastructures; examine relations to existing laboratories (e.g., CERN and particle physics laboratories); and launch common actions (e.g., a common fund for design studies).

The Governing Board (GB) is responsible for all management decisions of the network and for approval of all documents. (There is an overlap with the ApPEC SC.) The Joint Secretariat (JS) assures the day-today follow-up of the program. The PRC is responsible for the evaluation of the network's activities.

The roadmap was drafted in November of 2006 and covered high-energy gamma rays, high-energy cosmic rays, high-energy neutrinos, gravitational waves, dark matter, neutrino mass, low-energy neutrinos, and proton decay. Not included were space and dark energy; this decision will probably be revisited. The Phase I Roadmap was published in May of 2007.

Seven working groups prepared timelines of 55 astroparticle projects, detailing financial and human resources needed, milestones, enabling R&D, risks etc. In parallel, a detailed comparison of resources available in each agency was evaluated. First comparisons were presented in a workshop in September 2007 in Amsterdam. The workshop was also the occasion to have a first comparison with non-European strategies (NSF, DOE, and China). Currently, the working-group proposals are being evaluated by the PRC for science goals and technical readiness. Phase II will end with a workshop in Paris or Brussels in autumn 2008.

It was found that the European full-time-equivalent (FTE) employment in astroparticle physics was about 3000 people and that the investment was about 70 million € per year. The wish list for research exceeds the current funding by a factor of 3.

In the high-energy universe, gamma rays, cosmic rays, neutrinos, and gravity waves bring information on the origin of cosmic rays, sites of extreme phenomena for testing the fundamental laws of physics, sites that could become cosmological markers or probe the intergalactic space or even the space-time fabric itself, and sites of annihilation of dark matter. It is a task for physicists to bring new detection techniques to maturity, opening new windows of astrophysical investigation. The promise of opening windows to the universe is being fulfilled. The physics are big and rich. This community has a lot of infrastructure.

In high-energy cosmic rays, there is also a lot of infrastructure; central is Auger. The investment in ASPERA is 10 million € per year and 150 FTEs.

High-energy neutrinos are crucial. Currently, ANTARES [Astronomy with a Neutrino Telescope and Abyss environmental RESearch] has five lines. Another five will be connected within a week.

KM3NeT will start construction in 2011. It will image the galactic center.

A second model of world collaboration is the LIGO/Virgo Project/GEO 600 Project network for gravitational waves. Coherent analysis would produce a sensitivity increase, source-direction determinations from time-of-flight differences, polarization measurements, and a test of gravitational-wave theory and gravitational-wave physical properties through a LIGO/ Virgo Project/GEO 600 Project 4-month common run. The processes are now all completed, and the timescales have been coordinated.

Underground science would take one to 10^{-44} seconds in the history of the universe. There are six underground science laboratories in Europe: Gran Sasso, Sunlab, Centre for Underground Physics, Institute of Underground Science, Laboratoire Souterrain de Modane, and Laboratorio Subterraneo de Canfranc.

The dark-matter (DM) experimental status shows rapid evolution of the sensitivity of discriminating experiments, rapid progression of the cryogenic detector experiments [CDMS, Cryogenic Rare Event Search using Superconducting Phase Transition Thermometers (CRESST), and Expérience dont le but est la Détection des Wimps (EDELWEISS)], and impressive progress by liquid-target dark-matter experiments [XENON, WIMP Argon Programme (WARP), and ZonEd Proportional scintillation in LIquid Noble gases (ZEPLIN-II)]. The 10^{-8} pbarn SUSY-rich region should be reached within two years, but still more than 2 orders of magnitude of progress in sensitivity are still required compared to best present sensitivities. The goal is to attain about 10^{-10} pbarn by 2018. The PRC recommends pursuing three parallel experimental lines: argon, xenon, and cryogenic detectors. They recommend two dark-matter experiments (costing 20 million € and 70 FTEs per year), but will have to see how much money is available.

The KArlsruhe TRItium Neutrino (KATRIN) experiment is studying neutrino mass through single-beta decay. The Germanium Detector Array (GERDA) is studying neutrino mass through double-beta decay. The Cryogenic Underground Observatory for Rare Events (CUORE) will start operation in 2011. SuperNEMO [NEutrino Mediterranean Observatory] will have a 20-module tracking calorimeter with the first modules operational in 2011.

Neutrino mass can be investigated with isotope enrichment, which is being pursued by two routes: ion-cyclotron-resonance and laser-isotopic separation, which will be part of the underground laboratory extensions). It is hoped that the magnitude of θ_{13} and the mass hierarchy of the neutrino will be known by 2015.

The Large Apparati for Grand Unification and Neutrino Astrophysics (LAGUNA) is a successful EU Design Study that incorporates three large underground detectors, one water Cherenkov, one liquid scintillator, and one liquid argon, that work together in common. The R&D on three continents is coordinated through major conferences. The technology is there, but the costs must be lowered.

The Space Program just prepared its plan, calling for five space missions. The Dark Universe Explorer–SPectroscopic All-sky Cosmic Explorer (DUNE/SPACE) combination was ranked very high, and the proposers were asked to define a common mission in 2008. By September of 2009, two missions and two launches will be selected; and by 2011, downselection will be made to one mission and one launch. LISA/Dark Energy are common priorities across the Atlantic, leading to sharing.

ApPEC/ASPERA will

- Issue general priorities by autumn 2008,
- Form a common fund for design studies,
- Form a PAC in 2009–2010,
- Coordinate with the CERN strategy group,
- Discuss relationships of astroparticle physics with CERN and other particle physics laboratories,
- Coordinate with the astrophysics community (ASTRONET), and
- Handle the delicate problem of eventually different priorities.

Coordination with other regions must be carried out through such organizations as the OECD, Funding Agencies for Large Colliders (FALC), and the Particle and Nuclear Astrophysics and Gravitational International Committee (PANAGIC).

The European astroparticle physics roadmap process will define priorities, resolve emerging priorities (e.g., on ground or underground), consider the convergence of astrophysics space missions, and allow interregional coordination.

Cushman asked how one avoids overlap. Katsanevas replied that that occurs at the science level and that he was talking about the agency level. Cushman asked how the money flowed. Katsanevas replied that the agencies operate under an ambiguous relationship. There is money for organizations, but not for infrastructure or operations, just coordination. At meetings, money is awarded to several projects that share a common interest.

Crawford asked about the European Strategy Forum on Research Infrastructures (ESFRI). Katsanevas said that there is a committee to which one can promote projects. It is similar to the MREFC process for proposing projects. It facilitates interactions with the national agencies and governments.

As an introduction to the next two presentations, Shochet noted that the great majority of high-energy physics is publicly funded and should be openly available to the public. The following two presentations describe the issue of open publishing.

Gene Sprouse noted that the APS publishes the *Physical Review Letters* and the *Physical Review*, which are fully international journals whose submissions are rising linearly and whose revenues and reviewers are distributed evenly among the United States, Europe, and Asia. Revenues come almost entirely from libraries. The cost per article varies from $1or 2 (in an APS journal) to $33 per article.

Open access is when articles are available without barriers somewhere on the Web (APS copyright form allows this for a final published article, at the author's discretion), or the publisher's content is completely available from the publisher's site without barriers, or something in between.

All APS journals are open access according to that definition. There are two other journals that are open on our site without barriers. One, begun in 1998, is funded by sponsorship: *Physical Review Special Topics – Accelerators and Beams*. APS does not fully recover its costs; but because the journal is small, APS can absorb the loss. A new special topics journal, *Physics Education Research*, is funded by author or author's-institution charges.

Overhead charges were added for *Physical Review Special Topics – Accelerators and Beams* in 2005, and submissions have increased significantly since then. In 2004, the composition of Physical Review D was offshored; that step and digital submission have lowered costs well below revenues.

There are different models for subscriptions: under the current practice, libraries pay most of the revenues. For a library to supply content, it has to subscribe to all needed sources. The publishers have no incentive to cut costs. If the costs were borne by authors, authors would migrate to publishers with lower prices, subscription rates would fall, and standards would erode. If the revenue came from some other source and publications were supplied digitally, libraries would be cut out, publishers would prosper, and authors would be happy.

APS has a responsibility to publish good physics in all fields. To be able to continue to do this, the institution must remain financially viable. Any open-access initiatives must be sustainable with strong prospects for long-term support and they must be reversible in case the initiative is not successful and the publisher needs to revert to subscriptions. If these conditions are met, APS will consider making content available "Free to Read" on its site.

Currently, free-to-read services cost $995 for the *Physical Review* and $1300 for the *Physical Review Letters.* With these payments, APS will make a paper freely available on the web.

Shochet asked if libraries still bought print versions. Sprouse said that only 40% of the APS journals have a print component.

Wormser asked Sprouse if he saw any movement away from peer review. Sprouse said that, although flawed, the peer-review process is the best available. In January, the APS will start an award program to recognize the top 0.5% of its reviewers.

Salvatore Mele said that, in Sprouse's scheme, he would add an arrow from the libraries to the green pot of money that is distributed to the publishers to fund open publication. This is no longer the European model but the European–United States model: Grant anybody, anywhere, and anytime access to the (peer-reviewed) results of (publicly-funded) research and contain costs.

HEP is decades ahead in thinking about open access, partly because it is a small and connected community (<20,000 scientists) producing a small scientific output (<10,000 articles per year) with a small publishing landscape (fewer than 10 journals), and reader and author communities that largely overlap. To physicists, open access is second nature: posting on arXiv before even submitting to a journal is common practice. Open access is a grassroots movement; it is not library initiated. There is strong support from the LHC collaborations.

Journals are on the way to losing their century-old role as vehicles of scholarly communication. Still, evaluation of institutes and (young) researchers is based on high-quality peer-reviewed journals. The main role of journals is to assure high-quality peer-review and to act as keepers-of-the-records. The HEP community needs high-quality journals, its "interface with officialdom." Implicitly, the HEP community supports this role by purchasing subscriptions, even as it reads off arXiv. Subscription prices make the model unsustainable. As an "all-arXiv discipline," HEP is at high risk to see its journal canceled by large multidisciplinary university libraries (when that has not already happened).

5016 articles were submitted to arXiv:hep in 2005 and published in peer-reviewed journals. 90% of those articles are in theory, and by fewer than three authors. 83% of the articles are published in six leading journals. 87% of the articles are published by four publishers, and 57% by not-for-profit (nor-for-loss) publishers.

Open-access experiments have taken on several models. In the Sponsoring Model, institutions fund journals. There are no author charges. All content is free to read. In the Hybrid Model, authors can pay journals to make their articles open access; the rest of the journal is under subscriptions, and subscription rates are reduced according to the fraction of OA articles. Springer adopted this model in 2004, followed by APS and Elsevier. Their prices range from $975 to $3,000 per article, but have met little, if any, success. In the Author-Pays Model, all content of the journal is free to read. After acceptance, authors pay journals for processing fees. This approach has been successful in the life sciences (BioMedCentral) but sustainability problems are arising. In the SCOAP3 (Sponsoring Consortium for Open Access Publishing in Particle Physics) Model, a consortium sponsors HEP publications and makes them open access by redirecting subscription money.

Today, funding bodies through libraries buy journal subscriptions to support the peer-review service and to allow their patrons to read articles. Tomorrow, funding bodies and libraries will contribute to the SCOAP3 Consortium, which pays centrally for the peer-review service. Under this arrangement, articles will be free to read for everyone, supported by a mix of sponsoring and institutional membership on a worldwide scale.

Six journals cover 80% of the central HEP literature. Five core journals [*Physical Review D* (APS), *Journal of High Energy Physics* (Institute of Physics), *Physics Letters B* and *Nuclear Physics B* (Elsevier), and *European Physical Journal C* (Springer)] carry a majority of the HEP content and 10 to 30% of the nuclear physics and astroparticle physics content. The aim of SCOAP3 is to convert them entirely to open access and to reduce the prices of "packages," accordingly. Currently, one "broadband" journal, *Physical Review Letters* (APS) publishes about 10% of HEP papers, including nuclear and astroparticle physics. SCOAP3 plans to sponsor the conversion to open access of this fraction and to reduce the subscription price accordingly. SCOAP3 is not limited to this initial set of journals but is open to all high-quality HEP journals.

How much does this cost? *Physical Review D* (APS) operates with 2.7 million € per year (31% of arXiv:hep). The *Journal of High Energy Physics* (SISSA/IOP) needs about 1 million € per year (19% of arXiv:hep). The HEP open-access price tag is 10 million € per year. A published peer-reviewed article costs APS about 1500 €, and six to eight leading journals publish 5000 to 7000 articles a year.

SCOAP3 financing is to be distributed according to a "fair-share" model based on the distribution of HEP articles per country, accounting for co-authorship. A 10% allowance would be made for developing countries who, at the beginning, might otherwise not contribute to the scheme. The model is viable only if every country is on board. Allowing only SCOAP3 partners to publish open access simply replicates the subscription scheme and does not solve the problems, the need to buy and read what others write.

A study of HEP authorship in leading journals showed that the United States (24%), Germany (9%), Japan (7%), Italy (7%), United Kingdom, (7%) and China (6%) account for more than half of the publications.

A formal proposal was published in April. It is evolving from consensus-building to fund-raising. As of July, expressions of interest were being solicited and collected from potential funding partners: HEP funding bodies, libraries, and library consortia. Funding partners identify country-by-country schemes to redirect journal subscriptions to SCOAP3. Once a sizeable fraction of the budget is pledged, a tender will be sent to publishers and the final budget will be determined. A formal agreement is needed to establish SCOAP3. The goal is to have SCOAP3 operational for the first LHC articles.

25% of the "pie" has already been pledged from Germany, Italy, France, CERN, Sweden, and Greece. Many European countries are expected to join in the next weeks. Intense discussions are being conducted in Asia and the Americas. In Asia, contacts are established with Japan, China, and Korea. In the United States, a three-pronged approach is being taken: the possible redirection of subscriptions of DOE libraries, led by Fermilab and SLAC, is being discussed; redirection of subscriptions of individual university libraries not organized in consortia is being suggested; and redirection of subscriptions by large consortia is under negotiation. Several members of the North Eastern Research Libraries (NERL) are interested in the scheme.

Shochet contended that a large number of universities and colleges are involved with subscriptions, all operating under budget constraints produced by European journal costs. Mele agreed. Libraries are impressed with cost savings. Just a few consortia are needed to cover the costs; one does not need all the small colleges. However, small colleges are jumping on board.

Cahn said that Elsevier is the heart of the issue at Lawrence Berkeley National Laboratory (LBNL). It will take more than just the physics community to change Elsevier. Also, they will charge for back issues. Mele disagreed; Elsevier would be required to reduce the cost.

Crawford said that it is not politically easy to arrange to have a government board or agency to take the money away from publishers. Also, referees are not mentioned here. He asked how the refereeing process was to be guaranteed. Shochet added that Mele seemed to be asking the libraries to provide the revenues in the United States. Mele replied that libraries are sensitive to the communities they serve. They are a natural locus to make a decision how to serve their publics in a cost-effective manner.

Sprouse asked why countries would want to take money away from their libraries. Mele replied that libraries will be using their money in different ways to provide content. If a journal is good and cheap, it will get more volume and get more business.

Bagger observed that the U.S. libraries are prepared to pay for information. The Scholarly Publishing and Academic Resources Coalition (SPARC) would be very interested.

Olinto asked if this scheme would be viable with other communities. Mele stated that it takes a close-knit community to make this model viable. However, once it has been done once, it can be readily replicated.

Brau asked if he were talking about print copies. Mele said, no, just electronic versions.

Cahn asked Mele if he were looking for something from HEPAP. Mele replied, yes, at several levels, HEPAP members can bring this idea home to universities, consortia, and national laboratories. They should tell them that this is a good thing to do. HEPAP could also give it its blessing.

Shochet pointed out that the CLEO collaboration had asked to make a presentation to HEPAP and give a status report of the collaboration. **David Asner** would make the presentation.

CLEO-c begins its final data run on Dec. 7, 2007, and data taking will end on March 31, 2008. The collaboration is still strong and excited about its future. The CLEO-c collaborators will need support for graduate students and post-docs to complete analyses, produce Ph.D. theses, and publish papers.

CLEO-c is a unique high-precision, low-energy probe of charm quark physics. Charm production at threshold is ideal for many important measurements, some of which cannot be done elsewhere or cannot be done nearly as precisely. Threshold production allows clean electron-positron interactions with no additional particles produced; probing final states with one missing particle; detector coverage of 4π, with a ring-imaging Cherenkov counter for particle identification and cesium iodide-based electromagnetic calorimetry; and the "reconstruction" of missing mass. This is the best detector ever operated at 4 GeV. It takes a weak process to understand strong physics. CLEO made the first and only measurement of the $D^+ \rightarrow \mu^+\nu$ transition, the most precise measurement of the $D_s^+ \rightarrow \mu^+\nu$ transition, and the most precise measurement of the $D_s^+ \rightarrow \tau\nu$, $\tau^+ \rightarrow e^+\nu$ transition.

The CLEO-c leptonic results confront lattice quantum chromodynamics. Recent lattice quantum chromodynamics (LQCD) results give a D_s form factor of 241 ± 3 MeV, whereas experimental results give 274 ± 10 MeV, a 3.2-sigma discrepancy between the data and LQCD. With more CLEO-c data, a factor of 2 improvement is expected.

CLEO has had a broad impact on heavy flavor physics and quantum chromodynamics (QCD), producing focused and crisp challenges to theoretical techniques for QCD calculations, particularly techniques for nonperturbative QCD; measuring the f_D and f_{Ds} decay constants, validating LQCD assertions; and measuring semileptonic decays, allowing the testing of CKM [Cabibbo-Kobayashi-Maskawa] unitarity and stringent tests of LQCD.

At the LHC, measurements of CKM angles and other CP-violating and rare processes should show the effects of any new particles found. Precision electroweak results imply new physics at about 1 TeV, but precision flavor results imply new physics at 10 to 100 TeV. CERN is studying the relationship between the new physics observed at the LHC and flavor physics at a series of workshops. Validating QCD calculations is important for use in a wide variety of measurements that will elucidate the nature of this new physics. Proven techniques will be especially important if the new physics observed at the LHC has a strongly coupled sector.

In LQCD, a single formalism relates D/B to Ψ/Υ. There are more than 30 gold-plated quantities where few-percent LQCD calculations are possible. The Cleo data are the oldest and the most interesting in the search for η_c. 89% of the $\Psi(2S)$ data were recorded in 2006.

The CLEO collaboration is made up of about 110 scientists (60 FTEs) and 22 institutions. It is supported by the Natural Sciences and Engineering Research Council of Canada (NSERC), Particle Physics and Astronomy Research Council (PPARC, now part of the Science and Technology Facilities Council), DOE, and NSF. Projections of manpower show a decline in personnel: In November of 2005, there were 81 FTEs; in November 2008, it is expected that there will be 44. There will be enough manpower for data taking until March 31, 2008, and enough to continue physics analyses until 2011.

CLEO published its 450th paper in 2007. In the foreseeable future, the collaboration plans to analyze the unique CLEO-c data sample for 3 years after the end of data taking. The current CLEO results are based on a fraction of the data. It is anticipated that 70 to 90 publications and about 30 theses based on the full data sample will be published during the 3-year CLEO completion period. Cornell has submitted a grant proposal to the NSF requesting support to maintain the computing infrastructure necessary for this analysis effort. CLEO groups need base support for a physics-analysis program. Completion of the analyses will provide very precise values for many of the results.

CLEO-c has an important physics program and has done great work with a high yield per dollar. The collaboration is strong and will remain strong enough to continue into the completion era. The physics to be done in the completion era is important and will significantly improve many results. About 30 Ph.D. theses and 70 to 90 publications are expected in 2008 to 2011 (compared to 100 publications since 2003). The mature software and experienced collaboration make this physics output possible with the expected decline in FTEs, an exciting and strong finish to the CLEO physics program.

Cahn asked if it would really take 3 years to do the data analysis. Asner said that they had polled the collaborators, and they are at different stages of completion. The tail will likely go out 3 years. Cahn said that HEPAP should highlight the importance of the analysis of data after a project is complete.

Wormser asked if they were going to back up the data somehow. Asner said that there is a discussion about making the data an open archive. Most of these data samples will be eclipsed sometime after 2012.

A break was declared at 10:58 a.m. The meeting was called back into session at 11:33 a.m. to review the topics that would appear in a summary letter:

- The agency search committee
- University Subcommittee
- ILC
- Empanelling of the new P5
- Introduction of Dennis Kovar and his request for community input to a plan, showing new and lost science
- Dehmer’s new physics frontier proposals and the need for plans for the future
- Crawford’s primer on budget construction
- Sadoulet’s summary of the workshop on DUSEL’s first suite of experiments
- Primack’s relation of the Beyond Einstein mission selection of JDEM and LISA and the need for an agreement between DOE and NASA
- Bagger’s description of the Decadal Survey and the modification of that survey’s model by involving physicists
- Womersley’s positive COV report and HEP’s need for more manpower, which report was unanimously approved
- Seiden’s P5 report, listing the LHC, ILC R&D, dark energy, dark matter, and Daya Bay as the highest scientific priorities and the University Grant Program as requiring increased funding; which report was unanimously approved

- The new P5 to be chaired by Charles Baltay
- Kim's proposed path forward for Fermilab and the need for confirmation of the availability of personnel and a physics case
- A new charge to be addressed by P5, and HEPAP's review of the findings for timely transmittal to the agencies to inform budget deliberations
- European astroparticle physicists' coordination of their efforts
- Sprouse's appeal for high-energy-physics publications to be open and available
- Mele's description of the CERN model for publishing LHC papers in an open environment
- Asner's depiction of the status of the CLEO-c program, which has been very successful but needs continuing support for data analysis and the publication of findings, which will constitute the best-available measurements for a long time

Shochet asked if there were any other business to be brought before the Committee. There being none, the meeting was adjourned at 12:17 p.m.

Respectfully submitted,
Frederick M. O'Hara, Jr.
Recording Secretary
Jan. 11, 2008

Corrected – M.J. Shochet, 2/13/08

The minutes of the High Energy Physics Advisory Panel meeting held at the DoubleTree Hotel, Washington, D.C., on Nov. 29-30, 2007 are certified to be an accurate representation of what occurred.

Signed by Melvyn Shochet, Chair of the High Energy Physics Advisory Panel on February 13, 2008.