Minutes of the
High Energy Physics Advisory Panel Meeting
March 3-4, 2006
Latham Hotel, Washington, D.C.

HEPAP members present:

Charles Baltay	Angela V. Olinto
Daniela Bortoletto	Satoshi Ozaki
James E. Brau	Saul Perlmutter
Robert N. Cahn	Tor Raubenheimer
William Carithers	Steve M. Ritz
Alex J. Dragt	Nicholas P. Samios
Peter D. Meyers	Melvyn J. Shochet, Chair
William R. Molzon	Guy Wormser
Koichiro Nishikawa	

HEPAP members absent:

Joseph D. Lykken	JoAnne L. Hewett

Also participating:

Arden L. Bement, Jr., Director, National Science Foundation
Jonathan Bagger, Departments of Physics and Astronomy and Mathematics, Johns Hopkins University
Gary Bernstein, Department of Physics and Astronomy, University of Pennsylvania
Joel Butler, Particle Physics Division, Fermi National Accelerator Laboratory
Aesook Byon-Wagner, Office of High Energy Physics, Office of Science, Department of Energy
Sally Dawson, Physics Department, Brookhaven National Laboratory
Joseph Dehmer, Director, Division of Physics, National Science Foundation
Paul Grannis, Office of High Energy Physics, Office of Science, Department of Energy
Steve Kahn, Assistant Director of Research, Stanford Linear Accelerator Center
John Kogut, HEPAP Executive Secretary, Office of High Energy Physics, Office of Science, Department of Energy
Joseph Kroll, Department of Physics and Astronomy, University of Pennsylvania
Alfred Mann, Professor Emeritus, University of Pennsylvania
John H. Marburger, III, Director, Office of Science and Technology Policy, Executive Office of the President
Marsha Marsden, Office of High Energy Physics, Office of Science, Department of Energy
Hugh Montgomery, Associate Director for Research, Fermi National Accelerator Laboratory
Hitoshi Murayama, Department of Physics, University of California at Berkeley
Frederick M. O’Hara, Jr., HEPAP Recording Secretary, Oak Ridge Institute for Science and Education

Linda M. O'Hara, HEPAP Recording Secretary, Oak Ridge Institute for Science and Education
Raymond Orbach, Director, Office of Science, USDOE
Abraham Seiden, Physics Department, University of California at Santa Cruz
James Siegrist, Director, Physics Division, Lawrence Berkeley National Laboratory
Robin Staffin, Associate Director, Office of High Energy Physics, Office of Science, Department of Energy
Robert Svoboda, Department of Physics and Astronomy, Louisiana State University
Robin Staffin, Associate Director, Office of High Energy Physics, Office of Science, Department of Energy
James Whitmore, Physics Department, Pennsylvania State University
Andreene Witt, Oak Ridge Institute for Science and Education

About 95 others were also present in the course of the two-day meeting.

Friday, March 3, 2006
Morning Session

Before the meeting began, each of the attending HEPAP members was individually sworn in as a special government employee by a staff member from Human Resources, Office of Science (SC), U.S. Department of Energy (DOE). The Panel was then given an orientation session covering such issues as responsibilities and conflicts of interest.

Chairman **Melvyn Shochet** called the meeting to order at 10:07 a.m. and thanked the members for their participation. He said that this was a time of great opportunity for science. We anticipate important discoveries at the TeV scale. There is a growing appreciation of the importance of scientific endeavors in our society. We welcome the President's American Competitiveness Initiative (ACI). It is the Panel's role to vet the developed reports. He pledged to get the reports to the Panel members one month before a meeting and asked for questions in two weeks. He then asked the members to introduce themselves and state their affiliations and interests/work.

Jonathan Bagger recused himself from the Panel until April 1.

Shochet said that the Panel was honored to have three distinguished leaders address them at this meeting. He introduced the first, **Raymond Orbach**.

This is a very different year from the previous one. Last year, the SC budget experienced a 4% drop from the previous year, and SC had to make some difficult decisions and trade-offs. The Office wanted to maintain support that would maintain U.S. leadership in science. This year, the President's budget will support 2600 postdocs and researchers. In the State of the Union Address, President Bush said "I propose to double the federal commitment to the most critical basic research programs in the physical sciences over the next 10 years. This funding will support the work of America's most creative minds as they explore promising areas such as nanotechnology, supercomputing, and alternative energy sources." Bush also announced the ACI. The conjunction of the physical sciences and competitiveness is remarkable. This investment in physical science is a historic opportunity for our country, a renaissance for U.S. science and continued global competitiveness. The SC budget went up 14%. It was a quite remarkable address, a time for cheering.

This opportunity will not be given again. This proposal could double the SC budget from \$3.6 billion in FY06 to \$7.2 billion in FY16. However, one should note that this proposal would double the sum of the budgets of the three basic-research agencies [DOE, NSF, and the National Institute of Standards and Technology (NIST)]. If DOE does not respond, the monies will go to other agencies. Earmarks will decrease the level of funding in the current year and in future years, as well; every dollar lost in FY08 will be two dollars lost in FY09. This is the time to support the President's budget. As the steward of national science facilities, DOE is a player in competitiveness and energy security. The death of the Superconducting Super Collider (SSC) was a catastrophic moment for U.S. science. It meant the shift of the energy frontier to CERN, which made it less accessible to graduate students and which meant that the overall physical science budget went down.

The missions of DOE include being stewards of national science facilities [e.g., the Relativistic Heavy Ion Collider (RHIC) and the Continuous Electron Beam Accelerator Facility (CEBAF)]. As a result of private funding, RHIC is able to have a full run this year.

The consequence of the FY07 budget is that about half goes to facilities and about half to research. The funding level for facilities had decayed to 45% because of long-term flat funding. This budget will increase that percentage to 46%. This increase is intended to give the United States a lead in scientific facilities. Some of the highlights of that budget are

- The International Thermonuclear Experimental Reactor (ITER) is fully funded. This is a self-standing international agreement for a major scientific facility. It is the largest truly international experiment, with a complicated management and financial agreement. It will be the model for all future large-scale collaborations. The United States will contribute \$1.22 billion. The United States had left ITER. Then participation in it was unanimously approved at the Snowmass meeting and by the fusion energy sciences advisory committee. A National Academy of Sciences (NAS) committee under John Ahearn and a Lehman review also pointed to benefits of participation. On Feb. 10, the President announced that the United States would join ITER.
- In high-end computation, more than 250 teraflops will be provided on the floor in Oak Ridge (70 teraflops sustained), and 100 teraflops on the Blue Gene P at Argonne; the capacity of the National Energy Research Scientific Computing Center (NERSC) will be increased to 100–150 teraflops. At these speeds, scientific discovery can be done in many fields in which it could not be done before. If funding continues to increase, peak speeds will increase to 1 petaflop. Cray and IBM are major players.
- Linac Coherent Light Source (LCLS) construction continues; it will be the world's first X-ray free-electron laser and will provide an order of leadership beyond that of any other facility in the world and allow single-molecule structure determinations with a pulse rate of 350 per nsec.
- The Spallation Neutron Source (SNS) is being completed on-time and on-budget. It will be an order of magnitude more intense than ISIS.
- Four of five DOE nanocenters will begin operations in 2008, providing the United

States with resources unmatched anywhere in the world. They will be able to look at structure and dynamics.

- The International Linear Collider (ILC) would give the United States world leadership in the study of particle physics in the next decade at Fermilab. Killing the SSC allowed the high-energy physics energy frontier to move abroad. It is imperative to bring the ILC to the United States and maintain collaboration with colleagues in other regions. The ILC will be three to seven times more powerful than Fermilab. It will restore U.S. leadership in this field. The linear collider is the future of high-energy physics. This panel will help get the ILC built in the United States at Fermilab. The physics community is needed behind this promotion. On the morning of this meeting, an international ad hoc committee report offered a realistic sample site for hosting this project in Japan. A major effort must be made in United States if the ILC is to be built on U.S. shores.
- The CEBAF and RHIC are the primary nuclear physics programs in the United States. The upgrade of CEBAF will double its energy. RHIC goes fully operational in the President's out-year budgets. Supporting these facilities is what the advisory committees recommended.
- The National Synchrotron Light Source-II (NSLS-II) is slated to get $45 million for R&D and design in the FY07 budget, allowing it to leapfrog the third-generation accelerators and be the first fourth-generation machine with a 1-nm spot size. It will give us an edge in nanoscience. Because of this resolution, it will measure transition and vibrational properties at the same time. Its spatial and energy resolution will be the world's best. Nanoparticles will be grown in situ, and their properties studied. Stability is the challenge.

The previous week's issue of *Science* said that the scientific community did not support the funding of CEBAF and RHIC. However, that was an error. The Nuclear Sciences Advisory Committee (NSAC) recommended that support. Without the support of the scientific community, this funding will not be forthcoming.

The Office of High-Energy Physics (HEP) is slated to get an 8% increase in its budget from FY06 to FY07. This will make it a sitting duck when other programs are being cut. The President has committed to doubling funding for the physical sciences (the sum of DOE SC, NSF, and NIST budgets) over 10 years.

The increase in the user facilities' budgets will enable them to run at optimum levels. The increase in research funding is "starting to right the ship" that was teetering when inflation eroded its ability to perform. The budget increase is balanced between facilities at 51% and research at 47%.

In closing, Orbach quoted what he had said at the Congressional hearing on the budget: "We are indebted to the President for his foresight in recognizing the vital importance of America's continued leadership in the physical sciences to our nation's global competitiveness position in our quest for greater energy security. We are committed to holding up our end of the bargain by delivering truly transformational science and technologies – breakthrough advances that will provide new pathways to energy security and ensure America's continued global economic leadership in the years ahead."

Peter Meyers asked what lessons were learned from the ITER experience that could be applied to ILC. Orbach replied that a major lesson was that everybody is a partner until the money is put on the table. For example, Clavendon in Canada was considered the best site for ITER but Canada dropped out. The host country is expected to provide half the funding. One does not know who the partners really are until money is on the table; the Japanese are vital competitors. ITER is an incredible machine and has incredible costs: $500 million. The Europeans have different ways of doing numbers. The real test will be to see who the real parties are, the European Union or individual countries. It will be 14 years before ITER is completed.

Shochet asked for lessons from earlier projects. Orbach replied, cost issues.

Samios emphasized the importance of professional societies' support. Orbach responded that support from both the American Physical Society (APS) and the public were important. This is an exciting time for scientific research.

Wormser commented that France followed trends of the United States after a 5- or 6-year delay. It is important for the United States to make a firm statement on the future of high-energy physics and to state the intentions of the United States as the lead. Worldwide sharing is important in both space and time. Orbach replied that science has always been global. CERN [Conseil Européen pour la Recherche Nucléaire] and KEK [High Energy Accelerator Research Organization] need the United States. CERN needs to be given credit for its constancy in effort and support. The United States has always welcomed everyone at our facilities. The EU is struggling with the issue of user fees. There is a danger that this could create a backlash. If one country applies a user fee, then others would also feel the need to do so. It would ruin the fluidity of scientific research. High-energy physics has been open to everyone.

Cahn said that he was glad to hear Orbach's enthusiasm for ILC but that he was concerned about some phrases he was hearing. He did not yet have a copy of the report. One must consider the future if the United States does not get the ILC, whether for fiscal or international reasons. The case for its importance has to be built for colleagues outside high-energy physics. Orbach stated that that case building is the responsibility of the Panel. There are potential off-ramps. If there is a delay, one may be too late. One needs to take a risk. See the example of the Fusion Energy Sciences Advisory Committee (FESAP) ITER report and the Snowmass meeting. One must be ready to go when the science report recommends it.

Ritz said the priorities must be set and clearly stated and asked if there were any dos and don'ts so for effectiveness. Orbach replied that Robin Staffin would speak to that point.

Shochet introduced **John Marburger**. Marburger thanked the Panel for the reports on the quantum universe and said that the most recent, "Discovering the Quantum Universe," made the case for continuing the quest for the fundamental constituents of nature in a very appealing way. He asked for feedback on reaction to the reports and spoke of how to generate support for science.

He said that the President's American Competitiveness Initiative and the Advanced Energy Initiative both came after the NAS/Augustine report (*Rising Above the Gathering Storm*), but he did not believe that they were a response to that report. He said that many other reports also provided the context. The ACI expands federal funding for selected agencies with physical science missions, offers tax initiatives for industrial investment in research, improves immigration policy for people with advanced degrees, and has a strong education initiative. To benefit the physical sciences, there needs to be a permanent extension of the tax incentive for research and experimentation (it expired last December). Industry needs more than a year-by-year extension.

There is now a 9.3% increase in the budget for physical science (for DOE, NSF, and NIST), but it excludes technology transfer programs. Federal funding has been flat for more than a decade after the abrupt drop in defense funding in 1991, when the Cold War ended. The Galvin Report pressed the national laboratories for long-range plans (DOE is much further ahead in long-range planning than the Department of Defense). The recession in 1991 plus a link with putting money in high-energy physics when the Cold War ended weakened funding, and major forces came together to end the SSC. House Science Committee Chair George Brown said that the physical science community needed to make a new case for physical science research. Newt Gingrich spoke of the Endless Frontier. The report of Congressman Vern Ehlers reflected input from many sources and used economic competitiveness as a justification for science funding. (The report is available on the Office of Science website.) Attention needs to be paid to the growing gap in support for biological and physical science. Biology has recognized the need for physical-science research. Varmus's editorials on the need for physical-science research set an important precedent.

The 2002-2003 President's Committee of Advisors on Science and Technology (PCAST) report *Assessing the United States R&D Investment* came at the same time that the dot-com bubble burst and 9/11 placed pressures on the budget. The Bush Administration did expand funding for targeted areas along with a doubling of the National Institutes of Health (NIH). Now the ACI is emphasizing the importance of investing in the physical sciences. The NAS/Augustine report is a most visible proponent of this. It needs to be emphasized that science is likely to produce economic benefits, especially in nanoscale understanding and basic science. The nano-info-bio convergence is important to future economic growth. Increasing budgets should increase the vigor of space science but not emphasize it; their current funding is flat or negative. There are 56 space-science missions currently flying; there is substantial continued funding but there are budget problems with the Shuttle/Space Station that are not addressed by the ACI. The ACI funds fields with a likely economic advantage. One needs to understand the philosophy behind ACI to make the case for public funding. The case made in *The Quantum Universe* report sets the right tone. ACI aims to strengthen fields more characteristic of Basic Energy Sciences but is neutral toward high-energy or nuclear physics. There is an expectation of support for Basic Energy Science activities that can help future competitiveness.

This is an era of extraordinary demands on the budget. HEP labors at the deepest frontier of science and continues to be an important part of the package of federal support of science. He urged the Panel members not to slack off or to rest on ACI. Rather, they should make the case for the excitement and promise of HEP.

Carithers thanked Marburger for helping to bring this increase in funding about. HEP is not the focus of ACI, but the strategy should be to urge support of ACI but not to forget HEP. Marburger agreed. Carithers supported the "rising tide" analogy but was concerned that in the short term it did not appear to be the case for the National Aeronautics and Space Administration (NASA). Marburger said that the NASA budget will benefit in the long run but needs a better long-range vision. The same requirement is true for HEP. ILC will take a lot of bucks. The Presidential decision is in the future. The case needs to be made continually in public and to Congress. He likes the tone of *The Quantum Universe* with its focus on the excitement of the science. The promise and payoff of the next generation of accelerators is now much clearer so that a much stronger case can be made.

Shochet commented that Marburger in his talk focused on economics. In ACI there is also a focus on education and workforce issues. Marburger agreed and said that there is a challenge in making particle physics exciting to young people.

Cahn asked how these reports linked with the NASA experience. Marburger suggested that too small a time scale was being looked at. One cannot judge success on a single budget.

Olinto asked Marburger to comment on the Joint Dark-Energy Mission (JDEM) interagency cooperation. Marburger said that DOE is farther along in planning for this project than is NASA. There was a mismatch when they came together. There is a desire for more consonance between DOE and NASA.

Wormser said that he was surprised to hear Marburger imply that high-energy physics was not a good technology investment because the light sources of the next generation will be vital to explore frontiers. Marburger replied that to explore the frontiers, one needs to have frontier technology. There is a huge leverage of investment in light sources, which have much more impact and payoff than do accelerators. Material scientists, environmental scientists, geologists, chemists, and protein researchers all benefit from a light source. Basic Energy Sciences in SC is definitely underfunded in terms of its importance to the nation. All have to be balanced. One must grasp the big picture. The atomic-scale window has opened up. One needs to remember that, as costs increase, opportunities also increase. One has to invest. DOE is good at forming user communities.

Dragt asked about the DOE investment in light sources. Marburger said that a lot of science has benefited from research in the past. It is dangerous to oversell investment in science as resulting in spin-offs. Samios commented that one needs to view the cost/benefit issue from a higher intellectual plane. Marburger continued that intellectual excitement sells this stuff. Selling high-energy physics creates a problem not unlike that of space science but harder because it does not have the pretty pictures.

Murayama (who helped write *The Quantum Universe* report) said that he was pleased that Marburger liked the reports. The scientific community has to keep making the case based on the mysteries of science itself. Marburger agreed and told about Walter Kohn's video on solar power that was narrated by John Cleese. That is a good idea. Scientists have a responsibility to share excitement with our funders. He urged the Panel and the high-energy-physics community to be proactive. Gaining support for science is not just a series of one-shot deals but requires campaigns that last decades. There is a new generation in the classrooms and a new generation in Congress every 2 years. One must keep selling.

Shochet introduced **Arden L. Bement** to speak about the National Science Foundation (NSF) and the 2007 budget; the impact of the ACI on NSF; elementary-particle physics and its relationship with DOE and NSF; and education. On the budget, he said that he was excited by the President's requested budget, especially the ACI and the doubling of the NSF budget over the next 10 years. The requested budget includes a 7.7% increase in research and related activities (the core programs) and a 26% increase for funding major research equipment and facilities [e.g., Ice Cube, ALMA (Atacama Large Millimeter Array), and LIGO (Laser Interferometer Gravitational Wave Observatory)].

There is an exciting focus on physical sciences and a natural spillover into social and behavioral issues, a convergence of interests. In competitiveness, the central economic advantage comes when lead time is compressed. There is great momentum in China and India, and all nations recognize the importance of human resources and of the investment in education for the future. Everyone is chasing the same opportunities. Tom Friedman's "flat-world" theory can be carried to extremes. Our nation is spiky. Other spikes are growing up: China, India, South Korea, and Taiwan, to mention a few.

It is fun to think of particle physics; it is elegant, compelling, and broad in scope. There are frontiers, and all these topics are fascinating. He commended the leadership of the U.S. particle physics community, specifically in the LHC and ILC efforts, which are visionary projects.

A key to NSF's success has been its excellent partnership with DOE. Together, they provide stewardship to the accelerator infrastructure on which particle physics depends. Both NSF and DOE are relatively young organizations, but they both recognize the importance of a long-term commitment. DOE is taking leadership in providing an increasingly complex series of accelerator systems. NSF enables the university community to play major roles in the field.

The portion of the budget that bears directly on HEPAP includes an investment package that provides an additional $15 million for elementary particle physics. It will invest in the energy, neutrino, and cosmic frontiers. This increase in the Mathematics and Physical Sciences (MPS) budget arose from the facts that (1) the field of particle physics is poised at a frontier of discovery; (2) elementary particle physics is beset by challenges (it has been moving to Europe, and important projects have been cancelled); and (3) NSF has an important role in stewardship. *The Physics of the Universe* report has set the roles and defined the NSF stewardship role.

NSF recognizes that petascale computing is essential to deal with massive amounts of data. NSF will sponsor the Global Grid, which offers the promise of transformational changes.

Bill Gates has said that education is a matter of national security. The NSF says that an educated workforce for the 21st century is a matter of security. One needs to pay attention to the numbers because quantity has a quality of its own. Teaching is important at all levels. Failure early in the education process means the entire pipeline collapses. One needs to recognize the importance of the pathway between all educational institutions. K–12 education is an important priority at NSF. The 8-year old of today will be needed for heavy lifting tomorrow. There will be a future worldwide competition for workforce. Foreign talent may stay home. The NSF looks forward to the Panel's ideas, advice, and continued partnership.

Staffin asked where he saw the thrust of ACI branching out and expanding. Bement suggested more investment in frontier science. Other trends for more public investment may not be sustainable. The tightening of the economy creates deep swings. Public investment takes a longer time to get into effect. NSF believes they transfer knowledge through training.

Shochet declared a lunch break at 12:31 p.m. The meeting was called back into session at 2:20 p.m.

Shochet introduced **Hitoshi Murayama** to present a report from the subpanel studying the relationship between the International Linear Collider and the Large Hadron

Collider (ILC/LHC) and the science that they address. This is the first of four subpanel reports to be considered at this meeting.

Murayama presented the charge that was given to the Subpanel and reviewed the membership of the Subpanel. As stated, the charge to the Subpanel asked what the synergies and complementarities of the LHC and ILC were, how a linear collider would be used in understanding a Standard Model Higgs, and what the role of a TeV linear collider would be in distinguishing models and in establishing connections to cosmological observations. The way the Subpanel understood the charge was that the *Quantum Universe* questions are compelling. To address them, the Subpanel asked, why are accelerators needed? And, given the LHC coming online, why is the ILC needed? EPP2010 wanted a white paper on the technical argument with a deadline of August 2, 2005. But the charge called for a version that was less technical and designed for a wider audience. Therefore, in a series of meetings and teleconferences from March 25 through September 10, the Subpanel produced two reports, the first for EPP2010 and a later document for a wider audience.

The challenges faced in addressing the charge were

- Electroweak symmetry is the heart of the case for accelerators and is hard to explain.
- In explaining ILC, one does not want to hurt the importance of LHC; community input is needed.
- This report is to be directed at a broad audience and needs to be simply stated.
- Scientifically, it is not known what will be found at the TeV scale, and therefore nothing specific can be guaranteed; the best one can do is to talk about scenarios.

Nine discovery scenarios were described in the report:

- The Higgs is Different
- A Shortage of Antimatter
- Mapping the Dark Universe
- Exploring Extra Dimensions
- Dark Matter in the Laboratory
- Supersymmetry
- Matter Unification
- Unknown Forces
- Concerto for Strings

The Subpanel solicited feedback to earlier drafts of the report from LHC and ILC leaders and was encouraged by the constructive tone of the feedback received. Input was also sought from laboratory directors, HEPAP members, and Marburger. When Marburger's comments arrived after the report was in print, a bookmark was developed in order to include them.

The focus of the report is on science first. The nine great questions are mapped into three themes: the mysteries of the terascale, light on dark matter, and Einstein's telescope. The roles of the ILC/LHC in each of the discovery scenarios are explained with an effort made to dispel misconceptions. There is a misconception that if the LHC discovers more and measures more, then there is less motivation for the ILC. The report makes it clear that just the opposite is true The discovery of the Higgs particle will not be the end but will raise urgent questions. Particles are tools, not goals (physicists are not just particle collectors). These tools will help progress toward discoveries to resolve the laws of nature. When designing the report itself, a cover of notations on a blackboard was chosen. The title *Discovering the Quantum Universe* builds on the success of the earlier report.

The report reviews the *Quantum Universe* questions and discusses why accelerators are needed. Colliders are time machines and explore the terascale. Teravolts of particle accelerator energy will open up the terascale for discovery. Once the terascale is seen, the universe will never look the same. This is the new threshold physics is about to cross. The report explains the LHC and ILC and tries to communicate wow technology. In the section "Mysteries from the Terascale" the focus is on the Higgs field and how it works; supersymmetry is introduced. The LHC will have enough energy to survey the terascale landscape. Then a linear collider could zoom in to distinguish one theory from another. In "Light on Dark Matter," the report states how the LHC may identify a dark matter candidate in particle collisions. A linear collider could then zero in to determine its mass and interaction strength, taking its fingerprints and making a positive identification.. The report has a summary table of the relationships between possible discoveries at the LHC and the subsequent exploration of the basic questions of particle physics by the ILC. It explains what LHC might do and what ILC would add.

Discovery of a Higgs particle at the LHC would present mysteries of its own that would be even more challenging to solve than detecting the Higgs particle. Higgs is neither matter nor force; the Higgs is just different. Physicists suspect the existence of many Higgs-like particles: Why, after all, should the Higgs be the only one of its kind? They predict that new particles related to the Higgs play essential roles in cosmology, giving the universe the shape it has today. Experiments at a linear collider would zoom in on the Higgs to discover these innermost secrets.

About dark matter, the report points out that 4% of the universe is familiar matter; 23% is dark matter, and the rest is dark energy. Its identity is a complete mystery. Astrophysical evidence suggests that dark matter particles will show up at the Terascale. Physicists working at the LHC are likely to find the first evidence for Terascale dark matter. But is it really dark matter? Is it all of the dark matter? Why is it there? A linear collider would be essential for answering these questions, making precise measurements of the dark matter particles and their interactions with other particles. Linear collider experiments could establish both the what and the why for this chapter of the dark matter story.

In the section entitled Concerto for Strings, the report notes that string theory is the most promising candidate to unify the laws of the large and the small. If supersymmetry is discovered at the LHC and ILC, physicists will be able to test string-motivated predictions for the properties of superpartner particles. Here linear collider precision is essential, since the string effects appear as small differences in the extrapolated values of the superpartner parameters. A combined analysis of simulated LHC and ILC data shows that it may be possible to match the fundamental parameters of the underlying string vibrations. While not a direct discovery of strings per se, such an achievement would truly be the realization of Einstein's boldest aspirations.

There is a website set up to download the report after HEPAP approves it.

Ritz commented that it was great that the Subpanel sent drafts to various physics group and asked if it was also tested with the target audience. Murayama said no, but Judy Jackson tried it out with high school students. Ritz then asked if they came away with understanding. Ozaki said that he tested it with his wife, who commented that she learned quite a bit. He also noted that the font was small for older readers. Sally Dawson commented that nonphysicists on EPP2010 loved the previous version.

Shochet commented that the panel members had received the report a month ago; therefore there are fewer comments now.

Wormser posed another scenario: If the LHC finds nothing, what happens? Does the ILC go out of business? Or what about the possiblity that a weak particle exists but the machine is insensitive to its decay? Murayama said that the charge was to promote both the LHC and the ILC.

Shochet called for a vote, and the report was approved unanimously. Boxes of copies were brought in for distribution.

Shochet then introduced **Robin Staffin** to report on DOE and the 2007 presidential budget. Staffin was very pleased to see the new HEPAP together and ready to go. For a variety of reasons, legal and otherwise, the panelists are considered experts. The number of Panel members has been increased to 24; 20 are now on board. Science has driven the field to go in a broader direction. New members can offer a better range and more complete advice. What the Panel does is very valuable. The reports this Panel produces are the envy of other agencies, therefore DOE and NSF will ask for more!

Government employees make decisions; the Panel provides advice, and then the government employees take the heat. Not all decisions are technical. But the federal agencies take into account the recommended priorities.

High-energy physics reflects the international character of research, as seen in the number of languages you hear when you visit the nation's accelerators and colliders. The Panel reflects that international character with representatives from Europe and Asia. The field is too big and too expensive not to include international collaboration, which will help to stretch funding as far as it will go. When proposing an experiment, the proposer must consider what else is going on. Staffin thanked NSF for its superb cooperation, both in spirit and in execution. This is truly a joint committee, and Maury Tigner needs to be thanked for the suggestion to form it.

The bottom line of the FY07 budget was that HEP funding was up about 8% from FY06. Although HEP was not the first mentioned in ACI, it does quite well. Priorities include the Tevatron and B Factory, LHC support, and the core research program (with university research up 6% and laboratory research up 2%). ILC R&D is up $30 million, doubling. A few new neutrino initiatives will be started: the Electron-Neutrino Appearance (EvA) initiative and a reactor-neutrino-detector initiative to measure θ_{13}. Two other trends are long-term accelerator R&D and dark energy R&D. Staffin said that the table on the High Energy Physics budget is available on line.

The ILC R&D budget doubles in the FY07 budget request. Prominent industry has to get involved. The funding agencies involved have agreed to divide up costs. This is not approval for construction but for R&D to support the decision on construction.

With core research at universities up 6% and at laboratories up 2%, the goals include

- New neutrino experiments following the APS study "The Neutrino Matrix." The APS study recommended reactor and accelerator experiments in neutrino physics. Reactor experiments would measure θ_{13} with ν_e disappearance, and accelerator-based experiments would measure θ_{13} and mass hierarchy through matter effects.

- Accelerator R&D includes the ILC, one expensive item. If the field of accelerator physics is to advance, we must find a way to make accelerators cheaper. There is an increase of $5 million in long-range R&D program.

NuSAG was formed to assess the science case for the neutrino program.

The agencies want to know how this Panel would prioritize. Please rise above geographic, professional, and institutional affiliations and make an effort not to speak on behalf of your particular area of the field.

In addition to increases in ILC R&D, there is in the FY07 request an additional significant increase ($5 million or 18%) in the long-range R&D program that supports fundamental research into the physics of beams and accelerator technologies. The Advanced Accelerator Research and Development (AARD) panel will be asked to provide needed input for developing this program.

Other new initiatives include a high-intensity neutrino beam for CP violation experiments, neutrinoless double-beta decay experiments to probe the Majorana nature of neutrinos, an underground experiment to search for direct evidence of dark matter, and ground-based and space-based dark energy experiments.

Carithers asked about the international context of the $60 million for the ILC R&D. Grannis replied that detector R&D is a factor of 4 larger in Europe than in the United States, and Japan is about the same as the United States.

Ritz congratulated Staffin on the budget increase and asked how it would be distributed. Staffin replied that program managers' input and advice will be weighed, similarly to how LHC funds are distributed. Shochet asked if the these funds were to be distributed under solicitations. Staffin replied that that will be made clear soon.

Cahn asked what the priorities would be if the President's budget is not passed. Staffin replied that running the B-factory and Tevatron are priorities and running the existing facilities and getting ready for the ILC are the next priorities. There are other fields that are not as pleased as we are.

Bortoletto said she was looking at the increase in university programs but believed that universities have been suffering because their spending power had decreased as a result of inflation. Staffin said that the situation of universities was recognized and that the spending power of the laboratories has also gone down.

A break was declared at 3:40 p.m. Shochet called the meeting back into session at 4:02 p.m. and introduced **Joseph Dehmer** to speak on the NSF FY07 budget. Dehmer said that the two agencies, NSF and DOE, work together constructively, communicate well, and bring their own assets and style. He thanked all the members for serving on HEPAP, adding that this is a critical time and that there are a lot of charges moving through the system right now. These charges to the Panel were begun in an atmosphere that was extremely gloomy. One must always prepare for the next wave. That accidental coincidence can now be taken advantage of. With the rising budget and administration's emphasis on science, all of these planning exercises take on much more meaning. It is gratifying to see the numbers in the FY07 budget come around. This is an outstanding historic development.

At the rollout of the budget, one is expected to discuss priorities on how to use budgeted funds and to identify aspects of the scientific frontier and the academic community's involvement. This is what MPS says are its priorities: In FY07, there is $15 million added to the base for particle physics, which presents both opportunities and challenges. It will allow addressing the final big step for LHC operations and ILC R&D. In advancing the frontier of science, MPS in FY07 will put its investments into (1) elementary particle physics (EPP) at the energy, neutrino, and cosmic frontiers (we share some of these interests with Astronomy and some of the money goes to Astronomy); (2) physics of the universe; (3) fundamental mathematics and statistical science; (4) physical sciences at the nanoscale; (5) cyberinfrastructure and the cyberscience it enables; (6) the molecular basis of life processes; and (7) the physical science of environmental sustainability.

The budget of the directorate has increased to $1.15 billion, an increase of $65 million (6%) over that of FY06. About $250 million of that gets invested in the MPS facilities. MPS supports primarily physics and astronomy but also includes some ER activities. MPS supports, among others, the Cornell Electron Storage Ring (CESR), which is being phased out but still has connections to the ILC; the LHC with DOE (a signature piece for NSF); LIGO; the Michigan State University cyclotron (the flagship for nuclear science); Rare Symmetry-Violating Processes (RSVP, which unfortunately is now being phased out); and a lot of astronomy (ground-based observatories), which presents its own challenges.

The Division of Physics now has 11 programs. The traditional fields (AMOP Physics, Theoretical Physics, Gravitational Physics, EPP, Nuclear Physics, and Education) are still vibrant. Recent programs have been created to adapt to needs of the physics community (Particle and Nuclear Astrophysics, Biological Physics, Accelerator Physics and Physics Instrumentation, Physics Frontier Centers, and Physics at the Information Frontier).

MPS's priorities for FY06 are to provide a strong, flexible core research programs (more than 50% of the Division of Physics budget; the Committee of Visitors recommended increasing this to 55%); to support the physics of the universe (the EPP investment package; 10% per year; it started with $4 million, and astrophysics is now up to $16 million); to increase diversity by 10% per year; to strengthen theory by 5% per year; to pay attention to stewardship of facilities; and to cultivate new opportunities.

The OSTP report, *The Physics of the Universe: A Strategic Plan for Federal Research at the Intersection of Physics and Astronomy*, came out of the 11 questions posed by the National Research Council (NRC) report, *Connecting Quarks with the Cosmos*. These are the *big* science questions. *Physics of the Universe* prioritized the activities that the government can do to address those questions.

The NSF budget increased by 1.8% from FY05 to FY06 and is up 7.9% from FY06 to FY07 (in the President's budget request) to $6 billion. MPS is up 6% in the FY07 request. Research accounts in the NSF are up 7.7% in the FY07 request. The hope is to increase physics funding about 7% per year for the next 10 years. These are substantial increases. The Division of Physics has gone from $140 million to $250 million from 1997 to 2007. During that 10 years, funding was flat from 1997 to 2001, buoyant from 2001 to 2004, and flat again until this year.

Core research has declined in the percentage of funding from 65% to 55% between 1996 and 2004. Facilities increased from 27% to 33%, and centers increased from 5% to 10% during the same period. This shows the emphases placed on the sectors from year to year. There will be an open competition for new centers in 2008.

The Directorate's 664 awards support 969 senior personnel and about 4000 people overall. These awards support 536 postdocs, 370 other professionals, 997 graduate students, and 419 undergraduate students plus about 500 others at Research Experiences for Undergraduates (REU) sites. The directorate tracks active awards. Diversity is important to NSF; it needs to nurture a stronger field by funding new principal investigators (PIs). It is important to invest in the young with new ideas; women (a trend with significant numbers and sustainable growth); and minorities (the numbers are still low, and a substantial improvement has not been seen, but NSF is determined to make it happen). New PIs (the refresh rate) are about constant at 27%. Women are increasing (from 10% in 1996 to 17% in 2005), and other minorities are increasing very slowly (from 5% to 8% from 1996 to 2005).

The Deep Underground Science and Engineering Laboratory (DUSEL) has a highly interdisciplinary scope (including particle physics, nuclear physics, astrophysics, geosciences, engineering, biosciences, industry, and defense). All of these interests are shared with DOE. A lot of physics experiments would benefit from the lowest cosmic-ray flux possible anywhere (e.g., proton decay, neutrinoless double beta decay, dark matter detection, long-baseline neutrino experiments, solar and supernovae neutrinos, and low-energy nuclear cross-sections for nucleosynthesis research). The planning for this facility has been going on since 2001 and includes mention in Nuclear Science Advisory Committee (NSAC) long-range plans, *Connecting Quarks to the Cosmos*, the Earth Lab 2003 workshop, *Physics of the Universe*, *The Quantum Universe*, and the upcoming NuSAG report. All this started with Ray Davis's Nobel Prize-winning research on solar neutrinos in the 1960s. It will be a hazardous endeavor and must be done well. There have been several previous attempts. This effort has had a site-independent science-scope solicitation, a conceptual design solicitation (two awards were made for preconceptual designs), and a solicitation for technical description (which will be issued in June). This effort could lead to a Major Research Equipment and Facilities Construction (MREFC) candidate for FY09 funding.

Shochet asked how long it would take to construct the Underground Laboratory and when beneficial occupancy could be expected. Dehmer replied that it was a complicated matter. There are two potential sites for the facility, and those two sites have different time frames and costs associated with them. One site and design will be selected in the fall. The Undergound Laboratory is a candidate for FY09 funding if things come together, and it is on the table for P5. John Kotcher has joined the Physics Division and is responsible for stewarding DUSEL.

Samios noted that the government of South Dakota proposes taking over the Homestake Mine soon. Dehmer stated that South Dakota was aggressively pursuing the underground laboratory, but creating a safe environment for scientists was different from the experience with mining operations. Without analysis, that is risky; a safe operating environment has to be planned for.

Wormser asked Dehmer to elaborate on DUSEL's international connections. Dehmer replied that that referred to an international intellectual activity with all the leadership offshore, like the Sudbury Neutrino Observatory (SNO) and Super-Kamiokande. It is expected to be an international activity, and the details will be developed during the planning process. The scale of costs is much smaller than that of the ILC; it calls for a few hundred million dollars for the infrastructure. It would be an international project with scientists from around the world flowing in and out. All of NSF's activities are absolutely open (assuming one can get a visa). We have already engaged with DOE's NP and HEP offices to be there during the planning process so that their needs and aspirations can be anticipated. One does not need infrastructure to do dark-matter research.

Ozaki asked if the DUSEL support would be international. Dehmer replied that the infrastructure would be supported and managed by NSF.

Kahn asked about the NSF Physics Division's role in coordinating dark-energy astronomy. Dehmer replied that NSF was involved in the Atacama Cosmological Telescope (which has dark energy as part of its mission), the South Pole Telescope, the Large Synoptic Survey Telescope (LSST), and other ground-based dark-energy astronomical research. These activities are handled by the Astronomy Division. The Physics Division invests in those activities.

Shochet introduced **Peter Meyers** to make a presentation on the report and recommendations of the Neutrino Scientific Assessment Group (NuSAG).

NuSAG reports to both HEPAP and NSAC. This is the second report to those advisory committees. The subject of this report is the physics of neutrino mass and mixing. This is beyond the standard model of physics, and it is here now. There are connections to the big questions of high-energy physics and cosmology. This effort is experiment-driven, not theory-driven. Important new results are expected every year, but there are no guarantees. There has been a big U.S. investment over the past 5 years in neutrino experiments.

The next round is what NuSAG was charged to look at. There is lots to do in this area. This is a worldwide effort with much international collaboration, with a well-developed conceptual plan, and with reuse of a lot of expensive, existing facilities. There are opportunities for the U.S. program to take a leading role and to lift the worldwide program to a new, more-comprehensive level.

NuSAG was asked to make recommendations on the specific experiments that should form part of the broad U.S. neutrino-science program. One charge asked NuSAG to address the APS study's recommendation on a phased program of sensitive searches for neutrinoless nuclear double-beta decay. That topic was the subject of NuSAG's first report, which was approved in September of 2005.

Another charge involved the multidetector reactor experiment with sensitivity to $\sin^2 2\theta_{13} = 0.01$, an order of magnitude below present limits. The charge specified some experiments to look at:

- a U.S. experiment,
- U.S. participation in a European reactor experiment,
- U.S. participation in a Japanese experiment, and
- U.S. participation in a reactor experiment at Daya Bay, China.

Another charge concerned U.S. participation in a timely accelerator experiment with comparable $\sin^2 2\theta_{13}$ sensitivity and sensitivity to the mass hierarchy through matter effects. The options include

- U.S. participation in the T2K (Tokai to Kamioka) experiment in Japan (which has two options: B280 and 2km),
- construction of a new off-axis detector (NOvA), and
- liquid argon, which is currently directed to other applications and is in an R&D phase.

NuSAG was charged with looking at the scientific potential of each initiative, the timeliness on its scientific output, the likely costs to the United States, and the project's place in the broad international context. NuSAG was then to recommend a strategy of one or more experiments in that direction.

Membership of NuSAG was drawn from nuclear science and high-energy physics, from the United States and abroad, from theory and experiment, and from neutrino physicists and the broader field of particle physics.

In the current paradigm, three-neutrino mixing can be parameterized by three mixing angles, based on studies of atmospheric neutrinos, reactor or accelerator neutrinos, and solar neutrinos. The mixing angle of solar neutrinos is well understood, but the Liquid Scintillator Neutrino Detector (LSND) results are not consistent with this three-neutrino mixing. There are two possible mass hierarchies, normal and inverse, as well as an unknown lowest mass. There is an interesting interplay between the inverted and normal mass scales, and the experimental goal is to reach into the low-mass region by seeking high resolution in the observation of neutrinoless double-beta decay. The next round of experiments might

- further confirm three-neutrino mixing,
- determine if there is CP violation in leptons,
- sort out the mass hierarchy, and
- determine the maximal amount of atmospheric mixing.

These topics are all tangled together. The experiments before NuSAG propose to address several of them and to take the first steps on others. Several unknowns need to be addressed: $\sin^2 2\theta_{13}$, $\sin 2\theta_{23}$, the CP violation phase, and the mass hierarchy. The good news is that there is sensitivity to all parameters of interest. The bad news is that there is one measurement and four unknowns, all coupled together in one expression.

The appearance of $\nu_\mu \rightarrow \nu_e$ itself would be an important discovery but would give no specific value for $2\theta_{13}$. Different NOvA and T2K baselines would lead to different matter effects, which would lead to a resolution of the mass hierarchy, extending the reach for CP-violation discovery. Working with beams would put appearance of $\nu_\mu \rightarrow \nu_e$ and improved θ_{23} within reach. Also, NOvA would have a modest reach in mass-hierarchy resolution. Multimegawatt beams would increase sensitivity to mass hierarchy and CP violation by an order of magnitude.

Reactor neutrino disappearance gives a very clear resolution of $\sin^2 2\theta_{13}$ but no sensitivity to CP violation or mass hierarchy. If disappearance is seen, there would be confirmation of the paradigm, and $\sin^2 2\theta_{13}$ would be measured without ambiguities. In addition, reactor results could be combined with accelerator results to resolve the θ_{23} ambiguity with better and better precision.

The NOvA experiment uses the existing Fermilab NuMI beam. Its baseline is 810 km, but it is 12 km off-axis, giving a peak neutrino energy of about 2 GeV. It uses a 30-kT liquid scintillator detector at the far end of the beam and a movable near detector with the same technology. The estimated cost is about $165 million for the detector (the beam already exists). This cost estimate has not been vetted outside Fermilab. This detector would run for 5 years, and the beam would not be shared as it is now.

T2K uses the existing 50-kT Super-K as the far detector. A new Japan Proton Accelerator Research Complex (JPARC) accelerator is under construction and is pointed at the detector. The baseline is 295 km and is 2.5° off-axis, giving a peak neutrino energy of about 0.6 GeV. It has low energy, a short baseline, and low resolution. The T2K B280 experiment has a neutrino beam, an on-axis monitor, and a near detector at 280 m that is a fine-grain calorimeter. It is currently under construction in Japan. U.S. participation in the beam and detectors would cost about $5 million, a bargain. The Japanese contribution would be $165 million. Another proposal is to have a suite of near detectors and is referred to as T2K 2km. This facility is not yet approved in Japan; its total cost would be about $36 million with about a third of that being picked up by the United States.

The potential later phases of accelerator programs were not a part of the current advisory process, but they indicate what one can do with these expensive facilities. One possibility would be to increase the proton-beam power at Fermilab and T2K (under a long-range plan calling for a new injector). Another possibility would be to upgrade the detectors or to install very-long-baseline technologies. Plans are in place for such upgrades if the physics demand them.

The last thing that NuSAG was asked to look at was an R&D project for a liquid-argon detector. Liquid-argon detectors give better particle identification, tracking, and efficiency. The question is, does this technology scale up?

Sensitivity to θ_{13} not being zero was calculated as a function of the CP violation phase for each of the experiments and combinations thereof. The best 3σ discovery limits were calculated for NOvA plus a proton driver (PD) running both neutrinos and antineutrinos. For the resolution of the mass hierarchy, one gets much more performance from NOvA/PD plus T2K/4-MW/HK [Hyper-Kamiokande]. And for CP violation, the best determination is by NOvA plus T2K/HK. In all cases, combinations are more powerful than single facilities alone.

Robin Staffin said that DOE is supporting R&D at Fermilab on proton-driver technology but is not pushing CD0 at this time. Shochet said that there will be another charge tomorrow. Meyers said that approval was not being asked on the proton driver now.

Molzon said that at least one reactor is needed somewhere; it does not make sense to do experiments on NOvA without an expectation of a much larger beam in the future. Meyers said that the general issue is, do you aggressively pursue this physics or back off and do it step by step? People all over the world want to do this science. The question is does the United States do it or does it sit out?

NuSAG assessed three reactor proposals with two or more detectors: Double-Chooz, Braidwood, and Daya Bay. The detector mass of Double-Chooz is significantly less than that of the other two reactors, and its sensitivity is significantly less, also. It seems like Double-Chooz is going ahead; the cost of U.S. participation would be $5 million. The U.S. cost at Braidwood would be $65 million, and at Daya Bay $30 million.

All the experiments that NuSAG looked at were well-motivated and scientifically interesting. The region of $\sin^2 2\theta_{13} > 0.01$ seems to be a sensible target; it is an order of magnitude below current limits. Reactor experiments are not a faster path to this level of sensitivity, as was first assumed. Reactor and accelerator experiments give complementary information, with reactors measuring fewer parameters. (They do not do all the physics.) However, the reactor experiments do not have the ambiguity that accelerator experiments have. The experiments already under construction (T2K and Double-Chooz) do not and cannot do all the physics required. In combination, NOvA and a reactor can add mass-hierarchy resolution, would have about equal sensitivity, and would provide a substantial extension of CP-violation sensitivity. NuSAG concluded that

Braidwood and Daya Bay are scientifically very similar. There are some advantages to Braidwood and its symmetrical layout. There are also some complicated international and nonscientific issues involved here. The accelerator program is both enriched and complicated by future potential development paths.

NuSAG recommends that

- The United States can and should be a leader of the worldwide experimental program in neutrino oscillations.
- The U.S. program should include both accelerator- and reactor-based experiments.
- The United States should conduct the NOvA experiment at Fermilab.
- The United States should continue to play an important role in the Japanese program, focusing on T2K B280 in the short term and on the 2km on an appropriate timescale, if possible.
- The United States should support R&D on liquid argon to establish scalability to 10 to 30 kT.
- The United States should mount one multidetector reactor experiment sensitive to neutrino disappearance down to $\sin^2 2\theta_{13}$ of about 0.01. Both Braidwood and Daya Bay meet the scientific needs; one should be done.
- External issues rather than sensitivity are likely to be decisive. Determination of Daya Bay cost sharing with China must be clarified, and a full technical review of both experiments is needed. These experiments are not yet even proposals.
- U.S. participation in Double Chooz is encouraged as the quickest path to improved sensitivity. However, it should be given lower priority because it does not do all the physics needed.

The next round in neutrino oscillations needs a well-developed conceptual plan that calls for the reuse of expensive existing facilities. It will require a worldwide effort with much international collaboration, and there are opportunities for the U.S. program to take a leading role, lifting the worldwide program to a new, comprehensive level. Experiments should be carried out.

Shochet opened the floor to discussion. Battay asked if this assessment will continue into a long-range program as suggested by Marciano. Meyers replied that NuSAG did not spend a lot of time considering long-range ideas. It felt that the NOvA that was put before us should be considered on its own terms, not as part of a long-range plan. It asked if NOvA crossed a scientific threshold.

Ritz commented that we ask incrementally what does NOvA bring? It seems that NOvA has long-range potential but only modest sensitivity. Meyers commented that it is easier to get off than to get on. If the first phase is skipped, there may not be a second phase.

Nishikawa said NOvA Phase 1 experiments should be done and the results evaluated. One should not delay by looking for the perfect experiment; and that he believed that it would be a difficult analysis for Monte Carlo. Meyer commented that it is not difficult to make a neutrino beam with few antineutrinos in it. The opposite is difficult.

Brau asked him to address the time criticality. Meyers responded that the two experiments could be on similar time scales for sensitivity. They are reasonably advantageous. If you wait long enough, that ceases to be true.

Samios commented that NOvA should not impede other alternatives. A cap (e.g., $200 million) should be put on NOvA, and it should be reconsidered if the Lehman report goes over the cap. That way, if the decision is made to go in a different direction, it will not impede others.

Cahn commented that implicit in approval of this report is an approval of upgrades of the accelerator. Without upgrades (i.e., a proton driver), its results could be fairly limited. Shochet noted that incremental improvements in the Fermilab complex could improve performance to 1 MW.

Molzon noted that there is an interest in having an operating accelerator program after 2010 and that that is when the ILC would be coming about. He asked what would happen to the accelerator supplying NOvA at that point. Would it be the operating accelerator for some other programs and studies? Montgomery replied that a lot of different possibilities are being looked at. Neutrino experiments always want 150% of the protons available. He believed that a broader accelerator program could be supported.

Ritz then said that the question is whether to accept the report or make changes. If it is accepted, does NOvA then go to P5? Shochet responded, yes, P5 should be briefed on issues dealt with in the discussions embodied in the report. Ritz asked what would happen when NOvA went to P5. Would NuSAG like to have P5 make the choice? Meyers answered that that is the way it will go. But Shochet commented that he was not sure that P5 would be in any better a position to make the decision. Wormser commented that the timing of this approval process means that one should look at which experiment could be built fastest. NOvA has the advantage of having an accelerator already there.

Shochet suggested that the choices were to approve the report and pass it on, send it back to NuSAG with suggested changes, or approve it with explanatory language for P5.

Shochet introduced **Alfred Mann** to speak about the underground laboratory and its relationship to the NuSAG report.

Mann requested that HEPAP not approve the accelerator part of the NuSAG report, the third part of the charge. On the following day, the Panel will consider an additional charge on long-baseline neutrino physics. It is not NuSAG's fault but the narrowness of the original charge. There are other options not considered by the charge. The emphasis on neutrino physics has changed dramatically. There is now the possibility of DUSEL, which will change the field. The South Dakota Science and Technology Authority will take ownership of the Homestake Mine and will have miners move in during the next month to rehabilitate the mine to make it safe for scientists. In that underground laboratory, one can build two 100-kT detectors that can be ready by 2011 or 2012. The possibility of neutrino beams from Brookhaven National Laboratory or Fermilab should be considered.

Also, one could have 200 kT deeply buried to remove the cosmic-ray flux, which is not the case at Fermilab. The reach of a 200-kT detector (which can be added to) is significantly greater than that of a 25-kT surface detector. All of the constants in the equations have not been measured. The theory behind these constraints needs to be learned. One will not learn by making a quick experiment called in the charge "timely." The United States needs to aim at an ambitious program that can do other physics as well. To settle for a limited aim is to make a bad mistake. It gives away the future for neutrino physics in the United States. Neutrino work is time-consuming and costly. HEPAP should give a broader charge to NuSAG. Send them back to work and have them compare NOvA and DUSEL.

Meyers explained that these are complicated issues tied together. NuSAG, if it expands its efforts, could include those possibilities somewhat. Putting off the decision could end up hurting the United States. All combinations should be looked at, but some are on a faster track than others; delay could set us back. Costs and time scales are very uncertain.

Mann noted that the 200-kT detector is 8 times the size of NOvA's detector. There is only one other possibility than Nova: the underground laboratory with a 200-kT detector. Dehmer replied that there are two sites under consideration. One is the South Dakota Homestake Mine. The decision will be made between the two sites later this year. Baltay sympathized with Mann's comments but worried about charging off in the wrong direction.

Samios asked Meyer if he was sensitive to the need for a cap. Meyers replied, yes. Costs will factor into the decisions that will be made.

Shochet invited **Robert Svoboda** to speak on behalf of Double Chooz. Its goals are to obtain the first significant data on θ_{13} in a decade. The first measurement of θ_{13} was done by the Chooz experiments. Double Chooz is designed to get an answer quickly to the question of whether θ_{13} is big or small. One can get a big increase in sensitivity with a small effort. The experiment uses an existing facility. The far detector can start at the end of 2007, with the near detector following 16 months later. Double Chooz can surpass the original in three months, even with a single detector. The International Double Chooz Collaboration with 36 U.S. physicists, 7 university groups, and 3 national laboratories has done experiments on this detector. They have requested \$4.8 million for detector construction. Our collaborators are already building and will move in next month. A 1/5-size prototype has been running since last year. A U.S. group is responsible for critical systems (e.g., photomultipliers and high voltage). Double Chooz may not go ahead if it is not supported in the United States. A lot of equipment is needed to eliminate systematic errors. In Europe, it has been approved by both agencies [Le Centre National de la Recherche Scientifique/Institut National de Physique Nucléaire et de Physique des Particules (CNRS/IN2P3) and Commissariat à l'Energie Atomique (CEA)] in the spring of 2004. Electricité de France is putting up a significant amount of funding. France funded 25% of the detector and all the civil engineering. There is also strong support from Germany (the Max Planck Institute) and Spain. Construction could begin next year. The construction cost to the United States would be less than \$5 million, so Double Chooz would not impact NOvA or Braidwood. Double Chooz would only get to 0.03 but could do it very quickly, telling whether θ_{13} is big or small. The United States needs to put its cash on the table or go home. A quick decision is requested.

Samios asked what fraction of the final funding was in hand. Svoboda replied, 10 million euros; estimates of the cost are in the range of expected funding for the detector.

Baltay asked whether the other experiments would add anything if Double Chooz finds a high limit. Meyers said that he did not know; certainly one would still need greater sensitivity to unfold the ambiguities.

Shochet asked the Panel members to pick up copies of the new charges on the way out and to consider them overnight. He adjourned the meeting for the day at 6:45 p.m.

Saturday, March 4, 2006
Morning Session

Chairman Shochet called the meeting to order at 8:28 a.m. and asked **Aesook Byon-Wagner** to summarize how the governmental processes work, especially the budgeting process.

HEPAP was established in 1967 to advise the Federal Government on the national program in experimental and theoretical high energy physics research. It constitutes the official channel for advice from the field (scientific community) to the government. It operates in accordance with the Federal Advisory Committee Act (FACA) and all applicable FACA amendments, federal regulations, and executive orders. It is a relatively large Advisory Panel, having up to 24 members; it meets in public three or four times per year. It currently has joint ownership by DOE and NSF; before October 2000, it was strictly a DOE advisory committee. It reports to the Director of the Office of High Energy Physics (HEP) in DOE and to the Assistant Director of the Mathematical and Physical Sciences (MPS) Directorate in NSF.

Its charter allows HEPAP to provide periodic reviews of elements of the high-energy-physics (HEP) program and recommendations based thereon; advice on long-range plans, priorities, and strategies for the national HEP program; advice on appropriate levels of funding to develop those plans, priorities, and strategies and to help maintain an appropriate balance among competing elements of the HEP program; and advice on the scientific aspects of HEP issues of concern to the DOE and NSF, as requested by the senior managements in DOE and NSF.

HEPAP provides advice and recommendations; it does not make decisions. Making decisions requires budgetary authority and management responsibility (which come with accountability).

Subpanels exist to facilitate the functioning of HEPAP. The objectives of the subpanels are to make recommendations to the parent Panel with respect to particular matters related to the responsibilities of the Panel. Subpanels, appointed by the Chair of HEPAP in consultation with agencies, may meet in closed session but must report to HEPAP in open session. HEPAP considers the recommendations of the subpanels and acts upon them. HEPAP then reports to DOE and NSF. Much of the work of HEPAP occurs between meetings and is conducted by subpanels.

Information about HEPAP's charter, membership, meeting schedule, agendas, presentations, minutes, reports, and current subpanels can be found at www.science.doe.gov/hep/hepap.shtmwww.science.doe.gov/hep/hepap.shtm. Recent HEPAP subpanels include the Particle Physics Project Prioritization Panel (P5), Neutrino Scientific Assessment Group (NuSAG), AARD, Dark Energy Task Force, Cosmic Microwave Background (CMB) Task Force, and Physicist Resources Task Force.

In the Executive Office of the President, HEP interacts with two offices:

- The Office of Management and Budget (OMB), which advises and assists the President, develops and executes the budget, oversees implementation of administration policies and programs, and develops and implements management policies for the government.
- The Office of Science and Technology Policy (OSTP), which advises the President; provides science and technology analysis and judgment with respect to

major policies, plans, programs, and budgets; leads the interagency effort to develop sound science and technology policies and budgets by setting forth (along with OMB) the R&D priorities to guide the agencies when developing their budgets and co-chairing the National Science and Technology Council (NSTC) [which is comprised of the Committee on Science, Committee on Technology, Committee on Environment and Natural Resources, and Committee on National and Homeland Security]; and develops the annual OSTP/OMB Guidance Memorandum for R&D Priorities.

The Office of High-Energy Physics (HEP) is located in the Office of Science (SC) of DOE. The Secretary of Energy is Samuel Bodman, and the Department's budget is about $24 billion. Other offices in SC are Basic Energy Sciences (BES), Biological and Environmental Research (BER), Nuclear Physics (NP), Fusion Energy Sciences (FES), and Advanced Scientific Computing Research (ASCR). The Director of SC is Raymond Orbach, and the other advisory committees that report to him are BESAC, BERAC, NSAC, FESAC, and ASCAC.

The advisory process addresses questions on program direction. In addition to ongoing operations of the Office, HEPAP and its subpanels may be asked to assess and make recommendations about new, potential initiatives, including their

- Scientific potential (To what extent does the program/project have the ability to change the fundamental view of the universe?),
- Relevance [Is the science important to DOE–HEP and/or NSF–MPS mission(s)?],
- Value (Does the level of scientific potential match the level of investment?),
- Alternatives [Are there more cost-effective alternatives to get at the same (or most of the same) physics?],
- Timeliness (Will the results come at the right time to have sufficient impact?),
- International setting (Are similar efforts underway in other countries? Are there potential international partners for this effort?), and
- Infrastructure [Does the project exploit, or help to evolve, existing infrastucture (including human capital)?].

Many new initiatives involve other agencies or overlap with the topical interests of other advisory committees. In such a case, the assessment efforts may be shared by two or more advisory panels to address such questions as the overall shape of the field and the need for a "grand strategy," the priority to give to one area versus another, and the best project among several in an area. The latter question may be addressed by a scientific advisory group (SAG).

HEPAP frequently interacts and cooperates with several other advisory committees: the Nuclear Science Advisory Committee (NSAC) of DOE and NSF; Astronomy and Astrophysics Advisory Committee (AAAC) of NASA, NSF, and DOE/OHEP; and Structure and Evolution of the Universe Subcommittee (SEUS) of NASA's Space Science Advisory Committee. These committees currently have two cooperating SAGs: Neutrino Physics and Dark Energy.

NuSAG, the Neutrino Scientific Assessment Group, was initiated in 2005 and asked to provide scientific assessments on options for reactor neutrino experiments, off-axis neutrino experiments, and neutrinoless double-beta decay experiments. Its next charge is a scientific assessment of a next-generation, high-intensity neutrino beam facility. NuSAG is a joint subcommittee of HEPAP and NSAC.

Analogous SAGs are likely to be established for other scientific topics, such as dark matter and particle astrophysics.

P5 addresses the relative priorities of proposed projects/programs within the program and maintains the roadmap for the field. The first P5 was created in 2002 for 2 years and expired in November 2004. A new P5 was established in spring 2005 for 2 years. An "umbrella" letter created the panel, and it was followed by individual charges. P5 is expected to be asked to compare the recommended options from the SAG process and to prioritize programs relative to one another. P5 will likely be given an envelope of available funding for new initiatives and ongoing programs and be asked to prioritize within that constraint.

The National Academy of Sciences has an independent panel, Elementary Particle Physics 2010 (EPP2010), that is a "decadal survey" to

- Lay out the grand questions that are driving the field;
- Describe the opportunities that are ripe for discovery;
- Identify the tools that are necessary to achieve the scientific goals;
- Articulate the connections to other sciences and to society;
- Foster emerging worldwide collaboration;
- Recommend a 15-year implementation plan with realistic, ordered priorities;
- Strengthen connections with society;
- Sharpen the physics questions;
- Engage other scientific communities and international participants; and
- Place U.S. high-energy physics in the international setting.

During the federal budget process, the case for SC faces four big hurdles: Inside SC from February to April, it is subjected to guidance and program formulation. Inside DOE from April to July, it faces briefings, priorities, decisions, and revisions. At the OMB from August to December, it has more briefings, decisions, and revisions. And in Congress from February to perhaps September, it undergoes roll-out, hearings, and appropriations. Execution of the enacted budget then occurs from September to the following September. At any given time, 3 years of budgets are under way.

New construction projects of DOE pass through five critical decision points:

- CD-0, approval of mission need;
- CD-1, approval of alternative selection and cost;
- CD-2, approval of performance baseline;
- CD-3, approval of start of construction; and
- CD-4, approval of start of operations or project closeout.

DOE's approval process has different requirements for different types of expenditures. These required steps occur in parallel to the budget process. An MIE (major item of equipment) needs a CD-0 approval to be included in the budget request and a CD-3 approval to spend the MIE funds. A construction project needs CD-0 approval to include project engineering and design (PED) funding in the budget request; CD-1 approval to spend PED funds; CD-2 approval to include construction funding in the budget request; and CD-3 approval to spend construction funds. Approval is nominally needed by June to be included in the following fiscal-year budget request. The construction project phase is the 2 years from CD-0 to CD-2 and the 1.5 years from CD-2 to CD-3. Averaged data from real projects show the time to be 1.5 years from CD-0 to CD-1, 1 year from CD-1 to CD-2, and 1 year from CD-2 to CD-3.

Samios asked what would happen if Orbach were confirmed as Under Secretary of Energy. Staffin said that he had asked but had not gotten an answer.

Marvin Goldberg was introduced to describe the NSF budget process. The NSF was created by the National Science Foundation Act of 1950 to initiate and support basic scientific research in the sciences. Its structure was illustrated with an organization chart.

The NSF's program in Elementary Particle Physics (EPP) interacts with the whole NSF structure, including the Office of International Science and Engineering (OISE), the Office of Cyberinfrastructure (OCI), and the Directorate for Education and Human Resources. It also works closely with other offices.

EPP's goals are to advance intellectual frontiers, produce broader impacts and added value, empower universities, educate a diverse workforce, provide stewardship, and forge partnerships. An example of how partnerships add value can be seen in cyberscience with the development of the Tier 2c Grid with OCI, the UltraLight Network with OCI, and Trillium/Open Science Grid with OCI and DOE. An example in Education with Research is the development of QuarkNet with DOE's Office of Multidisciplinary Activities (OMA), Education and Human Resource Development (EHR), and DOE/HEP.

EPP is at a turning point. The discovery potential has never been greater. The LHC will dominate in the next 20 years, and U.S. projects will be phased out and/or be canceled. The next-generation energy-frontier accelerator requires a multibillion dollar investment and international cooperation. We are waiting for EPP2010 and starting to work on new initiatives. NSF supports more than 10% of the U.S. program and more than 40% of university activities. DOE is the primary steward of the national accelerator complex and will lead the ILC campaign. NSF will increase investment to broaden the field (e.g., through DUSEL) while supporting university groups across frontiers.

Other NSF opportunities include Major Research Equipment (MRE) awards for MPS projects that exceed a cost of $100 million during the construction project life and the Major Research Infrastructure (MRI) awards for developing university scientific infrastructure.

A planned program is the Accelerator Physics and Physics Instrumentation (APPI) program for accelerator physics R&D and mid-scale instrumentation. These grants are what is needed to do our science.

The NSF fiscal year for university-based support starts in September with a target date for proposals for the next fiscal year. In October, proposals are sent for ad hoc review. In the fall, project leaders visit. In December, there is an EPP panel review. Site visits are scheduled throughout the winter, as needed. During winter and spring, declinations are sent out. During the spring, funding awards are initiated. In the summer, funds are held for final awards and supplements. July is the deadline for career proposals for the following fiscal year.

The operations of the directorate are reviewed by a committee of visitors (COV). We give the COV all our proposals and our budget. COV panels are asked to prioritize proposals for different budget scenarios. This is a key aspect of the process because it requires the review committee to deal with the hard choices that the program officers face. The 2006 COV commended the proactive management of the portfolio, which has been kept lean and competitive. It also stated that it is absolutely necessary to carve out room in the budget to fund young faculty and to start new projects. It also commended the creative interactions with other NSF programs and divisions.

MRI has a $2 million upper limit, and the directorate will fill it in with budget authority. We have not done well with it. We are going to advertise it more. This is for universities. We need to get more and better proposals from universities.

Shochet asked for a report on the P5 Subpanel from **Abraham Seiden**, who listed the membership and pointed out that about one-quarter of the original membership was enlisted to retain corporate memory.

Current planning calls for the PEP-II [Positron Electron Project-II] B-factory at the Stanford Linear Accelerator Center (SLAC) to be operated until the end of FY2008 and the Tevatron collider at Fermilab through FY2009. We are asked to assess what factors or considerations might lead to stopping B-factory operations one year, or two years earlier than planned? When would we be in a position to make such a determination and what information would be needed? Similarly, for the Tevatron collider, what factors or considerations might lead to stopping operations one year, or two years earlier than now planned? What might lead to running longer than now planned? Again, when would we be in a position to make such a determination and what information would be needed? Ending these programs would make room for the funding of new initiatives. These goals need to be balanced.

The first meeting on September 8 and 9, near Washington, D.C., provided an opportunity to hear from the DOE and the NSF about both our charge and budget constraints for the field and to discuss issues related to addressing our charge. The other two meetings were at Fermilab on September 12 and 13 and at SLAC on October 6 and 7. At these meetings we heard about overall long-term laboratory plans, as well as the accelerator status, physics potential, and manpower and collaboration issues. We also had the opportunity at each laboratory for discussion with members of the international teams involved in these experiments, the program leaders, and the laboratory management.

The Tevatron collider and PEP-II accelerator are rapidly accumulating more data. The existing data sets will be doubled and then doubled again by the currently planned stopping dates. Each doubling of the data allows significantly more physics as both of these programs seek to discover how nature works at a deeper level. Early termination would reduce the physics that will come from these world-leading programs. With the material presently before P5, we see no reason to terminate the operation of either the Tevatron or PEP-II earlier than planned. However we have not yet studied in detail other High-Energy Physics projects that could be started if funds were available. P5 will be looking at the full U.S. particle physics roadmap in 2006. We plan to revisit the issue of the last year of running for the Tevatron and PEP-II programs in the context of a full roadmap. Input from a number of HEPAP subpanels presently looking at different aspects of the program will be an important component of our planning exercise in 2006.

A plot of Tevatron integrated luminosity projections indicated that the facility is producing the data that it was designed to produce, and it does not make sense to stop it now.

Shutting PEP-II off a year early would leave the staff there with an integrated luminosity of 653 fb^{-1} rather than 1004 fb^{-1}. However, the machine has not been running well lately, and the question is whether those problems will be solved.

In general, the physics collaborations are producing many front-line physics results and are exploring a window of opportunity to discover new physics. They are also functioning efficiently. Competition between collaborations [the Collider Detector at

Fermilab (CDF) and D0 at the Tevatron and between BaBar and Belle) not only provides crosschecks of difficult measurements but also has led to novel analysis techniques and exploitation of new methods for making important measurements (e.g., on the top-quark mass and on the angles of the unitarity triangle). The accelerators are performing extraordinarily well and pushing the performance frontier with new ideas and techniques. The PEP-II problems are vacuum issues. These programs provide models for international collaboration, including the sharing and integration of computing resources across continents during the LHC era.

The Tevatron collider, with its continued position at the energy frontier, will remain for the next few years an excellent window onto possible new physics. With ever increasing luminosity, the CDF and D0 collaborations are the dominant experiments with the potential to discover new physics through direct searches. Well-motivated examples include Higgs bosons, supersymmetric particles, new gauge bosons, excited fermions, and signals for extra dimensions. In addition, precise measurements of the top quark and *W* boson masses provide incisive tests and constraints on our picture of the electroweak interactions. Once we discover the source of electroweak symmetry breaking, for example Higgs bosons, the precision measurements will serve to constrain the physics picture needed to understand the new phenomena. The Tevatron also provides important measurements for the B_s system, where mixing and rare leptonic decays provide important tests of our understanding of flavor processes complementary to the B-factory measurements. With the new data expected in the next few years, the Bs measurements will confront directly the Standard Model predictions, allowing searches for new physics contributions. They need the whole data set to get there.

The PEP-II accelerator is routinely running at approximately five times the design averaged luminosity, allowing a much richer physics program for BaBar than originally planned. With the increased luminosity, there are other things that can be done. The BaBar collaboration is making world-leading measurements of the three angles of the unitarity triangle, in some cases using recently invented techniques, as well as the V_{cb} and V_{ub} CKM [Cabibbo Kobayashi Maskawa] matrix elements. The complete set of measurements represents an unprecedented check of the coupling of quark-flavors to the *W* boson, parameterized in the Standard Model by the CKM matrix.

P5 will be making a full roadmap later this fiscal year. We are deferring making final recommendations for the last year of running of the Tevatron and PEP-II facilities until after we can include the final year in a full updated roadmap. The question we propose looking at is the potential scientific impact of various options as well as the schedule and urgency for new initiatives. We therefore recommend that P5 revisit the issue of the final year of running, in the context of a better-understood roadmap, in Spring 2006 for PEP-II and Spring 2007 for the Tevatron collider. These dates allow a year of leeway in making funding decisions.

The issues faced by the Tevatron collider include the integrated and projected luminosity for this program, the status of the LHC, the manpower situation for the Tevatron program, the physics picture revealed by earlier Tevatron running, and any new physics discoveries coming from other parts of the program. The issue of whether any running beyond 2009 is warranted should also be looked at.

For PEP-II, the issues are the achieved and projected luminosity of both PEP-II and the KEKB accelerator in spring 2006 and the updated values for the key measurements planned by these projects, in particular the status of the hint of new physics in the CP asymmetries, as measured in gluonic penguins.

P5 strongly encourages DOE to engage those agencies that fund the major non-US collaborating groups participating in the Tevatron and PEP-II programs in a discussion of issues and timetables for the running of these facilities. Lines of communication with these foreign funding agencies should be strengthened, recognizing that they are partners who have made very significant investments and played a major part in the success of both of these programs. A very important issue at stake is the reputation and credibility of the United States in international HEP partnerships that will be considered in decisions about the ILC.

At the Tevatron, there are some interesting windows. If they attain 4 fb^{-1}, it would increase the exclusion area for the Higgs boson; of course it would be even more exciting if they found the Higgs in this space.

The collider program includes an important set of measurements on the B_s system. These include mixing, lifetimes, lifetime differences of the two mass eigenstates (which could have large lifetime differences), and the decay to the $\mu^+\mu^-$ final state. From the combined fit, a nonzero lifetime difference has been observed at about the 2σ level, $\Delta\Gamma_s/\Gamma_s = 0.12 \pm 0.06$, with a central value larger than the theoretical expectation.

The principal goal of the B physics programs at PEP-II and KEKB has become the search for new physics beyond the Standard Model. These programs carry out this search by looking for decays, mixing phenomena, or CP-violating asymmetries that, either individually or in combination, cannot be described by the four parameters in the Standard-Model CKM matrix. Evidence of new physics could take the form of inconsistent values for a single Standard-Model quantity when this quantity is measured in different ways, or a non-Standard-Model rate for a rare decay, or a non-Standard-Model CP-violating asymmetry.

A comparison of the theoretical limitations with the present experimental errors in the framework that includes the unitarity triangle angles and the penguin diagrams indicates that there is a way to go and that the error ranges need to be reduced. With the unitarity triangle, the measurements need to be sharpened by a factor of 2. The gluonic penguin decay modes are expected to agree in the Standard Model; they will be investigated with LHCb and the B factories.

Thanks to the discovery by both Belle and BaBar of a large number of new charmed and charmonium hadrons, some of whose properties are puzzling and a challenge and constraint on the understanding of nonperturbative QCD, one of the most exciting areas being pursued at both B factories is hadron spectroscopy. Increased statistics would likely result in the discovery of additional states, as well as helping to elucidate the properties of those already discovered. While such spectroscopy does not test the Standard Model, it does illuminate it, allowing us to refine our models for how to approximate QCD in a nonperturbative region.

P5 has received the new assignment to make a roadmap for the field and recommend priorities among options. Its goal is to complete this assignment by the end of this fiscal year. To get moving, it has scheduled a meeting in Washington for March 27 and 28. It will also hold meetings in April at Fermilab and SLAC to discuss the ILC and dark energy. P5 will get an update regarding PEP-II at the April SLAC meeting. The Subpanel will also be working in the interim.

Discussion of the P5 report was deferred to later in the meeting. Shochet called upon **James Whitmore** to give an update on the survey of physicist resources in high-energy physics through the end of the decade.

In 2004, a task force was formed by HEPAP to investigate the projected "needs" of experiments and "plans" for all U.S. HEP groups. He listed the committee members. The original charge called for the formation of a working group to study HEP manpower to answer the question of whether the field has the manpower to carry out the experiments to which the U.S. program is committed until the end of the decade. An initial report from the Working Group was presented to HEPAP at its meeting on September 23-24, 2004. (Final Report in June 2005.) The written report was given to R. Staffin and J. Dehmer and M. Shochet on January 31, 2006. A Feb. 17, 2006, version has a minor addition.

A survey was conducted of two communities: 18 experiments selected by the committee and 194 DOE and NSF PIs. A questionnaire was sent to the experiments, asking them to evaluate their needs in operations (carefully defined) and in analysis (carefully defined) from 2004 to 2009 in terms of faculty/staff, post docs, and students. 2004 was treated as a census year; foreign and U.S. personnel were considered separately. A questionnaire was sent to the PIs, asking them to evaluate their plans for faculty, research associates, postdocs, and graduate students for all projects from 2004 to 2009 under a severe, constant-effort boundary condition.

In August and September 2004, the Subpanel jointly prepared letters of introduction and instructions plus spreadsheets, including examples. They were sent to (1) all NSF experimental EPP grant PIs, including the Cornell Electron Storage Ring (CESR); (2) all DOE/HEP grant PIs, including Fermilab, Brookhaven National Laboratory (BNL), SLAC, Argonne National Laboratory (ANL), Lawrence Berkeley National Laboratory (LBL), and MIT Laboratory for Nuclear Science (MITLNS); and (3) spokespersons of the selected 18 experiments. From September through April, the Subpanel reminded, cajoled, begged, and threatened PIs and spokespeople to respond. Eventually, nearly 100% of the PIs responded in a useful way, and all experiments replied. Updates were given at each HEPAP meeting through the final report in July 2005.

The analysis of the PI responses from universities and laboratories was completed for 194 groups, 81 supported by NSF, and 136 supported by DOE. Some were funded by both agencies. There were 53 projects with two or more PIs responding. The respondents made up 603 group-projects, with about three projects per group, including, for 2004, 717 total faculty, 340 research scientists, 547 postdocs, and 712 graduate students. These were combined into one 50,000-cell database that was hand-checked to ensure that there were no errors. Subsequent discussion centered on the Tevatron experiments; CDF and D0 researchers at both of those experiments were sent a questionnaire as follow-up to their groups' survey results. The Subpanel met electronically to discuss results and fashion conclusions. The final results were presented to HEPAP on July 12, 2005, and a final writeup was finished in January 2006.

Both the PIs and the spokespersons were sent essentially identical letters that said "this survey is an accounting of your current effort and as such are presumably precise numbers. Since the strategy for the survey is 'constant effort,' the sum of each category of personnel is expected to remain equal to the FY 2004 totals." He described and explained the form that was used to collect data from the respondents. A graphic presentation of results showed that the Subpanel pretty much covered the field for particle physics.

Counting faculty is difficult. The Subpanel used percentage of research fraction (RF) as a metric, which allowed for a variety of comparisons and easy checking that the constant-effort rule was followed because it sums to a name. However, RF overcounts full-time equivalents (FTEs). Experiments use FTEs for postdocs and graduate students. But, essentially, FTE = RF. A standard in experiments is a 50% efficiency factor for faculty time. For laboratory scientific staff, RF is considerably higher than 50%. So, an estimated FTE (ESTFTE) was used for faculty counting: ESTFTE = 0.5(university professor RF) + (laboratory scientific staff RF). Postdoctoral and graduate-student counting totally correlated to faculty involvement (e.g., a 20% faculty person implies at least one student and/or one postdoc, while a 0% FTE faculty person implies zero.

For each of the groups (faculty/staff, postdocs, and graduate students), the data were divided by experiment, and for each experiment, they were broken down into U.S. physicists, host-laboratory physicists, and non-U.S.-institution physicists. This analysis was conducted for both total personnel and for operations and analysis personnel.

The Subpanel then wanted to compare what the PIs said to what the collaboration spokespersons said. The word “needs” is relatively straightforward for operating the experiments; the error is estimated at ±10%. The meaning is considerably less straightforward for analyzing the experiments. The same people do both, sometimes at different times during their involvement. Analysis intensity follows the integrated luminosity jumps. For future experiments, the estimate is of something other than “need.” It was reported as consisting of basically a mixture of real effort now ongoing in construction (like operations in running an experiment, again ±10%) plus a census of what groups intend to do in the future.

The data were broken down and graphed by field of physics, and these data were rolled up into the total figures. BTeV was canceled during the survey, so it was removed from the study.

The PI database contains a lot of information about what people are doing in high-energy physics. One can plot personnel needs over time by type of experiment (accelerator-based neutrinos, neutrinos, ILC, etc.). Then one can look at the results from 17 experiments in current (ongoing) and future terms. The Subpanel canvassed 75 to 80% of all experiments with the chosen ones. It did not ask for U.S. scientists needed. It took the total needs and benchmarked them to the 2004 U.S. actual ratio. That U.S. portion is what was reported. The data for different experiments were compared. There was good consensus between the PIs and the collaboration spokespersons. For D0 and CDF, one-third of the need was in operations, and two-thirds was in analysis.

On the basis of the PI data, there is a falloff in the Tevatron plans and a nearly linear increase of people migrating to the LHC. If all U.S. collider experiments are put together (D0 + CDF + BaBar + CLEOc) and one adds the numbers of people on U.S. ATLAS [A Toroidal LHC ApparatuS] and U.S. CMS (Compact Muon Spectrometer), one gets a nearly constant sum. This is a fairly closed set. On the basis of the collaboration spokesperson data, there is a rise in need (or anticipated need) as one goes from the current experiments to the future ones.

The Tevatron situation presents special challenges. Therefore, there was a special follow-up in June, focusing on the Tevatron. There is an apparent correlation among about 80 independent D0 and CDF PIs. A significant PI decrease occurs, especially after 2006. One difficulty is in defining "needs" by the experiments, and there is almost no certainty in predicting beyond FY08. This is all theoretical; nothing has happened yet. It suggests a potential problem to be investigated. Questions linger: Are these the real "needs" of the experiments? Are these the real "plans" of the PIs? The constant-effort rule was very difficult to contend with for PIs.

The Committee decided to send D0 and CDF institutional representatives a questionnaire that included the following questions for anonymous reply:

- Do these results surprise and/or concern you?
- Would you have liked to have kept a greater presence in D0 or CDF during the period from 2006 to 2009 than your response suggested?
- If you would have, what led to your decision to respond with a significant reduction in plans for CDF or D0?
- What factors influenced your projection to 2007?
- What would you have needed to believe about your particular circumstances in order for you to have responded with a greater presence in D0 or CDF?
- Should CDF and D0 collaborations just live with this apparent plan or should the Tevatron community promote a managed transition? Do you have a sense of what would constitute a managed transition?
- Would these apparent results have led you to have responded differently if you had known beforehand?

A draft of the questions was tried out on a few D0 people. One reported back, "One positive thing that I come away with is a greater sense of duty to D0. I can't now assume that other groups will keep D0 running as we shift to CMS."

In the results from the questionnaire, everyone emphasized that outstanding physics will come from the Tevatron. The redirection of physicist resources can compromise the physics. Premature migration would prevent postdocs and graduate students from gaining experience necessary for LHC analysis. Two issues dominated any shift from Tevatron to LHC:

- Some physicists need to participate in LHC on Day 1.
- Some reported implicit and/or explicit directives from agencies to shift from Tevatron to LHC.

Among these respondents, 60% say "physics," and 45% say "pressure" (including 5% who say both).

The constant-effort constraint was a reason for an apparent coherent response away from Tevatron. 65% said that, with incrementally more resources, they could devote additional students or postdocs to the Tevatron program. Small groups have a special problem because they have to make an either-or decision. Essentially all were in favor of a "managed" transition. Some suggested

- Specific ideas for streamlining of operations, analysis, and code changes;
- More inclusion of laboratory technical people into traditionally physicist roles;
- Prioritizing of physics goals;
- The need for close coordination among stakeholders, leading to a strategy; and
- Assurance that those who conformed would not suffer funding loss.

These responses were made in the framework of a constant-level of effort from the PIs. They were done in the context of three time-dependent uncertainties:

- The potential for exciting physics results,
- The uncertainty in the LHC schedule, and
- The uncertainty of the Tevatron's and B-Factory's future luminosity performance.

The Subpanel concluded that maximizing the physics return from the Tevatron and BaBar while simultaneously preparing for an active U.S. role in ATLAS and CMS may tax physicist resources of the U.S. HEP community, especially when factoring in the other efforts planned and under way in neutrino physics, astrophysics, cosmology, and cosmic-ray physics. With respect to the Tevatron and LHC, the next 2 years will be crucial in terms of understanding the evolution of the three uncertainties cited above, but the field cannot wait to see whether this will prove to be the case. Although one cannot be sure that additional resources will be required, navigating this transition will require an unprecedented, active coordination among (1) the running collider experiments (primarily, BaBar, D0, and CDF), (2) their laboratory managements, (3) U.S. ATLAS and U.S. CMS, and (4) the funding agencies to ensure it does not become a real problem.

There might be a serious problem at the Tevatron beginning within 1 to 2 years for those groups trying to evolve to LHC while simultaneously maintaining sufficient strength in CDF and D0. (For BaBar, this situation appears to be less severe at this point.) A focused effort on helping to maintain the Tevatron and B-Factory efforts of a small number of specialized groups/personnel may be required to alleviate potential problems. If necessary, a few-year supplement to the University Program budget could be required. The coordination should start immediately, and conclusions should be reached in a matter of a few months so plans can be formulated and remedies negotiated very soon.

The written report has been delivered to the Panel.

Shochet asked **Joel Butler** to describe what Fermilab had done in response to this study. A Tevatron Collider Experiment Task Force was set up out of concern about physicist staffing for Run II operations, data reduction, and physics analysis, an action catalyzed in part by HEPAP and P5. The charge by Fermilab Director Pier Oddone was to evaluate the personnel resources needed to operate CDF and D0 through 2009 and to perform the physics analysis of the data in a timely fashion; to compare these needed resources to estimates of the personnel resources available from the two collaborations and Fermilab; to identify shortfalls or gaps; and to suggest remedies.

The Task Force concluded that the needed physicist resources will be available to operate CDF and D0 through 2007 and to complete the most important data analyses (and a significant amount more) in a timely fashion. But there are some risks, and the Laboratory is working with domestic and international funding agencies to mitigate those risks.

The Task Force included CDF and D0 co-spokespersons; leaders of CDF and D0 operations, computing, and physics-analysis groups (including international representatives and Chip Brock for connection to the HEPAP resource study); and leaders of the Particle Physics Division and the Computing Division, the major suppliers of laboratory resources to the experiments. This group discussed and debated very difficult issues concerning the end game of experiments that many had spent their careers working on. They also carried the discussion to the full collaborations. The process took 6 months and will continue.

On the supply side, the number of physicist FTEs available to each experiment is expected to decline from 2005 levels by approximately 27% by 2007 and by 56% by

2009. The task force found that the HEPAP personnel survey and the memorandum-of-understanding (MOU) surveys carried out by the experiments agree well for 2005 and 2007. On the demand side, the original HEPAP estimate was a "top-down estimate" made during the midst of the Run II upgrade and when the data analysis was in an early stage of development (when the machine was unstable). Here, the collaborations and Fermilab have done a major amount of new work that only they can do. Both experiments thought that the HEPAP "needs" were overestimated.

As the detector matures, problems get eliminated, and quality monitoring, problem detection, and problem resolution get automated. The need for shift and on-call personnel has been decreasing steadily and will continue to do so. As the luminosity reaches an asymptote, triggers stabilize, and things get easier. However, other factors will create new problems. The larger data sets will require more computing and more human effort to process, and radiation damage and detector aging will require additional attention. The report makes several recommendations to decrease the number of physicists needed for operations. All of them are obvious.

The physics analysis was based on ensuring that analysis on a "core" set of ten high-impact physics topics is done in a timely fashion. These are the physics topics that are usually cited as the justification for achieving high integrated luminosities (the soul of the program). The algorithm development for these ten topics enables a much broader range of analyses, which can then be done with small additional effort if physicists are available. This strategy assures openness to the unexpected. Uncertainties on how much discipline can be applied to focus people on these primary analysis goals were the nub of the Task Force's discussions. Therefore, two somewhat different approaches were taken to establish a range of possible needs.

The results on the resource balance were in good agreement among HEPAP, U.S. researchers, and foreign researchers. There should be a comfortable amount of people to do the core program, and people will be available for other tasks, as well. In 2007, sufficient labor is expected according to both methods used. There are enough resources to deliver considerably more than just the core physics. By 2009, the situation is less clear, with one analysis showing that there is sufficient effort to deliver the core and the others showing modest shortfalls with respect to a broader physics program. The situation is certainly better than previous, less-rigorous analyses suggested. The problem is not seen to be one of pain but of risk management. The analyses indicated that the two experiments together will have 400 FTEs committed through 2009, even in the face of competition and uncertainties.

The Task Force made an extensive list of recommendations to Fermilab top management:

- Require the divisions to update the Laboratory staff profile needed to fulfill Fermilab responsibilities to complete the Tevatron program. This activity is ongoing.
- Communicate to Fermilab staff scientists engaged in the Tevatron collider program the Laboratory staff plan for the Tevatron and LHC and plans for future CMS membership opportunities. This activity is ongoing.
- Encourage the experiments and divisions to continue developing efficiencies that reduce the effective labor required to operate the Run II programs. This activity is in progress.

- Continue to promote the Tevatron program to incoming research associates and, starting in FY06, increase the number of CDF and D0 research associate positions by two each. The Laboratory is currently recruiting for two additional postdocs for each experiment.
- Periodically review with the collaborations' spokespeople the degree to which institutional MOU commitments are honored. This activity is being planned.
- Provide strong support for the LHC Physics Center (LPC) at Fermilab and for Fermilab's U.S. CMS posting activities. This activity is ongoing. In addition, expand the LPC to include limited support for members of ATLAS working on Run II. This suggestion is under consideration.
- Keep the international support solid.

The task force also made recommendations for joint DOE–Fermilab action:

- Increase visitor budgets for outside personnel by approximately a factor of 2. Travel and health insurance go a long way to bring the right people into the program.
- In concert with the collaborations' spokespeople, conduct negotiations with NSF and DOE and find funding sources aimed at retaining or enhancing support for university resources in the areas of greatest risk.
- Discus jointly with the LHC and Tevatron experimental leadership the difficulties faced by groups and individuals active in both programs. These groups frequently find it difficult to fully contribute to two programs through the Tevatron-LHC transition period.
- Explore the possibility of contributions from the funding agencies for the creation of Tevatron fellowships to support the main university students (five to ten per experiment).
- Similarly, explore the possibility of support from the funding agencies for the creation of hadron-collider fellowships to support postdocs (three to six per experiment) resident at Fermilab. The 3- or 4-year fellowships might initially focus on the Tevatron program with a transition to the LHC occurring late in the second or early in the third year of the fellowship.

Fermilab has begun to discuss with funding agencies how additional resources could help mitigate the risks that were discussed. With a problem of this type, modest increments and available resources can make big differences, especially in supporting more guests and visitors who are eager to work on the experiments. Improved funding for universities, expected in 2007, will certainly provide them with more flexibility. That should help. A firm commitment on 2008/2009 running would allow international partners, who provide half of the total effort and who have stated their support through 2009, to solidify their plans.

If the collaborations, Fermilab, DOE, NSF, and the international funding agencies work together, it will be possible to have a successful transition to the next phase of exploration of the high-energy frontier in which all investments are well served.

A break was declared at 10:47 a.m. Shochet called the meeting back into session at 11:03 a.m. and opened the floor to discussion of the reports presented.

Carithers asked how the P5 plan fits in with the EPP2010 long-range plan. Seiden replied that the P5 roadmap effort had not yet started, but decision dates may vary between the two studies, and some projects may be more advanced and ready to go sooner than expected, and the ILC may drive changes. Shochet stated that P5 is to focus on the next 5 years, and its roadmap is not to be static. Samios asked what they would do about people who are uncertain of their plans. Seiden said that the panel would look at things that looked promising and possible. It might identify areas of physics that look promising. Samios noted that that is a top-down procedure that can freeze out discoveries. Seiden said that P5 certainly does not want to freeze resources.

Wormser asked what happened to carry-over money. Staffin replied that, if a facility is turned off, the funds are at risk of being lost; there would be no carry-over money. It depends on how compelling the new opportunities are and how much science has been extracted from the current programs. For ongoing activities, it may be extremely difficult to turn off an experiment. Shochet said that a compelling case has to be made on what is best for the field.

Cahn pointed out that the physics goal had been defined as a search for new physics. That is not totally correct; the failure to find new physics may be just as telling as finding something new. Seiden said that the question is how much value to put on things. Some people would say that finding nothing would be less interesting. Cahn asserted that it is irrelevant in deciding whether to shut a project down. Shochet pointed out that there would then be no reason to shut a project down. Cahn said that the ultimate value of Belle and BaBar is the precision of the measurements. One can be confident that there will be new physics discovered at the LHC. Not finding something tells you something about the new physics.

Ozaki reminded the group that it had been stated that it was desirable to decide about the Tevatron in 2007. The proof of the pudding is whether the system will work. Seiden pointed out that a review at that point would allow at least an assessment of manpower adequacy. Most of the interesting things are bumps on backgrounds. There are many issues that will be clearer in a year. The rate of accumulation of data will be important.

Staffin noted that meeting some of the reporting times would be challenging. Schedules are partly driven by the budget process. The charge to P5 resulted from budgetary constraints; DOE wanted to ensure that some opportunities were not missed. The baseline plans are to run Tevatron to the end of FY09 and BaBar to the end of FY08. Research managers should always be evaluating whether the last efforts are worth it in terms of deferring other projects.

Perlmutter noted that there may be something missing. Seiden replied that P5 has not met yet, so it has not considered that possibility. It will be doing that this month. Perlmutter said that P5 should get as much from the Dark Energy Task Force as from other subcommittees.

Shochet called for comments on the physicists-resource report. Wormser stated that it seems to be a zero-sum game. It would help if the task force could add a recommendation that the communities exert pressure to support new hires and students. Whitmore pointed out that the charge was to consider constant funding. Shochet pointed out that the neutrino community is staying together, and the dark-energy community is growing. The problem was CDF and D0, where there had been an uncertainty that Fermilab has now addressed. Molzon said that there is movement in and out of the collider community, as reflected in the data but not in the report. Baltay said that the task force got Fermilab to act, and that was good.

Bortoletto asked what the follow-up would be and whether there has been any reaction from Fermilab in terms of fellowships. Whitmore responded that the Subpanel believes that its work is done. Butler replied that Fermilab has acknowledged the need to continue this evaluation and that it is expected that DOE and NSF will provide a good response for such things as fellowships.

Staffin noted that about half of the respondents felt that there is pressure from the agencies to move to the LHC. The agencies feel that current projects should be fully staffed; they do not encourage shifting (although they do shift resources to new initiatives). Montgomery said that Fermilab has started to set up some fellowships for the Tevatron and LHC.

Shochet asked the Panel if it would accept the P5 report. All agreed to accept the report. He asked the same question about the physicist-resources report. Agreement to accept the report was unanimous. Shochet said that he would mention the Fermilab response in the cover letter.

Dragt noted that Fermilab produces antiprotons and asked if there were other uses for an antiproton facility. Montgomery said that the response did not pan out into a significant effort, and the Laboratory does not have an active push in that area. Staffin added that the agencies had not gotten any proposals for such a capability.

Shochet brought up the new, draft charges from the agencies. Ritz asked if there were still a lower limit on the size of projects considered by P5 and what the scope of this report was. Shochet replied that P5 is still supposed to look only at the larger projects. Double Chooz is below that threshold. The question is whether the funding agencies and others will recognize that fact. Staffin said that the agencies had removed the funding interval. Byon-Wagner said that one could not consider one [small] project but not another. Seiden said that some things are too small (e.g., U.S. participation in Super-Kamiokande and some neutrino experiments). DOE will be supporting these projects, but P5 cannot look at *everything*. That is not to say that P5 is negative on these issues or that it limits small efforts. Perhaps that distinction could be reflected in the report. P5 is an evaluative, not a funding, group. Samios suggested that the word “major” be in the charge and defined there.

Shochet moved on to the charge for a Dark Matter Scientific Assessment Group (DMSAG). This is not the same as the Dark Energy Task Force or any future dark energy scientific assessment group.

Ritz said that he believed that the charge is excellent. He asked if it would be desirable to call this a direct detection of dark matter SAG or if the statement about direct detection were contextual. Staffin said that he would like to confer with the AAAC about that issue. Olinto said that the group should cover all of the big picture and point out where direct detection fits in.

Perlmutter noted that the P5 charge did not contain any reference to previous NAS reports. Staffin said that P5 was welcome to use those early reports but that EPP2010 was stressed because it is putting out a similar roadmap.

Shochet suggested that the new NuSAG charge be considered after the discussion of the current NuSAG report. A break for lunch was declared at 12:05 p.m.

Saturday, March 4, 2006
Afternoon Session

Chairman Shochet call the meeting back to order at 1:28 p.m. and asked for an interim status report on the activities of the Dark Energy Task Force. Robert Cahn began the presentation. The Task Force reports to three agencies and two subcommittees. He listed the Task Force members. The Task Force has held quarterly meetings and weekly phone conferences. The charge to the task force is, in part, "to advise the agencies on the optimum near- and intermediate-term programs to investigate dark energy and, in cooperation with agency efforts, to advance the justification, specification, and optimization of LST and JDEM." The Task Force is to summarize the existing program of funded projects; summarize the proposed and emergent approaches; identify important steps, precursors, R&D, etc.; identify areas of dark-energy parameter space existing or proposed projects fail to address; and prioritize approaches (not projects).

The organization of the perspective report is: context, goals and methodology, findings, recommendations, and appendixes. It starts with the statement that there is conclusive evidence for the acceleration of the universe and that dark energy makes up 70% of the mass-energy. There is the possibility that dark energy is constant in space and time or that dark energy varies with time. The impact of dark energy can be expressed in terms of an "equation of state":

$$w(a) = p(a)/\rho(a) \ ,$$

with $w(a) = -1$ for Λ. There is also the possibility that general relativity or the standard cosmological model is incorrect. Whatever the possibility, exploration of the acceleration of the universe will profoundly change our understanding of the composition and nature of the universe.

Dark energy appears to be the dominant component of the physical universe, yet there is no persuasive theoretical explanation. The acceleration of the universe is, along with dark matter, the observed phenomenon that most directly demonstrates that our fundamental theories of particles and gravity are either incorrect or incomplete. Most experts believe that nothing short of a revolution in our understanding of fundamental physics will be required to achieve a full understanding of the cosmic acceleration. For these reasons, the nature of dark energy ranks among the most compelling of all outstanding problems in physical science. These circumstances demand an ambitious observational program to determine the dark energy's properties as best as can be done.

The goal of dark-energy science is to determine the very nature of the dark energy that causes the universe to accelerate and that seems to comprise most of the mass-energy of the universe. Toward this goal, an observational program must

- Determine as well as possible whether the accelerated expansion is consistent with being caused by a cosmological constant.
- If it is not caused by a constant, probe the underlying dynamics by measuring as well as possible the time evolution of dark energy [for example by measuring $w(a)$; the task force parameterized w as w_0 and w_a].
- Search for a possible failure of general relativity through comparison of cosmic expansion with the growth of structure.

The dark-energy observational program should measure the expansion history of the universe [$dL(z)$, $dA(z)$, and $V(z)$] and measure the growth rate of the structure of the universe. All are described by $w(a)$. If there is a failure of general relativity, a possible difference in $w(a)$ may be inferred from different types of data. Some value for $w(a)$ should be obtained no matter how it is measured.

To quantify progress in measuring properties of dark energy, a dark-energy figure of merit is defined from a combination of uncertainties in w_a and w_p. The Task Force made extensive use of statistical (Fisher-matrix) techniques incorporating cosmic microwave background (CMB) and H_0 information to predict future performance. Its considerations follow developments in stages: what is known now; anticipated state upon completion of ongoing projects; near-term, medium-cost, currently proposed projects; and the Large-Survey Telescope (LST) and/or Square Kilometer Array (SKA) and/or Joint Dark Energy (space) Mission (JDEM). Dark-energy science has far-reaching implications for other fields of physics. Also, discoveries in other fields may point the way to understanding the nature of dark energy.

Gary Bernstein presented a pedagogical introduction to the field, an astronomy primer for dark energy. If one solves general relativity for the scale factor a of the universe, one finds that positive acceleration clearly requires that $w(p/\rho)$ must be less than $-1/3$, which is unlike any known constituent of the universe, a nonzero cosmological constant or an alteration to general relativity. Today, we have

$$H_0^2 = (8\pi G_N\rho/3) + (\Lambda/3) - (k/a^2) \; .$$

This equation can be rewritten and generalized so that it applies to conditions other than just now. To do that, we need to distinguish between nonrelativistic matter and relativistic matter and we need to generalize Λ to dark energy with a constant w, which is not necessarily equal to -1.

The expansion factor a is directly observable from the red shifting of emitted photons. Time is not a direct observable. A measure of the elapsed time is the distance traversed by an emitted photon. The distance–red-shift relation is one of the diagnostics of dark energy. Given a value for curvature, there is a one-to-one map between the distance traversed by an emitted photon and $w(a)$. Distance is manifested by changes in flux, subtended angle, and sky densities of objects and fixed luminosity, proper size, and space density. These are one class of observable quantities for dark-energy study.

Another observable quantity is the progress of gravitational collapse, which is damped by the expansion of the universe. Density fluctuations arising from inflation-era quantum fluctuations increase their amplitude with time. This increase can be quantified by the growth factor g of density fluctuations in linear perturbation theory. This growth–red-shift relation is the second diagnostic of dark energy. If general relativity is correct, there is a one-to-one map between the distance traversed by an emitted photon and $g(z)$. If general relativity is incorrect, observed quantities may fail to obey this relationship. If one can measure the fluctuations in the universe today and compared them to what (WMAP) sees, one has the growth factor.

What we are looking for in distances is a few-percent difference in the primary observables [distance traversed by an emitted photon and $g(z)$]. We have four main methods for measuring these observables.

- Dark energy can be measured with Type 1a supernovae. They all explode with the same luminosity, so measuring luminosity measures distance. q_0 is <0, so we have acceleration, and there must be dark energy.
- Baryon acoustic waves propagate in the baryon-photon plasma, starting at the end of inflation. When plasma combines to neutral hydrogen, sound propagation ends.

The total travel distance (r_s) is then imprinted on the matter density pattern. WMAP and Planck determine r_s and the distance to $z = 1088$. Galaxies are signposts in the universe.

- Galaxy clusters are the largest structures in the universe to undergo gravitational collapse. They are markers for locations with a density contrast above a critical value. Theory predicts the mass function $dN/dMdV$. We observe $dN/dzd\Omega$. We can observe the angular diameter distance. The mass function is very sensitive to M, which is very sensitive to $g(z)$. Galaxy clusters are visible in the visible and X-ray wavelengths, and they shadow the CMB.
- Dark energy can also be measured with weak gravitational lensing. Mass concentrations in the universe deflect photons from distant sources. Displacement of background images is unobservable, but their distortion (shear) is measurable. The extent of distortion depends upon the size of mass concentrations and relative distances. Depth information can be obtained from red shifts. Obtaining 10^8 red shifts from optical spectroscopy is infeasible; someone must measure "photometric" red shifts instead. Lensing also works on the cosmic background or 21-cm photons.

Cahn took up the discussion again. The Task Force's white paper is focused on four observational techniques: baryon acoustic oscillation (BAO) large-scale surveys that measure features in the distribution of galaxies, cluster (CL) surveys that measure the spatial distribution of galaxy clusters, supernovae (SN) surveys that measure of the flux and red shift of Type 1a supernovae, and weak lensing (WL) surveys that measure the distortion of background images because of gravitational lensing. Different techniques have different strengths and weaknesses and are sensitive in different ways to dark energy and other cosmological parameters. Each of the four techniques can be pursued by multiple observational approaches (radio, visible, near infrared, and X-ray observations), and a single experiment can study dark energy with multiple techniques. Not all missions necessarily cover all techniques; in principle, different combinations of projects can accomplish the same overall goals.

The four techniques are at different levels of maturity. BAO was only recently established. It is less affected by astrophysical uncertainties than are other techniques. CL is the least developed. Its eventual accuracy is very difficult to predict. Its application to the study of dark energy would have to be built upon a strong case that systematics caused by nonlinear astrophysical processes are under control. SN is presently the most hopeful and best proven technique. If photometric red shifts are used, the power of the supernova technique depends critically on the accuracy achieved for photometric red shifts. If spectroscopically measured red shifts are used, the power as reflected in the figure of merit is much better known, with the outcome depending on the ultimate systematic uncertainties. WL is another emerging technique. Its eventual accuracy will be limited by systematic errors that are difficult to predict. If the systematic errors are at or below the level proposed by the proponents, it is likely to be the most powerful individual technique and also the most powerful component in a multitechnique program.

A program that includes multiple techniques at Stage IV can provide an order-of-magnitude increase in the figure-of-merit. This would be a major advance in the understanding of dark energy. No single technique is sufficiently powerful and well established that it alone is guaranteed to address the order-of-magnitude increase in the figure of merit. Combinations of the principal techniques have substantially more statistical power, much more ability to discriminate among dark-energy models, and more robustness to systematic errors than any single technique. Also, the case for multiple techniques is supported by the critical need for confirmation of results from any single method.

In the figure of merit, an ellipse represents a 95% confidence limit. How can one tell that this is consistent? Is $w = -1$ everywhere? One may have an a for which $w \neq -1$. The uncertainty of w varies. How well can one distinguish w from -1? The results were robust.

If one measures w_p by some technique and then measures it by another way, one can overlay the results and get additional power from the Gaussian statistics of the multidimensional state, reducing the uncertainty.

Results on structure growth, obtainable from WL or CL observations, are essential program components in order to check for a possible failure of general relativity.

In our modeling, we assume constraints on H_0 from current data and constraints on other cosmological parameters expected to come from measurement of the CMB temperature and polarization anisotropies. These data, though insensitive to $w(a)$ on their own, contribute to our knowledge of $w(a)$ when combined with any of the dark-energy techniques considered. Different techniques are most sensitive to different cosmological parameters. Increased precision in a particular cosmological parameter may benefit one or more techniques. Increased precision in a single technique is valuable for comparing dark energy results from different techniques. Because different techniques have different dependencies on cosmological parameters, increased precision in a particular cosmological parameter tends not to improve the figure of merit from a multitechnique program significantly. Indeed, a multitechnique program would itself provide powerful new constraints on cosmological parameters.

In the modeling, a spatially flat universe is not assumed. Setting the spatial curvature of the universe to zero greatly helps the SN technique, but has little impact on the other techniques. When combining techniques, setting the spatial curvature of the universe to zero makes little difference because the curvature is one of the parameters well determined by a multitechnique approach.

Experiments with a very large number of objects will rely on photometrically determined red shifts. The ultimate precision that can be attained for photometric red shifts is likely to determine the power of such measurements.

The inability to forecast reliably systematic error levels is the biggest impediment to judging the future capabilities of the techniques. With BAO, theoretical investigations are needed of how far into the nonlinear regime the data can be modeled with sufficient reliability and further understanding is needed of galaxy bias on the galaxy power spectrum. With CL, combined lensing and Sunyaev-Zeldovich and/or X-ray observations of large numbers of galaxy clusters are needed to constrain the relationship between galaxy-cluster mass and observables. With SN, detailed spectroscopic and photometric observations of about 500 nearby supernovae are needed to study the variety of peak explosion magnitudes and any associated observational signatures of effects of evolution, metallicity, or reddening along with improvements in the system of photometric calibrations. And with WL, spectroscopic observations and narrow-band imaging of tens to hundreds of thousands of galaxies out to high red shifts and faint magnitudes are needed to calibrate the photometric red-shift technique and to understand its limitations. It is also necessary to establish how well corrections can be made for the intrinsic shapes and alignments of galaxies, how to remove of the effects of optics and (from the ground) the atmosphere, and how to characterize the anisotropies in the point-spread function.

Four types of next-generation (Stage IV) projects have been considered:

- An optical Large Survey Telescope (LST), using one or more of the four techniques;
- An optical/near-infrared JDEM satellite using one or more of the four techniques;
- An X-ray JDEM satellite, which would study dark energy by the cluster technique; and
- A Square Kilometer Array (SKA), which could probe dark energy by the WL technique and/or the BAO technique through a hemisphere-scale survey of 21-cm emission.

Each of these projects is in the $0.3 to 1 billion range, but dark energy is not the only (in some cases not even the primary) science that would be done by these projects.

Each of the Stage-IV projects considered (LST, JDEM, and SKA) offers compelling potential for advancing the knowledge of dark energy as part of a multitechnique program. According to the white papers received by the Task Force, the technical capabilities needed to execute LST and JDEM are largely in hand. The Task Force is not constituted to undertake a study of the technical issues.

The Stage IV experiments have different risk profiles:

- SKA would likely have very low systematic errors, but needs technical advances to reduce its cost. The performance of SKA would depend on the number of galaxies it could detect, which is uncertain.
- Optical/NIR JDEM can mitigate systematics because it will likely obtain a wider spectrum of diagnostic data for SN, CL, and WL than is possible from the ground, incurring the usual risks of a space mission.
- LST would have higher systematic-error risk, but can in many respects match the statistical power of JDEM if systematic errors, especially those caused by photometric-red-shift measurements, are small. An LST Stage IV program can be effective only if photometric-red-shift uncertainties on very large samples of galaxies can be made smaller than what has been achieved to date.

A mix of techniques is essential for a fully effective Stage IV program. No unique mix of techniques is optimal (aside from doing them all), but the absence of WL would be the most damaging, provided this technique proves as effective as projections suggest.

Shochet asked what program the Task Force expected to get under way to address item No. 15 (the mix of techniques is essential for a fully effective Stage IV program). Cahn replied that that is still being debated and is the reason why a final report has not yet been produced.

Dragt asked how big the LST was. Cahn replied that it is an 8.4-m telescope on the ground.

Carithers noted that one could show that dark energy is not the cosmological constant by two ways. Bernstein said that the uncertainty on w_p is the same as if you set w to 0. Cahn said that it could be shown if it is not close to the cosmological constant; however, that is not likely. Staffin said that measurements should be complementary. Cahn agreed. The different techniques have different shapes, and one can learn a lot if one forces them to intersect. It is a way to get more power out of the experiments. Staffin asked if budgets are additive. Cahn replied, yes. Many experiments use two or more techniques. It is a cost-effective way of getting good results. It is most important to use a multiplicity of techniques. Perlmutter noted that each method has to stand on its own two feet and will have a given uncertainty. Cahn responded that it is always helpful to have several measures, especially when you have surprising results.

Kahn asked if new resources would be required or whether the questions can be addressed with existing facilities. Cahn replied that the Task Force has not looked at what is achievable, but its final recommendations would probably say that that is a key issue.

Ritz noted that the P5 charge includes dark energy and asked if the Task Force had looked at how the report might be used and what else might be needed. Cahn said that the Task Force is trying to satisfy the needs of the agencies that gave the charge. It probably would not be wise to make conclusions about issues outside the expertise of many of the Task Force members.

A break was declared at 2:47 p.m., and the panel was called back into session at 3:09 p.m. to complete the discussion of the NuSAG report. Shochet said that one possibility would be to accept and pass on the report; another would be to add guidance to P5 in the cover letter; and a third would be to send the report back to the Subpanel. Baltay said that Double Chooz should be reflected more positively than it currently was in the report. Shochet said that he would like to hear the Panel's comments and recommend to the funding agencies that it go forward, if that were what the Panel decided.

Molzon asked how P5 would consider this report. Shochet replied that P5 has the charge to look at the big picture, including neutrino research. Seiden offered that P5 will look at everything on the table. Shochet said that P5 will look at the long-term program in many areas. Molzan asked what advice will be given to the funding agencies about the off-axis facilities. Seiden said that P5 will give an assessment in light of the potential of all the other programs.

Baltay said that he believed that NuSAG had done an excellent job that the Panel should support and let P5 use it as it will. Cahn said that the report is a little dated. There could be more on what could be achieved by pushing the machines a bit. Meyers replied that the group just used the numbers provided; it did not endorse the proton driver. Cahn offered that the issue could be dealt with by P5.

Shochet asked for a vote on accepting the report and said that he will mention the concerns in the transmittal letter. The report was accepted unanimously.

Shochet asked for comments on the new charge to NuSAG. Meyers noted that information from the relevant technical working group (to be gathered at a workshop this summer) will not be available until October, so having this report due in August is not optimal. He suggested an interim report at the June HEPAP meeting and the final draft after October. Byon-Wagner said that the Office would have to consult with NSF next week to see if that was acceptable. Meyers stated that the results of the workshop might significantly change what the group says in its report.

Montgomery pointed out that Fermilab will approach BNL in the fall about possible mutual interests and the long-term evolution of the neutrino program. Additionally, it is not known what will happen with DUSEL. A comparison of techniques seems reasonable. Fermilab hopes to develop a list of questions to be addressed. Nishikawa noted that there is a program in Japan, also. Some information might be able to be exchanged. Meyers pointed out that the group's charge asks it to survey the international community.

Wormser said that, if one just looks at these two options, one is not going to pick up on some exciting ideas. Meyers promised that the group will look over the horizon of the charge, including beta beams. Shochet stated that the second section of the second paragraph of the charge could be expanded. Meyers said that he did not know enough to say what to put in; neutrino factories, for example, are not within the proper timeframe. Shochet suggested that a telephone conference could be held to hash out these issues. Samios suggested that the term "*single and* multiphase" be added to the charge. Meyers pointed out that "multiphase" includes "single." The change does not add anything.

Cahn asked what a "one-megawatt class" meant. Shochet said that he believed that it referred to 1-, 2-, or 4-MW levels. Meyers replied that he believed that it meant to ask the question, does 1 MW get the job done or not?

Staffin suggested getting a handle on the costs of U.S. involvement at Daya Bay. The Chinese costs are broadly shared at the national, regional, and local levels, and they would warmly welcome U.S. participation. The United States will enter into discussions with them on what that participation might entail. Meyers suggested that it would be good to have nontechnical issues like that resolved in parallel with P5's technical assessment.

Shochet asked if the United States should participate in Double Chooz. Bortoletto commented that it is a very cost-effective next step in the neutrino program. Wormser said that it is not true that Double Chooz will necessarily proceed without U.S. participation; the project would be delayed if the United States did not participate. The United States should decide soon whether to participate. Shochet asked if there were any Panel members opposed to U.S. participation in Double Chooz. There were no responses. Brau stated that it is a great opportunity. Staffin observed that the prospective funding is not a small amount of money. If this Panel recommends funding Double Chooz, the money will have to be found somewhere else in the HEP budget, and that would probably delay some other program. Cahn asked what the U.S. contribution would be. Svoboda said, $4.8 million over 3 years from DOE and NSF. Cahn suggested that the summary letter should say that Double Chooz is well worth the cost. Staffin asked whether another reactor experiment would not be needed, then. Baltay said that it depends on what they find at Double Chooz. It could affect the design of the neutrino program that is being worked on. Meyers pointed out that another expectation of reactor experiments is that they resolve the ambiguities in this area. Nishikawa offered that K2K is already close to being able to provide cross-checks.

Shochet said that, in the summary letter, he would

- Express thanks for Orbach's, Marburger's, and Bement's attending the meeting;
- Express HEPAP's strong support for the report, *Discovering the Quantum Universe*;
- Express an appreciation for the increase in physics funding and the efforts that went into making that happen;
- Note that the NuSAG report was approved and transmitted;
- State that Double Chooz should be supported;

- Note that the physicist resources report was approved and transmitted and that the Panel was pleased with the positive response from Fermilab and with the report that there will be adequate manpower to continue work;
- Mention that laboratories, universities, and agencies should work together; and
- State that the Dark Energy Task Force presentation was informative and that the Panel looks forward to the final report.

Wormser announced that there will be a high-energy physics strategic plan drawn up in Europe and suggested that this Panel should stay abreast of those discussions and planning. Staffin said that he would be attending as an observer.

Shochet adjourned the meeting at 4:03 p.m.

Respectfully submitted,
Frederick M. O'Hara, Jr.
Linda M. O'Hara
Recording Secretaries
April 10, 2006

Corrected by
Melvyn J. Shochet
June 14, 2006

Marsha Marsden
June 16, 206

The minutes of the High Energy Physics Advisory Panel meeting held at The Latham Hotel, Washington, D.C., on March 3-4, 2006, are certified to be an accurate representation of what occurred.

Melvyn J Shochet ____________________________________
Melvyn J. Shochet, Chair, High Energy Physics Advisory Panel

_______June 19, 2006______
Date