Minutes of the
High Energy Physics Advisory Panel Meeting
July 6-7, 2006
Latham Hotel, Washington, D.C.

HEPAP members present:

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

HEPAP members absent:

Joseph D. Lykken	Tor Raubenheimer

Also participating:

Barry Barish, Director, Global Design Effort, International Linear Collider
Gene Beier, Department of Physics and Astronomy, University of Pennsylvania
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 (Friday only)
Robert Diebold, Diebold Consulting
Jonathan Dorfan, Director, Stanford Linear Accelerator Center
Gerald Dugan, Americas Regional Director, Global Design Effort, International Linear Collider
Tom Ferble, Office of High Energy Physics, Office of Science, Department of Energy
Paul Grannis, Office of High Energy Physics, Office of Science, Department of Energy
John Kogut, HEPAP Executive Secretary, Office of High Energy Physics, Office of Science, Department of Energy
Edward (Rocky) Kolb, Particle Physics Division, Fermi National Accelerator Laboratory
Marsha Marsden, Office of High Energy Physics, Office of Science, Department of Energy
Jay Marx, Executive Director, LIGO Laboratory, California Institute of Technology
June Matthews, Director, Laboratory for Nuclear Science, Massachusetts Institute of Technology
Hugh Montgomery, Associate Director for Research, Fermi National Accelerator Laboratory
Piermaria Oddone, Director, Fermi National Accelerator Laboratory

Frederick M. O'Hara, Jr., HEPAP Recording Secretary, Oak Ridge Institute for Science and Education
Randy Ruchti, Program Director, National Science Foundation
Abraham Seiden, Director, Institute for Particle Physics, University of California at Santa Cruz
Harold Shapiro, Professor of Econometrics, Mathematical Economics, and Science Policy, Princeton University
Robin Staffin, Associate Director, Office of High Energy Physics, Office of Science, Department of Energy
Andreene Witt, Oak Ridge Institute for Science and Education

About 80 others were also present in the course of the two-day meeting.

Thursday, July 6, 2006
Morning Session

Chairman Shochet called the meeting to order at 8:24 a.m. and introduced **Harold Shapiro** (Chair of EPP2010) to report on EPP2010.

It is hard to overestimate the critical nature of the next few years. A number of critical decisions need to be made. The current status of the U.S. program in a global context is changing. Mostly, we need to reverse the momentum of the program, which seems to be executing an exit strategy. The longer we wait, the worse our competitive position is going to be. A "different" and "scientifically pregnant" moment is occurring. The unusual features of this "moment" and this Committee are what make its report, *Revealing the Hidden Nature of Space and Time: The Final Report of the Committee on Elementary Particle Physics in the 21st Century*, distinctive.

The report examines and is framed by some initial conditions: the nature of the scientific opportunities, the current status of the U.S. program, and the current status of programs abroad. The report structure is characterized by an articulation of a set of strategic principles, an evaluation of alternative sets of priorities, and reasonable budget assumptions (at least including inflationary adjustments). Leadership cannot be maintained without at least inflationary adjustments. An order of priorities was selected; competitiveness, innovation, and the future position of U.S. science and technology came up at the last minute.

The Committee's membership was unusual in its breadth. It met at Stanford Linear Accelerator Center (SLAC); Fermi National Accelerator Laboratory (FNAL or Fermilab); Cornell; and Washington, D.C. It solicited community input through "town meetings," a web site, and written input. It visited High Energy Accelerator Research Organization in Ibaraki, Japan (KEK); Japan Proton Accelerator Research Complex (JPARC); Deutsches Elektronen-Synchrotron (DESY); and Conseil Européen pour la Recherche Nucléaire (CERN, now Organisation Européenne pour la Recherche Nucléaire).

The key questions in particle physics have been put forward in previous reports. These questions point to the potential transformation of EPP. The question of whether particle physics still matters arose. It was not obvious that the United States should strive to maintain leadership in EPP. How important *is* this subject of human inquiry?

The U.S. program's support has stagnated for the past 10 years, the intellectual center of gravity is moving abroad, and major experiments are coming to the end of their scientifically useful lives with no clear follow-on plan in place. This is a dangerous moment; however there is a silver lining: as facilities close or change focus, resources are becoming available within the program to support and launch new initiatives. But this support can be lost; and if lost, will not come back for a decade or more.

The committee spent a lot of time on the scientific role and the cost and schedule of the International Linear Collider (ILC), which is like the Large Hadron Collider (LHC) in that the United States cannot do it alone. It is a huge project. The ILC would only become tenable after costs become known and initial LHC results complete the grounds for decision making. If the LHC does not succeed, funding for the ILC would be zero. Is this a potential opportunity for the United States? If so, preliminary investment of risk capital is needed, and a successful U.S. bid-to-host requires taking initiative now. The next question is, is there a leadership strategy going forward, and to what role should the United States aspire? Leadership will be shared. The United States cannot be the leader in everything, but should aspire to a leadership role in the global program of particle physics. A diverse portfolio of activities is crucial. Vision, priorities, risk taking, flexibility, and responsible budgets are needed. Planning is needed to fully achieve the potential of particle physics.

The Committee's strategic principles are designed to set out the general framework or constraints within which particular priorities are selected. The priorities reflect both the long-term aspirations of the program and the necessary flexibility to adjust to the empirical facts that emerge from year to year. One set of priorities rejected any consideration of a major commitment to new accelerator facilities here. The Committee, however, adopted another set of priorities, which included investments aimed at the potential establishment of new domestically based experimental facilities to study terascale physics under laboratory conditions. The Committee believed that only a strategy that included a major commitment to exploit the terascale opportunities both at the LHC and with domestically based accelerator facilities would yield the highest risk-adjusted return for the United States program and the best opportunity to sustain (or regain) U.S. distinction in this area. Choosing not to pursue on a broad front the direct exploration of the terascale would be equivalent to walking away from leadership in particle physics.

The priorities selected by the Committee were to exploit the opportunities offered by the LHC; mount an internationally compelling bid to build the ILC on U.S. soil, recognizing that this is a risky proposition; seize the opportunities at the intersection of particle physics, astrophysics, and cosmology by coordinating and expanding domestic efforts; pursue an internationally coordinated, staged program in the physics of neutrinos and proton decay; and pursue precision probes of physics beyond the Standard Model, using available resources as a guide to overall level of effort while maintaining diversity.

The question must be asked, "What can we best build together?" In articulating its priorities, the Committee chose a strategy that leverages U.S. strengths for the global community and adds to the long-term vitality of the entire field. Also, can a competitive, globally relevant national program be sustained if the major new initiative is an accelerator-based neutrino program at Fermilab? This question was discussed a lot. The committee could not see a long-term leadership role for the United States in this scenario.

The take-home messages of the Committee's report are

- Particle physics is at a special time: There is great theoretical and experimental evidence that a revolution could be in the making.
- Particle physics in the United States is at a crossroads.
- Without clear, decisive action in the next few years, the U.S. program will deteriorate.
- The United States should continue to support a competitive program in this key scientific field.
- The Committee has outlined a strategy that it believes has the best chance to put the United States at the forefront of the field with a program of distinction and importance.
- The Committee's vision is that particle physicists, both here and abroad, and their students will be able to pursue whatever aspect of the field that they find intellectually exciting.
- To achieve this vision and to be accountable for the responsible use of public resources require that investments in new experimental facilities be "internationally optimized" and open to all scientists on an equitable basis.
- The Committee's work is an attempt to *start* an important process; many decisions need to be made.
- The particle physics community must come together on the strategic issues and on their aspirations and must define the path forward in considerably greater detail.

Shochet thanked the Committee for its important work and introduced **Sally Dawson**, the Vice Chair of EPP2010. The Committee made some specific recommendations: Conduct R&D to be prepared to build the ILC. Prepare a bid to site the ILC in the United States. It endorsed the P5 concept as the proper way to select experiments and locations for the national program; these decisions must be made jointly by DOE, NSF, and NASA. A broad-based program is needed, not just the ILC. The neutrino physics effort should be increased; this will be expensive and should be thought about in an international context.

Cahn estimated that the probability of getting the ILC here is between 20 to 50% and asked what the Committee believed would happen if the ILC does not pan out. Shapiro said that his guess was that the field would become smaller. Competition for resources would increase. What science would be compelling would have to be decided on. It is HEPAP's job to determine what position to assume. There are many more-certain ways to spend money, but risks should be taken.

Ritz noted that it was said that it was not obvious that EPP should continue and asked how the recipients of the report were reacting. Shapiro replied that the story is not that hard to make; people are just not aware of the situation and how critical it is. One can make a case for EPP in 15 minutes. People do not recognize what is going on or the international dynamics involved.

Brau asked what encouragement they got from international partners. Shapiro replied that they did not ask potential partners about their supporting a United States-led effort. Many are waiting for the United States to make a commitment. Many countries want the ILC, but many would side with the United States if the United States makes a commitment. This is not a big expense at the national level.

Hewett asked if the money for the ILC would come out of the HEP budget, and how this should be supported for long term. Shapiro replied that, if the budget situation does not improve in the next few years, there will be many other problems the country will have to deal with, and the problem of funding the ILC will become normalized. Economics will not be the big constraint. Someone has to want it.

Samios commented that the presentation was better than the report and pointed out that there must be a plan B in case the ILC does not pan out.

Wormser asked if there was enough commitment to EPP. Shapiro replied that there is support from DOE. Someone has to keep the area visible at a high level all the time. That is not the National Academy of Sciences (NAS) panel's job. That task must fall to the EPP community.

Matthews was struck by the need to reverse the momentum. The cancellation of programs and ending of SLAC and Fermilab programs are driving away young researchers. She asked if the Committee addresses the role of universities in EPP. Shapiro said that the Committee made a great effort to solicit input from young scientists. These experiments are very long-lived. One will not commit 10 years of one's life to something that may not pan out. The academic community must address the needs of students continuously. The Committee did not address that question. The Committee recommended a diverse portfolio. A single-end strategy (like the ILC) is a dead end.

Diebold asked him to expand on the recommendations regarding the advisory apparatus. Shapiro said that the Committee saw the national laboratories having their own advisory committees. They need a more coordinated approach. How to do that needs to be thought through. Bagger added that the field is becoming more international, and the advisory process has to be similarly international. Shapiro responded that the Japanese have integrated foreign nationals into their advisory process. Dawson added that the Committee thought that the advisory process needs to be improved.

Shochet introduced **Robin Staffin** to update the Committee on activities of the DOE Office of High Energy Physics (HEP).

Raymond Orbach has been confirmed as Under Secretary of Energy for Science. This is a position of great authority. He is still Director of the Office of Science (SC).

HEPAP exists in an age of reports. These reports are stitched together by P5 and sent to the full Panel for action. There needs to be a cohesiveness and consensus in the field.

The budget request for FY07 is being considered by Congress. The House has approved the full request. The Senate has approved the full amount of the bottom line but with some interesting directives on the internal organization and funding of SC. It reduces the request for ILC from $60 million to $45 million and ties it to results from the LHC. It put $15 million additional into participation in international Joint Dark-Energy Mission (JDEM) programs. And $8 million was taken out of the HEP budget for partial support of a new Office of High Energy Density Science.

There has been a FALC [Funding Agencies for the Linear Collider] meeting. The report will come out in about a month. The recommendations are similar to those of the NAS study.

Samios asked if the transfer of $8 million was justified. Staffin said that the short-term impact takes the money out of theory when theory support is very important. But the Office understands the value of close coordination in high-energy-density physics.

Cahn asked how concerned the Panel should be about the wording of the draft resolution. Staffin replied that he always takes seriously what Congress writes. This bill will be discussed in conference through the summer. Some education will occur during that process.

Shochet asked **Joseph Dehmer** to present an update on the elementary particle physics activities at the National Science Foundation.

The case for the Deep Underground Science and Engineering Laboratory (DUSEL) has been considered in many venues over many years. HEPAP's long-range plan does not spell out a path forward. In 2002, the Nuclear Science Advisory Committee (NSAC) strongly supported development of an underground laboratory to enable some aspects of nuclear research. The United States should consider a world-leading facility enabling a broad program of frontier research through generations of technical and scientific advances, providing infrastructure and detectors. Engineering could begin soon. The scientific case is very robust, and the project is well aligned with the mission of the NSF; it would have transformational impact on multiple disciplines, great education and outreach potential, and a unique vision within the government. Investment in R&D of approximately $6 million is planned for DUSEL and DUSEL-related experiments in FY07. The Engineering Division of NSF has offered more money. Interagency coordination and prioritization is proceeding through the National Science and Technology Council (NSTC) Working Group on Physics of the Universe. For particle physics, DUSEL introduces diversity into the portfolio, complementing the ILC.

In June 2006, conceptual design reports were submitted by the Henderson and Homestake teams; these will be evaluated. In September 2006, a solicitation was issued for proposals to develop a "preliminary" (baseline) design of a DUSEL, including an initial suite of experiments. In December 2006, the deadline for proposals will occur. In April 2007, an award will be made to design the DUSEL at the selected site. In October 2007, the deadline for the DUSEL Baseline Plan (including the cost, schedule, scope, and management) will occur. In December 2007, the NSF Baseline Review will be conducted. In March 2008, the DUSEL package will be ready for consideration by the Major Research Equipment and Facilities Construction (MREFC) panel.

Less than $500 million will be available for Phase 1. The conceptual design reports give a rough estimate of construction costs with <50% for infrastructure and >50% to experiments. They talk about the basic underground infrastructure: the subsurface construction of underground stopes (rooms) and drifts (tunnels) and utilities for experiments, the surface laboratory/office and outreach center, the underground clean-room facilities, the ultra-pure materials stock and fabrication facility underground, and the emphasis on safety systems. The initial suite of experiments will include the dark-matter detector and neutrino-mass experiment, the geosciences and biosciences experiments, and the engineering (rock mechanics, tunneling, and large excavations). There is significant long-term discovery potential, and most of the DUSEL physics experiments are also in the DOE 20-year facilities plan.

In summary, this proposal is one option for broadening the field. Both of these locations are in the comfort zone. HEPAP should discuss and comment on this program and strategy.

Ritz noted that DUSEL is being discussed in P5 and asked what the operating costs would be and how much would fall on the particle physics budget. Dehmer replied that the operating costs would be $40 million per year. If the budget undergoes growth, that will provide for this cost. NSF has a commitment to provide >50% of the Physics

Division budget to the core grant program. That commitment will not be altered.

Shochet asked if this is being discussed at the higher levels at NSF. Dehmer replied, no. That will come later. He would worry about science balance, not internal dynamics at NSF.

Samios asked if DOE was on board. Dehmer and said that DOE takes care of accelerators and NSF does the oddball stuff. The agencies jointly review. DOE does not want to be a primary sponsor of DUSEL.

Ferble asked if international support is being considered. Dehmer said that the NSF has an interesting proposal in hand, and it is moving it forward. It may garner international support as it is developed.

Shochet called upon **Jay Marx** to report on the Advanced Accelerator R&D Subpanel. The charge to the Subpanel was to undertake a comprehensive review of all aspects of the OHEP and NSF accelerator R&D programs with the exception of technical and management review of Linear Collider R&D and the LHC Accelerator Research Program (LARP). The panel was charged to address national goals, stewardship, scope, quality, relevance, resources, management, and training. The Subpanel had a mix of topics, scientists, and nationalities. It had several multiday meetings, including town meetings, and got input from laboratories, universities, industry, and members of the community.

To the Subpanel, short-term accelerator R&D is that related to existing or approved accelerator facilities (mostly ballistic). Medium-term accelerator R&D is that related to envisioned new accelerator facilities (mostly strategic development and targeted). And long-term accelerator R&D is exploratory research aimed at developing new and innovative concepts, new techniques and technologies, and advancing fundamental accelerator science (mostly exploratory and forward-looking research).

The Subpanel found an urgent need to strengthen accelerator science, technology and education in the United States to address long-term needs of particle physics, other sciences, and the nation. A very difficult challenge must be met: future energy-frontier accelerators that provide extremely high energy and luminosity within a feasible cost to society. The nation should continue to expand the contributions of accelerators to other sciences, the economy, security, and other national needs, enhancing recognition of the practical contributions of HEP to society. The Subpanel would like to see OHEP and NSF recognize this need and become the formal stewards of accelerator science and technology and of education in these disciplines.

OHEP has had a historical (informal) stewardship for fundamental accelerator science and technology. The Subpanel endorses the importance of this stewardship responsibility and recommends that it be formally recognized in the mission statement of OHEP by include the following: “The Office of High Energy Physics (OHEP) provides program planning, oversight, and funding for research in fundamental accelerator science and technology.” The Subpanel recommends that a new NSF program, Accelerator Physics and Physics Instrumentation (APPI), be established and funded. It would be a major step toward recognition of the value of accelerator science.

The Subpanel is concerned that there will not be enough accelerator scientists and engineers to meet future needs. Several issues limit the number of universities that provide opportunities to study accelerator science and engineering:

- Accelerator science and technology is not yet broadly recognized as an essential, vital, and exciting frontier research field.

- Most universities do not considered it an academic subject worthy of faculty lines.
- Few incoming graduate students are aware of either its existence or its contributions, challenges, and promise.

As a catalyst for new accelerator degree programs, the Subpanel recommends that an Accelerator Science Graduate Fellowship program in the DOE and NSF should be given high priority. It would enhance the visibility and stature of the field and help to attract the best students; it would also encourage universities to see the value of a program in accelerator science. DOE and NSF should expand opportunities for education and training in accelerator science. Given the special role of education in the NSF mission, a strong commitment to training and research in accelerator science would significantly enhance the recognition of this field in the universities.

The Subpanel identified key enabling science and technology needed for planned and envisioned particle-physics accelerator facilities, as well as facilities for other sciences. The overall quality of the R&D on these enabling technologies is very high, and the program addresses all of the key topics. The program is generally well-balanced given the level of available support. In the near term, the highest priority will be ILC R&D; but a balanced R&D program that addresses all of the important topics must be maintained. Sustaining excellence requires relatively stable funding, modernization of infrastructure, and a continuous inflow of well-trained new researchers.

A strong U.S. program in superconducting rf is essential for progress in accelerator science for HEP. The United States is currently behind Europe and Asia in capabilities, infrastructure, and industrialization in this area. The Subpanel is concerned that the support for muon cooling is below what is needed to sustain this program; support for the Muon Ionisation Cooling Experiment (MICE) leaves little for other R&D. Without increased support, essential intellectual resources will disappear. Within the LARP program, careful consideration should be given to the balance between producing hardware deliverables for CERN and activities (e.g., commissioning) that substantially enhance the intellectual and technical capabilities of the U.S. accelerator community.

A primary focus of the Subpanel is the longer-term accelerator R&D programs supported by OHEP and NSF. A healthy and stable program of long-term accelerator R&D is essential for HEP’s future. The long-term accelerator R&D supported by OHEP and NSF are unique programs that are effective and scientifically valuable. The overall quality of the U.S. programs in long-term accelerator and technology R&D is very high. Most of these programs are world class and, in many specific areas, world leaders.

The Subpanel has concern about R&D in long-term superconducting rf. Less than 5% of the U.S. effort on superconducting rf is on long-term R&D. This is inadequate, given the need for basic understanding of the physics of superconducting rf limitations, materials, and surface properties. The Subpanel recommends that OHEP and NSF increase support and build a healthy program to address the fundamental issues of superconducting rf and cavity properties, materials, and surface science.

It is important to encourage and support advanced accelerator R&D at the national laboratories. Such R&D has recently been constrained by programmatic and project-related goals, decreasing the flexibility to pursue new ideas or technologies that could form the basis of new and important capabilities. The Subpanel recommends that OHEP should accept proposals from the national laboratories to pursue long-term accelerator

R&D and should invest in appropriate research and supporting infrastructure. Previous subpanels suggested 4% of the HEP operating budget and emphasized the importance of stable funding. The need for more educational opportunities, strengthening accelerator science and the difficult challenges ahead (very high energy and luminosity, affordability, and low energy consumption) lead the Subpanel to recommend an increased investment during the next decade. This Subpanel recommends that the percentage of the OHEP budget assigned for long-term accelerator science should be 5% in FY07 and increased gradually and smoothly to 6% during the next 10-year period.

Coherence of management, oversight, and planning should be increased across the OHEP accelerator R&D portfolio. Within OHEP, oversight for accelerator R&D at universities and laboratories should be the responsibility of a single team of program managers. Medium and long-term accelerator R&D programs in OHEP should undergo a yearly review by a broad-based committee of accelerator scientists, including members who are cognizant of the possible longer-term accelerator based needs of the other SC and NSF programs. This committee should be appointed with overlapping terms to assure continuity. Long-term R&D should be additionally subjected to an expert review process to consider and prioritize all program elements; this will allow terminating the worst-rated programs while adequately supporting the leading ones.

OHEP should develop a strategic framework for its portfolio of medium-term and long-term accelerator R&D. This framework should be consistent with the overall strategic direction of particle physics and the anticipated needs of SC in the context of international efforts. This strategic framework must identify and develop new concepts for future energy-frontier accelerators that can provide the very high energy and luminosity needed for HEP within a feasible cost to society. The Subpanel recommends that OHEP develop such a plan for medium-term Advanced Accelerator R&D (AARD) based on the upcoming P5 roadmap and review and update this plan on a yearly basis.

Strategic principles need to be developed to guide management of longer-term accelerator R&D. Examples include: The breadth of long-term accelerator R&D should reflect the stewardship responsibility of OHEP. Highest priority should be given to producing new inventions, techniques, approaches, or technologies to extend the reach of accelerator-based physics and to research on fundamental aspects of accelerator science. Contributions from the universities, the national laboratories, and industry should all be encouraged. Collaborative activities and the education and training of students and postdocs should be strongly encouraged.

In summary, the Subpanel emphasizes the critical importance of accelerator science and technology to particle physics, other sciences, and the nation. It finds that there is an urgent need to strengthen accelerator science, technology, and education in the United States and to strengthen accelerator science as an important scientific discipline. More opportunities are needed for education and training of the next generation of accelerator scientists and engineers. The level and quality of short- and medium-term accelerator R&D need to be sustained. A healthy and stable program of long-term accelerator R&D is needed. And more-coherent management, oversight, and planning for the OHEP portfolio of accelerator R&D is required. The Subpanel, in its report, gives funding agencies information, ideas, and advice to help them work toward meeting these needs.

Shochet asked what the increase from 4% to 6% of operating funds was based on. Marx replied that this is the Subpanel’s guess of the required level.

Samios said that he believed that the amount should be beefed up. Accelerator R&D is critical. Marx replied that discussants of the report have made the same point. Brau suggested taxing the Office of Basic Energy Sciences (BES) for the support HEP provides them in accelerator design.

Ozaki noted that there is no funding for education. Marx replied that the Subpanel talked about that in the report in terms of training and education needs.

Bagger asked how well the program would evolve if there were no core ILC program. Marx replied that the Subpanel did not discuss that. It assumed the United States would be involved in the accelerator design no matter where the ILC was built.

Dorfan stated that it is the cost per GEV that needs to be worked on. More investment needs to be made in that area, and that fact should be stressed in the report.

Grannis noted that the Senate markup suggested that some large-term accelerator R&D be moved into a new office and asked what the Subpanel thought about such diffusion of effort? Marx said that he was nervous about accelerator R&D resources diffusing out of the HEP program. HEP's programs should take up the challenge. Staffin said that he had never met a Senator that did not ask for more funding. He expected HEPAP to prioritize these efforts. If something is to go up, what is to go down? Taxing another office in SC is not something we can do. If HEPAP does not balance the books, the Office will have to.

Matthews commented that there is no mention of nuclear research. Marx pointed out that the report recognizes the work done at the Jefferson Laboratory and elsewhere.

A break was declared at 11:03 a.m. The meeting was called back into session at 11:20 a.m. Ritz commented that the nation needs a way to move on in accelerator R&D and that was not in the roadmap. Shochet asked for approval of the report of the Advanced Accelerator R&D Subpanel. The vote for approval was unanimous. Shochet introduced **Rocky Kolb** to review the report of the Dark Energy Task Force (DETF), which report has been approved by the Astronomy and Astrophysics Advisory Committee (AAAC).

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. These circumstances demand an ambitious observational program to determine the dark energy properties as well as possible.

DOE, National Aeronautics and Space Administration (NASA), and NSF have with two charge letters commissioned a task force to look at dark energy. The DETF has 13 members, and its membership overlaps with AAAC, HEPAP, and the Science Definition Team (SDT). It has been asked to advise the agencies on the optimum near- and intermediate-term programs to investigate dark energy and to advance the justification, specification, and optimization of the Large Survey Telescope (LST) and Joint Dark Energy Mission (JDEM). The Task Force was not asked to prioritize projects. It conducted weekly phone conferences, held five meetings, issued 50 white papers, made briefings to agencies and advisory committees, and issued a preliminary report in April and a draft report in May that was accepted by the AAAC in June.

There is conclusive evidence for the acceleration of the universe. There are three possibilities: dark energy is constant in space and time (Einstein's cosmological constant, Λ), dark energy varies with time [or redshift z or $a = 1/(1 + z)$], or general relativity (GR) or the standard cosmological model is incorrect. It is not presently possible to determine the nature of dark energy. An observational program must (1) determine whether the accelerated expansion is caused by a cosmological constant; (2) 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)]; (3) search for a possible failure of GR through comparison of cosmic expansion with growth of structure. The goals of a dark-energy observational program are to measure the expansion history of the universe and to measure the growth rate of structure. All of these can be described by w(a). w(a) is a continuous function that must be parameterized; no parameterization can represent all possibilities; the Task Force chose $w(a) = w_0 + (1 - a)w_a$. This parameterization assumes that dark energy was insignificant at early times.If there is a failure of GR, a possible difference in w(a) will be inferred from different types of data.

To quantify progress in measuring the properties of dark energy, a figure-of-merit is defined from a combination of the uncertainties in w_0 and w_a. The DETF figure-of-merit is the reciprocal of the area of the error ellipse enclosing the 95% confidence limit in the w_0–w_a plane. A larger figure of merit indicates greater accuracy. The figure of merit serves as a quantitative guide to constrain a large, but not exhaustive, set of dark-energy models. There is no better choice of a figure of merit available at this time.

The Task Force made extensive use of statistical (Fisher-matrix) techniques incorporating cosmic microwave background (CMB) and Hubble constant (H_0) information to predict future performance with 75 models. The results reflect four developmental stages:

I. What was known at the end of 2005.
II. The anticipated state of knowledge upon completion of ongoing projects.
III. The knowledge produced by near-term, medium-cost, currently proposed projects.
IV. And the state of knowledge upon completion of the Large-Survey Telescope (LST) and/or Square Kilometer Array (SKA), and/or Joint Dark Energy (Space) Mission (JDEM).

Four observational techniques dominate the Task Force's white papers:

- Baryon acoustic oscillations (BAO), in which large-scale surveys measure features in the distribution of galaxies.
- Cluster (CL) surveys measure the spatial distribution of galaxy clusters.
- Supernovae (SN) surveys measure the flux and redshift of Type Ia supernovae.
- Weak lensing (WL) surveys measure the distortion of background images caused by gravitational lensing.

The 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 is only recently established; it is less affected by astrophysical uncertainties than are the other techniques. CL is the least developed; its eventual accuracy is very difficult to predict, and 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 powerful and best proven technique. If photo-zs (photometrically determined relative differences between observed and emitted wavelengths) are used, the power of the supernova technique depends critically on the accuracy achieved for photo-zs. If spectroscopically measured redshifts 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 also an 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 more than an order-of-magnitude increase in the figure of merit, 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. That said, results on structure growth, obtainable from weak-lensing or cluster observations, are essential program components to check for a possible failure of GR.

In the modeling, constraints are assumed on H_0 and on other cosmological parameters that are based on current data and expected measurements of CMB temperature and polarization anisotropies. These data, though insensitive to w(a) on their own, contribute to the knowledge of w(a) when combined with any of the dark-energy techniques considered. Increased precision in a particular cosmological parameter may improve dark-energy constraints from a single technique, which is valuable for comparing independent methods. Improvements in cosmological parameters, however, tend not to improve knowledge of dark energy from a multitechnique program. Setting spatial curvature to zero greatly helps SN, but has modest impact on other techniques. Little difference is produced when used in combinations. Optical, infrared, and X-ray experiments with very large numbers of astronomical targets will rely on photometrically determined redshifts. The ultimate accuracy that can be attained for such measurements is likely to determine their power.

The inability to reliably forecast systematic error levels is the biggest impediment to judging the future capabilities of the techniques. The techniques' needs are

- BAO: Theoretical investigations of how far into the nonlinear regime the data can be modeled with sufficient reliability and further understanding of galaxy bias on the galaxy power spectrum.
- CL: Combined lensing and Sunyaev-Zeldovich (SZ) and/or X-ray observations of large numbers of galaxy clusters to constrain the relationship between galaxy-cluster mass and observables.
- SN: Detailed spectroscopic and photometric observations of about 500 nearby supernovae to study the variety of peak explosion magnitudes and any associated observational signatures of effects of evolution, metallicity, or reddening, as well as improvements in the system of photometric calibrations.
- WL: Spectroscopic observations and multiband imaging of tens to hundreds of

thousands of galaxies out to high redshifts and faint magnitudes to calibrate the photometric redshift technique and understand its limitations. It is also necessary to establish how well corrections can be made for the intrinsic shapes and alignments of galaxies, to remove the effects of optics (and from the ground the effects of the atmosphere), and to characterize the anisotropies in the point-spread function.

Six types of Stage III projects have been considered:

- A BAO survey on an 8-m class telescope using spectroscopy
- A photo-z BAO survey on an 4-m class telescope
- A photo-z CL survey on an 4-m class telescope for clusters detected in ground-based SZ surveys
- A SN survey on a 4-m class telescope using spectroscopy from a 8-m class telescope
- A photo-z SN survey on a 4-m class telescope
- A photo-z WL survey on a 4-m class telescope

These projects are typically projected by proponents to cost in the range of tens of millions of dollars.

For Stage-III projects:

- Only an incremental increase in knowledge of dark-energy parameters is likely to result from a Stage-III photo-z BAO project. The primary benefit would be in exploring photo-z uncertainties.
- A modest increase in knowledge of dark-energy parameters is likely to result from Stage-III photo-z SN project. Such a survey would be valuable if it were to establish the viability of photometric determination of supernova redshifts, types, and evolutionary effects.
- A modest increase in knowledge of dark-energy parameters is likely to result from any single Stage-III CL, WL, spectroscopic BAO, or spectroscopic SN survey.
- The SN, CL, or WL techniques could, individually, produce factor-of-2 improvements in the DETF figure-of-merit if the systematic errors are close to what the proponents claim.
- If executed in combination, Stage-III projects would increase the DETF figure-of-merit by a factor in the range of approximately 3 to 5, with the large degree of uncertainty due to uncertain forecasts of systematic errors.

The Task Force looked at four types of next-generation (Stage IV) projects:

- An optical LST that uses one or more of the four techniques,
- An optical/near-infrared JDEM satellite that uses one or more of four techniques,
- An X-ray JDEM satellite that would study dark energy by the cluster technique, and
- An SKA that could probe dark energy by weak lensing and/or the BAO technique through a hemisphere-scale survey of 21-cm emission.

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

Each of the Stage-IV projects considered (LST, JDEM, and SKA) offers compelling potential for advancing our knowledge of dark energy as part of a multitechnique program. However, 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/near-infrared JDEM can mitigate systematics because it will likely obtain a wider spectrum of diagnostic data for SN, CL, and WL than possible from the ground, but incur 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 photo-z measurements, are small. An LST Stage-IV program can be effective only if photo-z 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. The technique mix may be comprised of elements of a ground-based program or elements of a space-based program or a combination thereof. No unique mix of techniques is optimal (aside from doing them all), but the absence of weak lensing would be the most damaging, provided this technique proves as effective as projections suggest.

The Task Force recommends

- An aggressive program should be pursued to explore dark energy as fully as possible, because it challenges our understanding of fundamental physical laws and the nature of the cosmos.
- The dark energy program should have multiple techniques at every stage, at least one of which is a probe sensitive to the growth of cosmological structure in the form of galaxies and clusters of galaxies.
- The dark energy program should include a combination of techniques from one or more Stage-III projects designed to achieve, in combination, at least a factor of 3 gain over Stage II in the DETF figure of merit, based on critical appraisals of likely statistical and systematic uncertainties.
- The dark-energy program should include a combination of techniques from one or more Stage-IV projects designed to achieve, in combination, at least a factor of 10 gain over Stage II in the DETF figure of merit, based on critical appraisals of likely statistical and systematic uncertainties. Because JDEM, LST, and SKA all offer promising avenues to greatly improved understanding of dark energy, we recommend continued research and development investments to optimize the programs and to address remaining technical questions and systematic-error risks.
- High-priority, near-term funding should be given as well to projects that will improve the understanding of the dominant systematic effects in dark-energy measurements and, wherever possible, reduce them, even if they do not immediately increase the DETF figure of merit.
- The community and the funding agencies should develop a coherent program of experiments designed to meet the goals and criteria set out in these recommendations.

The Task Force believes its legacy will be (1) a standardization of the parameterization of dark energy as $w_0 - w_a$, of the eight-parameter cosmological model, of the set of priors, and of the figure of merit; (2) a recognition of the importance of combinations; and (3) the DETF technique performance projections that include 32 data models, optimistic and pessimistic projections, and the four techniques in two stages and with five platforms.

Olinto asked whether Stage III would help one know the Stage IV systematic errors. Kolb said, yes; it would illuminate the techniques necessary for Stage IV.

Cahn stated that one should complete Stage III before going on to Stage IV.

Samios asked if anyone has come up with a model that is not covered and what the probability was that dark energy is conclusively there. Kolb replied that the observed data do not fit the cosmological model. He would not bet is life on it, but he would bet Mike Turner's life on it. There is a wide class of parameterizations; this is robust. But dark energy may be much stranger than we can model here. That is why the Task Force proposes pursuing the figure-of-merit method and using it to decide on the next steps.

Perlmutter suggested that the website where people can compare techniques might be more helpful if it had room in it for one extra parameter that reflected modified gravity so that people do not lose sight of the fact that that is also an important one for the complementary techniques to be able to achieve.

Baltay asked if they had any models that indicated what might be wrong with GR. Kolb responded that there are lots of ways to do it: the tired-light graviton approach, extra dimensions, etc. But there is no compelling model to adopt. There is the potential for determining where the problem is (observationally). One does not really know what one is searching for.

Wormser asked about the relationship with CMB experiments. Kolb said that, in the modeling, assumptions were made of what would be learned from the Planck Telescope. There was a CMB report; the post-Planck program will look at polarization effects of gravity waves. Planck will be it for awhile.

Ritz stated that the establishment of web-based vetting techniques should be encouraged in future subpanel studies, as was done here.

Staffin said that optimization should reflect the most return for a given investment. What do you get from Stage IV as opposed to Stage III? Kolb replied that there was no magic number in a figure of merit or other parameter. Staffin noted that there is a big difference between $10 million and $100 million. Cahn said that, in the end, one has to ask how fundamental this science is. This is very fundamental. One cannot quantify it. Staffin asked if one can do a Stage III and wait for Stage IV. Cahn stated that one must move forward on Stage IV to know how well it can be done. Kolb added that Stage III will affect how well Stage IV can be executed. One will get physics back at each of these stages. Bagger noted that the JDEM definition team foundered on just that question.

Shochet asked for approval of the report. The vote for approval was unanimous. A break for lunch was declared at 12:40 p.m.

Thursday, July 6, 2006
Afternoon Session

The meeting was called back into session at 1:46 p.m., and **Abraham Seiden** was asked to report on the P5 Subpanel.

The charge to the Subpanel was to propose a detailed roadmap for the U.S. high-energy-physics program for roughly the next 10 years, with particular focus on the decisions needed in the next 5 years, mindful of the international context. This roadmap should lay out the most compelling scientific opportunities that can be addressed in that timeframe. In addition, the Subcommittee was to provide a specific prioritization of the major elements of the roadmap. This prioritization should assume the future yearly budget envelope provided by the funding agencies. Projects to be assessed included:

- Operations of the Tevatron and BaBar
- U.S. contributions to LHC operations, computing, and upgrades
- The neutrino program
- DUSEL and its associated experiments
- ILC R&D
- Next-generation dark-matter experiments
- Dark energy experiments

Where relevant, the Subpanel was to consider the impact of potential program decisions taken elsewhere within the international HEP community, their relation to the programs of related fields, and their broader impact on science and society.

P5 has had four meeting to gather information and to work on the Roadmap and recommendations. These have included

- March 27–28 near Washington to hear from the agencies, hear about the LHC program, and learn about a proposal for a more precise $g - 2$ experiment
- April 18–19 at Fermilab with a focus on neutrinos, potential dark matter experiments, and the Neutrino Science Advisory Group (NuSAG) report
- April 20–21 at SLAC with a focus on the ILC R&D program, dark energy, and the DETF report; the Subpanel also reviewed the issue of the FY08 running of the B-factory

P5 received the EPP2010 report in April and discussed the report with Sally Dawson in a phone conference. It expressed its thanks to the EPP2010 committee for its enormous effort. P5 is enthusiastically trying to implement the vision EPP2010 has given.

A P5 meeting on June 12 and 13 aimed at starting to produce a roadmap. A letter has been sent to HEPAP with the P5 planning guidelines and recommendation on FY08 running of the SLAC B-factory in the context of a roadmap for FY08 so HEPAP can judge the recommendation more fully. The letter concerns mostly larger-scale efforts, but P5 strongly values a number of smaller projects, the development of new initiatives and associated R&D, and collaboration on projects abroad. The letter also does not explicitly mention the critical work in theoretical physics. These are all very important.

Dark Matter is an area where significant new results are expected in the near future, from continually more-sensitive direct-detection experiments, direct-production searches at the LHC, the search for dark matter annihilation signatures using cosmic photons, and the axion-search experiments. In direct detection, a number of powerful techniques are being developed that will likely lead to the possibility of using several large detectors, in an international setting, based on different techniques to search for direct interactions of dark matter. It should be possible to start construction of a large-scale detector early next decade. P5 looks forward to receiving the evaluation of the options by the Dark Matter Scientific Assessment Group.

An attractive option for locating a large Dark Matter detector would be in DUSEL. It is likely that an ambitious experiment to search for neutrinoless double-beta decay could be ready on a time scale similar to that for the dark-matter detector. These could be the initial flagship experiments from the particle physics community for DUSEL, providing a very exciting physics program. They should be included in the DUSEL MREFC plan. P5 would not, however, like to see the development of DUSEL delay the execution of these experiments. This is a competitive area.

P5 has been learning a lot from the DETF report and is still developing further science questions about (1) a better understanding of what the Stage-IV experiments add if one goes away from a linear assumption for $w(a)$ and (2) a better understanding of how well one can test other physics beyond just dark energy (for example GR). P5 is very enthusiastic about the possible Stage-IV experiments that significantly further the understanding of dark energy. The techniques for measuring the effects of dark energy have evolved during the past few years and now offer a way to broadly test the cosmological picture of the universe. These experiments would involve interagency collaboration, and developing a plan that works well within the various interagency constraints will be very important.

P5 has been able to use the NuSAG report as input to its planning. NuSAG has discussed three kinds of projects: detectors to search for neutrinoless double-beta decay, where several techniques are under development; reactor experiments that might unambiguously measure the third mixing angle in the neutrino sector; and accelerator-based experiments that might establish CP violation and determine the mass hierarchy.

The mature accelerator-based option within the United States is the NOνA experiment. P5 has spent a considerable amount of time with the proponents to understand the reach of the experiment. It has several important features: It can run neutrinos or antineutrinos depending on the state of knowledge at a given time; it allows upgrades by increasing the machine luminosity and detectors; and it provides the best near-term direction for resolving the mass hierarchy among the neutrinos.

The letter to HEPAP provides the P5 recommendation regarding the running of the SLAC B-factory for FY08. To produce a roadmap, P5 has adopted a number of planning guidelines:

- The LHC program is the most important near-term project, given its broad science agenda and potential for discovery.
- The highest priority for investments toward the future is the ILC. We need to participate vigorously in the international R&D program for this machine and its detectors as well as accomplish the preparatory work required if the United States is to bid to host this accelerator.
- Investment in a phased program to study dark matter, dark energy, and neutrino interactions is essential for answering some of the most interesting science questions before us.
- P5's recommendations will include rough dates for reviewing technical progress to select the most promising directions for new ambitious experiments.
- In making a plan, P5 has arrived at a budget split for *new investments* of about 60% toward the ILC and 40% toward the new projects in dark matter, dark energy, and neutrinos through 2012.
- The projects recommended for a construction start in dark matter, dark energy, and neutrino science should complete construction by approximately the end of 2012. This will allow maximum flexibility for decisions on future investments to be made toward the beginning of the next decade in the light of new science results, progress in new technologies, better definition of interagency contributions and plans, and progress on the ILC.
- Recommendations for construction starts on the longer-term elements of the

particle physics roadmap should be made around the end of this decade by a new P5 panel after thorough review of new physics results from the LHC and other experiments. A final decision regarding possible upgrade construction for the LHC, which will likely be a high priority, should also be made at that time.

- Among a range of funding options for the future provided to us, we have made our recommendations within a conservative funding plan. Significant additional discovery physics, more rapid progress on exciting projects in dark energy, as well as more rapid progress on ILC R&D would be possible with additional resources.

P5 has tried to follow the guidelines enumerated above in making a FY08 plan. The plan includes running of the Tevatron and the Fermilab neutrino program as presently foreseen. The groups at the Tevatron are to be congratulated on their recent discovery and precision measurement of B_S-meson mixing and the impressive first results from the NuMI-MINOS [Neutrinos at the Main Injector–Main Injector Oscillation Search] program. P5 looks forward to continued excellent physics from these programs. It also recommends strongly that FY08 see continued improvement in support for the University Program, as foreseen in the FY07 budget presently under consideration.

Within the roadmap, P5 recommends that the B-factory running continue in FY08, allowing completion of the BaBar physics data collection at close to 1000 fb^{-1} of integrated luminosity. The accelerator is running very well at present. The integrated luminosity through the summer of 2006 is expected to be about 415 fb^{-1}. The combined run in FY07 and FY08 would more than double that amount, allowing more incisive tests of the Standard Model and the search for new physics in channels where results are presently interesting but not definitive.

P5 also makes the following recommendations for other elements of the FY08 program, which are aimed at implementation of the EPP2010 vision for the field.

1. A program at the energy-frontier through physics at the LHC and vigorous R&D for an ILC should be strongly supported. FY08 will likely be the first year of significant data collection at the LHC, and the U.S. participants should be supported to vigorously engage in this first physics. International coordination of the ILC R&D should be encouraged to maximize progress toward the realization of this accelerator.
2. Construction should be started on three smaller projects that have significant potential for important physics:
    - The Dark Energy Survey, which combines measurements on baryon oscillations, cluster surveys, supernovae studies, and weak lensing to significantly improve the understanding of dark energy.
    - The next phase of the Cryogenic Dark Matter Search experiment, using a 25-kg detector deep underground to significantly extend the sensitivity for direct detection of dark matter.
    - The Daya Bay reactor experiment, contingent on satisfactory review of the costs, the construction plan, the technique, and the ability to control systematic errors to the required level. This experiment will significantly extend the reach for measuring the critical third mixing angle of the neutrino-mixing matrix, providing a factor-of-4 improvement in reach.
3. Construction should be started on the NOνA neutrino oscillation experiment using the NuMI beamline at Fermilab. This experiment is complementary to the other neutrino

experiments on a worldwide basis and represents the next step for the United States in a phased international program aimed at measuring the remaining parameters of the neutrino oscillation matrix, determining the mass ordering among the neutrino mass eigenstates, and finding out whether neutrinos violate the CP symmetry.

4. Numerous studies have identified a LST and a Dark Energy Space Mission as providing large steps forward in the study of dark energy and tests of general relativity, the picture of inflationary cosmology, and measurement of cosmic distributions of dark matter. The particle-physics community has been particularly active in developing candidates for each of these projects, which benefit from innovative work on detectors and data acquisition techniques developed in particle physics.

   Two projects, the Large Synoptic Survey Telescope (LSST) and the SuperNova Acceleration Probe (SNAP), are proposed as collaborative interagency projects. In the case of LSST, NSF has been the lead agency, with DOE providing substantial resources as the partner agency. In the case of SNAP, DOE has been the lead agency. P5 strongly re-affirms the compelling case for a Stage-IV dark-energy experiment (as described in the DETF report) and recommends that both LSST and SNAP be supported to bring these projects to the Preliminary Design Review stage in the case of NSF and LSST, CD2 for the DOE parts of LSST, and CD2 in the case of DOE and SNAP over the next 2 to 3 years (starting in FY07). Such support will allow sharpening of cost estimates, further interagency planning, further development of the collaborations, and continued work on the science potential, as discussed in the DETF report.

P5 places its highest priority on the new projects outlined above, which have been motivated by the EPP2010 vision. Should additional funds be available in FY08, the first priority would be to enable an even more ambitious start on ILC R&D.

Cahn said that there is some contrast between what some say about neutrinos and what is seen in EPP2010. There has also been a lot of discussion about how the power of the NOvA experiment depends on the intensity of the beam at Fermilab. He asked if P5 had discussed that topic at all. Seiden replied P5 is assuming the experiment is funded for $200 million and there is $35 million invested in the accelerator. The accelerator-upgrade projects seem quite sound, and P5 supports this strategy. It supports a U.S. investment of $200 million augmented by foreign investments. The present experiment has a chance to solve everything, including CP (when combined with T2K and others). P5 sees this as a phased plan. It is a field worth pursuing.

Wormser noted that there was no mention of NOvA foreign collaborations and also wondered about the future of flavor physics. Seiden replied that there are no foreign collaborators because no one knows if this program is going ahead, although five Italian universities have expressed interest. Mass hierarchy is the major target, so the experiment should be optimized for that. P5 supports B physics with its recommendation for BaBar. The physics case beyond BaBar and Belle needs to be evaluated. A decision on a super BaBar is three or more years away.

Cahn noted that the EPP2010 says one cannot do everything, but the P5 report seems to say that one can. He asked how all these programs fit in, especially the LST and JDEM. Seiden replied that other agencies and nations may contribute, but P5 is cautious about all of these programs. Some money will be recouped when BaBar and the Tevatron programs end. Cahn asked what the broader numbers are. Byon-Wagner replied that the numbers are in the DOE submission to Congress for the next 5 years.

Staffin asked if the Tevatron can be continued into FY10 if it finds interesting events and LHC is not running fully yet. Seiden answered that P5 postulated that it would run through FY09. Some other things may be delayed (e.g., NOvA). Shochet suggested that P5 would look at that again next year when it considers FY09 running of the Tevatron.

Bagger noted that P5 has a 10-year spreadsheet and asked what it has for JDEM. Seiden said that there is not enough money for it with an aggressive ILC start. Bagger asked how NOvA fits in. Seiden replied that it makes T2K a more powerful experiment. It is not a duplicative experiment. Ozaki asked what he meant by a 60-40 split in new investments. Seiden answered, the integral over 5 years. There are many elements of a diverse program than P5 feels are important.

Molzon asked how much discretion there is in funding small initiatives. Seiden answered that P5 has not tried to decide which smaller initiatives should be done. That is not P5's mandate, although those initiatives are important.

Samios questioned whether it was not better to do R&D to get to the next step rather than funding experiments that will not give the ultimate answer sought. Seiden said that the United States does not have a neutrinoless double-beta decay program. It will take a long time. Doing experiments to push the techniques is valuable.

Matthews asked if P5 made a recommendation on g-2. Seiden replied, no.

Staffin asked if P5 was proposing NOvA and higher beam intensities if the ILC has a quick start. Seiden said that if Congress quickly approved the ILC, HEP would need more money for ILC R&D and would not be able to support NOvA. But that is not the case. ILC R&D will probably go on for the next 5 years. NOvA *still* makes sense even if it is not brought up to multimegawatts.

Ritz noted that the upgradability of NOvA was a selling point, but not *the* reason, for proceeding with NOvA.

Oddone stated that it is difficult to argue that NOvA will impact the ILC. It is a completely different scale. If there is not enough money to do the ILC R&D properly, the neutrino program will not be done. The ILC is huge compared to anything else. An abrupt shutdown of a project makes managing a succeeding program very difficult. It is better to keep the required workforce trained and available..

Staffin asked how P5 saw the dark-energy experiments ramping up. Seiden said that Dark Energy Survey (DES) will take 3 years to ramp up. SNAP is not anywhere near ready to go. There are a lot of details to work out. Staffin asked if he saw a credible launch date. Seiden replied, no. What the U.S. share will be and what agency will shoulder it will take years to resolve.

Cahn asked if P5's plan will still be viable if SNAP and JDEM proceed well. Seiden answered, yes. Dark energy and neutrinos are decoupled from the ILC. Dark energy and neutrinos should be evaluated separately.

Shochet asked for approval of the letter. There was one abstention, and there were 16 votes for approval.

Gene Beier was asked to provide a status report on the Neutrino Scientific Assessment Group (NuSAG), which was jointly formed by NSAC and HEPAP and is co-chaired by Gene Beier and Peter Meyers.

NuSAG initially had three charges, to assess neutrinoless double-beta decay, reactor-neutrino mixing, and accelerator-neutrino mixing. These topics were assessed, reports were prepared and delivered, and those reports were approved by HEPAP and NSAC. Then another charge was added: Assuming a megawatt-class proton accelerator as a neutrino source, what would be the scientific potential, associated detector options, optimal timeline, and other scientific considerations associated with the various possible accelerator-detector configurations, including those needed for a multiphase off-axis program and a very-long-baseline broad-band program.

This charge arose from the American Physical Society's study, *The Neutrino Matrix*, which recommended a comprehensive U.S. program to complete the understanding of neutrino mixing, to determine the character of the neutrino mass spectrum, and to search for CP violation among neutrinos with a reactor experiment, an accelerator experiment, and a proton driver in the megawatt class or above producing a neutrino superbeam paired with a very large detector capable of observing CP violation and measuring the neutrino mass-squared differences and mixing parameters with high precision.

The off-axis approach produces a narrow-band beam at a particular energy. It limits the higher energy neutrino flux and is the approach adopted by NOνA. The alternative wide-band-beam approach can access multiple oscillation nodes simultaneously, but that requires very long baselines.

At approximately the time the NuSAG charge was issued, a Workshop on Long Baseline Neutrino Experiments was initiated by Brookhaven and Fermilab with a charge similar to the NuSAG charge and preliminary and final reports due July 15, 2006, and "before October" 2006, respectively.

The charge was delivered to HEPAP and NSAC in March 2006. An informational meeting was held May 27-28 in Chicago. Input was received from concerned parties, and areas where more information is needed were discussed. Questions/suggestions were sent to the Workshop in June 2006. The next meeting will probably be held in September or October 2006 after the Workshop's report is received. A preliminary draft report will be presented to HEPAP and NSAC in December 2006, and a final version in February 2007.

One question facing neutrino physicists is the value of the third mixing angle in the neutrino sector, another is whether or not CP violation occurs in neutrino oscillations, and a third is the mass hierarchy of neutrinos. At least some of these questions can be addressed through oscillation experiments. In a vacuum, the oscillation probability is given by an expression with an atmospheric term, an expression of interference-CP effects, and a solar sector. In matter, an appropriate analytical expansion has also been derived. For the experiments under consideration, matter effects are at a maximum at $E_\nu \approx 6$ GeV, and the CP asymmetry goes as $A_{CP} \sim 1/E_\nu$.

The oscillation-measurement goals are to

- Determine the U_{e3} magnitude to see if $\sin \theta_{13}$ is large enough to see any interference effects,
- Measure δ to see if CP violation is present,
- Resolve the mass hierarchy and determine the sign of Δm^2_{13}, and
- Resolve the θ_{23} degeneracy to see if it is less than or greater than 45°.

The off-axis beam requires about a 1-MW source, NOνA plus a big second detector, a greenfield installation, a near-surface site, and modifications to the NuMI beam. A wide-band beam requires a similar source, a very big detector, an underground laboratory, and a new beam. The off-axis beam has the advantage of being an incremental upgrading of an existing beam and detector. The wide-band beam has the advantages of measuring the full energy spectrum, supporting a broad physics program, and employing existing detector technology. The off-axis beam faces the challenges of statistics and detector technology. The wide-band beam faces π^0 rejection at higher energies.

The proton source requires modifications to the FNAL accelerator complex. The estimated cost for a 700-kW beam is $9.9 million; for a 1-MW beam, it is $32 million. The different proposed facilities have different baselines; and at different distances, different oscillations are revealed.

For wide-band beams, all ranges of neutrino energies are seen. For off-axis beams, one can select the neutrino energies that one wants.

For the off-axis approach with the NuMI beam at 810 km, the charge-current event rate peaks at 14 mr off-axis. Different beams can be extracted for different detector sites.

Three detector options are available:

- Liquid scintillator (not presented to NuSAG; needs to be much larger than NOνA)
- Liquid argon (presented in the off-axis context; may be applicable for the wide-band-beam approach)
- Water Cherenkov (monolithic or modular; must be underground to limit cosmic rays; may require DUSEL; may or may not work at Soudan)

One gets a lot more information with the liquid argon. Researchers are trying to determine the time scale for liquid argon detector R&D to see if this is a viable option. There are three water Cherenkov detectors: The UNO [Underground Nucleon Decay and Neutrino Observatory] detector requires fewer phototubes. The modular water Cherenkov detector uses established technology, and one only needs to build what is needed. Ten 100-kiloton modules would be needed, each looking like a scaled-up Super-Kamiokande.

After NOνA, T2K-I, Daya Bay, and Double Chooz, the International Scoping Study is looking at superbeams, β beams, and a ν factory. Europe holds three options:

- CERN Superconducting Proton Linac to Frejus,
- β beam to Frejus, and
- Need a neutrino factory if $\sin^2 2\theta_{13} < 0.01$.

In Asia, one could increase the JPARC beam intensity, build the Hyper-Kamiokande, or split the Hyper-Kamiokande in two. These ideas generally use multi-megawatt beams that are beyond the immediate NuSAG study.

NuSAG has sent 16 “suggestions” to the Working Group and is currently awaiting the Working Group’s interim report. Those suggestions include

- Using consistent assumptions and methodologies;
- Giving enough detail so results can be understood with a hand calculator;
- Specifying a level of simulation in current sensitivity estimates;
- Comparing sensitivities to those of NOνA and T2K; and
- Many more suggestions, some specific to water Cherenkov or liquid argon technology.

Baltay asked what had happened with Double Chooz. Staffin replied that, based on NuSAG’s recommendations, OHEP had decided to focus on Daya Bay first and then look at the next set of recommendations. It is not opposed to participation but first wants to make sure that Daya Bay succeeds.

Dawson asked Beier how he saw this happening. He replied that one needs to see how to organize. This time scale does not marry us to any approach.

Nishihawa asked how the questionnaire was structured. Beier answered that, in the 16 questions sent out, some were generally asked, and some were specifically sent to selected respondents.

A break was declared at 3:47 p.m. The meeting was called back into session at 4:03 p.m., at which time **Guy Wormser** [representing Laboratoire de l'Accelérateur Linéaire (LAL), Orsay] was asked to review developments in the formulation of a European strategy on particle physics.

The European polity is complex. Europe has more than 40 countries, of which 20 are member states of CERN. The European Union has 25 states and soon may have 27. It is difficult to tell where the European boundary is in particle physics; does it include Russia, Israel, and/or Turkey? Of the two major laboratories (CERN and DESY), one is internationally driven, and the other one is nationally funded but is open to international participaton. Several more accelerator centers are nationally funded: Instituto Nazionale di Fisica Nucleare at Frascati, Paul Scherrer Institute (PSI), and Rutherford-Appleton Laboratory (RAL)/Daresbury. Astroparticle Physics European Coordination (APpEC; the club of funding agencies) was recently created.

The European Union uses a variety of tools and councils: a multiyear R&D framework plan; the European Strategy Forum on Research Infrastructures (ESFRI) board and roadmap; the European Research Council; new construction initiatives, for which a call was launched in early 2007 for 600 million euros (the condition is that the initiative has to be in the ESFRI roadmap); the official delegation of ESFRI to the CERN Council's strategy process for HEP-related matters; the important European Union contribution to the HEP European programs, especially in accelerator R&D [Co-ordinated Accelerator Research in Europe (CARE), European Design Study Towards a Global TeV Linear Collider (EUROTEV), European Free-Electron Laser Design Study (EUROFEL), European Detector R&D Toward the International Linear Collider (EUDET), European Isotope Separation On-Line (EURISOL), and European Research Programme for the Transmutation of High-Level Nuclear Waste in an Accelerator Driven System (EUROTRANS); ~100 million euros] and grids [DataGrid, Enabling Grids for E-science in Europe (EGEE), and EGEE-II; 82 million euros]; and a strong expectation for a large superconducting rf facility, to be based at CERN.

In regard to the CERN Council strategy process, all details can be found at: http://council-strategygroup.web.cern.ch/council-strategygroup/. It was started in September 2005 under the aegis of the CERN Convention. The process has three phases: community input (the Orsay symposium in January 2006); the strategy group workshop (Zeuthen meeting in May 2006); and CERN Council approval (Lisbon on July 14, 2006).

The CERN Convention calls for the organization and sponsoring of international co-operation in nuclear research, including cooperation outside the CERN laboratories. This cooperation may include (1) work in the field of theoretical nuclear physics; (2) the promotion of contacts and interchange between scientists, the dissemination of information, and the provision of advanced training for research workers; (3) collaboration with and advising other research institutions; and (4) work in the field of cosmic rays.

The actors in the strategy process include the CERN Council [government representatives of member states with one vote per country and observers (Turkey, Israel, Russia, EU, UNESCO, USA, Japan, and India)]. The strategy group consists of the chairs, the preparatory group, the directors of the eight large European laboratories, 20 persons designated by the 20 member states, the CERN Council delegate, and invited members.

The Orsay symposium was organized by LAL Orsay for January 30–February 1, 2006. There was very strong participation of the European community. Presentations and discussions were summarized in a briefing book, which was given to the Zeuthen workshop participants.

The Zeuthen workshop produced a two-page bulleted strategy document, to be submitted to the special session of CERN Council (Lisbon July 14) for unanimous approval. Therefore, this document had to be unanimously approved at Zeuthen. It also produced a 20-page document backing this strategy document. The workshop was organized around many working groups, and a document was indeed unanimously approved.

The Draft Strategy Document contains (1) statements on the European role and structure in HEP and the need for a European strategy; (2) eight scientific statements (not very different from those of EPP2010 and mentioning links with nuclear physics and theoretical physics); statements on how to pursue the strategy process, the global scale of collaboration, the relationship with the EU, and the relationship with nonmember states; and (3) comments on three complementary issues: outreach, technology transfer to other scientific fields, and industry.

One key aspect of the European strategy process is the ability of the CERN Council to work in a dual manner: taking care of the CERN laboratory in Geneva and taking care of the European strategy as a whole. These are the same people, but they are acting in different functions. In the second function, they will approve strategic decisions that imply an increased CERN budget; in the first function, they will then vote on a budget increase. The first example of such a process is coming very soon: the draft strategy document stresses the need for extra R&D effort. Another question is, who will represent Europe in a global-collaboration mode? The first example of global activity is European representation in FALC. Another question is whether the CERN Council will manage the European contribution (financial and/or technical) to a global machine channeled through the CERN organization.

The scientific community is undergoing a difficult transition from a world of competition to a world of global cooperation. It is very important to realize that, today, some feelings of mistrust exist on both sides. In Europe there is a widespread feeling (also shared by some in the United States) that the overall scenario of United States participation in the LHC is a very good deal for United States. This does not necessarily imply that past agreements must be renegotiated, but it does raise questions about how compelling a U.S. ILC bid can really be and how to guarantee that the next large machine in Europe will be more globally financed than the LHC has been. In the United States, people are wondering why anyone is accusing the United States, which is respecting signed agreements. There are also questions about whether Europe can make decisions through the CERN Council requiring unanimous approval, especially with the large weight of the small countries. People are also wondering if the decoupling between the two roles of the CERN Council is strong enough to let Europe invest in something large that is not in Geneva. Furthermore, with the United States opening widely its various councils to European participation, why is not Europe doing the same?

These issues were (rather intensely) discussed at Zeuthen. A personal view is to use the LHC upgrade program as a short-term, moderate-cost first example of a future global machine. The LHC upgrade is key part of both European and U.S. strategies. Treating the LHC upgrade in a global mode allows one to exercise the model at a moderate cost scale (200 to 400 million Swiss francs); will soothe the LHC U.S. contribution "problem" while preserving International Committee of Future Accelerators (ICFA) rules for free usage of beams; and will encourage Europe to invest massively in a nonEuropean machine, not having to wait 20 years to see if reciprocity can be reached. This approach is rather in line with the new FALC role.

Many European countries (including some of the small countries) want to participate in the ILC. This participation can only be achieved through CERN. From about 2012 onward, *if* the CERN budget were kept at the present level, about 300 million Swiss francs per year would become available for new projects and could be split in three parts:

- LHC upgrades,
- Extensive R&D for the next large-scale accelerator in Europe, and
- Centralized participation in a worldwide machine not based in Europe.

European participation in the worldwide machine would be supplemented by the direct participation of European countries on a voluntary basis. For example, assuming the European centralized participation is 100 million Swiss francs per year for 10 years and this is matched by a European voluntary participation, Europe could contribute 2000 million Swiss francs to a non-European-based ILC.

The European strategy process is nearing completion. Given Europe's political complexity, this is a real achievement that should not be underestimated. From now on, it is very important to distinguish between the CERN Council with its new dual role and the CERN laboratory. The United States and European scientific priorities are quite well aligned, which presents a historic opportunity to strengthen international collaboration. Some level of mistrust exists today. Using the LHC upgrade program to dissipate that distrust and to accelerate the transition towards the new way of global collaboration needed for the ILC may be worth exploring.

Shochet asked how successful the group will be in acting for the benefit of Europe as a whole and for CERN specifically. Wormser replied that the CERN Council has representatives from across Europe. Shochet noted that, in ILC R&D, it is difficult to say, "This will be done in Europe, and this in the United States." He asked whether, if Orsay comes up with money, it will do what it wants. Wormser answered, yes! Acting cooperatively will take a lot of coordination. The CERN members will try to act on a reasonable basis.

Bortoletti raised two issues: (1) Wormser had mentioned the U.S. LHC contribution as a possible forgivable agreement and asked him to comment further on that. (2) The timing for different groups may be different. Wormser said that, in answer to the second issue, a group's concerns will be driven by science; today there is no sense of urgency. In answer to the first, participation will be tied to financial participation.

Bagger noted that EPP2010 calls for international cooperation and asked what Europe is looking at. Wormser replied that Europe is forging an interface with the outside world.

Oddone called attention to the fact that there is another player: Asia. Wormser said that he had used the example of the United States because the audience would understand it. Collaboration between Europe and Asia has been small but is growing. There is a sense in Europe that the United States has an HEP strategy but that Japan does not.

Staffin asked if some of the mistrust over the United States' underparticipation in LHC is directed to CERN's former management. Wormser replied, no; it is a sense of the United States' using more. Europeans need to get over this feeling. One must look at the 30% U.S. participation in the Compact Muon Spectrometer (CMS) and 50% participation in BaBar. The United States is the most important investor in HEP machines.

Dragt asked if there is a sense that the United States has been reliable in the past. Wormser answered, yes. The United States has multiyear planning, although there have been problems [like the Superconducting Super Collider (SSC)].

Ferbel asked if there were a recognition in Europe that the United States and Europe do accounting differently. Wormser responded, yes. Contingencies and manpower issues were discussed at Zeuthen. That difference is expected and must be reckoned with.

Jonathan Bagger was asked to give a summary of the LHC Theory Initiative.

The LHC is coming fast, and important theoretical work must be done before the LHC experiments can start producing physics. Specifically, accurate theoretical predictions are necessary for the LHC experiments to realize their full potential. It is crucial to understand both signals and backgrounds. Time is short.

The LHC theory initiative proposes a prestigious fellowship program for postdocs, students, and (perhaps) junior faculty that is modeled on the very successful Hubble Fellowship Program in astronomy and on the SSC Fellowship Program in particle physics. The aim is to create a vibrant, networked community of theorists working on physics directly relevant to the LHC.

The LHC Theory Initiative is a new way of approaching theory. Theorists often work on their own. This initiative calls for a virtual institute that uses collaborative tools to focus effort on LHC theory. It would establish a network of working groups to identify, prioritize, and solve the most pressing problems for LHC theory; and it would forge a fellowship program to build an LHC theory community.

It is run by a steering committee, which has held three town meetings. An agreement was reached to put together an NSF proposal. The proposal has intellectual merit in that it would provide calculational tools and theoretical results necessary for LHC physicists to compute higher-order quantum chromodynamics (QCD) and electroweak corrections in the Standard Model (SM), supersymmetric theories, and other beyond-the-SM models and to develop robust and well-tested Monte Carlo tools to confront data with various theoretical models. The broader impact would be to create the nucleus of a vital U.S. LHC theory community and facilitate the development in the United States of a world-class community in collider theory.

The highest-priority SM calculations would be improving parton distribution functions, including next-to-next-to-leading-order effects, with reduced uncertainties; improving calculations of QCD processes, such as multijet production, that will be used as calibration tools for the detectors; and calculating more-precise and -reliable background processes that are relevant for the Higgs search. A white paper on the website reviews this topic in detail.

The highest-priority signal calculations would be implementing new physics scenarios in Monte Carlo event generators; investigating how the different models can be distinguished in LHC experiments; and finding ways to determine the basic properties of new particles, such as couplings, spins, or electric charges.

The FY06 proposal was declined, but strong support was expressed for the concept at about $900,000 per year. A FY07 proposal is being prepared with encouragement from the community and from NSF. The initial steps are under way under the aegis of an organizing network and working groups. Input is still being sought. In regard to the inclusion of junior faculty, the question is how best to ensure a strong and effective linkage to ATLAS [A Toroidal LHC ApparatuS] and CMS.

Bortoletto said that this is an exciting initiative and asked if he could comment on the reduction of theory funding in favor of a new Office of High Energy Density Science. Bagger responded that why the money was taken out of the HEP theory budget was beyond him. Shochet stated that that situation is unsustainable and will be fixed.

The meeting was adjourned for the day at 5:15 p.m.

Friday, July 7, 2006
Morning Session

Chairman Shochet called the meeting to order at 8:30 a.m. and asked **Barry Barish** to begin the discussion of the ILC by presenting an update of the global design effort (GDE), which has existed for 11 months. The mission statement for the GDE includes the design concept, performance assessment, reliable costing, siting analysis, detector scope and concepts, and coordinated R&D efforts.

The GDE had 49 members at the beginning; it now has 64 distributed among the Americas, Europe, and Asia. Everything has been joint from the beginning. It is run by part-time personnel. The central operations budget is <1% of the R&D funding. ($377,000 per year, provided by DOE/HEP).

The GDE's job is to design a linear collider with an rf design. A big problem is creating enough luminosity in a linear machine (a restriction imposed by synchrotron radiation). The product of frequency and number of bunches is much smaller for linear machines, but it allows a small spot size. The game is to make a low-emittance machine and to squeeze the beam as small as possible (5 nm). Some of the technology has been demonstrated already at SLAC and in Japan.

A parametric approach will be taken (rather than a point design). No machine runs in the parameters it was designed for. The key decisions were probed at the Snowmass meeting, and 50 were identified. These were refined to the 10 most important. The luminosity parameter and rf gradient are the two crucial choices that have to be made. The other eight are interrelated, complicating the decision process. (Many design groups have to vet each decision.)

It took about a month to document the Snowmass meeting, resulting in a baseline. A straw-man solution for all the questions was put forward and discussed internationally, producing a conceptual design for a baseline machine, which was documented. Then the same thing was done for the alternative choices to put bounds on the costs. Many of these alternative choices are not mature enough to employ now but may be usable and adoptable in the future.

The Baseline Configuration Document (BCD) is now being used as the basis for the reference design and cost effort. It is being evolved through a formalized change-control process. A change-control board (CCB) has been established and serves as a proactive instigator of changes in the baseline design. Most decisions to date have been driven by performance; cost will become a driver as designs and costs become more available.

New members of the GDE were added with skills in design, engineering, costing, etc. A reference design board (RDB) and cost board has been established to internationalize the program management. An R&D board has also been established.

The GDE directorate meets weekly. The effort is now focused on developing the reference design. The configuration was frozen in January. The design/cost method was then reviewed. The initial design and cost will be set in July. The draft reference design report (RDR) will be reviewed in December. That review will take 6 months; then the technical design process can start. The machine will

- Be deep underground (100 m or more)
- Have a central campus where the beams come together
- Have two detectors and two interaction areas
- Have twin tunnels (accelerator and servicing)
- Take into consideration regional differences in safety etc.
- Employ a DC electron source with a 2-ns pulse
- Include a target for positron production
- Have a 6-km damping ring for the electron beam (needs a fast kicker because bunches are so close together)
- Use twin damping rings for positrons, alternating between them to avoid electron clouds
- Include a qualified superconducting rf cavity gradient of 35 MV/m.
- Employ electropolishing of cavity surfaces
- Use cryomodules similar to those used in X-ray free-electron laser (X-FEL) at DESY
- Have 10-MW multibeam klystron (MBK), 1.5-ms-pulse, 65% efficiency
- Include a complicated and long beam-delivery system with quite severe requirements on focusing, collecting and dumping, and collimating
- Employ large-scale, 4π detectors

The 500-GeV BCD machine and essentials for 1 TeV are being costed. The costing scheme is a cross between the International Thermonuclear Experimental Reactor (ITER) “value” and CERN “core” models. The RDR will provide information for translation into any country’s cost-estimating metric. The cost estimation assumes a 7-year construction phase.

Costing is based on a world-wide tender. Costs are in three classes: site-specific, conventional, and high-tech. Cost engineers will prepare the estimates.

A work breakdown structure (WBS) system and dictionary have been developed. These allow the rollup of costs by area systems.

The R&D program (1) supports the baseline; (2) supports baseline alternatives; and (3) is aimed at the combined requirements of the accelerator and the detectors. The goal is clear; the details are less clear.

The first thing GDE did was to prioritize the tasks. CERN has provided project-management tools that have been used to produce an ideal ILC R&D program. The GDE is working on the high-priority items first, is initiating two superconducting rf task forces, and is coordinating R&D on alternatives to the baseline through the CCB and RDB.

In summary, the baseline was determined, a reference design is being developed, and the technical design should be finished by the end of 2009. The R&D program is supporting the baseline and developing improvements to the baseline. Generally, the overall strategy is to be ready with a final design and plan in order to move forward when an informed decision is made, based on siting, international management structure, LHC results, etc.

Shochet asked how they were dealing with the cavity issue. Barish said that the process needs to be understood. The GDE is confident that it can be done but needs a demonstration program. In terms of costing, the GDE does a risk analysis.

Carithers asked how the process can be proposal driven and baseline driven at the same time. Barish responded that the GDE can identify problems but has no authority to assign anyone; someone has to come back with a proposal.

Hewett asked what the baseline design includes for the 1 TeV upgrade. Barish answered that the large-cost items like the tunnel have been left out. Hewett asked if a 200-GeV machine could be adapted easily and quickly. Barish replied, yes.

Bagger asked if any cost unknowns could delay the design and costing. Barish said that nothing merits waiting for.

Samios suggested that an option for dealing with the “two-crossing” cost issue would be to just design a tunnel with nothing in it for now. Barish replied that the GDE will look at various options, such as two tunnels, one interaction region, a push-pull system for detectors, and two independent detectors. It has to consider the costs and trade-offs of these alternatives.

Gerald Dugan was asked to present an update on the ILC-America Regional Team activities. The GDE leadership is evenly divided among the three regions (Europe, Asia, and the Americas), with each committee or board having three or four Americans and each area or technical system having at least one American participant. Canadian and Latin American participation needs to be recruited.

The program is organized with memoranda of understanding (MOUs) between the program and each laboratory. The work is broken down into a series of technically based work packages, documented in MOUs detailing the cooperative arrangement for the execution of work packages at each laboratory. Quarterly, laboratories report about financial status at the work-package level, and technical status semiannually. About 100 work packages for FY06 are organized into a WBS. The laboratory work packages, associated resources, and MOUs are posted on the ILC-Americas website wiki.lepp.cornell.edu/ilc/bin/view/Public/Americas/WebHome.

About half of the FY06 budget is taken up by the R&D on the cavities and cryomodules (25%), rf systems (15%), and beam-delivery system (9%). The other half is distributed among the other machine areas. Most resources go to SLAC and Fermilab. The electron and positron sources include photocathodes, laser, gun, and overall design and are being done at SLAC. For the damping rings, a refined baseline lattice has been produced, ELEGANT has been parallelized and multi-objective evolutionary optimization codes have been developed at Argonne National Laboratory (ANL). Injector/extractor line design and characterization, beam dynamics, wiggler vacuum system, quadrupole and sextupole design/costing, Accelerator Test Facility (ATF) injector/extractor kicker design and beam dynamics, and fast ion instability tests at the Advanced Light Source (ALS) have been conducted by Lawrence Berkeley National

Laboratory (LBNL). Wigglers, electromagnetic design, impact on ring dynamic aperture, and the use of the Cornell Electron Storage Ring (CESR) as an ILC positron-damping-ring test facility have been studied at Cornell. Studies of fast kickers have been carried out at SLAC, Cornell, Lawrence Livermore National Laboratory (LLNL), and FNAL. Electron-cloud measurement and mitigation have been studied at SLAC with secondary-electron yield (SEY) measurements, sample test chamber in the upgraded Positron Electron Project (PEP-II), grooved chamber tests, and clearing-electrode tests.

In terms of the main linac and siting, DOE has expressed interest in hosting the ILC at a site near FNAL. Fermilab has focused its R&D efforts on the ILC main linacs. The main thrust of the Fermilab ILC accelerator R&D is to establish U.S. technical capabilities in superconducting rf cavity and cryomodule technology. Fermilab is developing extensive infrastructure (1) to determine cavity processing parameters for a reproducible cavity gradient of 35 MV/m; (2) to design, produce, and test an ILC-specific cryomodule; and (3) to test one ILC rf unit at ILC-beam parameters, high gradient, and full pulse-repetition rate. FNAL is collaborating with DESY, Instituto Nazionale di Fisica Nucleare (INFN), KEK, CERN, Jefferson Laboratory, SLAC, and U.S. industry on the design of the next-generation ILC cryomodule (Type IV). Civil engineering of machine enclosures, study of U.S. sites on or near the Fermilab site, and cost estimates for conventional facilities are progressing. Fermilab has extensive infrastructure for testing, tuning, and measuring.

Cavity R&D is an FY06 focus. To speed progress on the ILC cavities gradient goal, a multilab collaboration has been formed that maximizes the utilization of existing U.S. superconducting rf infrastructure and develops Fermilab expertise and infrastructure in parallel. The plan is to procure up to 60 cavities by FY07 and to provide them to Cornell. Electropolishing will be done at ANL. Cavity testing is being done at Cornell, looking at different shapes for high gradients. Jefferson Laboratory focuses on electropolishing, is developing test cavities, and is investigating alternative technologies and superconducting joints.

At SLAC and LLNL, work is focused on power sources. A 5-MW L-band test stand is running in End Station B (ESB) with a Spallation Neutron Source (SNS) spare modulator. SLAC is doing the major work in beam delivery (diagnostics, collimating, focusing, etc.) and building hardware for the HTF2 [high transverse fields] at KEK. Brookhaven National Laboratory (BNL) is building very compact superconducting magnets (quadrupoles, octopoles, and sextopoles). SLAC has unique experience with operating the first linear collider, the Stanford Linear Collider (SLC). That experience includes availability simulation and analysis, commissioning strategy, machine protection systems, and machine tuning simulations. The emphasis is on high-availability, reasonable-cost components.

In FY07, we would like to conduct design and engineering efforts in support of the GDE Technical Design Report (TDR); cavity and cryomodule work; rf-system development; research on sources, damping rings, and beam delivery; investigation of global systems; and technical R&D in support of the U.S. regional interest. The laboratory requests have increased from $30 million in FY06 to $105 million in FY07 for ILC R&D. That request would fund

- Fabricating (in industry) 24 and processing (at national laboratories) 12 more ILC high-gradient cavities;

- Continuing R&D on large-grain and high-gradient cavities;
- Continuing R&D on electropolishing processing, field emission/dark current issues, and thin-film systems;
- Developing an electropolishing facility at ANL;
- Horizontally testing 10 cavities at Fermilab;
- Building the first U.S.-built cryomodule and receiving parts for a second cryomodule to be built in FY08;
- Completing the design of the Type IV (ILC-style) cryomodule;
- Completing the vertical test facility and a second horizontal test facility at Fermilab;
- Installing cryogenic-systems support for cryomodule tests at Fermilab;
- Upgrading and moving the Fermilab photoinjector to the ILC Test Area's NML enclosure at Fermilab; and
- Purchasing a 10-MW klystron and another bouncer modulator for the ILC Test Area's NML enclosure.

In the main linac rf systems, the FY07 funding will continue development of the Marx modulator and evaluation of the Diversified Technologies and SNS modulators (the modulator choice will be downselected by the end of FY07); purchase two 10-MW klystrons from Communications & Power Industries (CPI) and Toshiba; contract with CPI to develop a high-efficiency 5-MW klystron; investigate cost-reduction options for the rf distribution system and couplers; and continue development of the low-level rf systems.

All of these activities cannot be afforded, so the tasks will have to be prioritized. This process started with a Regional Team meeting to discuss FY07 requests and future plans (May 3-4 at SLAC). The FY07 requests have been reviewed by the GDE R&D Board and evaluated in terms of relevance, degree of duplication, and urgency for the global ILC R&D program. The GDE is also developing globally coordinated plans for cavity R&D needed to validate the project gradient goals; for demonstration of the operational gradient in a cryomodule; and for testing strings of cryomodules. Plans for the TDR effort will also be developed. The FY07 ILC program in the United States will be aligned with the GDE priorities and structured to conform to the GDE R&D plans to the extent possible on the required time scale. For technical R&D proposed in support of the U.S. regional interest, input will also come from the Ozaki panel. Recommendations to the DOE on the FY07 program can be provided by the end of July. For FY08 and beyond, help is needed to develop a multiyear plan. We need to eliminate duplication. Regional R&D requirements will be assessed by the Ozaki panel and also incorporated into the program.

The Americas Regional Team is playing a major role in the development of the ILC RDR and cost estimate. A vigorous R&D program in support of the GDE goals is under way in FY06 at national laboratories and universities throughout the region. Next year, as the project enters the TDR phase, a significant increase in resources will allow development of the TDR, expansion of the R&D program, and the start of technical R&D in support of the U.S. regional interest. The requested resources for an FY07 technically limited program exceed those expected to be available. A process of prioritization, in close coordination with the GDE, and including input about regional interests, is being carried out. This process will serve as a model for developing a multiyear R&D plan.

Wormser asked what the ratio was between the baseline and alternatives. Dugan said that alternatives are 10% or less than baseline efforts. Wormser asked what the American effort included. Dugan replied, site-specific design, R&D being done at U.S. laboratories, and some gray areas.

Cahn queried why, out of $100 million, $40 million went to regional interests. Dugan replied that the regional interest was broadly interpreted. Cahn asked what fraction would be regional in the future. Dugan answered that "regional" is a murky term; 40 to 50% is probably a good estimate. Cahn asked if other regions are also spending money in these categories. Dugan said that all of the infrastructure would be applicable to any site. Only the civil design is definitely regional.

Baltay asked what other countries are spending. Ozaki replied that Japan and other countries are supporting ILC R&D; they are the countries that have applicable technologies that are being developed for other purposes.

Paul Grannis was asked to review activities in the U.S. interest and activities related to ILC detector R&D.

A self-starting group, the FALC provides a small common fund to the GDE, which was chartered by the international linear collider steering committee, to which the Linear Collider Steering Groups for the Americas (LCSGA), Europe, and Asia also report.

The term "activities" refers to activities within the United States that are believed to be needed to enable a credible bid to bring the ILC to the United States. At present, these activities include developing a U.S. industry capability for key technologies (particularly fabricating superconducting rf cavities); setting up test facilities in national laboratories to advance the understanding of superconducting rf; testing prototypes and ultimately producing elements; guiding industrial development; evaluating potential U.S. sites for geology, ESH, environment, infrastructure, and machine-dependent design in advance of a real bid to host; and developing other facilities thought lacking in the global plan.

In the past several months, the LCSGA, in consultation with the Americas Regional Team (ART), DOE, and NSF, established a panel with S. Ozaki as chair to evaluate the activities in the U.S. interest, particularly in reference to budget priorities for FY07. The DOE and NSF offered comments to the LCSGA, outlining the definition of the topics to be considered, the desire to optimize existing infrastructure, the relationship with activities worldwide, and the need for understanding the priority relative to R&D and design activities being carried out by the GDE. The priority of detector R&D was explicitly excepted from the panel's purview. The ART plan is to fold the Ozaki panel recommendations into the larger matrix of inputs in making the FY07 budget requests to DOE.

As set up, the GDE (and thus ART) is not well structured to address specific regional needs and priorities, so these activities fall somewhat outside its jurisdiction. However, developing key technologies and assuring worldwide capability to produce components in sufficient quantity and quality to meet ILC schedules is of key importance to the GDE. It is presently assumed that significant U.S. production of superconducting rf cavities will be required, regardless of the ILC site.

Accelerators developed initially for HEP have expanded into many other sciences, and now find pervasive use across the DOE-SC programs. Future superconducting-rf energy-recovery linacs (ERLs) will likely expand these opportunities. Of the roughly 15,000 accelerators in use worldwide today, all but 100 to 200 are used for medical diagnostics and treatment, radioisotope production, electronics, food processing, etc.

It is reasonable to assume that future applications in all these fields will be transformed by new high-gradient superconducting rf acceleration technology. Although the ILC is driving much of this development, the impact will be felt across all DOE SC programs. It makes sense to envision an initiative within DOE/OHEP for the development of SC rf technology, with U.S. industrial capability and laboratory test facilities that serve the broad mission of DOE SC. Probably the most important ILC impact on broader science and technology will be from this superconducting rf technology R&D.

A global comparison was made of regional spending on ILC R&D (including superconducting rf infrastructure). It was found that the three regions are spending about the same amounts (within a few percent), assuming that the President's request for FY07 is $60 million, which aligns very closely with the expenditures in Europe and Asia for their fiscal years starting spring 2006.

A plan is needed to go forward. ILC R&D has grown to significant levels in the HEP budget (8% in the President's FY07 budget). A program this big needs a clear plan for the R&D phase, outlining the goals, what should be done when (milestones and deliverables), and resources (people, funds, and infrastructure). The April DOE/NSF review of ART recommended: "The committee calls for the development of an integrated multiyear R&D plan in the US showing resource needs and milestones, using significant input from the GDE." This plan is needed some time this year to defend the projected funding trajectory and to do effective oversight.

In regard to generic vs. ILC-specific R&D, the total laboratory and detector community requests for FY07 funding exceed the President's request by roughly a factor of 2, so programmatic decisions are needed. Some of the requests are for infrastructure or generic needs that serve broader purposes than ILC. At the President's budget level, all ILC-specific R&D will be charged to the ILC budget and reporting code. After validation that some expenditures are truly generic or have broader infrastructure, such expenditures could be placed on core-research budgets, to the extent possible.

The university ILC accelerator grants have been operating for the past several years. DOE and NSF instituted university grants for ILC-related accelerator R&D in 2002. This program was intended to stimulate interest in ILC, prior to GDE organization. In FY06, about $700,000 (DOE) and $200,000 (NSF) were allocated. The President's FY07 DOE budget has +$5 million (+18%) for general accelerator research. NSF is planning the APPI, funded at $2.8 million in FY06 with hopes to grow it in future. In fact, past proposals have spanned the continuum between generic and ILC-specific. ART is now in place to advise on overall ILC R&D priorities. It seems useful to transfer these ILC university program grants after FY07 to AARD or APPI programs or to ILC funds. The ILC Small Business Innovative Research (SBIR) program will continue.

Since 2002, DOE and NSF have conducted a program for university-based ILC detector R&D. For FY05 to FY07, these funds are distributed through subcontracts from an umbrella grant to the University of Oregon. FY05 grants totaled $700.000 (DOE) and $117,000 (NSF). The FY06 funding is $1,048,000 (DOE) and $300,000 (NSF), supporting 34 projects at 27 universities and 2 national laboratories. The funding distribution is very broad, mostly in tracking and calorimetry. This program is to continue in future years until there is an integrated ILC-detector program. As with accelerator

R&D, the detector program has some elements that are generically applicable.

Is it possible for people at universities to get into the ILC effort? Both DOE and NSF recognize the high priority placed by HEPAP and the recent NRC EPP2010 report on conducting a vigorous R&D program that could lead to the ILC project. Both agencies currently fund university grants for both detector and accelerator research with applicability to the ILC. These programs have been modest but have grown over the past several years. Both agencies respond to grants through the peer review process. They welcome proposals for which ILC detector or accelerator R&D is the whole or a component of the effort, as well as for generic research that may have some bearing on ILC issues. In addition, there is often some latitude within existing grant funds to consider new directions. The use of existing grant funds for ILC-related research depends upon the details of each proposal.

A 2005 World Wide Study panel chaired by C. Lois Damerell compared currently funded and self-estimated needs and money and people for detector R&D in the three regions over about 4 years. The United States and Japan lag behind Europe significantly. The U.S. effort was about 4 times less than Europe's, and was funded at about 35% of the estimated need. The United States is behind in detector R&D. About 80% of the $5.6 million in expenditures are SWF, come out of laboratory core research, and are spent at SLAC and Fermilab.

An informal and preliminary request by the American Linear Collider Physics Group (ALCPG) for the scope of the ILC detector university program in FY07 is about $3 million, with $1 million coming as a supplement early in the year. DOE and NSF have asked the ALCPG for a multiyear resource-loaded schedule that includes the prioritized goals of the R&D in the United States in the world context. The first draft is expected this summer, prior to any actions on supplemental requests. Any substantial increase in detector R&D funding will require this plan.

At the moment, there is no constituted body that is ideally suited to advise on the relative priority between machine-related R&D and detector R&D, although LCSGA could provide some useful perspective. Informal coordination of detector R&D at universities and labs is reasonably good, but a more tightly integrated approach by ALCPG is needed.

In conclusion, in the one year since the formation of the GDE,

- A good President's FY07 budget request has been received;
- Progress has been made in defining, designing, and costing the ILC;
- A GDE common fund has been initiated by FALC; and
- The coordination of the global effort on accelerator and detectors is better understood.

The remaining issues are (1) developing a plan and priorities for the ILC R&D phase, both accelerator and detector and (2) better coordination of accelerator, detector, and regional-interest efforts in the United States and worldwide.

Bortoletto noted that the U.S. R&D is closely tied to the GDE and asked if that were true in other regions. Barish responded that the program in Asia is expanding and is tightly coupled to the GDE. In Europe, there are many initiatives. The proposal process is evolving and getting more closely tied to the GDE.

Meyer asked about the time scale for site-specific and industrial efforts. Grannis replied that many processes have to be coordinated in time. The organizational evolution is key and difficult. The site-selection process must be defined. The goal is to have all these converge in 2010.

Carithers asked if the United States might be asking foreign partners to fund U.S. industry for technology if we produce the cavities. Grannis answered that Japan is spending significant funds at their sites in parallel to the spending in the United States.

Ritz asked what the plan was for reviewing program execution. Grannis replied that a program review was started this year under the NSF. Lehman-like reviews may start next year. FALC is discussing how to do that. Program reviews of detector designs will also be conducted.

Wormser asked about the time scale for detector development. Grannis said that the GDE is focused on 2010 with a construction-program start expected in 2012. Planning is being developed to conform to those deadlines.

Cahn asked if there would be discussion of the Senate bill. Shochet said that that topic would be covered in the discussion section of this meeting.

A break was declared at 11:01 a.m. The meeting was reconvened at 11:15 a.m. Staffin pointed out that there was a need for new personnel in the HEP office. Two division-leader positions were advertised, but no candidates were found; more vigorous recruitment is needed. HEPAP was asked to look for well-suited candidates. Ritz suggested that a letter from the HEPAP Chairman to the website of the Division of Particles and Fields (DPF) of the American Physical Society (APS) might elicit a good response.

Jonathan Bagger was asked to review ILC communications. The ILC will require a sophisticated communication strategy. For the global project, the GDE has created a professional communications team with representatives from each region. For North America, an independent Communications Committee has been set up under the auspices of the LCSGA. In each case, the goal is to develop a crisp, clear, and compelling message. The American group seeks to deliver that message to various audiences, such as the ILC physics community, the global HEP community, other physicists, other scientists, industrial partners, the press, the public, educators, and government officials. A committee has been set up to initiate action in this direction.

So far this year, basic communication tools have been developed (interactions with the community, an ILC website, the *ILC Newsline*, and an ILC brochure). In a huge step, *Discovering the Quantum Universe* was rolled out. It now has a companion website. It was hand-delivered to all congressmen by the R&D caucus.

The ILC Communications Committee was also active in the EPP2010 report rollout with accompanying press coverage, Washington briefings, and opinion pieces. An ILC speakers bureau needs to be populated. A workshop will be held at the GDE Vancouver meeting to discuss presenting the ILC to various audiences. There will be ILC talks at the APS and AAAS [American Association for the Advancement of Science] meetings.

The Committee is in the process of establishing a particle physics Envoy Program the goal of which is to create trained envoys who understand and represent the entire field and who will develop long-term relationships with policymakers in Washington. A steering committee has been set up, and activities are expected to begin in early autumn.

Ruchti noted that a 23-year-old researcher in 2020 is 9 years old now. Bagger stated that the Committee is reaching out to educators.

Perlmutter suggested including cosmology and other areas. Bagger answered, absolutely.

Shochet announced that HEPAP had received three new charges for

1. A Dark Matter Science Advisory Group to look at direct detection of dark matter and to report to HEPAP. Ritz asked whether that had been discussed before. Shochet replied, yes; this panel has been set up and has met.
2. A university subpanel to look at the grant programs run by NSF and DOE. Homer Neal has agreed to chair the panel. The panel has been populated, and they are scheduling their first meeting. This charge is fairly broad. Cahn noted that this is an opportunity to look at ways for the programs to learn from each other, especially in program management. Shochet responded, that is what is hoped for.
3. A midcourse review of HEP long-term goals. Three HEPAP members have been asked to conduct this assessment by December 31, 2006. Bill Molzon will chair this subpanel.

Montgomery commented that, when these long-term goals were set up, it was stressed that they were independent of the specific physics. Shochet stated that the Subpanel may consider modifying the performance measures.

A break for lunch was declared at 11:47 a.m. The meeting was called back into session at 1:33 p.m. Shochet listed the topics that he would cover in the summary letter to OHEP and NSF:

- U.S. leadership is desirable in particle physics, as called for in the NAS report.
- HEPAP was disappointed by the Senate action and would like to see funding for theory and ILC R&D at the level requested in the President's budget request.
- Support for the ILC in the President's budget request was viewed favorably by the international HEP community; it would be opportune to be poised for ILC construction when LHC physics results are in hand.
- HEPAP was pleased that the DUSEL program is on track for site selection and to be proposed for MREFC.
- The advanced accelerator R&D report was received and approved; the recommendations are essential to the future of the field
- HEPAP was excited about the DETF report and approved it.
- HEPAP was pleased with the progress made by P5 on the roadmap, and it looks forward to the final roadmap report; action items are listed in the letter from P5; the priorities cited there are in line with those of EPP2010.
- The Panel had heard the NuSAG status report and looks forward to the final report.
- It looks forward to the final priorities from the CERN Council Planning Group.
- The LHC Theory Initiative is engaging and exciting.
- The ILC is important to the future of the field; detector R&D funding is needed.
- HEPAP recognizes the need to fully staff OHEP with qualified personnel.
- The new charges were received, and subpanels were established to address them.
- There is a need to align the ILC R&D priorities with the overall GDE.
- HEPAP recognizes that it is important that Daya Bay gets off to a good start and understands that support for Double Chooz may be reconsidered.

A request for public comment was made; there being none, the meeting was adjourned at 2:04 p.m.

Respectfully submitted,
F. M. O'Hara, Jr.
Recording Secretary
July 25, 2006
Corrected – M.J. Shochet, August 22, 2006

The minutes of the High Energy Physics Advisory Panel meeting held at The Latham Hotel, Washington, D.C. on July 6-7, 2006 are certified to be an accurate representation of what occurred.

Melvyn J Shochet

Signed by Melvyn Shochet, Chair of the High Energy Physics Advisory Panel on August 24, 2006.