Minutes of the
High Energy Physics Advisory Panel Meeting
February 22-23, 2007
Palomar Hotel, Washington, D.C.

HEPAP members present:

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

HEPAP members absent:

Koichiro Nishikawa
Lisa Randall

Also participating:

Barry Barish, Department of Physics, California Institute of Technology
Dominic Benford, SAFIR Science Team Lead, Goddard Space Flight Center, National Aeronautics and Space Administration
Charles Bennett, Department of Physics and Astronomy, Johns Hopkins University
Tony Chan, Director, Mathematics and Physical Sciences, National Science Foundation
Joseph Dehmer, Director, Division of Physics, National Science Foundation
Steven Elliott, Scientific Staff Member, Los Alamos Scientific Laboratory
John Kogut, HEPAP Executive Secretary, Office of High Energy Physics, Office of Science, Department of Energy
Usha Mallik, Department of Physics and Astronomy, University of Iowa
Marsha Marsden, Office of High Energy Physics, Office of Science, Department of Energy
Peter Meyers, Department of Physics, Princeton University
Frederick M. O'Hara, Jr., HEPAP Recording Secretary, Oak Ridge Institute for Science and Education
Raymond Orbach, Under Secretary for Science, Department of Energy
Randal Ruchti, Program Director, National Science Foundation
Abraham Seiden, Director, Santa Cruz Institute for Particle Physics, University of California at Santa Cruz
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 60 others were also present in the course of the two-day meeting.

Thursday, February 22, 2007

Morning Session

Chairman Shochet called the meeting to order at 10:00 a.m. He welcomed the new members and introduced **Joseph Dehmer** to provide an update on the FY07 and FY08 budgets.

The NSF's flagship collider, CESR/CLEO [Cornell Electron Storage Ring], will run during FY07 and FY08 and will be shut down in FY09. The IceCube detector has finished its third season and has installed new strings of sensors. It now has 22 strings. It has been highly successful and will run three more seasons.

The Large Hadron Collider (LHC) will change particle physics in the next few years. The Physics Division got a 6% increase for research and related activities (R&RA), its full request. That increase will be used to ramp up from $0 to $18 million for the LHC. The main support is for principal-investigator (PI) and graduate-student travel.

DUSEL has four candidate sites that will be reviewed. It should be known this spring what site will be selected. This is a joint, coherent vision for the future.

The Laser Interferometer Gravitational Wave Observatory (LIGO) is operating at its design sensitivity and will collect a year of data this year. Advanced LIGO will be started in FY08.

Theory funding is being increased 5% per year for 5 years. It will go up 10% under the new FY07 budget.

The 2007 budget levels are not approved at the divisional level, yet, and will not be until March or April. In FY07, Physics has increased $14 million, with $8 million of that for particle physics. In FY07, it was extremely fortunate to get a 6% increase. CESR remained at the same level. There is a lot of excitement in a lot of fields, but their funding remains flat. The Physics Directorate would increase even more (8%) in the FY08 budget. That is when a lot of information will be coming in from many programs.

Midscale analytical instrumentation will receive significant increases in the FY09 budget.

Carithers asked when National Science Foundation (NSF) expected to go to the Science Board for major research equipment and facilities construction (MREFC) for DUSEL. Dehmer replied that they did not know yet; it might be done in an ad hoc manner. Given the budget cycle, the earliest it could happen is FY10.

Shochet introduced **Tony Chan** to give an update on NSF's Mathematics and Physical Sciences (MPS) activities in high-energy physics.

The Continuing Resolution, thanks to the community's support, provides 100% funding for NSF's R&RA accounts. The salaries and facilities funding is less than that in FY06 despite the increase in workload and travel. For FY08, every division in MPS is slated to get more than an 8% increase, largely because of the American Competitiveness Initiative (ACI). This increase was also affected by the Physics of the Universe and other initiatives.

LIGO has been running. Advanced LIGO is the only new start at NSF. IceCube has had nothing but good news; it is going well. MPS wants to be the steward for DUSEL. The selection process is very big.

ACI has a lot of industrial support.

The funding process with preconstruction, construction, and operation accounts imposes challenges. Operation costs can overwhelm the budget. A balance must be maintained. The MREFC panel has to be careful what it endorses. One must protect the core research.

Transformational research is often mentioned at NSF, which can only fund the proposals that come in. NSF is like a venture capitalist.

With the emerging nations, particularly those in Asia, the United States has to increase collaboration while remaining competitive. The human resources available in India, for example, are amazing. Major projects are so expensive, they have to be international.

Cahn asked how the scheduling of MREFC occurs. Chan replied that there are various “horizons,” like the five stages of DUSEL. The next stage is readiness. Then there is Board approval with all the NSF assistant directors. The first stages include blue-sky conceptualization, conceptual design, and technical design.

Cahn asked how long it is between Science Board approval and funding. Dehmer replied that once a project is in readiness, it gets inserted in the budget as soon as possible, usually very soon, depending on the space available in the budget. Chan added that there is a lot of competition among the NSF divisions. Other competitors are GENetic Imagery Exploitation (GENIE), National Ecological Observatory Network (NEON), and Earth Scope.

Bagger asked if the NSF has problems with items that are bigger than a piece of equipment and less than a big project. Chan agreed that the agency is well aware of that problem.

Carithers asked if a 7% increase was sufficient and necessary. Chan answered that, even assuming a 7% increase, there are a lot of new projects waiting to be funded. That increase also has to be used to protect the core research. Some budget lines go 15 years out. Funding decisions are easier if there is a budget increase.

Shochet noted that there can be discussions at the division level to advance an important program.

Olson asked where the $18 million came from. Dehmer replied that the director put in $4 million and MPS put in $14 million. That was a pioneering step.

Robin Staffin presented the DOE perspective on the FY07 and FY08 budgets.

The Government is currently in a period of transition. The clearer one’s priorities, the greater the loss of information. Last year, we transmitted to Congress a 5-year projected budget; it will be interesting to see how that will work out in the annual budgets passed by Congress. It is a grand experiment. As the LHC starts up, we will see if Einstein was right when he said that nature is not malicious.

One can now see how the FY08 budget reflects the P5 [Particle Physics Project Prioritization Panel] recommendations:

	FY06	**FY07**	**FY08 Request**
HEP	716.7	775.1	782.2
HEP Plus	746.1	815.1	843.7

[HEP Plus includes Plus Stanford Linear Accelerator Center (SLAC) funding from Basic Energy Sciences (BES)]

Spending power increases to $4.1 billion for the Office of Science (SC) and $775 million for the Office of High Energy Physics (HEP). Under the continuing resolution, FY06 spending rates have been in force since October 2006. Under the joint resolution, the FY07 budget for SC would be $3.8 billion; the HEP amount is to be determined through a redistribution of the FY06 budget amounts.

The overall priorities for FY07 are facility operations, maintaining staff, and the deferral of new projects to FY08.

In the FY08 budget request, Tevatron operations receive declining funding because equipment will have been installed and is running. LHC support increases. The B-Factory funding declines after FY07. Research increases. Accelerator science increases. General accelerator development increases. Other technologies decrease. Other projects (e.g., dark energy) increase. Among projects and initiatives, NOvA [the NuMI Off-Axis ν_e Appearance experiment] increases, the International Linear Collider (ILC) plateaus, SuperNova Acceleration Probe (SNAP) decreases, the Dark Energy Survey (DES) is initiated, and reactor neutrino research (Daya Bay) ramps up. All of this totals $782 million.

The ILC R&D budget process has developed a new body, the ART, which has put together its first budget on the basis of polls of universities and national laboratories. For FY08-FY09, it will have a more rigorous procedure with 11 work breakdown structures (WBSs). Each WBS category has a work-package leader who is responsible for developing priorities, work plans, and effort coordination.

Based on the budget guidance ranges from DOE and on ART input on the relative balance of WBS categories, work plans were proposed by the WBS leaders. In the budget guidance, detector R&D was assumed to be part of the ILC funding line. Advice from the Linear Collider Steering Group of the Americas was sought to help establish the relative balance between accelerator and detector budgets.

Those involved in superconducting radiofrequency (SCRF) recognize that the technology will have broad applications and an overlap with ACI objectives. The ILC will be a driver of this technology. SCRF is requesting $23.45 million in FY08 for

- R&D facilities for developing new cavity-fabrication methods, surface processing, and materials characterization;
- test facilities for single cavities, cryomodules, cryomodule strings, beam injection, and the infrastructure needed; and
- developing productive interactions with U.S. industry to bring its capability to world standards.

In dark energy, the National Aeronautics and Space Administration (NASA) and DOE are jointly sponsoring a National Academy of Sciences (NAS) study, the Beyond Einstein Physics Assessment Committee. Its report is due by fall 2007 to advise NASA by identifying the highest priority among the five proposed NASA Beyond Einstein missions. Should the top priority be the Joint Dark Energy Mission (JDEM), DOE and NASA would propose to proceed with this mission. In the interim, DOE continues to support R&D for JDEM.

One can compare the P5 roadmap versus the FY08 budget request:

P5	FY08 Request
The highest priority is investigations at the energy frontier; that is, the full range of activities for the LHC program and the	• Support for LHC physics through university grants (~$26 million) and the LHC research program ($50 million)

R&D for the ILC	• ILC R&D continues at $60 million with increased support for SCRF R&D
A near-term program and dark matter in dark energy and specific neutrino measurements at CDMS, DES, and Daya Bay along with support for long-term R&D in these areas	• Daya Bay fabrication begins in FY08 with a total project cost of $29 million • DES fabrication begins in FY08 with a total project cost of $20 million • The 25-kg CDMS is to be considered for FY09 • Long-term R&D for dark matter, dark energy, and neutrinos continues
Construction of the NOvA experiment at Fermilab along with a program of modest machine improvements	• NOvA fabrication begins in FY08 with a total project cost not to exceed $260 million; accelerator and improvements to provide additional beam power to NuMI is included

The P5 assumption for the HEP base FY08 budget was $785 million; the actual FY08 HEP budget request is $782 million. The ILC R&D ramp-up profile and the NOvA construction schedule must both be slowed with respect to the most aggressive proposals if costs are to match the assumed annual budgets. The FY08 split is about 55% for the ILC and 45% for all others, not including small projects below the P5 threshold, SCRF R&D and infrastructure that supports ILC and other possible projects, and generic dark-energy R&D. This budget is not out of kilter with P5. The Office took the priorities of the community and tried to conform the budget to those priorities. A lot will hinge on the affordability of the ILC.

Carithers noted that the ILC R&D budget seems to be $83.5 million and asked how accurate that was. Staffin said that the infrastructure exists for a range of things. If ILC went away, SCRF funding would continue. He would not add it to the ILC line. There is synergy.

Bagger asked if he had decided where these facilities will be. Staffin replied that Fermilab is a lead laboratory, but clearly several national laboratories and universities will be participants.

Samios asked when the FY07 budget would be set. Staffin replied that that was out of his hands.

Wormser asked what fraction of the ILC budget would be used for siting activities. Staffin said that isn't clear yet.

Bortoletto asked if there had been any review of Daya Bay. Staffin replied that there was a review at Berkeley in October. The committee said that it was technically feasible. A CD-1 [critical decision] review is scheduled for April 10.

A break for lunch was declared at 11:38 a.m.

Thursday, February 22, 2007
Afternoon Session

Chairman Shochet called the meeting back into session at 1:29 p.m. to hear **Raymond Orbach** review the DOE FY08 budget. This is the second year of the

President's ACI, which calls for a doubling of the budget for the physical sciences over 10 years. The FY07 budget is not quite on track for that doubling, but the FY08 aggressively pursues the goal of doubling that support.

During the next few years, the high-energy physics community will see great scientific opportunities; the theoretical projections are exciting. These, in turn, will pose profound challenges. We must make the right choices on the right timescales to ensure the vitality and continuity of the field for the next several *decades*. We need a strategy to 2020 or 2030. SC is doing 10-year budgets. It must provide 5-year budgets to Congress. It has mortgages over that range of years.

Three events are notable: (1) The U.S. accelerator-based program will complete within the next several years two highly successful experimental campaigns, the Tevatron at Fermilab and the B Factory at SLAC. They both are making very significant advances in the field. They must be recognized for running far above their original design luminosities. (2) The LHC is scheduled to commence operations in FY07, ushering in a period of new and exciting scientific opportunity in FY08 and FY09. SC is a major contributor to the LHC in scientists and equipment. The FY07 budget will complete the contribution. (3) The Global Design Effort (GDE) just recently released a reference design for the International Linear Collider (ILC), a positron machine that holds great promise for deepening our insight into the terascale. These three programs frame the world of high-energy physics today.

The P5 roadmap articulates a broad set of scientific opportunities and compelling priorities. P5 should continue the dialogue on the future of this field.

DOE is committed to a vigorous R&D program for the ILC with $23 million for SCRF cavities. Our R&D partnerships include Asia, Europe, and the Americans. An R&D program is needed to design and develop these cavities, including industrial participation.

We need to learn from our experiences. We *can* build machines on time and under budget. Completing the R&D and engineering design, negotiating an international structure, selecting a site, obtaining firm financial commitments, and building the ILC could take well into the mid-2020s, if not later. That could be 15 to 20 years without the Tevatron. If the ILC were not to turn on until the middle or end of the 2020s, the right investment choices need to be made to ensure the vitality and continuity of the field during the next two to three decades and to maximize the potential for major discovery during that period. The budgets are critical for that path. We must prepare ourselves for that trajectory.

Our commitment to providing facilities has eaten away at the core research budget. We are trying to restore the 50:50 balance between facilities and core research. The FY07 budget is $133 million short of that balance. The hope is that the balance will be restored with the FY08 budget.

The number of postdocs, faculty, and students was reduced by 2200 in the FY06 budget. The FY07 budget restores 1000 of those positions, and the FY08 budget would restore the rest, to a total of 25,500 researchers. Universities are about half the users. The core research is openly competed. The FY08 budget is an attempt to repair the damage done during the past decade to our scientific workforce.

Shochet asked **Abraham Seiden** to respond to Orbach's report from the perspective of P5. He reviewed the P5 report, pointing out the major science questions of mass, of undiscovered principles of nature, of the dark universe, of unification, and of flavor. These science opportunities fall into five categories: the energy-frontier projects of the LHC and ILC, a program to study dark matter (which is complementary to the work in astrophysics), a program to study the nature of dark energy (which is collaborative with the work in astrophysics), a number of projects in neutrino science, and precision measurements involving charged leptons or quarks.

P5 also made a budget assumption based on the DOE five-year budget plan and the closing of the Tevatron and PEP-II [Positron Electron Project II] with recouping of those funds. P5 also looked at a doubling of the physical sciences funding over 10 years. It was disappointed in the FY07 budget situation. It used the following planning guidelines.

- The LHC is the most important near-term project,
- the ILC is the highest-priority long-term investment,
- investment should be made in a phased program investigating dark matter, dark energy, and neutrino interactions,
- the budget for new projects should be split 60:40 between ILC and dark matter/dark energy/neutrinos, and
- construction recommendations should be developed about 2010 after the initial LHC results are known.

P5 grouped major construction and R&D activities into three categories with different degrees of priority:

- LHC and R&D for ILC,
- Dark energy/dark matter (including space missions and DUSEL), and
- NOvA.

It also recommended additional reviews by P5 toward the end of this decade to look at projects that could start construction early in the next decade, such as the ILC, LHC upgrades, DUSEL and other dark matter and neutrinoless-double-beta-decay experiments, stage IV dark energy experiments, and the status of flavor physics. A later review of neutrino physics will also be required.

From these considerations, a detailed roadmap of R&D, construction, operation, decision points, and results was laid out for the potential programs and facilities.

It was a relief to hear of the restoration of core-research funding. There will be many interesting things found from these physics.

Shochet said it is not clear what the schedule is for the ILC. A lot of physics results can be seen coming out over a broad range of projects during the next 10 to 20 years. Internationalization of governance and finance of the ILC is needed. The P5 roadmap was based on given budgetary guidance. It would be good to smooth out (move forward) the large increases in FY10 and FY11. Long-term accelerator R&D operates under the uncertainty of where the next breakthrough will come from.

Cahn noted that the P5 report raises the question of a possible different direction than the ILC resulting from the LHC findings. Seiden replied that that is why P5 has recommended that the review be scheduled. If the case for the ILC is made by LHC results, we want to move forward quickly. Orbach noted that, if the R&D is not done, we will not be ready to go forward, and an aggressive R&D program is essential. Cahn agreed.

Samios asked whether, if the ILC is delayed, the funding can be used for other, bold programs. Orbach replied that the International Thermonuclear Experimental Reactor

(ITER) was an eye-opening experience. It took 3 years to put the agreement in place. If such a “delay” occurred with the ILC, other opportunities would open up.

Lykken said that that should be kept in mind when considering upgrades to the Tevatron. Wormser suggested making a list of things that would be nice to do. This “backup plan” should not be looked upon as an excuse to plan for other projects. Orbach replied that he did not intend this to be a “backup plan” but rather a recognition that there may be other opportunities.

Carithers commented that there are many markers at 2010, but it sounds like SC would like advice earlier than that. Orbach responded that 2010 is a good estimate of when one would understand the needed energy scale for the ILC. The issue of “what next” is what is being asked for. The timeline has to be realistic for getting results out of the ILC. What does that timeline look like? Carither’s asked if that information was wanted within a year. Orbach said that that was correct.

Eno asked if he was saying that there may be more money available for interesting projects. Orbach replied that the amount of money that will likely be available is clear. He was asking about investments in the health of the field and where it should be going. HEPAP should look at a longer timescale, perhaps 10 years beyond 2015. Those investments need to be made now. That is where HEPAP’s help is needed.

Barry Barish was asked to report on the reference design for the ILC.

The United States spends more than $800 million per year on high-energy physics, and it must follow the priorities it sets to move the field forward scientifically. It must not shirk from that responsibility. The United States is in a unique position because of the transitions in the U.S. program at Fermilab and SLAC and the opportunities that those transitions offer. The United States has the opportunity to take the lead on the ILC and to recapture a leadership role in high-energy physics and even to host this frontier facility. The ILC offers a time-dependent opportunity.

The GDE has produced a detailed and complete description of a design for the ILC. It believes that this reference design will achieve the physics goals. A plan has been defined with priorities for the R&D program. The cost scale of the project has been determined and has produced guidance for the engineering phase for value engineering and trade studies. The project is in an excellent position to take on the next phase (to produce a construction-ready proposal) effectively and efficiently.

The costing line in 2007 dollars is $4.87 billion in shared costs, $1.78 billion in site-specific costs, and 13,000 person-years. The LHC will lead the way and has large reach in energy and techniques, including quark-quark, quark-gluon, and gluon-gluon collisions at 0.5 to 5 TeV with a broadband initial state. The ILC is a second view with high precision for electron-positron collisions with fixed energies that are adjustable between 0.1 and 1.0 TeV with a well-defined initial state. Together, these are the tools for the terascale.

Positron-electron collisions will be between two elementary particles that are well defined in energy and angular momentum, will use full center-of-mass energy, will produce particles democratically, and can mostly allow fully reconstructing events.

Examples of using these features include (presuming that the LHC discovers the Higgs) confirming that it really is the Higgs, measuring the quantum numbers (the Higgs must have spin zero), and measuring the spin of any Higgs it can produce by measuring the energy dependence near threshold. Further, (1) one can determine the underlying model; (2) one can take precision top-quark measurements, which allow one to put bounds on axial ttbarZ and left-handed tbW for the LHC and ILC compared with deviations in various models; (3) one can study supersymmetry; and (4) one can answer whether quarks and leptons unify, perhaps by counting extra dimensions.

The Quantum Universe listed nine questions. The ILC addresses virtually all nine.

The International Committee for Future Accelerators (ICFA) published a list of six physics goals for the ILC. It concluded that the Reference Design Report (RDR) design meets these requirements, including the recent update and clarifications of the reconvened ILCSC parameters group. [The parameters group was reconvened and concluded that removing safety margins in the energy reach is acceptable but should be recoverable without extra construction. The maximum luminosity is not needed at the top energy (500 GeV); however, the interaction region should allow for two experiments. The two experiments could share a common interaction region if the detector changeover could be accomplished in a week.]

A baseline configuration was documented in December 2005. This configuration was refined to a reference design, published in February 2007. That design calls for a maximum center-of-mass energy of 500 GeV, a peak luminosity of 2×10^{34}, a beam current of 9.0 mA, a repetition rate of 5 Hz, an average accelerating gradient of 31.5 MV/m, a beam pulse length of 0.95 ms, a total site length of 31 km, and a total power consumption of 230 MW.

A "value" costing system was used, as was done for ITER costing. This project is being costed very early without a lot of engineering drawings. The system is based on agreed-upon "value" costs and provides an estimate of "explicit" labor in man-hours. It is based on a call for world-wide tender, the lowest reasonable price for a required quality. It does not take any credit on outsourcing to lesser-developed countries. It breaks the estimate into three classes: site-specific, conventional, and high-tech.

The estimated costs have been in hand since last July and have been reviewed and vetted since then. Some 20 design changes that could reduce costs were identified and reviewed at several levels. The cost was thereby reduced by 25%, and decisions on some possible cost-saving opportunities were deferred to the engineering phase. The main cost drivers are the main linac and the construction, fabrication, and supply (CF&S). Manpower totals 22 million person-hours, and its main cost drivers are installation and management.

Costs were modeled for a 7-year construction schedule. No major spikes occurred.

How good is this estimate? The methodology is a practical way of developing agreed-to international costing. Its strength is that it is a good scheme for evaluating the value of work packages to divide the project internationally. Its weakness is that it is difficult to sort out real regional differences from differences caused by different specifications etc. Six months were spent developing the methodology, a good WBS dictionary, technical requirements, and the requested costing data. Another six months were spent doing cost vetting and cost/performance optimization. This is a very complete cost analysis for this stage in the design. A lot of sanity checks were performed, checking costs against costs of existing facilities.

The technical facilities include a double tunnel. There is not a large price differential between one and two tunnels. The conventional facilities include 72.5 km of tunnels, 13 major shafts, 443,000 cubic meters of underground excavation, and 92 surface buildings.

The design of the tunnels is quite detailed at this point. There are just small differences in the cost for the tunnels from site to site.

The main linac costs have been estimated regionally and can be compared. Understanding the differences requires detailed comparisons in industrial experience, differences in design or technical specifications, labor rates, assumptions regarding quantity discounts, etc. The gradient choice and its cost impact were looked at. The chosen gradient of 31.5 MV/m cannot be produced reliably now. Cryomodules are being made now in several countries. American and European costs vary widely.

A risk analysis of these cost estimates is now being performed. Three documents will present the RDR Overview, Draft RDR, and the *Gateway to the Quantum Universe*.

The detectors need design work. There is no international committee. Four concepts of detectors have emerged. They have to go from four to two concepts. They do not have design specifications but can determine some performance goals. These goals have been pulled together. These detectors will be harder to build than those of the LHC because of the closeness of the inner vertex layer, pixel size, and thinness of the vertex detector layer.

In summary, the ILC is the consensus long-term priority for particle physics worldwide. In the United States, the priority of the ILC has been reaffirmed by EPP2010 [Elementary Particle Physics 2010]. A solid reference design has been produced. The United States has the opportunity to take the initiative toward making the ILC a reality. What is needed is a strong commitment to support the R&D and engineering design to bring the ILC to construction-ready status at the end of this decade.

Samios asked about going to a TeV. Barish replied that the design group did not want to ask for an empty tunnel. A footprint for 1 TeV (including all the dumps and bins) was put in. An extra, empty tunnel was not included.

Shochet asked how the R&D could be carried out most expeditiously and worldwide. Barish replied that they had been working on that issue incrementally. The U.S. regional people work well together. The Japanese and British have also accepted review. There now is a reference design. Some things will have to be demonstrated in the laboratory. ICFA will review the R&D program in April, and a formal plan will be drawn up next summer. R&D and engineering need to be tied together; that is evolving and should be done in the next six months.

Ozaki asked if he had a plan to maintain momentum in the R&D program if there is a delay. Barish replied that the schedule for a construction proposal is fairly realistic. The design team would be ready to break ground in 2012 with a 2019 completion date. There will be delays (for environmental studies etc.), but he did not know how to manage that.

Eno asked if any parametric studies had been done. Barish answered that a 7-year schedule had been developed, but in a 4½-year construction schedule, time-motion problems might occur. Taking a year longer might save money; a parametric study needs to be done to limit inefficiencies.

Chan asked what contingencies are being planned for. Barish replied that a risk analysis is being conducted. Cost creep must be watched, but one can limit that with good management. A lot of cost studies that could contain costs have not yet been done. Contingency is a different way to do things.

A break was declared at 3:38 p.m., and the meeting was called back into session at 4:01 p.m. to hear **Usha Mallik** report on the activities of the Demographics Subpanel.

For quite awhile, the need has been seen for statistics on the trends in career tracks followed by students and professionals in high-energy physics. HEPAP and the American Physical Society's Division of Particles and Fields (DPF) took ownership of the committee set up to collect these statistics. In 2003, funding to the Particle Data Group (PDG) for this effort was cancelled. Later, after appeal from HEPAP, funding was restored. In 2004 to 2005, responsibilities for the database were transferred from Mike Barnett to Mike Ronan.

Ronan established modern and flexible database software. There was good communication between the Subpanel and Mike Ronan with interim cross-checks of the data collected. New capabilities were incorporated into the database, improving its overall quality.

There was no unique identifier in the database. So, the Subpanel suggested adding a unique ID to each individual for tracking purposes. The archival database would not change, but a modified database would be created for analysis purposes, correcting obvious mistakes and adding extra information for tracking and identification. Tests on a small piece of the new database went well. The Subpanel had planned to send an appeal to the community along with the NSF-DOE questionnaire, but the meeting planned for October 06 was cancelled because of the death of Ronan.

Each year the new data is collected as a separate database. All previous data are put into the archival database, and the two are compared for internal consistency. Multiple passes are made to correct mistakes and create files with the unique identifier.

The main pieces of information used are the surname; first name; initials or middle name (if available); and year PhD earned. These are the same, are carried over from institution to institution, and (often) are unique.

The basic approach is to read in the main database line by line, fix obvious problems, assign a unique number to each individual, look for the entries belonging to each individual, (optionally) check for further potential problems, and write out an updated database in which each individual is "tagged" with a unique number. An error-checking process is then conducted.

Finally, the data are looked over and compared manually. Now real analysis for tracking individuals can begin. The goal is to get a first set of analysis results in 1 year. A cross-check with the new database keeper will be necessary, however.

Bill Carithers at Lawrence Berkeley National Laboratory with professional help is continuing the collection and processing of the data. Maintaining continuity is critical; the process needs to be picked up from where it was left off. Currently, the project to produce the tagged database is well under way.

Shochet noted that, because dark energy is becoming important to high-energy physics, three JDEM PIs were asked to describe their programs at this meeting. Two PIs of ground-based programs will make presentations at the next HEPAP meeting. The first presenter at this meeting was **Saul Perlmutter**, speaking on the SNAP.

Scientifically, this is an extremely important and demanding measurement because we are looking for the signature of a revolutionary change in our picture of physics, a previously unknown component that makes up most of the universe or an error in general relativity or evidence of more than four dimensions or a clue to combining gravity/general relativity (GR) with the other forces/quantum chromodynamics (QCD).

Given the scientific challenge of unusually good control of systematics, a design based on complementary and cross-checking methodologies is called for. All projects use at least two of the three (or four) known approaches. With so few methods available, each one has to stand on its own feet as robustly as possible. SNAP is designed to use the Type Ia supernova and weak-lensing methods.

In the expansion of the universe, it either first decelerates and then accelerates, or it always decelerates. It then may expand forever or it may collapse.

The supernova measurement sample requires about 2000 well-measured supernovae and must study the cosmologically significant redshift range up to 1.7. The uncertainty in the variation of the dark-energy equation-of-state improves significantly out to redshift z of about 1.7.

One needs to measure the brightness of the supernovae and then recognize differences between them and correct for evolving dust extinction, which requires the measurement of three colors. One also needs detailed features through the spectrum into the near infrared. Going to space makes these measurements possible over the full redshift range.

To complement the supernovae study, one uses gravitational weak lensing. Observed galaxy shapes are distorted by the gravitational field of mass concentrations along the line of sight between the galaxy and our telescopes. This effect can be very small and yet detectable statistically after averaging over the measured ellipticity of many galaxies. One needs millions of these measurements. There are lots of galaxies to play with, a good fraction of which cannot be measured from the ground because of atmospheric smearing. Space measurements are more than 10 times better than ground measurements. One also needs to know the galaxy redshift. As one goes through filters, effects show up in the near infrared, pushing this research into space so that these measurements can be made.

The envelope can and must be pushed in control of systematics. Pushing the envelope in technical innovation is not needed or desired. The science is hard, and the implementation is mostly pedestrian. This experiment would require the smallest launch vehicle in its class, a standard bus, a traditional telescope, only one instrument bay, one focal plane, and very few moving parts.

The L2 orbit is a stable platform with a simple design, accomplished with fixed solar panels and antennas. All instruments are on a single focal plane with fixed filters. The field is huge in comparison to that of the Hubble telescope, running from 6000 to 2 million times the Hubble Deep Field (HDF). Also, SNAP is in nine colors. SNAP will be comprehensive, have an independent test of flatness to 1 to 2%, and be complementary.

The biggest jobs are procuring sufficiently good sensors and assembling a Mosaic camera for space, all based on years of successful R&D supported by DOE. This effort began with a proposal to DOE in 1999, leading to today's international program. The DOE R&D was focused on detectors and electronics. A new charge-coupled-device (CCD) technology was developed at LBL that resists radiation damage. Near-infrared sensors now exceed the original SNAP goal. Dark current has been brought down, and matching application-specific integrated-circuit (ASIC) electronics have been developed that convert analog detector signals to digital values, operate at 140 K, and have been subjected to irradiation and cryogenic testing. A spectrograph was developed in France with NASA/Goddard. The focal plane effort has included R&D at dozens of institutions. A large range of detailed engineering studies has been conducted on all this effort.

The 4-year R&D program has had several technical reviews, one of which said, "the Committee felt that there were no technical issues that would preclude readiness of the mission."

Getting this experiment into space is the next hurdle. There are five projects competing for one NASA funding slot. If JDEM is not selected, Centre National d'Etudes Spatiales (CNES) may be a possible launch source. This past November, it initiated a study of SNAP and French participation in the experiment. The European Space Agency (ESA) has developed a program line called Cosmic Visions that could include a dark-energy mission for launch in 2015 or later. The ESA is expected to issue a call later this year to start the process. One could launch this experiment on a Delta IV rocket or on a Soyuz.

Cushman asked about funding from NASA and DOE. If this experiment were put on another launch vehicle, would NASA pull out? Perlmutter said that he did not think NASA has considered what they would do. It seems a natural project for them to be involved in.

Samios asked how much it weighed. Perlmutter replied, 1800 kg.

William Molzon was asked to present the results of the review of DOE's long-range goals. Long-term (~10 year) research goals for the HEP program were set in 2004 by HEPAP and HEP. Quantitative measures were developed as a component of the Office of Management and Budget (OMB) Program Assessment Rating Tool (PART) evaluation process to see whether the DOE meets, partially meets, or falls short of these goals. The quantitative measures are meant to be representative of the broad program goals. SC developed a set of milestones for 2008 to help gauge progress toward the long-term goals. This report provides an evaluation of how the field is doing in meeting the long-term goals. An intermediate level of performance between success and minimally effective is also defined, a change to the PART evaluation system. In some cases, it is not useful because the original performance levels leave little room for intermediate performance.

The long-term goals are to

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

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

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

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

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

Success for the fourth goal consists of confirming or refuting the present evidence for additional neutrino species; confirming or ruling out the current picture of atmospheric neutrino oscillations; measuring the atmospheric mass difference Δm^2 to 15% [full width at 90% confidence level (CL)], if confirmed; and measuring a nonzero value for the small neutrino mixing parameter $\sin^2 2\theta_{13}$ or constraining it to be less than 0.06 (90% CL, ignoring CP and matter effects). NuMI [Neutrinos at the Main Injector] delivered a 0.17 MW average (0.29 MW peak), achieving a 2008 milestone. MINOS [Main Injector Oscillation Search] reported atmospheric ΔM^2 as (2.74 + 0.44 – 0.26) × 10^{-3} eV^2, achieving a 2008 milestone. Atmospheric neutrino oscillations were confirmed in KEK-to-Kamioka (K2K; KEK is the High Energy Accelerator Research Organization) and MINOS, meeting a long-term goal. Projecting the first MINOS result to 7.5 × 10^{20} delivered protons (1.27 × 10^{20} in the first year) gives 90% CL full width ~15% statistical. Many systematic errors should decrease with increased statistics, and prospects for achieving the long-term goal are good. MiniBooNE has shown a projected sensitivity of 3σ coverage of the putative Liquid Scintillator Neutrino Detector (LSND) signal region with the full neutrino data sample, with results expected late 2006 or early 2007. The collaboration is working to improve its analysis, but meeting the 2008 goal is not assured. If the analysis is not significantly improved, reaching a sensitivity sufficient for 95%

exclusion or 5σ detection at the LSND signal value would require at least a significant new commitment of running and perhaps improvements to the beam and detector. The Double Chooz reactor neutrino experiment in France is expected to start operation in 2007 with one detector and in 1 or 2 years later with two detectors. It expects to reach a sensitivity to $\sin^2 (2\theta_{13})$ of 0.02 to 0.03 in 3 years of data taking. The long-term goal is expected to be reached. The Daya Bay reactor neutrino experiment could extend the $\sin^2 (2\theta_{13})$ range by a factor of 3 or so over the Double Chooz sensitivity. The NOvA experiment could also extend the $\sin^2 (2\theta_{13})$ range on a somewhat longer time scale.

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

Success for the sixth goal consists of discovering at more than five standard deviations the particle responsible for dark matter or ruling out (at a 95% CL) many current candidates for particle dark matter (e.g., neutralinos) in many SUSY models. Accelerator searches for dark matter candidates (e.g., neutralinos) are addressed in the section of this report on supersymmetry. Discovering or ruling out candidate particles is likely to be achieved during early LHC running. Confirming a particle detected at the LHC as the dominant source of dark matter particles will require significant analysis and, in some scenarios, more detailed information (e.g., from the ILC). CDMS-II [Cryogenic Dark Matter Search] expects to reach a sensitivity of 1 to 5×10^{-44} cm^2 (depending on mass) within 2 years using their full detector. The 2008 milestone is likely to be met. SuperCDMS 25 kg could reach a sensitivity of about 10^{-45} in approximately 2012, which would cover the mass range of many current candidates for dark-matter particles and would meet the long term goal for weakly interacting massive particle (WIMP) dark-matter candidates. Either a larger version of SuperCDMS or one of a number of promising techniques using liquid noble gasses could reach higher sensitivity. If R&D on some of the noble-gas techniques is successful, sensitivity approaching 10^{-46} could be reached on this timescale. The Axion Dark Matter Experiment [ADMX] expects to reach an interesting coupling sensitivity in 2 years over the full mass region in which axions could account for all dark matter. Technical improvements (lower temperature) could push the coupling sensitivity over this range by another factor of 2 and extend the mass range to about 10^{-4} eV/c^2.

Gladney asked if there were a weighting system. Molzon said that these results were forwarded to DOE. Staffin said that he had no idea whether there was such weighting.

Olson asked if this MiniBooNE issue was a new development. Molzon replied, no, they have been fighting several issues for several years.

Eno asked if the goals had been reassessed. Molzon replied that that should be done, perhaps in 2008.

Jonathan Bagger was asked to provide an update on the activities of the University Research Program Subpanel. The charge is to look at the Program's goals, scope, quality, relevance, human resources, resources, structure, management, and broader impacts.

The Subpanel is having a wide-ranging discussion about the Program, asking: Are there reasons for concern about the health of the University Program? Is the current model of the university high-energy physics research group still valid? Is the current balance of support between nonaccelerator- and accelerator-based research about right? Is the balance correct between theory and experiment? What should be the role of universities in accelerator and detector design and construction? How can the computational capacity needed at universities be provided in the LHC era? How can the University Program link most effectively with neighboring disciplines, such as astronomy and nuclear physics? What is the state of theory, phenomenology, or the student pipeline at universities? What is the expected involvement of university researchers in the ILC R&D, and how will this be managed? How can university groups initiate and execute smaller initiatives? Why has the community failed to use collaborative tools to facilitate university participation in large, worldwide projects? How can we make the field more attractive to future generations of graduate students? How can we use the special features of the field to attract underrepresented students? What is the appropriate role for university-based research scientists in high-energy physics?

These discussions have touched on a number of topics: the loss of infrastructure in universities, support for theory graduate students, the plight of smaller experiments, the process for providing advice to P5, the involvement of universities in ILC R&D and LHC upgrades, research scientists, and collaborative tools.

The Subpanel set up several sub-subcommittees and garnered community input from meetings, DPF mailings, a website, and a PI survey. A March meeting is scheduled for writing the report.

The Subpanel is now focused on analyzing survey results, completing its findings and recommendations, and preparing its report. It is on track for completing the draft report and providing it to HEPAP well in advance of the summer meeting.

The meeting was adjourned for the day at 5:25 p.m.

Friday, February 23, 2007
Morning Session

The meeting was called into session at 8:28 a.m. The Dark Matter Science Advisory Group (DMSAG) report considered here will be reviewed by external reviewers suggested by AAAC and HEPAP. Approval of the report by HEPAP will be voted on after the review. **Steven Elliott** was asked to present the DMSAG report.

The charge was to look at the direct detection and study of dark matter. The Science Advisory Group (SAG) was to look at the most promising experimental approaches; their relative advantages and disadvantages; the optimum strategy to operate at the sensitivity frontier while making the investments required to reach the ultimate sensitivity; the present state of the world wide dark-matter program; and the necessary guidance and constraints for this program.

At two meetings, presenters reviewed the field; and at closed meetings, the information was pulled together. In the past decade, breakthroughs in cosmology have transformed our understanding of the universe. A wide variety of observations now support a unified picture in which the known particles make up only one-fifth of the matter of the universe with the remaining four-fifths composed of dark matter. The evidence for dark matter is now overwhelming, and the required amount of dark matter is becoming precisely known.

We do not know what dark matter is. The current constraints on.dark-matter properties show that the bulk of dark matter cannot be any of the known particles. The existence of dark matter is at present one of the strongest pieces of evidence that the current theory on fundamental particles and forces is incomplete. Because dark matter is the dominant form of matter in the universe, an understanding of its properties is essential to attempts to determine how galaxies formed and how the universe evolved. The discovery of the identity of dark matter is among the most important goals in basic science today.

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

The U.S. projects CDMS and ADMX are leading the field in dark matter of sensitivity. Rapid advances and detector technology have reached interesting areas and can go further. The R&D for the future has also made great advances. There is a broad spectrum of such technologies.

The theory of the strong interactions naturally predicts large CP-violating effects that have not been observed. Axions resolve this problem by elegantly suppressing CP violation to experimentally allowed levels. Cosmology and astrophysics set the allowed axion mass range from 1 μeV to 1 meV, where the lower limit follows from the requirement that axions not provide too much dark matter and the upper limit is set by other astrophysical constraints. There is a specific mass range that one wants to look at for axions.

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

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

WIMPs are particles that interact through the weak interactions of the Standard Model and have mass near the weak scale of about 100 GeV to 1 TeV. Such particles have strong motivations. WIMPs appear in supersymmetric theories and many other model frameworks independently motivated by attempts to understand electroweak symmetry breaking.

These new particles are naturally produced by the Big Bang with the cosmological densities required for dark matter. From a completely model-independent viewpoint, this last property implies that the weak scale is an especially promising mass scale for dark-matter candidates and that experiments that probe the weak scale are required to determine if this possibility is realized in nature.

If one assumes the WIMP to be initially in thermal equilibrium, when expansion outpaces annihilation, WIMPs freeze out. Then one can calculate the decrease caused by the expansion of the universe and that change caused by annihilation and creation. If one integrates it over time, one gets the density now. It goes as one over the annihilation cross-section.

For direct detection of WIMPs, people usually assume a spherical distribution with a Maxwell-Boltzmann velocity distribution and look for elastic nuclear scattering. The overall expected rate is very small, with some models going to 10^{-46} cm^2.

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

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

Some discrimination techniques are ionization, scintillation, and heat phonons. Different experiments use different combinations of these techniques. The CDMS Collaboration has pioneered the use of low-temperature phonon-mediated germanium or silicon crystals to detect the rare scattering of WIMPs on nuclei and to distinguish them from backgrounds. With this powerful technology, operating deep underground in the Soudan Mine in Minnesota, the CMDS group has produced the most sensitive WIMP search in the world, and their reach is projected to grow by factor of 8 by the end of 2007.

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

Future plans include a run in Soudan through 2007 with the existing setup, a run in Soudan with two new supertowers through 2009, and a run at SNOLAB (Sudbury Neutrino Observatory). They hope to make seven supertowers.

A lot of developments are occurring in the field. It has been energized by the emergence of noble liquid gasses (argon, xenon, and neon) in various detector configurations, as well as by new ideas for use of warm liquids and various gases under high or low pressure. These techniques offer an increased reach in sensitivity by at least 3 orders of magnitude for WIMPs, the possibility of recoil-particle-direction measurement, increased sensitivity to spin-dependent interactions, and detector sizes well beyond the ton scale. The complementarity of detector capabilities provides a range of target types suitable for establishing a WIMP signature and diverse background-control methods.

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

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

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

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

The gaseous detectors are more speculative. The theory is that, with a low-pressure gas, one can identify dark matter by observing diurnal periodicity. The direction of the recoil nucleus must be reliably measured. With a high-pressure gas, ionization and scintillation signals are also available at normal temperature. They could provide reasonably sized competitive detectors at high pressure (5 to 10 atm for xenon and 100 to 300 atm for neon). The room-temperature requirement could simplify design and operation.

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

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

Other approaches to dark matter include indirect detection in experiments like GLAST [Gamma Ray Large Area Space Telescope], IceCube, PAMELA [Payload for Antimatter Matter Exploration and Light-Nuclei Astrophysics], HESS [High-Energy Stereoscopic System], and VERITAS [Very Energetic Radiation Imaging Telescope Array System], which offer many new dark-matter detection possibilities that are complementary to direct detection. If WIMPs are a significant component of dark matter, the properties that make them excellent dark-matter candidates also imply that they are very likely to be produced at future colliders. The limits of neutrino telescopes on WIMP-proton spin-dependent cross-sections are currently about 100 times more sensitive than those of direct-detection experiments. WIMP-annihilation signatures in galaxies produce pairs of gamma rays that are subject to detection by gamma-ray telescopes. They also produce particle-anti-particle pairs that can be detected. Because WIMPs couple by weak nuclear interactions to ordinary matter, it may be possible to produce dark-matter particles at colliders, either directly or via decays of other new matter states. The signal would be events with visible particles but with missing energy-momentum. By picking a supersymmetry model plus input parameters, one could calculate sparticle mass spectrum, mixings, production cross-sections, and decay rates. If these are observed at the LHC, then, by fitting all observables, one could measure important astrophysical quantities. The point is that there are regions where indirect detection is more sensitive than direct detection and direct detection is more sensitive than the LHC.

We recommend that the United States advance the search for dark matter with a variety of physical and technical approaches. U.S.-led experiments currently lead the world in sensitivity of the direct-detection searches for both WIMPs and axions. This leadership should be preserved. In addition to supporting the running and improvement of existing detectors, the United States should strongly support R&D for the next stage of technology development with a goal of steady progress toward ton-scale and larger detectors. DOE and NSF should increase funding for the direct detection of dark matter from the present $2 to $3 million to $10 million a year. The prospect of detecting dark matter while the LHC is operating amply justifies this increase. Such a figure is also consistent with the recommendations of P5 and EPP2010.

The Subpanel recommends the completion and operation of CDMS-II and the funding of two SuperCDMS superpowers at the Soudan site. If dark-matter funding is increased to the range suggested above, the Subpanel supports the design and construction of the necessary refrigeration system for SuperCDMS in SNOLAB, contingent upon a full evaluation of the field to be completed by mid-2009.

The Subpanel recommends that the ADMX collaboration be supported to operate the existing detector and, pending success of Phase I, to take the necessary steps to reach greater sensitivity through lower system temperature.

The Subpanel recommends the development of superheated-liquid detectors. The program proposed by COUPP appears to be well balanced and has recently been approved by the Fermilab Physics Advisory Committee. On the basis of the performance and background levels presented by the DRIFT collaboration, the Subpanel recommends the development of a single prototype detector module with the principal goal of demonstrating track reconstruction and directionality determination.

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

Eno noted that there is no comment about the axion experiment. Elliott replied that there are other axion experiments. He did not know why there is not strong competition. If Livermore has positive results, others will want to verify them.

Bortoletto asked if the group had any recommendations on streamlining R&D and avoiding duplication. Elliott replied that the group believed that two-phase xenon should go forward. The different projects are similar and should come together. Otherwise, there will be a shoot-out review.

Lykken asked if 2009 was the best time to pick a dark-matter experiment for DUSEL. Elliott responded that one wants to understand how these different technologies will go forward. 2009 seems like a good time to do that.

Cahn asked if DUSEL is critical to this program. Elliott answered that the program would be seriously slowed if there were no DUSEL. No given experiment would fail, though.

Olson asked if DUSEL would be better than SNOLAB. Elliott responded affirmatively. It would allow more diversity. These are all long-lasting programs, and SNOLAB could be oversubscribed.

Wormser asked how international these programs were. Elliott replied that virtually all these experiments have international collaborations. There were several European and Japanese members on this Subpanel.

Samios asked if COUPP were trigger limited. Elliott replied that the dead time is not very long, and one is looking for very dense tracks to cut down background.

Gladney asked how much of the funding was for the 25-kg CDMS. Elliott answered, on the order of 30% or higher.

Perlmutter asked how the Subpanel's recommendations fitted into those of P5. Elliott answered that P5 does not quantify the costs of these programs. That is what this Group adds to the table. Otherwise, the two organizations are similar. Perlmutter asked if P5's dollar numbers were the same. Elliott replied that the two groups use the same sources for dollar numbers. Seiden said that the magnitude is similar. P5 relied on this SAG to review R&D-state programs. The agencies need to take this report and provide the funding for R&D so P5 can see what these experiments are.

Cahn asked how HEPAP should provide comprehensive input into the budget decisions and whether the support for these projects would shut out other small projects. Shochet commented that it would have been better if this report had preceded P5. There will be a P5 in the future. For small items, the answer depends on timing, typical interest, and other factors.

Bagger noted that the $10 million is consistent with the interest in the field, with P5, and with NSF advice.

Wormser asked if the SAG had considered letting this Dark Matter (DAMA) project's result go. Elliott responded that many groups are checking out that result. Others are looking at other nuclei and will run at higher sensitivities. DAMA is looking at a very small modulation of the large WIMP signal.

Charles Bennett was asked to summarize the Advanced Dark-Energy-Physics Telescope (ADEPT).

The dark-energy equation of state can be stated as $P = w\rho$. The options have huge implications for fundamental physics; w can be a constant, not constant, or irrelevant because general relativity is wrong. What is needed are complementary space-based and ground-based measurements. ADEPT is designed to do from space what needs to be done from space in a cost-controlled manner. ADEPT is a dark-energy probe, a redshift survey of 100 million galaxies with slitless spectroscopy with nearly cosmic variance limited over nearly the full sky producing the first and final generation $1 \leq z \leq 2$ BAO measurement. During the survey, supernovae will go off and be observed at $0.8 \leq z \leq 1.3$ with no additional hardware or operating modes, producing angular diameter distance, luminosity distance, expansion rate, and other interesting things.

ADEPT is a dedicated facility in the spirit of the Wilkinson Microwave Anisotropy Probe (WMAP), the cost of which was $145 million. ADEPT controls costs by having a 1.3-m mirror, a single instrument, a pre-determined survey mode of operations, no new technology, and a small team.

Dark energy was discovered in 1998. In 2003, WMAP observed baryon acoustic oscillations (BAOs). Although there are fluctuations on all scales, there is a characteristic physical and angular scale at 148 Mpc, a characteristic physical scale of the universe. As one goes forward in time, the photon-baryon fluid advances more rapidly than does the cold dark matter, and the universe has a superposition of these shells. At $z = 1090$, photon pressure decouples, the cosmic microwave background travels to us, the sound speed plummets, and the wave stalls at a radius of 148 Mpc. Dark matter and baryons are pulled together gravitationally and seed galaxy formation. The BAO generates a 1% bump in the galaxy correlation function at 148 Mpc. In 2005, the bump was detected in z = 0.35 SDSS.

The physics of these BAOs is well understood, and their manifestation as wiggles in the CMB fluctuations spectrum is modeled to very high accuracy. The value of r_s is 148 ± 3 Mpc. The sound horizon scale can thus serve as a standard ruler for distance measurements. Lingering doubts about the existence of dark energy and the composition of the universe dissolved when the WMAP satellite took the most detailed picture ever of the CMB.

There will be an enormous amount of data from ground sites in the next few years up to $z = 1.8$, where ground-based observations get difficult. Space observations have a role to play at higher redshifts. The ADEPT BAO/supernovae redshift overlap will tie $z \sim 0$ to $z = 1090$, and BAO will check supernovae systematics. ADEPT will survey 10^8 galaxies, a full-sky BAO. ADEPT is complementary to the Large-Scale Space Telescope (LSST); 10^8 galaxies are fundamentally different than 10^9 galaxy shapes. However, ADEPT and LSST are a powerful combination.

The systematic errors of BAO are nonlinearities. Nonlinear gravitational collapse occurs on small scales; large-scale features are unaffected. The BAO technique improves with redshift. Redshift distortions arise from a halo's peculiar motion relative to the Hubble flow. Clustering bias arises when two overdense regions fall toward each other and two underdense regions fall away from each other. This cancels to first-order unless galaxy bias causes us to misweight the over- and underdense regions. Clustering bias and redshift distortions do not create features at preferred scales of 148 Mpc. Computer simulations verify BAO at the 1% accuracy level; but 1% is not adequate, and we are now running 0.1%-accuracy-level simulations. Any small errors will be correctable with such simulations based on known gravity physics. The conclusion is that acoustic peaks are robust.

The Dark Energy Task Force (DETF) said that "This is the method least affected by systematic uncertainties and for which we have the most reliable forecasts of resources required to accomplish a survey of chosen accuracy. This method uses a standard ruler understood from first principles and calibrated with cosmic-microwave-background observations. An advantage of BAO is that it does not require precision measurements of galaxy magnitudes, though if photo-zs are used, then precision in galaxy colors is important. In contrast to weak lensing, BAO does not require that galaxy images be resolved; only their three-dimensional positions need to be determined."

ADEPT Type Ia supernova systematic errors include intrinsic supernova luminosity unknowns, the incomplete understanding of the physics of explosions, unsure photon propagation interactions, and the limited calibration accuracy. BAO is used to provide a high-quality check whether supernovae extend another order of magnitude before reaching systematic limits. If one assumes 0.3%, about 1000 are needed.

In terms of tests of modified gravity, there are small-scale laboratory experiments and solar-system tests. Modified gravity changes the correlations between the CMB and galaxy surveys. All of these phenomena are accessible with current and future data and provide stringent tests of general relativity on cosmological scales. In weak lensing and growth of structure, ADEPT's 3-D positions of 10^8 galaxies will calibrate ground photo-zs for the Panoramic Survey Telescope and Rapid Response System (Pan-STARRS), LSST, etc.

The DETF drew up a figure of merit. Improvements must model $w(z)$ with many depths of field (DOFs), must account for the combination of space data with anticipated ground data, and must estimate appropriate futures systematic error limits.

Calculations of $w(z)$ over redshift show that future ground measurements, ADEPT plus space supernovae measurements, and then the addition of BOA will narrow down the possible value of $w(z)$, progressively ruling out competing models.

Why go to space? One needs infrared to measure $z > 1$ to the experimental/cosmic limits. This cannot be done from the ground. The sensitivity to infrared is 1000 times better in space. Space provides a full-sky sample homogeneity. These factors enable ADEPT to reach the cosmic variance limit, they open the investigation of dark energy at $z > 1$, and they harvest full BAO information at the important range of $1 < z < 2$.

ADEPT has only one instrument, one detective type, and one observing mode. Its 1.3-m mirror is scaled from the current Geo-Eye 1 payload. Geo-Eye 1 slews multiple times per orbit; has eight low-noise reaction wheels; has an 1800-kilogram observatory; is due to launch this year; is fully redundant; has a 7-year lifetime; has a price tag of $209 million; has a camera and optical telescope developed by ITT; and has a 1.1-meter clear-aperture three-mirror anastigmat telescope. No technology breakthroughs are required.

Dark energy is a fundamental scientific mystery. There is a need to measure $w(z)$ or to determine that general relativity is wrong. Physicists require reliable results.

Samios asked if one could turn all of those telescopes looking at the Earth around. Bennett replied that the Earth is a high-signal source; what is being looked at is a low-signal source.

Cushman asked what the cost cap was. Bennett replied, $600 million. Staffin said that DOE will judge it by the physics, not the cost.

A break was declared at 10:16 a.m. The meeting was called back into session at 10:41 a.m. to hear **Dominic Benford** describe DESTINY, the Dark Energy Space Telescope.

Dark energy makes up 73% of the universe, dark matter 23%, intergalactic gas 3.6%, and stars etc. 0.4%. *Science* magazine listed dark energy as one of the top questions of 2005.

Measurements have shown that dark energy is 70% of the universe and that the density of the universe is dropping.

DESTINY was awarded a JDEM concept study by NASA in 2006. It is pursuing a DOE grant to augment the NASA grant. The target is to have high-fidelity definition by mid-2008 and to be prepared for the Astronomical Observatory (AO) flight in late 2008.

It is a straightforward 1.65-meter telescope placed at L2 orbit to conduct a Type Ia supernova survey over $3^{\circ 2}$ in the first 2 years and a weak-lensing survey over $1000^{\circ 2}$ in the third year. The goal is to measure w_0 to 0.05 and w_a to 0.20.

The design philosophy calls for doing in space only what must be done in space and to leverage ground-based observations. It also calls for the use of the minimal instrumentation required, a highly automated survey, and all spectra all the time. A science team has been assembled.

The DETF called for a mix of techniques, which is essential for a fully effective dark-energy program. The primary survey will leverage the maturity of the supernova standard-candle technique (with data from existing supernova studies) to precisely determine the dark-energy equation of state. We need to know the Hubble diagram and the supernova light curves. We have to go to high redshifts to go back in time (to 1.7). We have to go to space because the near infrared is available only in space. Crucial near-infrared observations are impossible from the ground for the required photometric accuracy.

We have a proven observing strategy. Supernovae are the only things that will change in a field. We will start off with a galaxy field; eventually, supernovae will go off and their galaxy's spectra can be subtracted to determine the supernovae. This is done over three months. We will take a series of monochromatic flat fields, obtain high-fidelity external and internal flats and ground tests, monitor with internal flats on orbit plus field stars, make absolute photometric calibrations with DA white dwarfs, and isolate supernovae spectra with differencing. The ad hoc spectral flat will be extracted from the data cube of monochromatic flats. After doing this, we will get simultaneous spectra and photometry (red shift and brightness). This will provide equal precision and more accuracy than broadband filters alone.

Combining all the data, one gets a signal-to-noise ratio of more than 100 and always gets optometry around maximum light. A sample is taken every 5 days. Supernovae are the most direct and precise approach to study dark energy. A lot of extra data will be obtained and will be used to improve systematics.

There are many ground observations; DESTINY will complement those data and extend them with a little overlap.

In weak lensing, we measure the shapes of galaxies. The dominant noise source is the random intrinsic shape of galaxies. Large-N statistics extract lensing influence ("shear") from intrinsic noise. We will measure the shape of supernovae before and after their light goes through the web of mass and compare the two measurements.

Ongoing work continues to refine the dark energy and cosmological parameters. Preliminary results from ESSENCE [Equation of State: SupErNovae trace Cosmic Expansion] are consistent with $w = -1$. We will compare the resultant data against the results of various models. Weak lensing and supernovae will not give the same results but will overlap.

The DESTINY supernova survey was motivated by the unique role of a near-infrared space telescope for observing Type Ia supernovae at $z > 0.8$. All spectra all the time gives a rich data set that allows for future developments of Type Ia supernovae standard candles and minimizes machine complexity. The weak-lensing survey uses sharp and stable point-spread function (PSF) and near-infrared for depth. The instrument follows from the Hubble Space Telescope and the James Webb Space Telescope (JWST); its unique aspect is the large mosaic of H-2RG sensor chip assemblies (SCAs). The analysis techniques are well understood for both supernovae and weak-lensing observations. Absolute photometric calibration of supernova survey data is a challenge and a major part of present the study.

The design is to take both supernova and weak-lensing survey fields located near both ecliptic poles. There will be no targeting or acquisition of specific objects. These will be highly automated and repetitive blank-sky surveys. No real-time or time-critical operations are required. The steady-state operations mainly comprise monitoring of the data stream, spacecraft health, and occasional maintenance. Location at L2 gives a stable spacecraft and simple operations. Delivery to L2 can be done with an Atlas V with ample mass margins.

The performance requirements include a survey time of 2 years for supernovae and 1 year for weak lensing; survey areas of 3.2° squared for supernovae and 1000° squared for weak lensing; a science field of view (FOV) of 0.18° by 0.72°; $0.4 \leq z \leq 1.7$; near-infrared broadband filters; a 0.13" resolution; 0.01" pointing; a stability of 0.01"/900 s; a data rate of 13 GB per day; and passive thermal control.

The detector arrays are made by two vendors; the technology is well-established.

Peter Meyers was asked to report on the status of the Neutrino SAG (NuSAG). Important work is coming out of the Brookhaven/Fermilab group, and NuSAG's final report will be presented at the July HEPAP meeting.

The current charge is to consider the scientific potential, detector options, timeline, needed scientific inputs, and addressable additional physics of a megawatt-class proton accelerator as a neutrino source for a multiphase off-axis program or a long-baseline broadband program of neutrino research. T2K and the NOvA experiment currently use off-axis neutrinos to create narrowband beams. BNL and Fermilab have created a working group to study the options. The main scientific objectives are to determine the mixing angle θ_{13}, the neutrino mass hierarchy, and the presence or absence of CP violation in oscillation experiments.

The paradigm is three-neutrino mixing with three mass eigenstates with three mixing angles and a CP violating phase. Two of the three mixing angles have been measured, and both of them are large. θ_{13} has not been measured; δ is the CP-violating phase. This is beyond SM physics.

The other dimensions are the mass eigenstates that measurements show are different, but it is not clear if the distribution is normal or inverse. This is the mass-hierarchy problem.

Goals of the next phases of the worldwide experimental program in neutrino oscillations are to fill out the understanding of three-neutrino mixing and oscillations. What are the orderings and splitting of the neutrino mass states? What are the mixing angles? Is there CP violation in neutrino mixing? There is a worldwide effort to do all of this.

A ν_μ is seen to turn into a ν_e. This process can be described mathematically, but there are several unknowns in that mathematical expression, such as the mass hierarchy, δ, and $\sin^2 2\theta_{13}$.

Another approaches the disappearance of electron antineutrinos from reactors. Looking at that can tell if θ_{13} is > 0 and when compared with results from the accelerator-based ν experiments, can perhaps determine the mass ordering and CP violation. Reactor experiments measure only θ_{13} but without ambiguity. Their results can be combined with accelerator results to break degeneracies in some regions if there is sufficient precision to resolve the ambiguities present.

Phase 1 of this enterprise is the suite of currently approved or planned reactor experiments at Double Chooz and Daya Bay. Double Chooz is expected to increase the 3σ sensitivity to $\sin^2 2\theta_{13}$ to about 0.05 by 2012. Daya Bay is expected to increase the 3σ sensitivity to $\sin^2 2\theta_{13}$ to about 0.02 by 2013. In accelerator experiments, T2K is expected to improve the 3σ sensitivity to the probability of $\nu_\mu \rightarrow \nu_e$ to about 0.01 by 2014. NOvA is expected to improve the 3σ sensitivity to the probability of $\nu_\mu \rightarrow \nu_e$ to about 0.005 by 2016. Together, they are expected to exhibit some sensitivity to the mass hierarchy at the highest currently allowed values for θ_{13}.

Phase 2 (the subject of the current charge) consists of the next round of accelerator experiments, which is to extend the mass hierarchy and CP-violation sensitivity down to a $\sin^2 2\theta_{13}$ of about 0.01, where one runs out of steam with conventional beams.

The charge letter asked NuSAG to assume a megawatt-class proton accelerator as a neutrino source and to address the following questions about the different accelerator-detector configurations needed for a multi-phase, off-axis program and a very-long-baseline broadband program: the scientific potential; associated detector options, including rough cost; the optimal timeline, including the international context; other scientific inputs needed; and additional physics that could be addressed.

Both T2K and NOvA use off-axis neutrinos to create narrowband beams, and both layout potential programs including upgraded accelerator power, beams, and detectors. An alternate approach that uses a wideband beam has been proposed by a BNL group. Concurrently with NuSAG's deliberations, BNL and Fermilab convened a study group spanning both approaches. The general consensus was that the Fermilab Main Injector would be the proton source for either approach in the United States.

One starts with a beam that is mostly muon neutrinos, and one looks at the oscillations to electron neutrinos by looking for electrons in a distant detector. There are two principal backgrounds: (1) neutral pions, where the two gamma rays are not distinguishable from a single electron and (2) an intrinsic background in the number of electron neutrinos in the beam from the accelerator. The latter background is irreducible. Backgrounds are measured in the near detector to reduce systematic error.

In $\nu_\mu \rightarrow \nu_e$ appearance experiments, an accelerator produces positive pions, which decay into muon neutrinos. The muon neutrinos are propagated about 1000 km to see if they turn into electron neutrinos. The signal watched for is electrons produced by electron neutrino reactions in a distant detector. That signal is masked by two types of background, one of which can be rejected (by water-Cherenkov, segmented-liquid-scintillator, and liquid-argon detectors) and one of which cannot.

These produce systematic errors.

The off-axis approach takes advantage of pions of all energies at a fixed angle from the pion-beam direction (which produces neutrinos of about the same energy, a narrowband beam). The loss of flux has the benefit of decreasing neutral-current neutral-pion background. Electron neutrinos are also produced from kaons at different energies.

But the narrow-beam approach brings with it ambiguities and degeneracies. At a single energy and baseline, a perfect measurement of the probability of $\nu_\mu \rightarrow \nu_e$ could yield 0.02, which establishes θ_{13} as greater than zero. However, that measurement is consistent with $0.025 < \sin^2 2\theta_{13} < 0.075$, both mass hierarchies, and any CP phase δ (including zero). New measurements are needed to determine the mass hierarchy and perhaps whether CP violation occurs.

So, how does one proceed? The off-axis-beam approach is the experimental way of looking at the bi-probability plots. The narrow energy band suppresses production of background neutral pions from neutral current interactions by reducing the high-energy-neutrino flux. (However, the background from electron neutrinos in the beam, mostly from high-energy K meson decay, is irreducible.) It uses an upgraded NuMI (Neutrinos at the Main Injector) beam and may require a second detector at the second-appearance maximum.

Experiments that use a wideband-beam approach use a broad spectrum of energies (and therefore a more intense beam), which allows one to use longer baselines to enhance the matter effect. The neutral-pion rejection looks good at 60 GeV; 120-GeV data are not yet available.

The U.S. experimental scenarios using these approaches all start with the Fermilab Main Injector, which has achieved a maximum beam power of 315 kW. It will initially be upgraded to 700 kW and then to 1.2 MW. Less beam power is produced at lower energies. The off-axis approach uses a detector with about 100 kt of liquid argon on or near the surface and the NuMI beam. The wideband beam, very-long-baseline experiment would use a 300- to 500-kt water-Cherenkov detector underground (in DUSEL) with a new neutrino beam. With the wideband-beam approach, the location of DUSEL affects the discovery contours. If the baseline is too short, a matter effect is small; if it is too long, some sensitivity is lost.

Other physics, such as that on nucleon decay and low-energy neutrino astrophysics, may be possible with these facilities, although they may accrue additional costs.

The water-Cherenkov technology is known, is large, requires an underground facility, and has high costs and long construction times determined by the manufacture of the photomultiplier tubes. It would benefit from R&D on new light sensors. The liquid-argon technology can reconstruct events in detail, leading to excellent neutral pion rejection; requires aggressive R&D to prove its feasibility at scale; and must demonstrate that it can work at the surface.

These factors translate to the two approaches under consideration as follows:

- The positive aspects of the off-axis approach are the reduced neutral-pion background, the known neutrino energy, the use of the existing NuMI beam, the common detector technology for near and distant detectors, and the possibility of an incremental deployment. The negative aspects of the off-axis approach are the need to deal with the ambiguities associated with a single energy if operating at a single oscillation maximum, the very low event rates at the second-maximum site, the need to be on the surface, the intensive R&D needed on liquid-argon detectors, and the differences in the beam quality between the near and far detectors.
- The positive aspects of the wideband-beam approach are the full energy spectrum for resolving ambiguities, the proven detector technology, the broader physics program allowed by DUSEL, and the encouraging recent progress in neutral pion rejection in the water-Cherenkov technology. The negative aspects of the wideband-beam approach are the large, all-at-once cost; the possible timeline constraints imposed by DUSEL; the cost uncertainties associated with the photomultiplier tube coverage; and the need for different types of detectors at the near and far locations.

In summary, NuSAG is educated on the issues, the findings on technical issues are mostly in place, the BNL/Fermilab study group is working on directly comparable sensitivity calculations for the different scenarios, the need for an observation of a nonzero θ_{13} is clear, the value of θ_{13} determines the needed detector masses (and costs), and the needed R&D has been identified. The NuSAG report will be available before the next Nuclear Science Advisory Committee (NSAC) and HEPAP meetings.

Molzon asked if NOvA would really have new results by 2015; the P5 report showed that NOvA would not start until 2013. Meyers answered that the specifications were being taken directly from the proponents, and they may not be comparable. NOvA would start running when the detector is completed, and the curve drops off very rapidly. The values provided may need to be interpreted. Molzon asked whether, if one knew θ_{13}, it would limit the strategies to be pursued (among detector types). Meyers responded, possibly. That issue has not been pursued quantitatively. The off-axis program has more difficulty getting down to the lowest θ_{13}.

Bagger asked if Fermilab can shoot to all the potential DUSEL sites. Myers answered, yes, although the short baseline to Soudan may be a problem. This aspect should be considered in the DUSEL site selection. Only Homestead and Henderson have been checked.

Bortoletto asked when the BNL/Fermilab study would be completed. Meyers answered that they will likely wrap up well before the next HEPAP meeting.

Cushman asked whether the wideband option required a longer baseline for the same figure of merit. Meyers replied that they would see benefits from a longer baseline: it would get broader parameters, and it could move to a higher energy.

Olson asked whether DUSEL was not deeper than needed for this experiment. Buyers responded that it was, but it was quite adequate. A break for lunch was declared at 12 o'clock noon.

Friday, February 232, 2007
Afternoon Session

The meeting was called back into session at 1:30 p.m. The points for the draft summary letter were discussed.

- It was noted that Orbach is extremely supportive of ILC; it has the highest priority in the SC mid-term facility plan. He had reminded the Panel about uncertainties in its schedule.
- HEPAP reiterates its support of the scientific program outlined in the P5 report.
- HEPAP is very impressed by the Reference Design Report for the ILC. It is a major step toward realizing the ILC with its enormous scientific promise. It is imperative that the R&D be carried out as effectively and efficiently as possible. HEPAP encourages the GDE to establish a coordinated international R&D program based on international agreements to provide an engineering design by 2009. Raubenheimer suggested adding "R&D and engineering." Ozaki asked if 2009 would be too soon. Raubenheimer suggested saying 2009 or 2010. Molzon asked if someone could clarify the relative efforts represented by engineering design and R&D. Raubenheimer said it was about 25% engineering. The

Engineering Design Report has to be based on DOE milestones. One can do sufficient engineering with 25%; it is not a build effort.

- HEPAP was pleased to hear about the significant increases in the NSF Physics Division budgets in both FY07 and FY08, including increased funding of theory.
- HEPAP supports the DUSEL planning but is concerned about the MPS budget having to absorb DUSEL operating costs. Molzon asked if something should be put in about a long-baseline neutrino cavern. Shochet noted that there was still some concern about where the operating costs would come from. It is hoped that the MPS budget will be increased to cover those costs. P5 was enthusiastic about DUSEL, with the funds covering two projects of interest here. Samios suggested voicing support, not just concern.
- HEPAP heard that the FY08 budget for the DOE Office of HEP is an increase of 3.5% compared with the proposed FY07 budget. However, the FY07 continuing resolution provides only $3.8 billion for SC, down from the proposed $4.1 billion. The P5 recommendations were compared with the FY08 budget request. The agreement is very good. HEPAP was glad to hear that the Daya Bay review committee concluded that the experimental goals could be reached. Bagger suggested referring to the SCRF R&D.
- Orbach reported that the ILC has great scientific promise, but the date it would start operation is uncertain. It is thus imperative that the rest of the program be optimally designed. The program recommended by P5 will yield important scientific results about the energy frontier, dark energy, dark matter, and neutrino properties through at least 2020. Wormser asked if it would be useful to mention the need to stick to the schedule presented by Barish. Shochet said that he would not want to push Orbach to adhere to a schedule that he does not think is politically possible. Kephart said that it was his impression that the FY09 budget could be flexible enough to accommodate R&D. Shochet commented that there are significant increases in FY10 and FY11 and smoothing out that spike to FY09 could ease the transition. Something about that suggestion could be put in the letter. What is in there now is focused on the next step. P5 suggested that 2010 is the best time to consider a construction-start date. The letter will try to encourage an early start to the internationalization of the project, perhaps through bilateral agreements.
- Mallik described the demographic survey. An important change under way is adding identifiers in the database so that individual careers can be tracked.
- Because the study of dark matter and dark energy is becoming an increasingly important element of the HEP program, we began a series of educational presentations on possible Stage-IV projects. At this meeting, we heard from the three JDEM-mission concepts. Saul Perlmutter described the SNAP detector, Charles Bennett described the capabilities of the ADEPT experiment, and Dominic Benford presented the DESTINY concept. Samios suggested adding that each experiment covers at least two techniques.
- Molzon presented HEPAP’s midterm assessment of the DOE long-range goals. The report, which was transmitted to the agency in December, concluded that excellent progress is being made toward almost all of the goals. The exception is the confirmation or exclusion of the LSND neutrino result.

- Bagger reported on the work of the University Research Subpanel. Its written report will be ready for HEPAP review prior to the next meeting.
- Steve Elliott reviewed the science and draft conclusions of the Dark Matter Scientific Advisory Group. The comments of HEPAP members had been forwarded to the Committee. The next step will be an external review. We will vote on approval via email. If there are substantive changes, a presentation will be made at the July HEPAP meeting.
- The NUSAG draft report will be completed soon. HEPAP will consider it at its summer meeting.

The meeting was adjourned at 2:05 p.m.

Respectfully submitted,
Frederick M. O'Hara, Jr.
HEPAP Recording Secretary
April 9, 2007

Corrected – M.J. Shochet, June 15, 2007

The minutes of the High Energy Physics Advisory Panel meeting held at The Latham Hotel, Washington, D.C. on February 22-23, 2007 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 June 15, 2007.