Draft Minutes
High Energy Physics Advisory Panel
October 22–23, 2009
Hilton Embassy Row
Washington, D.C.

HEPAP members present:

Hiroaki Aihara
Marina Artuso
Alice Bean
Patricia Burchat
Lance Dixon
Graciela Gelmini
Larry Gladney
Boris Kayser
Robert Kephart
Steven Kettell
Wim Leemans
Daniel Marlow
Ann Nelson
Paris Sphicas
Kate Scholberg
Melvyn J. Shochet, Chair
Henry Sobel
Maury Tigner
William Trischuk
Herman White

HEPAP members absent:

Priscilla Cushman
Sarah Eno
Stephen Olson
Lisa Randall
Sally Seidel

Also participating:

Barry Barish, Director, Global Design Effort, International Linear Collider
Frederick Bernthal, President, Universities Research Association
Glen Crawford, HEPAP Designated Federal Officer, Office of High Energy Physics, Office of Science, Department of Energy
Joseph Dehmer, Director, Division of Physics, National Science Foundation
Cristinel Diaconu, Directeur de Recherche, IN2P3/CNRS, France
Robert Diebold, Diebold Consulting
Marvin Goldberg, Program Director, Division of Physics, National Science Foundation
Judith Jackson, Director, Office of Communication, Fermi National Accelerator Laboratory
Young-Kee Kim, Deputy Director, Fermi National Accelerator Laboratory
John Kogut, HEPAP Executive Secretary, Office of High Energy Physics, Office of Science, Department of Energy
Dennis Kovar, Associate Director, Office of High Energy Physics, Office of Science, Department of Energy
Kevin Lesko, Nuclear Science Division, Lawrence Berkeley National Laboratory
Marsha Marsden, Office of High Energy Physics, Office of Science, Department of Energy
Howard Nicholson, Program Manager, Office of High Energy Physics, Office of Science, Department of Energy
Piermaria Oddone, Director, Fermi National Accelerator Laboratory

Frederick M. O'Hara, Jr., HEPAP Recording Secretary, Oak Ridge Institute for Science and Education
Moishe Pripstein, Program Director, Division of Physics, National Science Foundation
Steven Ritz, Department of Physics, University of California, Santa Cruz
David Sutter, retired from the Office of High Energy Physics, Office of Science, Department of Energy
Andreene Witt, Oak Ridge Institute for Science and Education
Darien Wood, Department of Physics, Northeastern University

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

Thursday, October 22, 2009
Morning Session

Chairman **Melvyn Shochet** called the meeting to order at 9:00 a.m. He introduced **William Brinkman** to give an update on the activities of the Office of Science (SC) of the Department of Energy (DOE).

The Office has gotten three budgets approved by Congress in the past year: FY09, American Recovery and Reinvestment Act of 2009 (ARRA), and FY10. Of import are (1) pushing the country to do something about climate and energy, (2) the Energy Frontier Centers, and (3) the Bioenergy Research Centers. This Advisory Panel should take some pride about what has been accomplished by the accelerator program. The Nobel Prize in chemistry went to a three researchers who used that program for their research.

SC does three things: discovery science, science for national needs, and user facility operations. The Office of Basic Energy Sciences is the largest office in SC, and the Office of Biological and Environmental Research is expanding. The Office of Advanced Scientific Computing Research (ASCR) is also expanding and implementing and operating the largest, fastest open-science computers in the world.

SC operates the Spallation Neutron Source (SNS), the most powerful accelerator in the world; ITER; five nanoscale centers; the Joint Genome Institute, which is now studying microbes; EMSL; and the Atmospheric Radiation Measurement (ARM) centers. All in all, this is a broad spectrum of facility capabilities. SC will have about 25,000 users at its facilities in FY10, about one-half from universities and about one-third from national laboratories with the remainder from industry, other agencies, and international entities. Basic Energy Sciences (BES) facilities account for about one-half of SC's users; 17% of SC's facility funding goes to the light sources. High-Energy Physics (HEP) accounts for about 12% of SC's users at the Tevatron and B-Factory.

Funding of SC programs in FY10 is led by BES at $1.7 billion, followed by HEP at $819 million and Biological and Environmental Research (BER) at $604 million. SC funding goes to research, facility operations, facility construction, and major items of equipment (MIE).

President Obama has reinforced President Bush's desire to double the budgets of research agencies over 10 years with 2006 being the base year. 2006 was an interesting year with a low budget request and an appropriation that was about the same as the request but lower than the previous year's appropriation. Some programs (e.g., HEP) got mauled, and some (e.g., ASCR) were well on the doubling path. In 2010, the request follows the doubling path. The comparison of the SC request versus its appropriation does not include stimulus money.

People do not appreciate the enormity of SC's education program, undertaken to train the next generation of scientists and engineers, although at nowhere near the level of support provided by the National Science Foundation (NSF). In FY09, 4,400 grad students and 2,700 postdocs were supported by SC; about 550 undergraduates interned at DOE laboratories, about 280 K–16 educators trained at DOE laboratories; and about 300,000 K–12 students, 21,000 educators, 3,000 graduate students, and 4,200 undergraduate students participated in DOE laboratory activities. The DOE National Science Bowl attracts about 22,000 high school and middle school students annually. SC initiated the Graduate Fellowship Program for steady-state support of 400 graduate students with ARRA funds and the FY10 request. The Early Career Research Program has 68 slots and more than 3000 applicants.

For FY10, SC requested $4.7 billion and got $4.9 billion. Congress did not approve all the hubs. It was an uneventful year in terms of arguing over budgets. Three hubs were approved out of eight. The Department failed to explain itself early on and confused Congress about the Department's intents and desires. The three successful hubs are fuel from sunlight, energy-efficient building systems design, and modeling and simulation for nuclear fuel cycles and systems.

With the ARRA, SC got 51 projects, totaling $1.6 billion, including National Synchrotron Light Source II (NSLS-II; $150.0 million), the Thomas Jefferson National Accelerator Facility (TJNAF) 12-GeV upgrade ($65 million), science laboratory infrastructure construction ($108.5 million), acceleration of MIE ($171.1 million), NuMI Off-Axis v_e Appearance (NOvA) MIE ($55 million), advanced networking ($86.8 million), ARM Climate Research Facility ($60.0 million), upgrades to SC user facilities - $391.0 million), EMSL ($60.0 million), light source instrumentation/enhancements ($24.0 million), nanoscale science research center instrumentation ($25.0 million), laboratory general plant projects (GPP; $129.6 million), scientific research ($562.1 million), energy frontier research centers ($277.0 million; forward-funded 5 years), energy sciences fellowships and early career awards ($97.5 million; forward-funded 3-5 years), and management and oversight ($8.0 million). No heavy-duty mortgages were created.

The HEP ARRA projects are the Advanced Plasma Acceleration Facility MIE, advanced technology R&D augmentation, Fermi general plant project augmentation, NOvA MIE, research and infrastructure augmentation at universities, and superconducting radio frequency (SCRF), totaling $216.4 million.

The HEP strategic directions for 2010 and beyond include

- Ensuring successful operation and scientific impact of the Tevatron at Fermilab and the Large Hadron Collider (LHC) at CERN [Conseil Européen pour la Recherche Nucléaire (now European Organization for Nuclear Research or Organisation Européenne pour la Recherche Nucléaire)];
- Participating in the planned upgrades of the LHC;
- Conducting a comprehensive R&D plan to lay the foundations for a future TeV-scale lepton collider;
- Continuing a world-leading program in neutrino physics;

- Planning for increased proton beam power at Fermilab, a long baseline neutrino experiment, and a program of rare decays; and
- Continuing a targeted program of experiments to investigate dark matter and dark energy.

The Office welcomes a lot of advice on these directions. It is impressed with the progress on the wake-field accelerator at Lawrence Berkeley National Laboratory (LBNL) and hopes that this leads to a new class of accelerators. It is important to look at and evaluate each of the neutrino experiments and to see how each fits into the grand schema.

Marlow asked if accelerator development would continue to progress if there were no high-energy research. Brinkman replied that he did not know. John Galayda was a high-energy physics theorist, and he has now built the LCLS at Stanford. The wake-field concept is enormously promising. Burchat pointed out that SLAC now has a division that blends HEP and BES personnel, and BES is picking that division up and nurturing it.

Diebold asked Brinkman for his thoughts on the International Linear Collider (ILC). Brinkman replied that the costs ($15 to $20 billion) have pushed it out of current consideration. He would like to see Fermilab look at something novel rather than to pursue the biggest facility.

Bernthal applauded Brinkman's comments on education and noted that the importance ascribed to this topic is cyclical. Brinkman answered that DOE has sometimes gotten hammered over its support of education. Jobs are now center stage in Washington, and education for those jobs is getting a lot of attention.

Sutter asked when the United States will develop an open and public science policy (i.e., approved by Congress and covering many years). Brinkman replied that Congress is going to preserve its right to approve budgets each year. However, its actions have been very appropriate. When they have not supported science, it has been when DOE has fouled up and not justified its requests.

Artuso asked what advanced computing would be conducted beyond 2010. Brinkman responded that ASCR is executing an exascale computing strategy that faces terrific challenges. Semiconductor technology is running out of steam. One used to count on a factor of 2 improvement every 18 months. That rate of improvement is not occurring anymore.

Oddone commented that national need does not seem to include discovery science. That type of assignation weakens discovery science, and the United States needs discovery science. Brinkman agreed. Discovery science is very important. The biofuels work involves a lot of discovery science, as does the catalysis work. Making SC an applied technology program would be a disaster.

Gelmini noted that Brinkman had made a comment supporting accelerator efforts in the United States as desirable but said that a strong science case needs to be made. She asked whether this means that there is not a strong case now and, if so, what the justification would be. Brinkman answered that DOE and the NSF are trying to push a long-baseline neutrino experiment. They need to present this in the context of the worldwide neutrino program.

Dennis Kovar was asked to provide an update on the activities of DOE Office of High Energy Physics (OHEP). OHEP, with input from the scientific community, has developed a long-range plan that maintains a leadership role for the United States at the three scientific frontiers that define the field. Several projects are under construction (dark energy, and NOvA) and under design. ARRA made a big contribution to these projects, including the LHC, long-baseline experiments, etc. Guidance is being awaited from Astro2010 and CERN on the LHC.

It is hoped that FY09 and FY10 signal a change in the funding trend. Funding is now at Scenario B of the funding schema of the Particle Physics Project Prioritization Panel (P5). In FY09, HEP got a cost-of-living increase. It increased research significantly, and operations [specifically at the Stanford Linear Accelerator Center (SLAC)] decreased. Proton-accelerator-based research is increasing to 50% of the research budget; electron-accelerator-based research is decreasing. About $15 million of ARRA money is going to university enhancement, $52.7 million to superconducting radio frequency (SRF) infrastructure, $20.0 million to advanced technologies, $15.0 million to the Long-Baseline Neutrino Experiment (LBNE), $55.0 million to NOvA, $33.7 million to Advanced Plasma Accelerator facilities, $25.0 million to GPP at Fermilab, and $3.6 million to the Small Business Innovative Research Program. This funding was allocated in such a way as to meet ARRA objectives with no mortgages.

The funding for the Early Career Research Program will be increased to $16 million per year. The FY10 budget has been passed by Congress and is awaiting the President's signature. SC got a 3.2% increase. HEP got a 1.9% increase. These investments have to be made in infrastructure, or there will not be anything for scientists to do down the road. A strong effort is being made to see that research projects are developing on time and on budget. Research funding will increase to maintain the cost of living. The overall research budget is $347.1 million this year. Supporting the research community will be a priority.

At the energy-frontier facilities, there is a strong case for running the Tevatron through FY11. CERN has a working group (WG) on possible geographic and scientific enlargement of CERN. The United States (DOE and NSF) provided input to the CERN WG deliberations on September 3, 2009. The United States proposes that its relations with CERN remain basically the same as now, a project stakeholder/CERN-observer (not a CERN member state) and will participate in the LHC program until the end of the U.S.–CERN memorandum of understanding in 2017. This participation includes detector/accelerator "replacement" or "modest upgrades" (Phase-I LHC upgrades). In the future, the United States will decide what its role might be for an LHC major upgrade (sLHC or Phase II). In regard to a next-generation tera-electron-volt facility, an international "ILC decision" awaits results from LHC and commitments of interested participants. This decision had been envisioned to happen about FY12, but most now believe it will happen later. HEP plans to support ILC R&D through FY12 and has asked the U.S. ILC Team to articulate the options and needed funding for beyond FY12. The United States needs to remain involved in the ILC, and it needs to plan how to go forward.

The HEPAP-envisioned "world-class" intensity frontier program entails an evolution of the Fermilab program, and the envisioned "world-class" intensity frontier program entails the development of an underground detector. These goals are ambitious and will take significant combined (DOE, NSF, and other country) resources. NSF is proposing a Deep Underground Science and Engineering Laboratory (DUSEL) with a suite of experiments that includes a large detector (for neutrino oscillations and proton decay); the

Europeans have a large underground detector in their strategic planning; and the Japanese are also interested in the science. DOE and NSF have had discussions with the Office of Management and Budget (OMB) and OSTP on how to coordinate planning. NSF is supporting the conceptual design of the DUSEL facility and a suite of experiments. DOE HEP is seeking mission need (CD-0) approval for the LBNE that includes the neutrino beam and a large underground detector. DOE and NSF are working to coordinate their efforts, avoid duplication, and optimize their investments. A joint DOE–NSF statement has been submitted by DOE and NSF.

At the electron-accelerator-based physics intensity frontier, BaBAR data need to be analyzed and archived. Disposal of Positron Electron Project II (PEP-II) components await an Italian decision on their proposed SuperB accelerator. Instituto Nazionale di Fisica Nucleare (INFN) has requested that all the PEP II components be provided for this facility, OHEP will need to make a decision in FY10 and is requesting that SLAC do an assessment of options (costs, benefits, etc.) for U.S. involvement before the end of the calendar year.

In nonaccelerator physics, DOE and the National Aeronautics and Space Administration (NASA) have been working on identifying the path forward on a Joint Dark-Energy Mission (JDEM). Two concepts [the International Dark Energy Cosmology Survey (IDECS) and the Observatory for Multi-Epoch Gravitational-lens Astrophysics (OMEGA)] have been presented to Astro2010. The cost of each of these missions is large, and current budget projections show that large-class missions may not be possible. NASA and DOE have agreed to examine a "probe class" mission capped at $650 million. The Project Offices at Goddard Space Flight Center and LBNL are being asked to develop these concepts. Directors of GSFC and LBNL have committed to facilitating these efforts.

A group of scientists needs to be assembled to make sure that the agencies are going in the right direction. Guidance is being sought from HEPAP and from Astro2010 for an optimum dark-energy strategy.

In advanced technologies, historically, the United States has been a leader in the development of advanced accelerators. The developments have been largely driven by HEP's program in the quest for higher energies and intensities and more-demanding beam properties. That leadership is now being challenged by facilities elsewhere. There appears to be recognition by governments of the importance of accelerator competency and infrastructure. OHEP has begun to address this technology gap. In FY07, it started to nurture the development of critical accelerator capabilities in the United States and it is participating in the international ILC R&D effort. Significant ARRA funding is being directed toward accelerator R&D and, in particular, industrialization. OHEP is sponsoring a symposium during the week following this meeting to make a more direct connection between fundamental accelerator technology and applications and to obtain guidance on the needs of federal programs and the private sector.

The DOE Early Career Research Program will be a new funding opportunity for early career researchers in universities and DOE national laboratories. It will make 5-year awards of approximately $500,000 per year for national-laboratory researchers and $150,000 per year for university researchers. Competitive peer-reviewed proposals were due September 1. There are 150 proposals for 12 awards. Peer reviewers are needed.

In the Office, the detailees and Intergovernmental Personnel Act staff (IPAs) are essential. The Office is trying to hire permanent personnel. Applications for two program-manger positions and one engineer/physicist close soon.

Shochet asked whether the research program's growing with inflation assumes a constant workforce. Kovar replied, yes. The LHC program cannot grow. It is dominating the Office's programs now. The Office will make sure that the maintenance of LHC has the needed number of people. Programmatically, research funds will be put into the areas HEPAP has identified as important. Shochet asked whether Astro2010 was explicitly asked for a balanced ground-space dark energy program. Kovar responded that it had been asked for and that that request had been reiterated just the week before this meeting. Their guidance on optimizing the program is considered critical.

Burchat asked what motivated the $350,000 per year difference between university and national-laboratory personnel in the Early Career Research Program. Crawford answered that, for university grants, graduate students and summer salary for a professor are supported; for national laboratories, full-year salary (including overhead) is supported. This differential may be adjusted in the future. White asked if the numbers of universities and of national laboratories were considered when calculating the levels of support. Crawford replied that there were 150 proposals: 100 from universities and 50 from national laboratories. The money will not be distributed 50-50. There will be more university grants than national-laboratory grants. There is a funding target, but it will have to be seen what the quality of the received proposals is.

Bean asked what the criterion was for deciding when there are enough people in proton-accelerator-based research. Kovar replied that a review of the proton-accelerator-based national-laboratory programs has been conducted. University grants will be evaluated in the future. There have been a lot of requests for new money for additional people. That expenditure may not be justified.

Leemans asked what was hoped to result from the following week's symposium. Kovar responded, (1) a report that points out accelerators' broad impacts on national programs and (2) opportunities for huge impacts from accelerator R&D breakthroughs. A lot of money is going toward the ILC. Another pot of money is going toward long-term basic R&D. This workshop may identify areas that are not currently being supported or that are not being supported enough to meet the needs of SC and to facilitate technology transfer to the private sector.

Artuso asked how the DUSEL decision process is related to other projects in the world. Kovar said that the science reach depends on what value of θ_{13} is found. Today, one cannot tell what is going to come out of the science.

Sphicas asked what the plan was for the physicists who will be displaced by the shutdown of the Tevatron. Kovar answered that there will be pressures to shift funding to other areas (e.g., dark energy). There will likely be funding at the LHC for upgrades. There are plans for funding accelerator and detector upgrades; but frankly, a case will have to be made for the science to be obtained and the U.S. role There are no guarantees for funding.

Marlow stated that it would seem like the amount of money for the Early Career Research Program should be set, and the chips should be allowed to fall where they may. Crawford responded that this is an SC program (not an OHEP program), and a distribution across SC is being made. OHEP has 12 of the 65 Early Career slots.

Sutter stated that one of the most difficult areas is the transition from an advanced concept to something useful, the so-called midterm R&D. There, one has to do something large to show that one can engineer the concept so it can be used in a facility. He asked if that was going to be part of the stewardship program or part of the project. Kovar replied that, if one has to build a prototype, there should be a stewardship project coming out of it. There continues to be a gap between basic science R&D and a prototype. It is hoped that this upcoming workshop helps bridge that gap.

Dixon stated that the energy frontier is important because of the access to direct information, so a balance should be maintained between the energy and intensity frontiers.

Kim said that, last year, national-laboratory scientists could not apply for Outstanding Junior Investigator awards. She agreed with DOE that the Early Career Research Program should continue to be balanced.

A break was declared at 10:41 a.m. The meeting was called back into session at 11:03 a.m., and **Joseph Dehmer** was asked to give an update on the activities of the NSF.

Keith Dienes is the new program manager for Theoretical Physics.

The NSF budget has gone up 15% during the past 2 years. For FY09, FY10, and FY11, the budget tracks a doubling curve. The Research and Related Activities (R&RA) account increased 10.6% in the FY10 request. The Major Research Equipment and Facilities Construction (MREFC) account decreased 22.8% because of the completion of some construction projects. MREFC is about 5% of the NSF budget. The budget of the Directorate for Mathematical and Physical Sciences (MPS) increased 9.9%, and may be lower (about 7%) after the House and Senate marks. Within MPS, Physics increases 7.9%. These are good numbers.

In 2002–2003, there was a National Research Council (NRC) report (*Connecting Quarks with the Cosmos*) that came up with 11 big questions and that said that NSF and DOE needed to work together on these questions. A study conducted by the two agencies made six recommendations, all of which have since been addressed; the Physics Division funds projects to address each of these recommendations. The decisions to pursue these projects are made from the bottom up through a very robust process.

DUSEL is being envisioned as a unique, dedicated international underground education & research center that would support a set of potentially transformational experiments in multiple disciplines. The U.S. particle, nuclear, and astrophysics communities have selected DUSEL as central to their national programs. At the same time, the engineering, geology, and biology communities are proactively engaged and are part of all aspects of DUSEL planning.

Many of the cosmic investigations have moved to Japan and Europe. Perhaps now is the time for more of that work to be done in the United States. The shutting down of the Homestake Mine in 2000 triggered the physics community's interest in pursuing that research. Solicitations and workshops led to a list of experiments, experiment designs, and site selection. A draft preliminary design report (PDR) was adopted in 2009. A final PDR may be reached in late 2010.

The P5 report repositioned the HEP community. Two major statements were:

- "The Panel recommends a world-class neutrino program as a core component of the U.S. program, with the long-term vision of a large detector in the proposed DUSEL laboratory and a high-intensity neutrino source at Fermilab."

- “The panel endorses the importance of a deep underground laboratory to particle physics and urges NSF to make this facility a reality as rapidly as possible. Furthermore the panel recommends that DOE and NSF work together to realize the experimental particle physics program at DUSEL.”

About the same time, the Nuclear Science Advisory Committee (NSAC) of DOE and NSF issued a Long-Range Plan that recommended “a targeted program of experiments to investigate neutrino properties and fundamental symmetries. These experiments aim to discover the nature of the neutrino, yet-unseen violations of time-reversal symmetry, and other key ingredients of the New Standard Model of fundamental interactions. Construction of a Deep Underground Science and Engineering Laboratory is vital to U.S. leadership in core aspects of this initiative.”

Four solicitations looked at ten sites and 25 proposals for initial experiments. The Homestake site was selected, and four experimental awards were recently made.

The engine that is driving this effort includes:

- NSF/DOE’s agreement to establish the DUSEL Physics Joint Oversight Group (JOG) immediately after the release of the P5 report (May 2008);
- Representation from NSF/PHY, HEP, and DOE’s Office of Nuclear Physics (NP);
- A building on the successful NSF and DOE collaboration on the LHC;
- Joint coordination and oversight of the DUSEL experimental physics program;
- Quarterly meetings; and
- The agencies’ consulting, and participating as observers, on all reviews of DUSEL and related experiments.

Some good indicators of progress are a joint statement of intent and the transmittal of a letter of intent (LOI) to the Office of Management and Budget (OMB). These documents provide a baseline plan to guide further efforts. The large detector cavity has been sited, and a method for excavating it has been devised.

A well-expressed process has been developed at NSF for considering large (>$100,000) projects. DUSEL is in the conceptual design report (CDR) stage of the process, which translates into DOE’s CD-1 period.

For DUSEL, the Homestake site selection was made only about 2 years ago. Enormous progress has been made on all fronts. The community is now developing a preliminary design, which will provide the basis for DUSEL’s consideration as an NSF MREFC construction project. The current goal is a baseline design that lays out the community’s vision for DUSEL. The first NSF annual review of the DUSEL design project was held at University of California at Berkeley (UCB) in September 2009. An award of $29 million (in addition to the $15 million already awarded) will go to UCB for DUSEL design.

The National Science Board (NSB) states that an award should be made to UCB for preliminary design of DUSEL for an amount not to exceed $29,092,000 for 24 months. The Board is to receive a status report twice a year from NSF on the preliminary design management during the lifetime of the award. The first report is expected at the February 2010 Board meeting. DUSEL will be included in the NSF large-facilities portfolio review at the May 2010 NSB meeting. Board approval will be requested by the Director for any DUSEL planning and design awards subsequent to this award. Furthermore, the Board directs NSF management to undertake a broad, independent review of DUSEL to establish its priority so that it can inform the May 2011 portfolio review. This review will be the first time that the science and extrascience considerations will be weighed at the same time.

DOE is setting up DUSEL programs at the national laboratories. NSF is setting up its program offices, also.

Seven proposals from engineering and geo/geo-bio were selected for funding: fracture processes, coupled processes, subsurface imaging and sensing, fiber-optic strain monitoring, CO_2 sequestration, ecohydrology, and deep drilling. NSF remains committed to a rich, diverse, and multidisciplinary DUSEL research program.

The aggressive target timeline includes NSF Project Review 1 in January 2009, NSF Project Review 2 in February 2010, NSF Preliminary Design Review (PDR) Project baseline in December 2010, and presentation of the DUSEL MREFC proposal to the NSB in Spring 2011. All of these target an October 2012 construction start.

Now that preliminary design work is under way, NSF and DOE want to talk with international partners. NSF is interested in establishing DUSEL as a facility of intrinsically international character. NSF and DOE will be actively pursuing international partnerships and welcome collaborative discussions with colleagues at any time. The mounting of experiments by foreign sponsors is envisioned as an inherent component of the DUSEL program.

In September 2008, there was a meeting of the AStroParticle ERA-NET (ASPERA) at which Carlo Rubbia said "I have been for decades one of the most strenuous supporters of the LHC. However, I believe that we cannot predict where and if the next major discoveries/surprises may come from. Ultimately, the LHC and the other experiments are fighting together, as did David and Goliath. The discovery of SUSY [supersymmetry] may be a real 'bonanza' for the present (and future) colliders, but its relation to the now-credible dark matter is by no means obvious or granted. Likewise, the neutrino sector may reserve for us incredible new discoveries. Proton decay will never be observable with accelerators. Gravitational waves are about to be discovered in the laboratory and in space. Events from the sky and underground have an immense role to play in the future. Now that LHC is on the verge of operation, European physics and CERN have the obligation of concentrating some of the efforts and funding also on a broader range of other activities in the framework of a wider collaborative effort with the rest of the world." The scientific community is now in a position to execute this advice.

Sobel asked if there were a vision for transitioning stewardship from the South Dakota Science and Technology Administration (SDSTA) to NSF. Dehmer replied, no. There is a problem looming, and there is no clear vision of how to address it. South Dakota is paying $10 million per year to keep the pumps running. The preliminary design had to be completed before this problem could be addressed.

Kayser asked if there would be three cavities available from the beginning. Dehmer responded that one cavern takes 3 years to excavate. The rate limiter is the removal of the crushed rock. As a result, the three cavities will be built sequentially. Experiments can be run in the first cavity while the others are being constructed. It will be a challenge to produce clean-room capability 1 mile underground.

Kayser asked what the study would do. Dehmer answered that it would give an up-to-date analysis of the value of the proposed program. Scrutiny is always good. It is hoped to get the study under way soon so it is not rushed in its deliberations. It will produce an important piece of analysis.

Barry Barish was asked to give an update on the ILC.

Today, a muon collider is not realistic. The field of high-energy physics has been driven by the energy frontier for years.

The total ILC program is $100 million per year out of a global $2.5 billion per year HEP program. The underlying technology, SRF acceleration, is developing rapidly and is a strong candidate for next-generation particle accelerators [the X-Ray Free Electron Laser (XFEL), ILC, Project X, etc.]. Also, there are broad applications beyond particle physics. SCRF accounts for about half the total ILC R&D program.

The worldwide outlook for 2012, when the LHC may have produced enough information to plan an ILC, is focused on technical enabling R&D. We have limited flexibility to manage the needed ILC design and engineering development. We do not control these resources, but the resources are well-matched between ILC technical and institutional priorities with some exceptions: positron-system beam demonstrations and conventional facilities optimization and site development.

Two years ago, a design was created with two linacs that puts the business part in a site the size of Fermilab (plus the linacs). Costing such a project is complicated by different personnel and funding measures in each country or region involved. A "value" costing system is being used that provides basic, agreed-to "value" costs and an estimate of "explicit" labor (man-hr). We then try to base a call for a world-wide tender for the lowest reasonable price for the required quality. The items were then divided up into separate estimates for each sample site, a global capability estimate, and regional high-tech (cavities, cryomodules, etc.) estimates.

That process produced a design with a total value estimate of $6.62 billion US 2007 plus 24 million person-hours' explicit labor (about $1.4 billion U.S.). It was reviewed by two international committees. It totals $8.07 billion U.S. 2007. For the United States to be a partner in an ILC hosted elsewhere, it would cost about $1.3 billion. About half the project's cost would be borne by the host.

There is no official U.S. cost for hosting the ILC. The factors are

- Some contingency needs to be added item by item (conservatively about 20%, increasing the total cost from $8 to $10 billion).
- The costs need to be escalated to "then-year dollars." Escalating for 15 or 20 years would be increase the costs by about 100%.
- U.S. costs will only be a fraction of total project costs whether the project is offshore or onshore.
- Thinking in "then-year dollars" in the far future can be quite misleading because inflation of wages, gross domestic product, etc. will scale differently from country to country.

The major goals of the technical design phase are laid out in the ILC R&D Plan for the Technical Design Phase, which is updated every 6 months. Specific goals are

- Conduct high-gradient SCRF R&D to demonstrate the gradient by 2010 with a 50% yield,
- Preview new results from the Freie-Elektronen-LASer in Hamburg (FLASH),
- Demonstrate the fast-kicker performance and final focus design at the Accelerator Test Facility 2 (ATF-2) at KEK [the High Energy Accelerator Research Organization in Ibaraki, Japan],

- Conduct electron-cloud tests at the Cornell Electron Storage Ring Test Accelerator (CesrTA) to establish mitigation and to verify that one damping ring is sufficient,
- Study possible cost-reduction designs and strategies for consideration in a re-baselining in 2010.

In the SCRF cavities, it is desired to achieve a high gradient (35MV/m), develop multiple vendors, and make them cost-effective with a focus on the high gradient, production yields, cryogenic losses, radiation, and system performance. To see what the different variables are doing, we are trying to standardize the steps. We are still missing a systems test of a cavity string with beam acceleration, which is needed for industrialization to be undertaken.

A TTF/FLASH 9-mA experiment has come close to simulating the output needed to run a beam.

The Technical Design Program runs in parallel with the R&D. A new baseline will be set next spring to achieve a TDR by 2012. An updated cost estimate will be produced in 2012, which will need a margin against possible increased component costs. The reference design was reviewed, and errors and design issues were identified, the design was iterated and refined, and more critical attention was focused on difficult issues. Nine areas were picked for cost containment. One was a single-tunnel solution for the main linacs and ring-to-main-linac, with two possible variants for the high-level radio frequency. Safety and reliability questions were raised about the single-tunnel design. These questions are now being looked into. The parameter space of the facility was also examined. About $1 billion might be saved in the $8 billion cost by design changes.

The other option being looked at by an international group is a two-beam accelerator. It could go to higher energies with a normal-conducting accelerator. A demonstration is being constructed at CERN. Nonaccelerator portions of this design overlap with the ILC R&D, so they are being conducted jointly. A 3-TeV machine has huge risks. It is not even known how to make it. Risk, detectors, and timescales are all trade-offs. A recent management meeting at CERN reviewed collaborative status and looked at possible areas for additional cooperation. Conclusions from that meeting include that the existing working groups were deemed a success and two more were added (damping rings and positron production). Jean Pierre Delahaye has joined the ILC Global Design Effort (GDE) Executive Committee, and Brian Foster has joined the Compact Linear Collider (CLIC) steering committee.

The other thing being done is the implementation plan, which includes internal governance, project structure, finance models, globally distributed mass production, and in-kind contributions. Studies have been started in each of these areas.

We are also looking at when and how to lay out the case. For after 2012, global plans are being developed, the main elements of which are continuing SCRF R&D, especially systems tests and industrialization; selective design efforts (e.g., positrons); siting; etc.

In summary, the GDE will be well-positioned to propose a robust, well-developed project on a 2012-2013 timescale. The earliest start for construction is about 2015, assuming that the science case, funding, siting, etc. are in place. CERN has stated its intent (or desire) to host a linear collider (either ILC or CLIC). This eventuality must be considered seriously because of CERN's stable funding. Its earliest start would be about 2018. Japan or the United States are possible alternatives for hosting, with Russia an outside possibility. Japan is actively working toward the possibility (with a strong support group, siting efforts, industrialization, etc.). Hosting efforts in the United States are presently dormant, but the option remains viable in that key U.S. technical infrastructures, etc. are being developed.

Shochet asked how the team had recovered personnel from the budget disaster. Barish answered that the team lost 40% of its funding (from the United States and the United Kingdom). In the United Kingdom, only 25% of the people have been recovered, but they are the ones most wanted. In the United States, about 100 or 125 out of 200 have been recovered. Some duplication internationally has been eliminated, but there are still some holes in staffing.

Sphicas asked why the single tunnel is a safety issue. Barish replied that one needs an egress every so often. Egress is an issue in most countries. Historically, facilities have gotten exceptions to these rules. All the European safety requirements are being looked at.

Trischuk noted that, with 50% for the host and 15-20% for non-host partners, more than two of the latter would be needed. Barish responded that the governance studies have moved away from the regional model and toward a country model. The assumption is that more than two countries would be partners.

Marlow noted that a safety issue (that is, not being able to have people in the tunnel when the machine is running) ties safety to reliability and asked how close they were to having a highly reliable machine. Barish answered that current accelerators operate 80 to 90% of the time. If one increases the number of components, that would bring operability to about 10%. So, one has to do much better than is currently being done. Therefore, it is being investigated whether the mean time between failures for each component can be reduced. That study will continue for another couple of months.

Sphicas asked if there were a scenario for higher-energy operation. Barish answered that costs and risks are the questions. There are technical issues that keep one from going to higher-energy accelerators. Even now, the question is open whether one can build detectors.

A break for lunch was declared at 12:53 p.m.

Thursday, October 22, 2009
Afternoon Session

The meeting was called back into session at 2:09 p.m., and **Young-Kee Kim** was asked to report on the Tevatron collider program.

The Tevatron had its first collision in 1985. There is a lot of support to run it through 2011. Run 11 (2002–2009) had excellent performance. The machine has been reliably running since after the shutdown in 2007.

Since 2002, the antiproton usage has increased from 2.3 to 16.9 nb^{-1}/mA, and the store planned termination has increased from 30% to 88%. The delivered luminosity is being maximized by continuing to make small improvements with short payback times, optimizing running conditions to take advantage of improvements, striving to increase overall machine reliability, and conducting no long-term shutdown. It is expected that the total integrated luminosity will reach 12 fb^{-1} by FY11.

The detectors are operating well with high efficiency. Operations have been streamlined, and many operational and monitoring tasks have been automated. There are no effects expected on the physics program from the condition of the silicon detectors. Some DØ Layer 1 sensors will reach their bias-voltage limit before 12 fb^{-1}, but they will be covered by Layer 0 and outer layers. In the Collider Detector at Fermilab (CDF), the bulk of the ladders will be fully depleted through 12 fb^{-1}, but signal-to-noise projections indicate that no tracking degradation should be expected.

Computing for analysis and collaborations will continue for 5 years after the Tevatron turnoff.

CDF has 602 users from 15 countries and 62 institutions; D0 has 507 users from 18 countries and 90 institutions. The effort required to run the experiments used 107 full-time equivalents (FTEs) for D0 in 2008 and will use 100 in 2011. The 95 FTEs for CDF in 2009 is 30% fewer than years ago.

On the basis of feedback from institutions, the number of FTEs available for CDF in 2009 is 292; in 2010 it will be 249; in 2011, 191, and in 2012, 141. In 2008, D0 had 307 FTEs; in 2011, it will have 170. In FY11, there will be 75 FTEs to run the experiments for CDF and 117 to conduct the physics. For D0 in 2011, the numbers are 100 to run the experiments and 70 to conduct the physics.

In the past few years, collaborations have decreased less than 10% per year, which is much smaller than expected (about 30%). Principal investigators (PIs) have moved their groups to LHC more slowly, many postdocs who thought they would work 1 to 2 years at the Tevatron and then go to the LHC did not move, assistant professors have stayed longer, and new graduate students have joined. Out of 35, about 12 have worked on LHC experiments before joining D0. This migration is attributed to the success of Tevatron program, the impact young people can make at the Tevatron, the leadership they can take on, and the LHC delay.

The Tevatron physics program includes mixing, Cabibbo-Kobayashi-Maskawa (CKM) constraints, and charge-parity violation; heavy flavor spectroscopy; new heavy baryon states; tests of quantum chromodynamics (QCD) and heavy flavor production; top-quark and W-boson masses; top quark properties; diboson production and Standard Model (SM) gauge coupling; reaching the standard model Higgs sensitivity; new exclusive/diffractive processes; searches for supersymmetry, extra dimensions, other exotica; and probing the terascale as luminosity increases. These projects address questions of fundamental importance.

The Tevatron has produced stable tools and an excellent understanding of the detectors and the data. Productivity is higher than ever. Nearly 200 new results have been produced between summer 2008 and summer 2009. Tevatron results dominate high-energy-physics conferences. Still, in some areas, the surface is only being scratched; there is much potential for further precision, reach, and observation. The data are being exploited from all angles. There have been about 100 journal articles published this year. About 60 PhDs have been produced during the past few years, about 350 conference presentations are being made each year, and about 3500 physicists have participated in CDF and D0 experiments.

Rare Standard Model processes (e.g., diboson production) have been observed. More luminosity allows access to smaller cross-sections. New heavy baryons were observed in

2006. These new particles yield a few events per fb^{-1}, leading to new areas of research at tens of fb^{-1}.

The charge-parity-violation (CPV) phase in the B_s system is being measured, and other constraints are being added from other recent measurements. The Tevatron is producing the most precise measurements of the W boson and top quark ever made; it will take a long time to get better measurements. This increase in precision leads to knowledge about Higgs constraints. Now the mass of the Higgs is known to be less than 157 GeV. With 10 fb^{-1}, the mass of the Higgs will be known to be less than 117 GeV. The Standard Model Higgs (if it exists) is being produced *now* at the Tevatron. There is enough energy. It is just not produced that often; and when it is produced, it is buried in backgrounds. It is a story of luminosity, passion, persistence, and luck. How to look for it is known; and the search is, in fact, closing in. Over the past years, there has been a dramatic infusion of people, effort, and ideas aimed at finding the Higgs. With some luck and continued improvement in analytical techniques, the mass of the Higgs will be known with 2 sigma probability with the FY11 run.

So far, there are up to 70 analyses for CDF and D0. Progress on the Higgs search is being made through analysis improvements and greater integrated luminosity. Observations have been running largely within one sigma of expected values. Some mass and tan β range has been excluded in supersymmetric models.

A charge-conjugation, parity, and time-reversal (CPT) test is going on via a free quark and antiquark mass difference. A search is under way for dark photons.

In conclusion, the Tevatron program continues to be remarkably successful. The accelerators, detectors, triggers, and computing are expected to operate well through 2011. The accelerators are producing nearly double the integrated luminosity, and full support for analysis computing capability is being provided. It is estimated that the collaborations will make available 170 to 190 FTEs with 100 (or fewer) required to run each experiment, and there are enough physicists to do a wide spectrum of analyses. Young scientists are staying on, and new students join the effort.

The program will try to extract as much information from the data as possible. The program will be revisited to prioritize analysis, determine what should go on with increasing luminosity, determine what should wrap up, and provide easy access to tools. Physics with the FY11 run include doubling the dataset, conducting unique physics, analyzing legacy measurements, extending the discovery potential, and investigating Higgs possibilities.

Marlow asked about the target date for accumulating 12 fb^{-1}. Kim replied that the target date is the end of FY11. Marlow asked if the degradation of the detectors has been considered in the integrated luminosity. Kim answered, yes. There was a study that showed that there would be no significant effect on integrated luminosity; in addition, there will be other improvements in the process and in analytical procedures.

Dixon asked if the backgrounds are being modeled the same way in the Standard Model Higgs search results. Kim responded, yes.

Kim announced that, in November, there will be two workshops on physics with a high-intensity proton beam and on muon-collider physics.

Kevin Lesko (UCB) was asked to report on DUSEL.

We are having to develop an enduring international underground laboratory with a best-in-world class scientific program of research, education, and outreach and to do it as quickly and cost-efficiently as possible. The goal is to look at a broad spectrum of science: in neutrinos, to discover new physics; in dark matter, to identify about 25% of the universe; to probe the limits of life in extremes, in isolation, and in new lifeforms; to study matter and antimatter's symmetries and asymmetries, the universe at extreme energies, and the physics of the early universe; to understand, probe, and predict the occurrence of natural resources; to learn how to engineer safer, deeper, and larger structures; to welcome, attract, excite, and engage students through education and outreach; and to answer imperative societal questions.

DUSEL is proposed as an MREFC project with a line-item, multiyear construction project for the facility and an integrated suite of experiments. The site is the former Homestake Gold Mine in the Black Hills of South Dakota. Facility planning and experiment integration is supported with a cooperative agreement between the NSF and UCB. The site is currently being prepared and risks mitigated by the South Dakota Science and Technology Authority (creating the Sanford Lab) with state and private funds.

There are three principal players in this project: (1) The DUSEL project team is developing the facility design, integrating the experiments with the facility, overseeing the proposals and the construction project, and ultimately operating the facility. It currently has Solicitation 3 (S3) and PDR awards totaling $47 million through calendar year 2010. (2) SDSTA is operating the site preparation and Sanford Laboratory with $126 million in state-controlled funds. (3) Experimental collaborations are enunciating scientific research goals; initially managing the S4 awards and developing the experiments; performing critical R&D; developing experiment designs, project plans, and hazard assessments; and performing detector construction, installation, and operations. They are being funded at $21 million over 3 years.

The project has an engineering team and a scientific team. SDSTA has a team that is maintaining the mines. It has major financial support from the State of South Dakota but will exhaust its funds about end of 2010. There is a partnership to "achieve DUSEL," which will assimilate Sanford Laboratory at MREFC construction. Facility work has been initiated on the lifts and shafts; pumps; facility stabilization and rehabilitation; initial operations, environment, and safety programs; and an early science program. An agreement in principal has been reached with Barrick Gold Corporation to use a nearby open cut for rubble disposal during excavation. Alternative sites have been identified. The project is currently in the readiness stage and preparing the preliminary design. A generic suite of experimental designs is at the conceptual level or better. A proposal describing the total NSF capital costs is being prepared. And DOE roles and contributions are being discussed by the JOG.

The schedule is synchronized with the scientific goals and resources, with input expected from the science collaborations by April 2010, the DUSEL design package to be assembled with generic experiments during summer 2010, and NSF reviews completed in time for a spring 2011 NSB consideration.

The S4 awardees are the Enriched Xenon Observatory (EXO); the 1-Ton Germanium Detector (GE1T); Multi-ton Argon and Xenon (MAX); the 20-ton LXe (Liquid Xenon) Detector (LZ20); Germanium Observatory for Dark Matter (GEODM); Chicagoland Observatory for Underground Particle Physics (COUPP); Low Energy Solar Neutrino Spectrometer (LENS); Dakota Ion Accelerators for Nuclear Astrophysics (DIANA);

Facility for the Assay and Acquisition of Radiopure Materials (FAARM); Towards a Transparent Earth; Fiber Optics Array; Fault Rupture; Thermal, Hydrological, Mechanical, and Chemical (THMC); Geologic CO_2 Sequestration; Ecohydrological Facies; and Monitoring. Additional proposals are expected in all disciplines. Existing efforts at Sanford are Marjorana Demonstrator, Large Underground Xenon (LUX) detector, SD 2010 (Center), Seismic Arrays, Biosampling, Hydrochemistry, Characterization Efforts, and the Deep Underground Gravity Laboratory (DUGL).

A DUSEL Research Association (DuRA) is now being assembled on the basis of traditional facility user groups and a DUSEL Research Executive Committee and program advisory committees (PACs) for Sanford Laboratory and DUSEL.

The MREFC proposal will consist of a facility and a generic suite of experiments that permits facility design to continue and ground breaking and fixes capital budgets for the suite of experiments and the facility. Following NSB approval, experiments will be reviewed and selected for construction. Approval will follow NSF's peer-review guidelines. Reviews will include significant input from the facility team; DOE involvement in the process is anticipated.

A JOG has been established by DOE and NSF. They have signed an LOI to the effect that DOE and NSF would jointly develop the DUSEL science programs. That LOI has been transmitted to OMB. The JOG is focusing on the Long Baseline Neutrino Experiment, for which the beamlines would be provided by the lead laboratory, Fermilab; the detector would be provided by Brookhaven National Laboratory (BNL); and the staffing would be provided by the NSF-funded S4 collaboration. Project coordination and senior management groups have been established.

Site-assessment contracts have been initiated and are focused on risk reduction. The selection process also considered a firm's capacity to also perform design. Assessment contracts were awarded to RESPEC Consulting and Services for geotechnical engineering services, to Arup for the underground infrastructure, and to HDR CUH2A for the surface campus infrastructure. Contracts are in negotiations for the preliminary design; the scope, schedules, and deliverables are currently being adjusted to match design funding and schedules. Those contracts are with Arup for underground infrastructure, HDR CUH2A for surface campus infrastructure, Golder Associates for excavation design, and Arup for underground laboratory design.

1.5 million cubic yards of rubble will be disposed of 1 km away in an open-cut mine. Surface-facility concepts are being developed.

The 4850 Level developmental baseline for the PDR includes three laboratory modules, one large cavity, and options for two additional future large cavities. Core samples are being taken now for the first large cavity, and the rock is looking very good.

The large-cavity water Cerenkov detector with 50,000 phototubes would have a 53-m inner diameter and a 60-m height. It was clear from the week-long inspection that much progress has been made in developing and initiating a site-investigation plan. On the basis of the site inspections, the Large Cavity Advisory Board (LCAB) is confident that the first 100-kt cavern, with a right-cylinder configuration, can be constructed safely and economically at the 4850 level. Laboratory modules are being planned for the 4850 and 7400 levels.

The Davis cavity will be available from 2013 on. Laboratory modules 1 to 3 will be available in about 2016 or 2017; and laboratory module 4 will be available about 2018.

Challenges and opportunities include the preparation and integration of the DUSEL science programs with the facility designs, continuing site preparation and risk mitigation, long-lead items (e.g., the Ross Shaft upgrade and rehabilitation), and DOE and NSF cooperation and scope definition.

In summary, the DUSEL facility design and experiment integration are advancing and are on track for a preliminary design by the end of 2010. An exceptional, multidisciplinary science program is being developed and integrated into the MREFC proposal. And effective initial cooperation has been demonstrated between NSF and DOE.

Shochet asked if the time required for the National Academy of Sciences (NAS) study were accounted for. Lesko replied, yes. There should be no interference with the schedule because of that study. Shochet noted that the generic suite of experiments might include liquid argon or water detectors and asked how they were going to deal with that. Lesko responded that that is not a new thrust, and overcapacity has been designed into the ventilation and other systems.

Kayser noted that the NSB might want to know about real, not generic, experiments. Dehmer replied that there was a long lead time on the facility. The NSF told the MREFC and NSB that a generic suite of experiments would be proposed with a cost cap. Therefore, there will be cost controls. Also, the large scope makes allocation between agencies complicated. The devil is in the details, but both review boards accepted that. NSF has committed to optimizing the science achieved globally. Only those that need the new facility will get to use the new facility. The NAS study's central intent is to be independent and broad. The real issue is how valuable is this multidisciplinary activity from a scientific perspective. The NAS's study will build on other studies conducted by HEPAP et al.

A break was declared at 3:35 p.m. The meeting was called back into session at 4 p.m.

Daniel Marlow was asked to present a report from the HEPAP Informal Working Group on University Issues.

In February 2009, the chair of HEPAP appointed a group of members to focus on the HEP University program and to bring to the attention of HEPAP and the agencies systemic problems in the program. This group is not a subpanel responding to a charge. It is simply a group of HEPAP members looking at basic demographic information to monitor diversity trends, providing a response to the HEPAP subpanel report on the University grants program, and assessing the status of the technical infrastructure at universities.

A letter was written to the agencies, asking for their blessing and cooperation. The Group thanked the agencies for providing the demographic information, providing a response to the subpanel report, and finding time to meet with the Group.

The DOE university demographics have been flat for faculty, graduate students, postdocs, administrators, research scientists, and engineers for the past 3 years. DOE funding for various categories is also relatively flat except where facility operations were shifted to research at Fermilab and SLAC.

NSF demographics are relatively stable. NSF funding has largely been increasing slightly except for the physics frontier centers and major research initiatives.

The agencies provided written responses with some important qualifiers (e.g., OHEP no longer has a university program to address University issues).

In its report, the HEPAP University Grants Program Subpanel made 16 recommendations. This Group polled the agencies to see what responses had been made to the Subpanel's recommendations. The recommendations and the relevant responses were:

1. The University Grants Program must be strengthened in order to achieve the goals of the national high energy physics program, as articulated by Elementary Particle Physics 2010 (EPP2010). This strengthening requires increased investment and careful attention to building and sustaining levels of personnel and infrastructure necessary for successful university research groups.
   DOE: Our funding in FY09 indicates that university groups have done a good job of proposing research that meets the needs of the program, since the funding level for these groups is in excess of cost of living since the FY07 report. In addition, in FY09 it was determined that a strong case had been made for needed infrastructure for university performers and $10.7 million of the $15 million of ARRA stimulus funding was provided to nearly 100 proposals. These funds will support a variety of equipment purchases (for example, accelerator hardware and magnet and vacuum equipment) as well as enhance computer capabilities, purchase lab equipment, collaborative tools, and other hardware.
   NSF: The Physics Division is committed to funding "PI" proposals (as opposed to Facilities or Projects such as LHC Operations) at a level that is more than 50% of the division budget.
2. Group sizes should be sustained and increased where appropriate and supported by peer review. The agencies should make a special effort to support long-term research scientists as an integral part of this group structure, particularly when they provide expertise essential to the experimental program or leadership at a remote laboratory.
   DOE: We do not in general favor open-ended support for long-term research scientists on grant funding (as opposed to, say, support for graduate students or postdocs), but we do consider each individual case on its merits. Overall, university research group sizes increased from FY07 to FY09 (although the numbers are not final yet).
   NSF: no specific response
3. Funding directed at university-based theoretical particle physics for the purpose of increasing the number of HEP-grant-supported graduate students should be given a higher priority in the overall HEP program. Support for students and postdocs doing calculations related to upcoming experiments is particularly urgent.
   DOE: Funding for theoretical physics at universities from FY07 to FY09 increased by more than 10%.
   NSF: no specific response.
4. University-based technical development should be funded at a level commensurate with its great importance. The investment should be adequate to provide the necessary equipment, technical and engineering support, and infrastructure.
   DOE: In FY09, ARRA funding of $10.7 million was provided to fund proposals submitted in response to a request for proposals to enhance university infrastructure.

NSF: no specific response

5. The University Grants Program should fund the development and mounting of small and mid-scale university-based experiments that are highly rated by peer review and, where appropriate, by the Scientific Assessment Groups (SAGs) and P5. This may require supplements to the University Grants Program..
DOE: It is in the arena of nonaccelerator-based experiments that we find experiments with a much larger fraction of university participation. Experiments proposed by university groups go through the same SAGs as lab experiments. Funding is sent to the appropriate site, that is, to a university if it's leading the experiment.
NSF: The NSF pointed to its 2007 report, *Enhancing Support of Transformative Research at the National Science Foundation*.
6. A University Grants Program Committee (UGPC) should be formed to consult with University Grants Program agency managers on the issues facing the University Grants Program. The chair of this committee should be chosen cooperatively by both agencies and by the chairs of HEPAP and of the American Physical Society Division of Particles and Fields (DPF), Division of Astrophysics (DAP), and Division of Particle Beams (DPB). This chair should serve as a spokesperson for the university community.
DOE: Done: Sarah Eno, Chair.
NSF: We are not clear on how this would work with these many organizations. Our initial thought is that this Committee would advise HEPAP and that agency representatives would be available for consultations.
7. The SAGs should regularize their role in reviewing projects. Each SAG should actively monitor and prioritize the experiments and R&D in its area. It should evaluate both physics goals and technical design. The SAGs should report to P5, timing their reports so that they are available to P5 when needed. The SAGs should review all experiments with expected construction costs above $5 million, along with smaller ones seeking review. This includes both experiments that are affiliated with a U.S. laboratory and those that are not. Additional SAGs should be created as needed to cover all areas (taking care to avoid proliferation).
DOE and NSF: The agencies discussed this and decided it was not beneficial to establish standing SAGs.
8. HEPAP should establish mechanisms for prioritizing experiments whose cost is above $5 million but below the P5 threshold. The prioritization process should take advantage of input from the SAGs and should reflect the breadth of the field.
No specific response from either agency
9. We applaud the Committee of Visitors (COV) process and endorse its continuation. Among the issues that future COVs should address are: Mechanisms for the consistent review of lab- and university-based researchers. The competitive review of proposals, through panels or other means, within the University Grants Program. The workload of University Grants Program staff. Implementation of a DOE database comparable to the one used by NSF that makes institutional, funding, demographic, and programmatic information readily available

DOE: We will have our next COV review in 2010 and we will endeavor to address these issues.
NSF: We note that the COV praised the EPP program and calls out our many alliances with other programs at NSF. The major co-funding alliances are with Education/Human Resources (EHR), Office of Cyber-infrastructure (OCI), and the Office of International Science and Engineering (OISE). Quarknet, Open Science Grid (OSG), and the Partnership in International Research and Education (PIRE) are examples.

10. As much as possible, universities should be funded through merit-based peer-reviewed proposals, rather than through specific project-based funds.
    DOE: All DOE-sponsored research at universities is funded on the basis of merit-based peer-reviewed proposals. DOE projects do not fund research activities at universities. DOE projects can fund project-specific deliverables at a university in the context of a work package subject to DOE project management regulations.
    NSF: For ILC awards we are trying "collaborative proposals" in which awards are reviewed and funded in the same way as base grants, but have significant project discipline. Goldberg noted that the NSF has some paths for peer review, and it follows those paths.
11. The agencies should support university technical infrastructure as part of grants, including hardware development. In addition, project managers should utilize university resources because they are economical and effective, and they should report on this optimization at major project reviews
    DOE: DOE supports technical infrastructure as part of a detector R&D program at universities. In addition, ARRA funding is providing a significant boost to university infrastructure through the procurement of scientific equipment. While it is the job of project managers to utilize resources economically and effectively and to report periodically on their progress, it is not their job to support university (or lab) technical infrastructure as an institutional steward.
    NSF: There is the NSF-wide MRI program, we hope to establish a Physics-wide infrastructure program when budgets permit, and the rest are funded in merit-reviewed base grants
12. The agencies should continue their efforts to ensure that the vision for LHC computing is realized. This includes working across and within agencies to ensure sufficient network and computing capacity.
    DOE and NSF: DOE and NSF provide substantial resources for LHC software and computing, both at national labs and universities. DOE-HEP has partnered with NSF (EPP) and DOE-ASCR in providing support for the Open Science Grid. In addition, DOE-HEP supports the main LHC-dedicated Trans-Atlantic link and has recently funded 46 LHC Tier-3 centers at A Toroidal LHC ApparatuS (ATLAS) and Compact Muon Spectrometer (CMS) universities. The U.S. ATLAS and U.S. CMS collaborations have demonstrated, through data challenges and peer reviews, that they have sufficient networking and computing capacity for a 2009 start in data taking.
13. The agencies should support efforts to ensure that both U.S. sites and key sites abroad are equipped with remote videoconferencing systems that are reliable, robust, and readily available.

DOE: While DOE is not in a position to guarantee the robustness, reliability, and availability of any videoconferencing system, we are providing support for videoconferencing equipment and associated infrastructure through the ARRA process.

14. The agencies should support the increased travel and subsistence costs of university researchers participating in the LHC and other overseas experiments.
    DOE: Ever since the US LHC effort started to have a significant presence at CERN (approximately 2004), DOE-HEP has provided a special supplement for travel to CERN for US universities. In FY09, DOE-HEP provided $2.5 million in supplementary funding for ATLAS and CMS travel at universities, over and above the travel allocations in existing grants. Similar travel supplements were provided for university physicists participating in the Tokai-to-Kamioka (T2K) experiment in Japan.
    DOE: All of these are done as judged from peer review and program-manager assessment. Artuso mentioned that many universities do not have access to these resources. Kovar replied that peers take into account the capabilities and resources of the proposing universities. Shochet added that, over the years, the method of funding of technical infrastructure changed to the projectization of the funding. An engineer cannot work on a project for 2 years, go away for a year, and then return for a year. Kovar noted that national-laboratory programs face the same problem.
    NSF: The NSF funded a competitive ATLAS and CMS student support program and extra travel and travel supplements for LHC, SuperB, Beijing Spectrometer III (BES III), and Astroparticle Experiments in France and Italy. It funded Research Experiences for Undergraduates (REU) students to work on experiments in Europe and Russia (with OISE).

15/16. The agencies should foster outreach by, for example, funding new positions dedicated to facilitating and coordinating university outreach efforts. Additional support should be made available to enable undergraduates and high school teachers to participate in experiments offshore. In addition, support should be continued for an NSF REU program at CERN, following discussion of its structure with representatives of interested university groups.
    DOE: no specific response
    NSF: Noted that Kathleen McCloud was appointed Program Director for Education and Interdisciplinary Research (EIR), and pointed to its July 2007 policy statement, Merit Review Broader Impacts Criterion: Representative Activities (www.nsf.gov/pubs/gpg/broaderimpacts.pdf); its Grant Proposal Guide (www.nsf.gov/pubs/policydocs/pappguide/nsf09_1/gpg_index.jsp); and the description of its Education and Interdisciplinary Research Program (www.nsf.gov/funding/pgm_summ.jsp?pims_id=5610&org=PHY&from=home).

The group decided to conduct a survey to study the state of university infrastructure. One question asked what the respondent would do with a grant increment of $150,000. Respondents predominantly believed that postdocs and new graduate students were important. In experimental work, many institutions could take on more students if funding were available. About half of the institutions would use additional money for some type of technical infrastructure. Travel is also important.

Question 2 asked if software jockeys or hardware workers were being turned out. 24% said all students worked on hardware, and 20% said that fewer than 35% of students worked on hardware. These data indicate a high degree of variability in the direct hardware experience students receive, influenced by the project type and stage and the institution's emphases and resources. This spread of hardware experience will likely affect future scientific endeavors.

Question 3 was about the use of technicians at a national laboratory. The technical resources at the laboratories are seen by the university community to be a valuable resource, but many expressed concern about their ability to gain access to this resource. It is expensive, and the costs are difficult to reconcile with the declining facilities at the universities. The hardware-development opportunities at universities are unique and should be preserved.

Question 4 asked if opportunities had to be passed up because of the lack of infrastructure. Several responses commented on the fairness of the project-management strategy. Most groups commented that they lack infrastructure, which adversely affected their ability to participate in hardware projects. Some expressed concern about the funding process and the complexity of modern experiments.

In summary, while a number of universities are finding creative ways to maintain technical capacities and training capabilities, others are struggling because of the complexity of modern experiments, decreased support from their home institutions, and decreased support from the agencies. This situation will affect the ability to build the next generation of experiments.

Kayser noted that although the focus of the survey was infrastructure, the responses focused on the need for additional postdocs and graduate students. Goldberg noted that, about 12 years ago, a similar question was asked, and the same answer came back.

Oddone commented that, in the old days, an experiment could be supported by one or two engineers at a university. That is no longer true. Adding an engineer does not do that much good anymore. A university can afford only limited infrastructure and little support. Few universities are doing small experiments.

Diebold said that the response to such a question depends on the infrastructure that the university has to begin with. Many will not have any, and an engineer would not help them.

Oddone added that universities make alliances to work at national laboratories to get access to HEP infrastructure. This works in the East Coast, West Coast, and Midwest, but not in Alabama. That problem has not been solved. There needs to be a mechanism to put infrastructure at different universities. Marlow replied that respondents were skeptical that they could get good technical support at a national laboratory.

Shochet said that it would be good to get students involved in the early design work.

Kovar acceded that a large fraction of universities do not see the national laboratories as places where they can work. Also, project managers put together the team for a project without any peer review. Shochet added that they look at the cheapest opportunity available, not the educational opportunities afforded. It varies from project to project. Artuso said that universities have a lot to offer. Infrastructure is more than an engineer. Infrastructure could be put at universities. Goldberg replied that the NSF had looked at setting up infrastructure at universities to supplement what was available with those centers being available to other universities. That strategy never worked.

Marlow pointed out that there is a tradition of students spending extended periods of time at national laboratories, so the national laboratories have a leg up. Burchat said that 48% of the respondents said that they had worked at a national laboratory and are interested in working at a national laboratory in the future.

Dehmer said that this survey effort is a worthy exercise but that it has not achieved focus. One must consider the fact that program managers look at peer-reviewed proposals. What has come out so far is failure to achieve aspirations. You should ask, "is there advice to give to optimize the portfolio beyond its current capabilities?" If all the money went into students, it would not be productive. The agencies need to know *how to change the system*. Compare alternatives; do not just ask how to spend an extra $150,000. Marlow admitted that the responses are not very directive. White said that, if one limits oneself to just getting more students, one limits oneself in the breadth of skills those students will need to be able to contribute to society.

Kim asked what the group was to do next. Shochet said that these issues will be raised, the problems will be identified, and how to solve those problems will be figured out. Kovar commented that it is a legitimate role for HEPAP to put together an agenda for agency consideration. The communities are the ones to address these findings. They can submit proposals to request and deploy additional resources.

Dehmer said that he would not expect a quick fix or analysis. The real purpose was to give voice to a core part of the community. But it will take time to raise the tough questions. The agencies want to improve how they do business.

Cristinel Diaconu was asked to discuss the preservation of high-energy-physics data.

The Hadron-Electron Ring Accelerator (HERA) ended collisions in 2007, and there will be no follow-up before at least two decades. The B-factories have shut down, and the next generation will not appear until around 2017. The Tevatron is shutting down in 2011, and the majority of the physics program will be taken over at LHC; however, its p-pbar is unique, and no follow-up is foreseen. What is the fate of the data collected at these facilities?

The community was polled about the importance of data preservation. The majority said it was very important or crucial. However, many said there was no way to do it. Often, the best data are collected in the program when funding is declining and people are emigrating. Dedicated resources need to be planned.

An international study group was formed, covering collider experiments, computing centers, funding agencies, and about 50 contact persons. It started in September 2008, held several remote meetings, held two workshops, all with the objective of preparing a draft report and making the report available for debate in the HEP community. That report has been submitted to the International Committee of Future Accelerators (ICFA).

The physics case is that collected data sets are mostly unique and have a true scientific potential for (1) long-term completion and extension of the physics program; (2) cross collaborations; (3) data re-use; and (4) scientific training, education, and outreach.

A significant number of publications are issued after the termination of programs in collaborations. Preserved data would make possible more combined analyses across experiments. Such collaborations already exist. Data can also be re-used to improve precision on former measurements, apply new and improved theoretical predictions, check new physics in the old data samples, and investigate discrepancies. History may repeat itself. 10 to 20% of measurements are dominated by nonexperimental errors that can be improved upon by theory or simulation. Examples are Tevatron/LHC, B and super-B factories, and low energy. Archived data can also be used for scientific training, education, and outreach to improve HEP education and to connect HEP-emerging countries with the HEP science and community.

These data include digital information (event files and databases), software for simulation, reconstruction, and analysis; documentation (papers and manuals); meta-information (news and messages); and expertise (people).

Models of preservation include providing additional documentation, preserving the data, preserving the analysis-level software and data format, and preserving the reconstruction and simulation software and basic-level data.

The technical issues are that computing centers are expected to store the data, which might be 10 PB per experiment, at a total cost that is double current costs. Technology evolution, data migration, and software maintenance are some of the real issues. Interfacing with experiments needs to be defined in terms of procedures, contacts, and resources and in terms of supervision and custodianship of data sets with archival expertise.

The last issue is governance. Preserved data sets should be subject to scientific supervision to ensure authorship; access; channels to outreach and education; endorsement by experiment, laboratory, and funding agencies; and HEP global solutions.

One can envision a transition scenario and resources at the experiment level that include contemporary analysis, long-term analysis, data preservation, archived data, and open access. This arrangement would cost about 1% of the original investment.

An international organization should include a research portal, project review board, publication review board, outreach portal, advisory committee, steering committee, and an international study group with a group chair.

The preliminary recommendations include

- Opening up future scientific opportunities through an urgent and vigorous action to ensure data preservation in HEP.
- A dedicated project in each experiment to assess the technological requirements.
- The introduction of an interface to the experiment know-how and the creation of a data archivist position.
- A synergistic action of all stakeholders: experimental collaborations, laboratories and funding agencies with a clear and internationally coherent policy.
- An International Data Preservation Forum as a reference organization.

A panel of advisers provided very positive feedback, gave strong support for a global approach, suggested documenting more examples for the physics case, suggested more-quantitative preservation models, requested clarification of the physics supervision and the relationship with the open-access philosophy, encouraged the full-preservation model, asked for the time scales associated with the preservation models, and suggested exploring the models used in astrophysics. The document will be made public by the end of the year, including the advisory-committee and ICFA recommendations.

Two workshops will be held. The first, in December 2009, will review the proposals for preservation models, develop more-quantitative estimations, take steps toward a global organization, and include public discussion. The second, in June 2010, will prepare a second report, reviewing concrete programs. Possible funding programs will be followed closely.

The conclusions of the group are that data preservation in high-energy physics is based on a relevant physics case, is timely, has the potential to provide research at low cost, provides image enhancement for high-energy physics, and should be endorsed by the funding agencies and foreseen in the experiments' budgets. It also concluded that international cooperation is the best way to proceed. There is a unique opportunity to build a coherent structure for the future.

Shochet pointed out that there is often a problem with inertia and asked what the reaction was of leaders of projects that do not have archival processes. Diaconu replied that they felt that the issue had to be addressed and that this was the time to do it.

Wood said that the magnitude of data produced by D0 is much greater than what currently exists. Archiving the data would be a tremendous challenge, and no plans have been set to archive those data. Shochet noted that, if they wait until funding goes away, it is too late. Kim added that there is a consensus that, in the long run, something like this has to be done.

Gelmini asked for how long the data should be retained. Diaconu replied that that has to be assessed in terms of technology turnover and other factors.

Marlow said that, if one could have open access, the chances of use would be increased. The documentation of the large programs is poor, and software use is learned only because of a cadre of technical-support personnel. A standardization of the documentation might help for when that technical support is not available. Diaconu said that that was absolutely right. Often, though, the people are not gone, just reassigned. Marlowe asked if there were any case studies that evaluated the necessity for different levels of preservation. Diaconu said that there were not any such case studies, yet; however, it is planned to do that.

The meeting was adjourned for the day at 5:54 p.m.

Friday, October 23, 2009
Morning Session

The meeting was called to order at 9:00 a.m. **Judith Jackson** was asked to speak about high-energy physics communications, which are characterized by specific goals, audiences, messages, and tactics (purposes). Such communication has a different function than education and outreach.

Particle physics is all about collaboration. In Hamburg in 2001, it was decided to do particle physics communications the same way one does particle physics. The effort was called InterAction. Its mission is "to support the international science of particle physics and to set visible footprints for peaceful collaboration across all borders."

The *Quantum Universe* model has the communicators and physicists on the same team from the beginning. In that model, communicators are integrated into the process *ab initio* rather than as an add-on at the end of the process. The goal is to produce a strong and healthy US research program at the frontiers of 21st-century particle physics. P5's identification of the three frontiers of particle physics was a great help in pursuing this goal.

The messages place new emphasis on applications of particle physics (like the accelerator-based vulcanization of radial tires). The messages reflect international collaboration (D0 involves speakers of 89 languages).

Fermilab produces the news magazine *Symmetry*. It is now being published as a blog, which allows cost savings, instant distribution, and rapid feedback. *Symmetry* distributes about 20,000 print copies per month. Its web site, which includes the feature Symmetry Breaking, gets about 150,000 unique visits per month. *Symmetry* has about 1500 Facebook friends and 750 followers on Twitter. About 15% of its web traffic is produced by Facebook and Twitter.

Other InterAction activities have been fielded:

- Fermilab publishes *International Science Grid: This Week on the Web*, funded by DOE and NSF.
- NOVA will air Brian Greene hosting "Fabric of the Cosmos."
- OHEP paid for the startup of the U.S. LHC Communication website.
- A lecture series was produced on the real science behind the "Angels and Demons" motion picture. This initiative worked out well. The public lecture nights were an excellent example of collaboration; universities got in the spotlight; it was an opportunity to reach thousands.
- InterAction's website is a major tool for particle-physics communication. Its news service, InterAction Newswire, has about 2350 subscribers to which it distributes press releases.
- "Quantum Diaries," which presents personal insights on particle physics, gets about 500 visits per day and is growing.
- Fermilab employees sent Gran Sasso children art supplies after the earthquake there. The children made a mural for the Fermilab cafeteria.
- Other efforts include rap videos on the LHC and particle physics that have gone viral on the web. The LHC rap has been viewed more than 5 million times and was featured on National Public Radio.

One InterAction initiative is peer review to strengthen communication at laboratories and in global particle physics. It uses combined InterAction communication experience and expertise to analyze and improve communication at member organizations. There is a global team of InterAction members and others operating in a Lehman-review format that results in findings, comments, recommendations, and a closeout report to management. TRIUMF [originally, the TRI-University Meson Facility] requested the first peer review. The reviews cover publications, community relations, electronic communications, media relations, internal communications, and education and outreach.

The Fermilab communications team is available to serve the particle-physics community.

Pripstein asked if the communications team got together with science writers to see how its messages get across. Jackson replied, yes. There are close relationships with other science writers, but they are not systematic. Traditional journalism is still there but is going away. The blogosphere is an unknown quantity at this point. Communications is changing rapidly.

Artuso asked how one distinguishes between fundamental science and applied technology. Jackson answered that one has to figure out what one's audience is interested in. One has to be ready with a variety of messages to deal with everything that might be asked.

Leemans asked if the subtitle of *Symmetry, The Particle Physics Magazine*, should be broadened. Jackson agreed that that could be considered. Particle physics is a big tent that should include a muon collider. Slicing it up into beam physics, accelerator physics, etc. is only important to physicists, not to the broader audience, generally.

Paul De Luca, who was scheduled to speak on high-energy-physics applications in medicine did not appear.

Steven Ritz was asked to report on the HEPAP Particle Astrophysics Scientific Assessment Group (PASAG).

Together with the energy frontier and the intensity frontier, the cosmic frontier is an essential element of the U.S. HEP program. Scientific efforts at the cosmic frontier provide unique opportunities to discover physics beyond the Standard Model and directly address fundamental physics. The primary areas covered by PASAG are dark matter, dark energy, cosmic particles (high-energy cosmic rays, gamma rays, and neutrinos), and the cosmic microwave background (CMB). It did not cover all areas of nonaccelerator physics. Topics not addressed include low-energy neutrinos, low-energy cosmic rays, nucleon decay, tests of gravity and gravitational waves. This is a dynamic field. The PASAG's report is based on a snapshot of where the field stands right now.

Projects at the cosmic frontier naturally exist at the boundary between particle physics and astrophysics. Some projects are obviously very close to the core of particle physics; other projects straddle the boundaries between fields and, in some cases, would not happen without significant HEP participation or leadership. These projects are designed to answer very important scientific questions and, in many cases, have the potential to uncover new directions for particle physics. Our prioritization criteria for HEP investment account for these issues. How to put these straddlers into the overall framework was struggled with, and it is also confusing to the funding agencies. PASAG developed some prioritization criteria. It is necessary to understand in sufficient detail the related astrophysical phenomena. The astrophysics investment is sometimes necessary to realize the particle physics benefit. The relationship is symbiotic: particle physicists have much to offer these important related fields of study and often have a major impact on them.

The criteria developed are: Is the science necessary? To what extent does the project address fundamental physics (matter, energy, space, time)? Are the anticipated results at least one compelling result or a preponderance of solid, important results? Anticipated results should not be marginal, either in statistics or in systematic uncertainties, relative to the needed precision for clear science results. Does the effort offer a large leap in key capabilities, significant new discovery space, and the possibility of important surprises? Is particle physicists' participation necessary to provide the transformative techniques and know-how without which the project would not happen. Here, leadership is a higher priority than participation. The scale matters, particularly for projects at the boundary between particle physics and astrophysics. Relatively small projects with high science per dollar help ensure scientific breadth while maintaining program focus on the highest priorities. A program should be broad *and* balanced. There are programmatic issues, such as cooperation vs. duplication/competition at the international scale.

There are other efforts going on, such as the NRC Astro2010 "Decadal Survey" of activities in astronomy and astrophysics, jointly funded by NASA, NSF, and DOE. This multiplicity of inquiries is not a problem. Even where there is overlap, the different studies are informing each other.

Four budget scenarios were given to PASAG in the charge. Scenario A postulates a constant effort at the FY08 funding level (i.e., funding in FY10 at the level provided by the FY08 Omnibus Bill, inflated by 3.5% per year and continuing at this rate in the out-years). Scenario B postulates a constant effort at the FY09 President's Request level (i.e., funding in FY10 at the level provided by the FY09 Request, inflated by 3.5% and continuing at this rate in the out-years). Scenario C postulates a doubling of funding over a 10-year period starting in FY09 (i.e., funding in FY10 at the level provided by the FY09 President's Request, inflated by 6.5% and continuing at this rate in the out-years). And Scenario D postulates additional funding above Scenario C associated with specific activities needed to mount a leadership program that addresses the scientific opportunities identified in the EPP2010 or P5 reports.

PASAG also had to look at the resources available. To calculate the phased resources available for construction and operation of new projects, the committed funding for existing projects and ongoing science analysis (the "base," estimated with some simplifying assumptions) was subtracted for each year. For the entire FY10–FY20 period of this study, the total (in then-year dollars) available for new projects was $266 million, $389 million, and $640 million for scenarios A, B, and C, respectively.

There are a lot of science opportunities out there. In dark matter direct detection, there are next-generation (G2) facilities capable of reaching sensitivity levels better than 10^{-46} cm^2 (about a factor of 400 better than present-day limits and a factor of about 10 better than expected for the experiments already under construction). These facilities have typical target masses of approximately 1 ton, with a construction and operation cost in the range of $15 to $20 million. Third-generation (G3) experiments surpass 10^{-47} cm^2. Target masses would be many tons with a construction and operation cost around $50 million. For dark energy, several Stage-IV projects have been proposed, including the space-based JDEM and the ground-based Large Synoptic Survey Telescope (LSST), which are large, and the medium-scale ground-based Big Baryon Oscillation Spectroscopic Survey (BigBOSS) Project. In the area of cosmic particles, the Auger North has been proposed to provide a factor of 7 increase over the existing capabilities of Auger South in statistics about the highest-energy cosmic rays. To understand features in the cosmic ray spectrum at lower energy, the Telescope Array Low Energy extension (TALE) has been proposed.

Three projects in very-high-energy gamma rays have been proposed: The large-scale Advanced Gamma-Ray Imaging System (AGIS), a joint effort with the European-led Cherenkov Telescope Array (CTA) project, would provide at least an order of magnitude improvement in sensitivity and new capabilities. The High-Altitude Water Cherenkov HAWC observatory, a different kind of ground-based very-high-energy gamma-ray detector at much smaller scale, would provide a factor of 15 improvement in sensitivity over its predecessor, Milagro. And a small proposal would upgrade the existing Very Energetic Radiation Imaging Telescope Array System (VERITAS) detector. A relatively small level of support has also been proposed for Fermilab participation in the QUIET II (the second Q/U Imaging Experiment) CMB experiment.

The PASAG subfields have different histories and issues. For dark matter, the 2007 Dark Matter Scientific Assessment Group (DMSAG) report provided a detailed survey of experiments designed for direct detection of dark matter along with a roadmap for future investments. To obtain the information needed to update the DMSAG report, PASAG issued a request for written information from the experiments. For cosmic particles, there has not been a devoted scientific assessment group, so PASAG issued a request for written information that was similar to the one for dark matter and invited the major projects in this area to make presentations. For CMB, which is primarily funded by agencies other than those HEPAP advises but for which small investments by HEP have had a large and visible impact, PASAG was specifically asked to comment on QUIET II, which also made a presentation. To make this assessment, PASAG also reviewed the overall importance of the science of the CMB to particle physics.

For dark energy, there have been several panels, including the 2005 Dark Energy Task Force (DETF) and the 2007 Beyond Einstein Program Assessment Committee (BEPAC), that have evaluated dark energy goals and a subset of the proposed projects. The two large projects that would have HEP funding (LSST and JDEM) have been extensively reviewed. A moderate-scale project, BigBOSS, is very new, so PASAG heard a presentation from that project. A coherent overall strategy, optimizing observations both from the ground and space, taking into account the priorities of both the astronomy and physics communities, has been lacking. PASAG is not constituted to perform this integration. However, because dark energy is a very high scientific priority, PASAG sought to define the scope of dark energy within the broader particle astrophysics program. The detailed allocation to projects in the different budget scenarios awaits a coherent plan. The Astro2010 Survey, which is ongoing, will presumably play a key role in this planning. As input, issues of importance to HEP for participation in dark energy projects are provided in Section 6 of the final PASAG report.

Under Scenario A, for dark matter, the PASAG sought to maintain the current world-leading program, but world leadership would be lost toward the end of the decade. Two G2 experiments and the 100-kg SuperCDMS–SNOLAB [Super Cryogenic Dark Matter Search –Sudbury Neutrino Observatory Laboratory] experiment are supported. The technology selection for the G2 experiments should occur soon enough to allow the construction of at least one G2 experiment to start as early as FY13. No G3 experiments can be started in this decade. Progress will be slowed, risking loss of U.S. world leadership. However, because of the risk of picking the wrong technology, this loss is preferable to descoping to only one G2 experiment. These dark-matter experiments are getting into very interesting areas of sensitivity, and a breakthrough might occur at anytime. In dark energy, it is not possible to have major HEP hardware and science contributions to any large project. World-leading participation is supported in only very limited areas. The area of high-energy cosmic particles, is severely curtailed in this scenario to preserve viable programs in dark matter and dark energy; only the VERITAS upgrade and HAWC are possible. Even in this very lean scenario, the diversity offered by these two projects is a priority, and their impacts are large for a relatively small investment. Auger North and AGIS are not possible. This lack of support would be a retreat from U.S. leadership in high-energy cosmic rays and high-energy gamma rays. In CMB, QUIET II is supported, along with possible other small investments in CMB research, provided the prioritization criteria in Section 2 are clearly met.

In Scenario B, for dark matter, the current world-leading program is maintained, but with some risk later in the decade. Two G2 experiments and the 100-kg SuperCDMS-SNOLAB experiment are supported. The technology selection for the G2 experiments should occur soon enough to allow the construction of at least one G2 experiment to start as early as FY13. Only one G3 experiment can start in this decade. Based on what is known at this time, to mitigate the risk of picking the wrong technology, a broad second-generation program is a higher priority than starting a second G3 experiment. For dark energy, Scenario B may provide just enough funding for significant participation in only one large project, but at significant risk because the total costs are still uncertain and the one project probably will not adequately address all the scientific issues. A program with world-leading impact in dark energy is possible, but in a limited way. The overall funding profile requirements are uncertain, but the straight-line budget scenario does not appear to allow sufficient resources for a fast start early in the decade, and some adjustments to the profile would be necessary. For high-energy cosmic particles, the VERITAS upgrade; HAWC; and a reduced, but still leading, AGIS that is fully merged with CTA are the highest priorities in this scenario. Auger North is not possible in Scenario B; this would be a retreat from U.S. leadership in high-energy cosmic rays. CMB is the same in all the scenarios.

Scenario C would produce a world-leading program in dark matter. Two G2 experiments plus the 100-kg SuperCDMS-SNOLAB experiment are supported. The technology selections should occur soon enough to allow construction to start on at least one experiment as early as FY13. Two G3 experiments can start in this decade. In dark energy, a world-leading program is also enabled, with coordinated activities in space and on the ground. Significant HEP roles in one large project are possible, along with a moderate-scale project and/or a substantial role in a second large project. As in Scenario B, the straight-line budget scenario does not appear to provide sufficient resources for a fast start early in the decade. Although the overall funding profile requirements are uncertain, some adjustments to the profile would likely be necessary. In high-energy cosmic particles, a world-leading program is also enabled with the VERITAS upgrade, HAWC, a reduced but still leading role in an AGIS that is fully merged with CTA, and U.S. leadership of Auger North.

In Scenario D, augmenting the program in Scenario C with an additional $200 million investment over the decade would enable major roles in two complementary, Stage-IV dark energy projects, ensuring continued U.S. leadership in this field and providing the best chance of a major breakthrough in dark energy in this decade.

There are some subtle issues. PASAG could only use the numbers provided by the agencies. Scenarios A and B forced extremely difficult choices. In any scenario, if the funding available for a project was judged to be insufficient to support a world-class result, the project was removed. Similarly, in any scenario if only R&D-level funding could be accommodated with insufficient funding for construction, the R&D was also removed from the program. It is therefore important to revisit these choices if sufficient resources outside of HEP become available. In the cases of JDEM, LSST, and AGIS, the Subpanel recommends contributions from HEP agencies that are a portion of the total project costs.

In all three scenarios, projects in dark energy represent the largest total investment, reflecting the very high scientific priority and the fact that large projects are required to make significant progress in this area. Even with that large fractional investment, there are significant challenges and risks, particularly in the leaner scenarios. In Scenario A (with a budget envelope of about $140 million), it is not possible to have major HEP hardware and science contributions to any large dark-energy project. Scenario B (about $200 million) is still very risky because it is near the threshold for significant participation in only one large project, requiring great vigilance and careful consultation with the scientific community. For example, because JDEM is not currently well defined, yet is very expensive, there is at present considerable risk that a large fraction of the total available resources will be spent on a project that does not provide a scientific return that matches HEP priorities while precluding any significant participation in other dark-energy projects that could. In Scenario C (about $350 million), a world-leading dark-energy program is possible with coordinated activities in space and on the ground. In Scenario D (about $540 million), a world-leading program is assured. This scenario would fully fund the DOE hardware and scientific contributions to an optimized combination of Stage-IV dark-energy experiments. This portfolio would likely include a large ground-based imaging survey, a space-based survey with higher angular resolution and infrared capabilities, and a massive spectroscopic galaxy redshift survey, executed through the most cost-effective combination of ground and space approaches. HEP leadership and the resultant dark energy measurements would be strong and secure.

On the topic of balance, in Scenario A, while more resources would be required to have full participation in even one large dark-energy project, PASAG advises not to reduce the dark-matter project investment below a level critical to maintain leadership. As the dark-matter experiments scale up in size, it is important to have at least one frontier sensitivity experiment operating at all times throughout the decade. A discovery could be imminent. Continued support for theoretical research is an essential part of a strong particle astrophysics program.

CMB measurements are important to particle physics as a unique probe of the extremely high-energy processes associated with inflation. Given the central importance of the CMB to our understanding of energy, matter, space, and time and the unique contributions HEP can provide to CMB science, small investments are highly recommended in all budget scenarios if the prioritization criteria are clearly met. Several of the national laboratories and other institutions now have small groups active in this area. Additional investments in CMB projects should be made when the HEP community can provide unique capabilities. Relatively small (up to a few million dollars per year) investments in CMB research would be appropriate.

In the AGIS, the United States has played a leading role in the study of high-energy cosmic particles (cosmic rays, gamma rays, and neutrinos) from space. This field sits at the interface between high-energy physics and astrophysics, enabled by techniques and personnel drawn from both areas. The main goals of the field are to understand the acceleration processes in cosmic sources that produce particles with energies well beyond what can be achieved on Earth and to use these particles to search for physics beyond the Standard Model. AGIS well exemplifies this interdisciplinary nature, having significant capability for indirect detection of dark matter in addition to its main goal of exploring the TeV gamma-ray sky. The novel AGIS design concept has the potential to offer much better instrument performance over the baseline design of the planned CTA. Given the expense, only one large array is likely to be built. To make sense programmatically and technically and to maximize the effect of a U.S. investment, AGIS and CTA should move quickly toward a joint project. AGIS is also under review by the Astro2010 Survey; should it be highly ranked in that study, it would be expected that a significant fraction of the AGIS cost would be borne by the U.S. programs in astronomy and astrophysics.

In regard to Auger North, establishing the high-energy cutoff in the cosmic-ray spectrum was a great achievement of the past decade. This achievement also fundamentally changed the intellectual landscape for the study of the highest energy cosmic rays, removing the need to explain them with new physics, such as exotic, massive particles or topological defects at the grand unified theory (GUT) scale. Now, the scientific focus is on finishing the quest to determine the astrophysical origin of the highest energy cosmic rays. Auger North is shovel-ready, and the world is looking to the United States for leadership. The Astro2010 survey is ongoing: Auger North may be highly ranked in that survey, in which case astronomy and astrophysics agencies will presumably then plan to fund it, and the costs to HEP will be lower. If not, then Auger North can only be substantially supported by HEP in the best funding scenarios.

For DUSEL, given its current status and the uncertainty in the funding that could be made available, PASAG chose not to assume the funding of experiments through DUSEL in the budget planning exercises, even though the U.S. dark-matter program would be greatly strengthened by it. DUSEL is central to the future dark-matter and neutrino experimental programs, both of which require large underground laboratories. DUSEL would provide a unique location with needed infrastructure in the United States. In addition, the funding for dark matter that may be available when DUSEL goes forward would enable key enhancements of the variety, scope, and schedule of the program. PASAG hopes that DUSEL happens.

Exciting times are ahead for particle astrophysics, with many new results emerging from operating projects and even more expected soon from the projects currently under construction. Even in the leanest budget scenarios, the full budgets for the projects that are already under construction or that are currently operating should be maintained. Every operating project should have a well-defined sunset review date and a realistic plan for possible extended operations. Sunset reviews and decisions must carefully consider international and multi-agency perspectives.

In summary, the PASAG priorities are generally aligned with the recommendations for the cosmic frontier in the 2008 P5 report. Dark matter and dark energy remain extremely high priorities. Dark-energy funding, which receives the largest budget portion, should not significantly compromise U.S. leadership in dark matter, where a discovery could be imminent. Dark-energy and dark-matter funding together should not completely zero out other important activities in the particle astrophysics program. The recommended programs under the different scenarios follow the given prioritization criteria.

Specific comments were made on the different areas. In dark matter, to advance the CDMS technology, PASAG recommends a technical review of SuperCDMS in FY10 to evaluate the performance of the new detectors currently in operation at Soudan. Funding for the 100-kg SuperCDMS-SNOLAB experiment should begin as soon as the detectors meet the design requirements. A future xenon program that avoids duplicate efforts and meets the technical requirements for low background should be supported in any of the funding scenarios. For axion detectors, the Axion Dark Matter Experiment (ADMX) has completed Phase-I construction and is operating well. It is estimated that it will take 1 to 2 years to cover 10^{-6} to 10^{-5} eV down to the first of two model benchmark sensitivities, the Kim, Shifman, Vainshtein, and Zakharov (KSVZ) model. Phase II of the experiment will cover the same range down to the lower Dine, Fischler, Srednicki, Zhitnitsky (DFSZ) model. This phase requires a dilution refrigerator to go from 1.7 to 0.2 K. This is a unique experiment, and its continuation through Phase II is supported in all budget scenarios.

In dark energy, the 2008 P5 report recommended support for a staged program, as defined by the 2006 DETF, of dark energy experiments as an integral part of the U.S. particle physics program. PASAG reaffirms this staged approach and recommends funding to complete those Stage-III dark-energy experiments receiving particle astrophysics (PA) support [i.e., the Dark Energy Survey (DES) and the Baryon Oscillation Spectroscopic Survey (BOSS)]. For all budget scenarios, timely pursuit of a Stage-IV program that can obtain another order of magnitude or more improvement beyond Stage III in metrics for dark energy and gravity tests, as specified by the DETF and Figure of Merit Science Working Group, should happen. Formulation of a detailed plan for achieving a comprehensive and optimal dark-energy portfolio under all funding scenarios is needed. Astro2010 is an essential component of this process. The JDEM design process should be coupled to plans for ground-based projects to ensure that JDEM offers the possibility to significantly extend the capabilities of ground-based experiments.

Uncertainties about the technical approach, scope, and organization of JDEM remain. PASAG knows of no actively engaged science panel currently advising the JDEM Project Offices. While the responsibility for the project rests with the project management, it is essential that the observatory design and approach be a close, collaborative effort between dark-energy scientists in the community and the project team to ensure a scientifically successful mission. Support of JDEM as a particle-astrophysics project should imply that the methods and talents of the HEP community are applied to JDEM at its design, instrument-construction, and science-analysis phases.

In terms of other projects, LSST is a well-developed design and collaboration with very strong HEP participation in design, management, and construction plans, as well as in the LSST Collaboration. Continuing support of LSST preparatory work is recommended so that ground possibilities are known for timely planning of a coherent ground–space dark-energy effort. An ambitious ground-based imaging survey over most of the accessible extragalactic sky is an essential element for nearly all approaches in a cohesive ground–space dark-energy strategy. BigBOSS is in early planning and will require a lot of telescope time. Substantial immediate BigBOSS R&D is recommended to assess its capabilities for studying baryon acoustic oscillations.

In conclusion, exciting times are ahead for particle astrophysics. It is well-aligned with the other frontier areas. For example, the same type of dark-matter particles may be produced anew in the LHC, while relic copies are detected both underground at low energy and from outer space at high energy. Each of these sources will provide a needed piece of the puzzle. This is a particularly exciting time of convergence of theory and experiment, particle physics, and astrophysics. It is a strong entrepreneurial spirit providing great discovery potential. Cultural differences between scientific communities are not necessarily impediments but are, rather, reinforcing capabilities enabling important new opportunities.

Scholberg asked Ritz if he could say something about high-energy astrophysics neutrinos. Ritz replied that PASAG would need a proposal to consider. IceCube is an ongoing project, but no specific proposals for future work in this area were found.

Nelson commented that it is not known what funding scenario prevails and asked what would be the timing of these decisions. Ritz responded that that is the job of the agencies. This advice is to ground those decisions. Shochet added that this report provides ammunition for the agencies to increase their funding. Kovar said that the agencies have no idea what the budget is going to be in the out years. One does not start a project until one knows that one can complete it. One has to have a science case to start a project. Once a project is started, Congress generally provides support to complete the project if there are no cost or time overruns.

Dehmer thanked the PASAG and said that it did exactly what was needed. This is an extremely useful result.

Burchat asked how PASAG assigned funding starting points given the blurriness between DOE and NSF. Ritz said that they had sat down with agency finance personnel and subtracted known commitments from expected funding levels. It takes a lot of work to do that.

Burchat asked if they had recommendations on how to go forward on dark energy Ritz replied that it is hoped that Astro2010 will provide the next logical step and opportunity to guide the agencies. Shochet stated that one cannot do everything. One has to optimize the science within the budget envelope. If Astro2010 is not sufficient, the agencies might constitute another group for guidance on how to proceed on dark energy. Burchat asked if Astro2010 were optimizing across funding possibilities? Ritz answered that everything Astro2010 is doing is available on its website.

Leemans said that the United States has a better design in AGIS then the Europeans have in CTA. Ritz noted that PASAG said that some collaboration with CTA would lead to a better result. R&D would be needed to determine what is practical, and collaboration would be important.

Dixon asked if there were a budget number for the CMB project. Ritz pointed out that the agencies make those decisions. It is around a few million dollars.

Nicholson asked about liquid argon. Ritz stated that no technology has a lock on the slot. PASAG wanted to make sure that there is a slot and to let the best technology take it.

Trischuk asked where the two G2 experiments go. Ritz replied that PASAG would not consider sites. Trischuk asked when they would go. Ritz responded that it is hoped that they would start around 2015.

Shochet asked for a vote on submitting this report. The vote was unanimously in favor of submitting the report to the agencies.

Kovar thanked the PASAG members for their very hard work. This group struggled with the tough decisions, and the guidance they offer will be extremely valuable. The criteria are extremely thoughtful and will be very helpful in shaping the program.

A break was declared at 10:57 a.m. The meeting was called back into session at 11:20 a.m.

Shochet listed the points that he will include in his letter to the agencies about this meeting:

- Brinkman's description of SC's successes during the past year.

- Kovar's description of the activities funded: CERN, ILC, NASA-DOE JDEM mission guidance, and future R&D.
- Dehmer's noting NSF's support for DUSEL, that a broad independent study of DUSEL is being called for by the NSB, that a LOI has been sent to OMB, that funds for Homestake maintenance will be needed, and that construction could start in 2013.
- Barish's description of the ILC GDE, which he likened to ITER and which noted that much in SCRF is broadly important in science, that cavities are improving, and that a project-implementation plan has been started.
- Kim's noting that the Tevatron performance has been excellent, that the current run is to go through 2011 with 12 fb^{-1} to be produced along with many publications.
- Lesko's pointing to DUSEL's generic suite of experiments, Joint Oversight Group, Brookhaven National Laboratory detector work, and Fermilab's beamline studies.
- Marlow's description of university infrastructure status, which indicated that most respondents would hire more postdocs and graduate students and that technical resources should be made more available to universities.
- Diaconu's reporting on two data-preservation workshops and a forthcoming report, on four data preservation models considered, on the need for an archivist for each project, and on how the status of technology makes this problem tractable.

Items would be added to reflect the reports received during the current day's session.

The floor was opened to public comment. There being none, the meeting was adjourned at 11:31 a.m.

Respectfully submitted,
F. M. O'Hara, Jr.
HEPAP Recording Secretary
Nov. 16, 2009

Corrected by
Melvyn Shochet
HEPAP Chairman
January 30, 2010

The minutes of the High Energy Physics Advisory Panel meeting held at the Hilton Embassy Row Hotel, Washington, D.C., on Oct. 22–23, 2009, are certified to be an accurate representation of what occurred.

Signed by Melvyn Shochet, Chair of the High Energy Physics Advisory Panel on January 30, 2010.