**Minutes for the**
**High Energy Physics Advisory Panel Meeting**
**May 18-19, 2005**
**Radisson Hotel, Washington, D.C.**

HEPAP members present:

Keith Baker
Joel Butler
Alex Dragt
Frederick Gilman, Chair
JoAnne Hewett
Young-Kee Kim
Peter Meyers (Wednesday only)
Stephen Peggs
Steven Ritz (Wednesday only)

HEPAP member absent:

Paul Langacker

Also participating:

Barry Barish, Director, LIGO Laboratory, California Institute of Technology
Raymond (Chip) Brock, Department of Physics and Astronomy, Michigan State University
Aesook Byon-Wagner, Office of High Energy Physics, Office of Science, Department of Energy
Robert Cahn, Physics Division, Lawrence Berkeley National Laboratory
Glen Crawford, Office of High Energy Physics, Office of Science, Department of Energy
Sally Dawson, Physics Department, Brookhaven National Laboratory
Joseph Dehmer, Director, Division of Physics, National Science Foundation
Robert Diebold, Diebold Consulting
Jonathan Dorfan, Director, Stanford Linear Accelerator Center
Thomas Ferbel, Office of High Energy Physics, Office of Science, Department of Energy
Judith Jackson, Director's Office, Fermi National Accelerator Laboratory
Edward (Rocky) Kolb, Particle Physics Division, Fermi National Accelerator Laboratory
Joe Lykken, Particle Physics Division, Fermi National Accelerator Laboratory
June Matthews, Director, Laboratory for Nuclear Science, Massachusetts Institute of Technology
Marsha Marsden, Office of High Energy Physics, Office of Science, Department of Energy
F. M. O'Hara, Jr., HEPAP Recording Secretary
James Siegrist, Director, Physics Division, Lawrence Berkeley National Laboratory
Michael Salamon, Astronomy and Physics Division, National Aeronautics and Space Administration
Robin Staffin, Associate Director, Office of High Energy Physics, Office of Science, Department of Energy
Michael Turner, Assistant Director, Mathematical and Physical Sciences, National Science Foundation

Rainer Weiss, Department of Physics, Massachusetts Institute of Technology
James Whitmore, Program Director, National Science Foundation
Michael Witherell, Director, Fermi National Accelerator Laboratory
Andreene Witt, Oak Ridge Institute for Science and Education
John Womersley, Office of High Energy Physics, Office of Science, Department of Energy

In the course of the two-day meeting, about 75 others were also present.

**Wednesday, May 18, 2005**

Chairman **Frederick Gilman** called the meeting to order at 9:04 a.m. and welcomed the members. A great deal had happened in a short time since the most recent meeting, and the Panel would correspondingly hear about a number of developments and set the stage for others in the near future. His comments would concentrate for now on two areas: budgets and the International Linear Collider (ILC).

In regard to budgets: At the Panel's most recent meeting, the FY06 budget proposal had just been released. Along with the whole DOE Office of Science (SC), the Office of High Energy Physics (HEP) was down in then-year dollars, and the fallout included the loss of BTeV as a project.

There have been many people from across the science communities who have communicated with those in the Congress and Administration about the implications of the FY06 proposal. He would cite one example, with which the Panel may not be familiar; it is by no means the most significant, just one that he was familiar with. At the beginning of April, five existing chairs of DOE science advisory committees visited Washington. When one combines the effects on each of the research programs in DOE SC and then embeds it in a 5-year plan of further decreases, the whole picture is, in understated language, extremely worrying. The five chairs spent the day visiting with staff on the Hill as well as well as senior people in the Executive Branch, including Jack Marburger and Sam Bodman, the Secretary of Energy. The chairs came away somewhat optimistic that the Congressional appropriations process will add a few percent to the SC budget. The markup for DOE in the full House Appropriations Committee should come during this first day of the HEPAP meeting.

In regard to the ILC: At the time of the Linear Collider Workshop at SLAC, Barry Barish's acceptance of the job of leader of the Global Design Effort (GDE) for the ILC became official. Barry is moving quickly. One can already see his organization starting to take shape and his plans for a conceptual design report and cost estimate. The Panel is to hear from Barry later, and Gilman left it to him to relate the progress that has already been made and much more that is yet to come. Very importantly, there is a tangible sense of momentum for the ILC.

Since the Panel's most recent meeting, EPP2010 sent its expected letter to the HEPAP chair with its questions about the ILC. Sally Dawson will address the Panel at this meeting about EPP2010 developments. The Panel members had all received an electronic copy of Gilman's letter of response to EPP2010, together with the answer to its questions about ILC R&D. The answers to the questions on the physics case and the international aspects will be given to EPP2010 at their August 2005 meeting.

At the beginning of this meeting, HEPAP had six subpanels in various stages. Gilman suspected that before the meeting is finished, it will have more. He would be briefly reporting to the Panel on the next morning about the status of those subpanels.

Gilman asked **Robin Staffin** to review events in SC and to report on program planning. Staffin announced that the agencies will issue two new charges, one for the Particle Physics Project Prioritization Panel (P5), and one to study the national accelerator R&D program. Several subpanels had previously been announced, for example the Neutrino Scientific Assessment Group (NuSAG).

P5 was been in existence for several years. One purpose of this subpanel was to hold the roadmap. What they did was extremely valuable. Therefore, a roadmap with budget scenarios will be requested from that subpanel.

At the present, the United States has a very strong program in HEP. It has invested strongly in facilities, and the field is reaping the benefits of the investments made in the 1990s. In the near term, the Large Hadron Collider (LHC) will produce data and will change the field. The physicists are shifting toward Europe, and the United States wants to respond to that shift to CERN [Conseil Européen pour la Recherche Nucléaire, now Organisation Européenne pour la Recherche Nucléaire].

The ILC is coming together not only in the physics community but also in governments. There is now a GDE, and the statesmanship that is being exercised is laudable. The recommendations of the International Technology Recommendation Committee (ITRC) have gained the acceptance of the world scientific community and ILC funding agencies. Raymond Orbach has said that the ILC is SC's highest priority in HEP, given funding criteria.

Neutrino physics is at an exciting juncture, and HEPAP has a joint advisory group with Nuclear Science (NS).

However, the budget is constrained, so in order to pursue these opportunities, some funding must be redirected. To do that, HEPAP's advice is needed. The question to P5 is, at what time would the investments in HEP R&D in the United States have a greater effect if they were redirected at other efforts? When does one make this transition?

Several opportunities have presented themselves. A graph of the budget showed funding to be very flat. The budget request for ILC research has increased from $20 million to 23 million or 24 million. As the Tevatron research drops off, opportunities for new facilities and initiatives will open up. P5 is being asked to consider four scenarios:

- Scenario A: Run both facilities as long as is currently planned: Tevatron until 2009 and PEP-II [Positron Electron Project] until 2008; assume that this implies very limited funding for any new initiatives and no significant ramp-up in ILC R&D until 2009.
- Scenario B: Stop both as soon as possible (end of FY 2006); assume that all resources would go into new initiatives and ILC R&D ramp-up.
- Scenario C: Curtail PEP-II sooner than planned, while continuing to run Tevatron; resources would go into new initiatives and ILC R&D ramp-up.
- Scenario D: Curtail Tevatron sooner than planned, while continuing to run PEP-II; resources would go into new initiatives and ILC R&D ramp-up.

The scenarios should be considered in the international context; there will be a KEKB (an asymmetic electron-positron collider for B physics at the High Energy Accelerator Research Organization in Ibaraki, Japan) and an ILC. P5 should also assume a constant funding level but should not assume the geographic and programmatic distributions to remain static. P5 should also assume that making funds available through redirection will (1) likely strongly impact the ability to carry out smaller initiatives within the roadmap (e.g., research on neutrinos, dark energy, and dark matter) and (2) likely only weakly impact the start date for ILC construction because that date will largely be determined by other factors. The draft report should be submitted by October 2005, and the final report by December.

The most recent review of the United States' accelerator program was 25 years ago. HEPAP is being asked to conduct a comprehensive review (exclusive of ILC and the LARP [LHC Accelerator Research Program]) of the DOE and NSF accelerator R&D programs. What is desired is a description of the scope, quality, relevance, resources received, management, and adequacy of training of the United States' accelerator R&D.

The charges will be forwarded to HEPAP shortly for these two subpanel studies.

Dehmer noted that two areas are not covered by these charges: dark matter and high-energy cosmic rays. They will be addressed in the future.

Meyers asked about the Scientific Assessment Group on Experimental Non-Accelerator Physics (SAGENAP). Dehmer replied that SAGENAP is no more. Its legality was always questionable in view of the Federal Advisory Committee Act. It is being replaced with several subpanels.

Siegrist noted that HEP's accelerator technologies are important to other SC offices and asked if this fact will be brought home. Staffin said that these reports will not focus on that fact but will focus attention on the broad role of accelerators in science.

Ritz noted that there are conferences that will cover some SAGENAP roles. However, there are some small topics with clever ideas that might not get the attention or support they deserve because of the attention paid to the large projects. A number of these topics were addressed by SAGENAP in the past. Dehmer said that that is a good point. Some of the large projects (e.g., dark matter) are looking at detectors and other small-funding projects, and they recognize the need to support these smaller projects. Staffin added that there will still be mechanisms (e.g., grants and laboratory funding) to support those types of ideas.

Cahn asked if P5 had been reconstituted and whether it has a membership. Staffin replied, no. Cahn said that the task being presented to P5 seems enormous to be carried out so quickly.

Hewett expressed the hope that P5 will consider the international effects of early shutdowns and the need for a clear plan of what will replace any program that is scheduled for shutdown. Staffin noted that P5 would be asked to consider *timing* as well as the *roadmap*.

Gilman asked **Jonathan Dorfan** to review the status of the B Factory at the Stanford Linear Accelerator Center (SLAC). The Linac/PEP-II complex produces an electron beam and a positron beam that are held in two storage rings. During the 2004 downtime, the current (the main driver for luminosity) was increased, beam stability was increased, and the optical performance was improved. An electrical accident in 2004 shut down the accelerators. Safety procedures were reviewed and validated. PEP-II started up again in April 2005. They have experienced good productivity since then with luminosity increasing to two-thirds of the historic peak value. BaBar is now in full logging mode. The turn-on is going very well. The run plan is to go through FY08. Run 5 started during the week of April 15, 2005. The peak luminosity has increased to $6 \times 10^{33}$, pushing toward $9 \times 10^{34}$. A maintenance break will occur in October 2005. Then the machine will run through July 2006. A 3-month downtime will be followed by full-out running to 2008. Improvement activities are scheduled during that final downtime. Peak luminosity will be increased to $2 \times 10^{34}$. At that point, we will have four times the data we now have.

The BaBar collaboration has 11 countries, 80 institutions, and 623 physicists, 50% from outside North America.

BaBar focuses on CP violations, but its capability is much broader: it is characterizing the unitarity triangle by directly measuring the angles of that triangle by measuring phases. He showed the data from a BaBar event: three electrons, two pions, three other pions, and a few gamma rays. A B meson can be seen as the data are unfolded. Five layers of silicon in the detector make it very powerful. If one sees a difference in matter/antimatter, one can measure the charge-parity violation (CPV). In the unitarity triangle, measuring the $\beta$ angle is most common. The angle $\alpha$ is also interesting. The indirect measurements have an error of 16°. BaBar is already improving on that.

The most interesting potential anomaly with respect to the Standard Model includes going through a strange quark, which introduces loops that will present an opportunity for new physics beyond the Standard Model. Loops' interference will change the value of $\sin 2\beta$ from the value observed in the golden modes. These other modes are now starting to be measured. The different measurements of the golden modes agree and establish a benchmark. Most measurements of other modes with strange quarks fall below that benchmark by as much as $4\sigma$. This occurrence may be a harbinger of new physics. We will have to wait until 2006, when the luminosity is increased, to see if those new physics evolve. The period from 2006 to 2008 will be crucial in determining what is going on because that is when the individual modes will be able to be observed and measured. BaBar continues to put more modes on the table and will characterize more modes as the size of the data sets increases with luminosity.

But BaBar is not limited to looking at CP violations. Pentaquarks are a hot topic, so BaBar looked for them. No evidence has been seen, yet.

BaBar and Belle have produced an enormous number of contributions to the literature.

The BaBar dataset will be doubled by summer 2006 and doubled again by the end of 2008. One of the justifications for that doubling is to determine if supersymmetry is observed when individual penguin modes are able to be analyzed in 2008.

PEP-II and BaBar will operate straight through the summer to catch up on the time lost to the electrical accident.

Dragt asked what Belle's luminosity will do. Dorfan replied that, by the end of 2008, BaBar will have an integrated luminosity similar to Belle's. It is important that both B factories do well. KEK's program is facing the same outlook: needing to make way for the ILC.

Diebold asked if luminosity could be doubled again after 2008. Dorfan responded that the final improvements will be made by 2006. They did not see any other tricks that could be exploited beyond that.

Gilman declared a break at 10:41 a.m. He called the meeting back into session at 10:53 a.m., asking **Michael Witherell** to review the research program at the Fermilab accelerators. Fermilab is in the middle of a campaign to optimize the science done throughout this period. The entire laboratory is organized to support the accelerator effort. It is building and installing luminosity upgrade projects from 2004 to 2006. Luminosity is planned to be delivered continuously from 2004 to 2009. An efficient detector operation is being maintained with modest upgrades. The computer capacity is being increased to keep up with the growing data sample. Fermilab is working with the collaborations to do the best physics possible with Run-II data.

The projected integrated luminosity (PIL) has been the same for the past year. A big question is what effects electron cooling has. Fermilab has a good shot at increasing the PIL significantly during the next couple of years.

Run II's integrated luminosity is increasing impressively and is expected to continue to increase. Record performances have been set in the past month. The accelerator operations have been better than ever seen before, and many operations are being run simultaneously.

The experiments Collider Detector at Fermilab (CDF) and D0 are operating well with greater than 90% data-taking efficiency. For CDF, all aspects of analysis are mature, and the publication rate is jumping up. D0 is experiencing a similar surge in the breadth and number of publications, such as in the search for supersymmetry (SUSY).

That is the story on the collider program. Fermilab also has a neutrino program. It is the only laboratory in the world to do neutrino experiments with an accelerator. The Main Injector Oscillation Search (MINOS) is currently

- verifying the dominant $\nu_\mu \rightarrow \nu_\tau$ oscillations;
- precisely measuring the atmospheric $\Delta m_{23}^2$, which is about 10%; and
- searching for $\nu_\mu \rightarrow \nu_e$ oscillations, a $3\sigma$ discovery about a factor of 2 below the Chooz [reactor] limit.

The Neutrinos at the Main Injector (NuMI) construction project is complete, and MINOS is starting to operate for physics. A problem arose on March 23: a leak in the NuMI target cooling system, but it was found to not affect operations. At the present time, running conditions are stable.

MiniBooNE (a detector at the Booster Neutrino Experiment) is designed to follow up on the Liquid Scintillator Neutrino Detector (LSND) evidence of a $\nu_\mu$–$\nu_e$ oscillation at high $\Delta m^2$. If MiniBooNE confirms LSND, it will change the worldwide neutrino program overnight. The beam and experiment are running well. A new horn was installed during shutdown. It reached $5.45 \times 10^{20}$ protons on target, beyond the demanding milestone of $5 \times 10^{20}$. The result on $\nu_e$ appearance will be known by the end of 2005.

Fermilab has granted Stage 1 approval to NOvA, the NuMI Off-Axis $\nu_e$ Appearance Experiment. It provides the most sensitive search for $\nu_\mu \rightarrow \nu_\varepsilon$ at the wavelength of atmospheric oscillations; a good measurement of $\theta_{13}$ if it is not too small; and a unique capability to resolve the mass hierarchy. NOvA is the first stage of a flexible program, where each stage can be planned according to what has been learned in previous stages. Stage 1 approval means that the science is compelling; this is the machine to do it; and this is within the capability of the laboratory.

The past year at Fermilab saw the Run-II integrated luminosity increase from about 0.5 to 1.0 $fb^{-1}$; the record luminosity increase from 0.7 to $1.23 \times 10^{32}$ $cm^{-2}$ $s^{-1}$; Recycler being integrated into routine operations; new records being set for stacking rate using slip-stacking; the installation of the e-cooling apparatus into the Recycler, and a lot of physics results. In neutrinos this past year, the NuMI Project was completed; the NuMI beamline was commissioned; MINOS operation was started; the MiniBooNE total protons on target was increased from $2.6^{20}$ to $5.5^{20}$; and the replacement MiniBooNE horn was installed.

In summary,

- the Fermilab accelerators are simultaneously providing beams for unique particle-physics programs (CDF and DZero at the Tevatron, MINOS in the NuMI neutrino beam, MiniBooNE in the Booster neutrino beam, and the Main Injector Particle Production (MIPP) Experiment with secondary hadron beams);
- the collider is operating at record levels;
- the neutrino beams are operating reliably;
- all experiments are running well;
- exciting physics is coming out and it will get better;
- the Cryogenic Dark Matter Search (CDMS) has the best limits on dark matter by a factor of 10; and
- Auger results will be the highlight of the cosmic ray conference in the August.

Ritz asked what sets the timescale on the MiniBoone results. Witherell replied, a year ago, they were convinced that they had the systematics down and that they would have enough data by the end of FY05.

Gilman asked **Sally Dawson** to review EPP 2010.

The U.S. National Academy of Sciences reviews each field of physics every 10 years. The most recent survey of elementary particle physics was completed in 1998. But since then, much has changed. Dark energy has been discovered, neutrino mass has been discovered, and precision electroweak measurements have been made. A new survey is needed to lay out the grand questions that drive the field, find the opportunities that are ripe for discovery, identify the tools to achieve the scientific goals, articulate the connections to other sciences and to society at large, foster collaborations with scientists around the globe, and recommend a realistic implementation plan.

The EPP 2010 Committee is charged (1) to identify, articulate, and prioritize the scientific questions and opportunities that define elementary particles physics and (2) to recommend a 15-year implementation plan with realistic, ordered priorities to realize these opportunities. Emphasis is to be placed on ranking science priorities. For the Committee, the Linear Collider and support of LHC physics are dominant questions. It also seeks to investigate the roles of neutrino physics, cosmology and astrophysics, and the underground laboratory.

She listed the members of the Committee. This is not your usual committee. There are many nonphysicists included to strengthen the Committee's connections with society at large and to sharpen the physics questions. Non-particle physicists are also on the committee to help engage the scientific communities. The Committee also has international representation to place U.S. particle physics in an international context. The overall goal is to present a compelling vision for EPP and to create an action plan that will allow the achievement of the goals.

The Committee has held two meetings, one in Washington and one at the SLAC. It has meetings scheduled at Fermilab and at Cornell. It is also taking field trips to CERN, DESY [Deutsches Elektronen-Synchrotron], and KEK and seeking information from the Asian Committee for Future Accelerators (ACFA), European Committee for Future

Accelerators (ECFA), and International Committee of Future Accelerators (ICFA). It expects to issue a report by December 2005.

At the Fermilab meeting, the Committee heard about that facility's accelerator-based programs, R&D and the path forward for the ILC, neutrinos at accelerators, cosmology, astrophysics, the national program, and how to fit aspirations into a modest budget. The Committee got an international perspective on how to make national decisions in an international framework, and held a town meeting.

At the Cornell meeting, the Committee heard from the HEPAP Subpanel on LHC and ILC, gained additional international perspectives, and held a town meeting.

The committee is posing questions to the community. The first set is about the Linear Collider. Responses have come in from several groups already, and those responses are extremely useful. The report of the HEPAP Subpanel on LHC and ILC will be important. The second set of questions is about neutrinos, astrophysics, cosmology, etc. The committee invites written comments at epp2010@nas.edu, and all communications will be made public.

The questions about the ILC are

- How does a Linear Collider address the compelling questions of particle physics? Is a Linear Collider clearly the right machine to address these physics objectives?
- What physics does a 500-GeV Linear Collider address? What are the arguments for going to an energy scale of 1 TeV? How would results from the LHC change these arguments?
- What are the physics arguments for operating a Linear Collider during the same time frame as the LHC?
- How would the combination of the LHC and a Linear Collider answer questions that could not be addressed by either machine alone?
- What physics would a Linear Collider address that would be impossible to probe at the LHC?
- How would the physics discoveries from experiments at a Linear Collider be useful to other branches of science?

The questions regarding the R&D plan for the ILC are

- What general R&D is required to arrive at a construction decision, and about how much would it cost? What is the relative difference in R&D costs between a 500-GeV and a 1-TeV Linear Collider?
- What are the characteristic timeframes and constraints for an R&D program that leads to a construction decision?
- What are the greatest technological risks?
- How would decisions about the necessity of different R&D paths be made?
- How could the R&D be useful even if the ILC did not proceed to implementation?
- Is it possible to give a reliable estimate of the overall cost of the project?
- Does the U.S. accelerator science community have the capacity and capability to do the work necessary to make a bid to host a Linear Collider?

The questions related to international planning are

- How would a Linear Collider be managed and operated in the context of an international laboratory?
- How can the U.S. funding mechanisms (with yearly budget decisions) connect with a long-term international project?

- How would cost overruns be handled?
- What is the model for distributing the costs between the host country and other participants?
- What arguments can be made for hosting an ILC in the United States?

Kim asked how many comments had been received from the community. Dawson replied that about 20 sets of comments had been posted on the website. These responses are *very* important.

Butler asked how many of those respondents were in their 20s. Dawson answered, zero. Butler noted that those are the people who will be most affected and should be encouraged to write. He asked if people are excited about the future. Dawson replied that it is too soon to tell.

Ritz asked about her comment concerning neutrino questions. Dawson said that this Committee does not need to consider the prioritization of those questions because the community has already done that. There is a neutrino study to refer to. Ritz suggested circulating the questions to the community and allowing comment. Dawson said that they could do that.

Gilman opened the floor to discussion. He asked for comments about P5 and the other new subpanel. Kim asked if the answers from P5 were to apply to next year. Staffin said that that depends on the scenario recommended by P5. This is an optimization. The existing operations need to be balanced against the R&D needed for future operations. There is no presumed answer in mind.

Butler noted that people have invested funds from international sources. If programs are to be terminated early, one needs to make the international partners aware of that possibility early on. Staffin replied that he understood. The result may be that not enough information exists to justify early termination. This process is one way of giving the community input into the decision making. Butler pointed out that one needs to take into consideration the effects on the good will of the international partners in considering such changes. Staffin said that he communicated such concerns with his counterparts from other countries.

Cahn said that it is extremely important that international collaborators have a chance to make a case for this science before termination decisions are made. Staffin asked if other programs did that. Ritz replied, yes; this information is put into plans and agreements to begin with, and then they undergo senior review processes.

Matthews noted that the Nuclear Science Advisory Committee (NSAC) had been charged with deciding whether to shut down the Thomas Jefferson National Accelerator Laboratory (TJNAL) or Relativistic Heavy Ion Collider (RHIC). She said that this process is very divisive. Staffin said that he was worried about that divisiveness, but if the community is asked to guide such decisions, that would show a level of maturity for that community.

Ritz said that he considered the ways that questions are being posed to be good. One should not be scared to answer hard questions.

Meyers voiced the opinion that this seems like the right thing to do, but the process makes him jumpy because, even when something is going well, it can be swept away. This may be the proper desperate measure for desperate times, but it made him uncomfortable.

Cahn said that the question comes down to whether one wants the community to participate in this decision or does one want HEP to make a decision unaided. P5 is a committee that is well respected and would make the community more comfortable with both the decisions and the process.

Kim said that the question then becomes how to do it to make sure that it is done well.

Butler asked how reality was to be injected into the process. Staffin responded that he intended to get wise people. Other than that, he did not have a well-formed answer.

Gilman steered the discussion to the accelerator-based subpanel and asked if non-U.S. members will be invited. Dehmer said that they do not know, yet. Staffin said that it has been suggested to make an experimentalist the chair.

Matthews suggested that the group could be a HEPAP subpanel but could also be a joint HEPAP-NSAC subpanel. Staffin stated that, if it is need-driven, he would guess that there would be some large differences between accelerators and, say, light sources.

Witherell said that a subpanel to look at all accelerators across the United States would have to come from SC, not HEP.

Peggs stated that a review is welcome, but the integration is of technologies, particularly superconducting technologies. Brookhaven National Laboratory (BNL) needs to be included, therefore. HEP's and NP's accelerator R&D has a different philosophy or accent than that of Basic Energy Sciences (BES). A good balance is needed between university and national-laboratory representation on such a subpanel. It is vital to understand how activities like LARP fit in with the core program. Staffin replied that one needs to see where the synergies are. Everyone is under financial stress. One needs to make sure that such a program continues to benefit the HEP community. Peggs added that, looking at the superconducting technologies, they are vital to the economic well-being of the nation. Staffin asked him if he was suggesting that there be nuclear physicists on the subpanel or that it be a joint subpanel of HEP and the Office of Nuclear Physics (NP). Peggs replied that he believed that it should be a joint subpanel.

Matthews said that one needs to look at future R&D and who is going to perform that R&D. Education of those future technology drivers is important.

Gilman declared a break for lunch at 12:10 p.m. He called the meeting back into session at 1:34 p.m. and introduced **Rocky Kolb** of Fermilab, who participated by telephone, to update the Panel on the activities of the Dark Energy Task Force, created by DOE, NSF, and the National Aeronautics and Space Administration (NASA) and reporting to HEPAP and the Astronomy and Astrophysics Advisory Committee (AAAC). The Task Force has had one meeting and has two scheduled in the next few months. Its report is scheduled to be prepared by the end of 2005.

The charge to the Task Force is to

- Summarize the existing program of funded projects;
- Summarize proposed and emergent approaches;
- Identify important steps, precursors, and R&D;
- Identify areas of dark-energy parameter space that existing or proposed projects fail to address; and
- Prioritize approaches (not projects).

"The DETF [Dark Energy Task Force] is asked to advise the agencies on the optimum near- and intermediate-term programs to investigate dark energy and, in cooperation with agency efforts, to advance the justification, specification and optimization of LST [the Large Survey Telescope] and JDEM [Joint Dark-Energy Mission]." There is a range of interpretations of the charge that the Task Force is trying to resolve.

Before 1998, there were hints on dark energy in large-scale structure, theory, the cosmic microwave background, supernovae, and the Hubble constant. The confirmation state is now passed, and now there is confidence in the acceleration of the universe. Now one can focus on w and w′, how the acceleration started, and how it is evolving. Weak lensing, large-scale structure surveys, evolution of structure, etc. can be used to study w and w′. Many of these projects will allow getting the best use of the LST and JDEM.

The Task Force is focusing on the near term and trying to coordinate the current efforts. One needs complementary techniques to get a sharper result than one would get from just a single technique. The Science Definition Team (SDT) is concentrating on the science requirements for the LST and JDEM. The problem is to figure out what can be done in the near term to minimize the costs of the LST and JDEM in the long term.

The Task Force has issued a call for white papers at http://www.nsf.gov/mps/ast/detf.jsp. The purpose of these white papers is to educate the Task Force so it can give informed recommendations. It needs to know how projects can be put together in a program and the common needs of the various projects in terms of technology and astronomy information.

Baker asked if any timeframes were being recommended for the programs. Kolb responded that the Task Force is not charged to evaluate the individual projects, but it does have to make some assumptions about the timelines. The Task Force is charged to consider emergent, promising approaches, like looking at the baryon wiggles to learn about dark energy.

Dragt asked how one sees baryon wiggles. Kolb responded that one uses large-scale surveys, looking at the visible-light power spectra of galaxies.

Gilman asked **Barry Barish**, who participated by telephone, to talk about the Global Design Effort (GDE) for the ILC.

The ITRC recommended that the Linear Collider be based on superconducting rf technology. This recommendation referred to a technology, not a design. The final design is expected to be developed by a team drawn from combined warm and cold linear-collider communities, taking full advantage of the experience and expertise of both.

What is next? A new global design based on superconducting rf technology will be undertaken by the combined warm and cold experts. The experience from the Stanford Linear Collider (SLC), Final Focus Test Beam (FFTB), Accelerator Test Facility (ATF), and TESLA Test Facility (TTF) need to be fully capitalized on as the project moves forward. The R&D will be coordinated by an International Central Design Team, which the ITRP endorses. The first collaboration meeting will be at KEK in November.

The technology recommendation was presented to the International Linear Collider Steering Committee (ILCSC) and ICFA on August 19 in a joint meeting in Beijing. ICFA unanimously endorsed the ITRP's recommendation on August 20.

The FALC [Funding Agencies for the Linear Collider] met and stated "The Funding Agencies praise the clear choice by ICFA. This recommendation will lead to focusing of the global R&D effort for the linear collider and the Funding Agencies look forward to assisting in this process. The Funding Agencies see this recommendation to use superconducting rf technology as a critical step in moving forward to the design of a linear collider."

FALC is setting up a working group to keep a close liaison with the Global Design Initiative with regard to funding resources. The cooperative agreement of the Funding Agencies on organization, technology choice, and timetable is a very strong signal and encouragement.

The first ILC workshop was held Nov. 13 to 15, 2004, with 220 participants. There was broad geographic representation. They set up five working groups:

WG1: Overall Design
WG2: Main Linac
WG3: Injector, including damping rings
WG4: Beam Delivery Systems, including collimator, final focus, etc.
WG5: Cavity Design for higher gradients, etc.

Each has three convenors, one from each region. This is the functional way the program will be moved forward.

Nine proposals were submitted for sites for the ILC. A search was started for a site after the November meeting. The GDE will likely operate through the completion of the conceptual design report (CDR) and detector CDR (i.e., 2007). Construction may start before 2010, and operation might start 2015. It is important not to design something that is obsolete before its construction is complete. Things that will improve performance and/or lower costs need to be nurtured.

The first task is to staff the GDE. A dispersed-collaboration model will be used for staffing. A small administrative staff is being hired at Fermilab. The GDE will be organized regionally with three regional directors. A job description for regional directors was written and circulated; 19 nominations were reviewed by a committee, which narrowed the list down to five people. Parish and his advisers chose Gerry Dugan from that list. Fumihiko Takasaki (KEK) was chosen as the Asian director. Europe had not yet selected its director. Still to be selected are the engineer/costing engineer, civil engineer, the experts, and missing skills for each region. About 30 to 40 people will be involved, representing about 20 full-time equivalents (FTEs).

The schedule calls for beginning to define the configuration (August 2005); issuing a Baseline Configuration Document by the end of 2005; putting the baseline under configuration control (January 2006); and developing the CDR by the end of 2006. Three volumes will come out of this effort: the CDR, a shorter glossy version for nonexperts and policy makers, and the detector CDR.

The GDE also needs to organize the ILC activities globally and coordinate the international R&D program. The first step will be to appoint Regional Directors within the GDE. They will serve as single points of contact for each region to coordinate the program in that region, bring up a website, coordinate meetings, conduct collaborative R&D, etc. The second step will be to coordinate worldwide R&D efforts in order to demonstrate and improve the performance, reduce the costs, attain the required reliability, etc.

He showed a picture of a generic linear collider with a source, pre-accelerator, damping ring, bunch compressor, main linac, collimator, focusing lens, and extractor. He said, we know what we are designing to. A scope document sets the basic parameters:

- $E_{cm}$ adjustable from 200 to 500 GeV

- Luminosity integrating to 500 $fb^{-1}$ in 4 years
- Ability to scan between 200 and 500 GeV
- Energy stability and precision below 0.1%
- Electron polarization of at least 80%
- Upgradeable to 1 TeV

There is a set of facilities to work from, such as the experimental test facility at KEK, the FFTB facility at SLAC, and the TTF linac. Clearly, test facilities are needed at Fermilab etc.

The main costs are in the superconducting main linac.

When a cold design was chosen, a frequency was also a chosen, and that, in turn, sets a lot of other parameters of the linear collider. The big costs of the final facility are in the civil engineering (for the tunnel) and the superconducting rf (SCRF) linac (for the rf source and structures).

Some of the tasks that must be accomplished are in the main linac. There are 17,000 accelerating cavities in the machine. The current performance goal is 35 MV/m, (operating at 30 MV/m). The performance of these accelerating cavities needs to be improved with electropolishing. Consistently high-gradient cavities need to be produced. They currently have a wide spread that will need to be reduced. The best cavity shape for producing the highest gradient should also be investigated.

The positron source needs to have the largest possible number of positrons per pulse. But how does one make positrons? There are three concepts: undulator-based (TESLA Technical Design Report baseline), conventional, and laser Compton based. The last is just a "good idea" at this point. Undulators are the most likely candidate at this time.

The damping-ring design is open to suggestions. One possibility is the "dog-bone" design. It uses a long tunnel and has some flexibilities but unfortunately has some sensitivities, as well. Other designs are on the table: one from Fermilab and a racetrack design from KEK.

The final focus is the subject of a working group that may have to remain open until there are detectors to provide focus for.

In summary,

- The FALC endorsed the decision and wants to be engaged in the next steps.
- The warm laboratories (KEK and SLAC) quickly reorganized toward the design of a cold machine.
- The community self-organized toward a cold machine at a workshop at KEK in November.
- The GDE began in March with the appointment of its director.
- The Snowmass meeting will take the first steps toward defining a baseline for the ILC.
- The baseline configuration is to be determined and documented by the end of 2005.
- The CDR is to be issued by the end of 2006.

Kim asked how the 40 people are communicating and how the detector groups are to be governed. Barish said that he was trying to build a functional communication system (which is currently quite poor) and to use one site for all meetings. The detector people have been operating independently of the accelerator people. He has been trying to bring the detector and machine people together and get them on the same time scales and the same product goals. He is also making the three leaders of the detector working effort part of the GDE.

Hewett asked a question that Barish rephrased as, what real investment in the initial machine needs to be put forward to accommodate the future upgrade? He did not have a good answer to that question.

Jackson noted that there were actually six working groups. There was also a group on strategic communication. Communication has to be integrated into the ILC conceptual design.

Dragt asked how long it took to write the TESLA and KEK CDRs; a year sounded like a short time. Barish responded that depends on how mature the concept is. There is nothing that says it *has* to be done by December 2006, but he would like to see it done by then.

Gilman opened the floor to comment. Dragt asked Jackson if she were doing anything to make the ILC exciting to the public. Jackson responded, yes, and that will be an ongoing process. Here in the United States, there is a good base to start from, but the United States will need to work collaboratively with its counterparts around the globe.

Ritz asked whether there was a good enough understanding of the manufacturability and of the design itself to justify the funding that is being asked for. Peggs responded that a recent workshop at SLAC on industrialization showed that this has to be taken very seriously. Ritz noted that it takes a lot of money to do it properly up front to avoid even larger costs for redoing it later.

Kim asked if all of this will come out of the $25 million requested for the ILC. Staffin replied, yes, and added that Kenji Saito had demonstrated how to fabricate superconducting accelerating cavities more cheaply.

Gilman declared a break at 3:16 p.m. He called the meeting back in session at 3:52 p.m., introducing **Raymond (Chip) Brock** to provide an update on the Physicist Resource Survey.

One year ago, HEPAP university representatives conducted an informal survey, asking what types of people would be needed by projects and what types of activities were being engaged in. Subsequently, a charge letter from Gilman asked if the field has the manpower to carry out the experiments to which the United States is committed until the end of the decade.

Last fall, the Subpanel sent questionnaires to every NSF and DOE HEP grantee and to administrative staff at the Cornell Electron Storage Ring (CESR). A set of experimentalists were also surveyed. Almost 100% of the DOE and NSF principal investigators (PIs) replied. The PIs were supposed to assume constant effort; other spokespersons were to assume expanded efforts. The data analysis began the week before this meeting. As an early validity check, the data were compared with other data from PhD surveys and found to be comparable.

Experimentalists were asked, "There are two emphases in this assessment: a reasonably precise accounting of the current effort within your experiment (the FY2004 numbers) and an accurate estimate of your experiment's needs for out-years." This is essentially a census of personnel for each experiment.

A 30,000-cell spreadsheet has been constructed that can be analyzed.

These data were used to project needed physics personnel (PhDs, postdocs, and graduate students) for the future. The data reported by PIs correlates well with experimentalists' reported data. However, it is too soon to report out-year trends; more consistency checking must be done. No problems have been identified. The Subpanel will meet to assimilate these results, and a written report will be issued and supplied to each respondent.

Dragt asked if it is a problem that current projects are expected to continue and new projects are expected to create more demand for physicists. Brock said that that is why a study was needed. Staffin asked what he meant by "needs." Brock replied that it was left to the projects to determine what that meant in the context of their experiments. There is the assumption that the foreign contribution will remain constant; the 2002 ratio was used. Staffin asked if there were good reasons to believe that the number of foreign people here will drop. Brock replied that he would leave that to the program managers.

Kim noted that a big drop in foreign participants has not been seen in CDF. Staffin asked how one explains that, given the machines overseas. Witherell responded that this is an international endeavor.

Diebold asked what happens to the people working on BaBar. Brock said that he did not know. Its effort does go down along with that of the BTeV with its cancellation.

Gilman asked **Aesook Byon-Wagner** to discuss resource trends and distributions. In the budget request, proton-accelerator-based research represents 53% of the request, electron-accelerator-based research 19%; non-accelerator research 6%, theory 7%, advanced R&D 13%, and mandated spending is 2%. From 1996 to 2006, the total funding increases slightly and then drops. However, with 3% inflation, this is actually a steady decrease in buying power. The largest fraction of funding in each year is provided to the national laboratories, but the actual funding for national laboratories has been decreasing over this period whereas the university funding has been slightly increasing. The human resource data for theory research and experimental programs at universities shows a decrease in faculty of 10%, an increase in postdocs of 10%, and a decrease of students of 30% for a total decline in personnel of 12% between 1992 and 2001. The number of groups funded is similar in 1992 and 2001. Comparable data for 2003 to 2004 indicate no change for faculty and slight increases for postdocs and graduate students; looked at another way, the data showed decreases in theorists but increases in experimentalists.

Laboratory managements have used voluntary reductions in force, no refill for attrition, deferred raises, reduced travel, and reduced new hires as ways to cut costs. At the same time, the national laboratories' costs for doing business (environment, safety, and security) have risen. In addition, they *have* to spend 2 to 4% of property value on maintenance. So, annual costs are increasing. However, laboratory funding has decreased 4%. All the national laboratories except Lawrence Berkeley National Laboratory have suffered HEP employment decreases in scientific and technical staff.

The percentage of salary, wage, and fringe benefits (SWF) payments in the total budget is increasing for each national laboratory. Their effective SWFs range from 60 to 85%, much higher than that of CERN, which is about 30%. Budget and staff sizes have steadily declined during the past few years. The age profile of the staff has been raised as a significant concern in all national laboratories during their annual program reviews and operations reviews. Budgets are dominated by salary. CERN's effective SWF during 2003-2007 is about 30% of its actual total spending per year. In facilities operations reviews and other assessment processes, Fermilab and SLAC were cited for a low level of resource allocation for support activities compared to other national laboratories. This led to characterizations of stretched out too thin; stressed; might not be sustainable; falling behind in infrastructure maintenance, human resource services, etc.; inadequate support on procurement, legal, etc. Unless budgets are carefully planned and managed, the foundation of any future initiatives, especially any large initiatives, will be lost.

**James Whitmore** commented on resource trends and distributions for the NSF. The EPP base increased from $55.48 million to $73.00 million between FY00 and FY05. He listed the activities supported. The trends from 1997 to 2005 were

+$2 million for EPP,
+$15 million for astrophysics,
–$2 million for CESR,
–$1 million for theory (which is increasing, although it looks like it is decreasing because of accounting procedures), and
+$8 million for LHC operations.

From FY02 to FY05, there was virtually no change in the number of universities or the total number of people supported:

| | Faculty | Postdocs | Grad Students | Universities |
|---|---|---|---|---|
| FY02 | 252 | 174 | 182 | 31 |
| FY05 | 251 | 177 | 184 | 33 |

The percentage of the EPP funding that each experiment gets is Tevatron: 14.2%, Neutrino: 5.7%, LHC: 15.2%, DESY/CERN: 3.7%, BNL/TJNAL: 3.3%, CESR: 3.9%, SLAC: 1.3%, other: 3.1%, particle astrophysics: 17.3%, and theory: 32.3%. The DOE contributions to these EPP research projects are roughly 10 to 30%.

Ritz commented that the ratio of faculty to students for experimental nonaccelerator groups seems to be low. Whitmore replied that that does seem to be an anomaly for that group.

Staffin commented that these data would be an interesting input to the EPP 2010 study.

Gilman opened the floor for discussion. There being none, he adjourned the meeting for the day at 4:56 p.m.

## Thursday, May 19, 2005

Gilman called the meeting into session at 8:56 a.m. He announced that on the previous day the House markup had added $22 million to the HEP budget and $200 million to the SC budget. The HEP additions were for the ILC and neutrinos.

**Michael Turner** announced that he did not have anything to report.

Gilman noted that HEPAP has six subpanels:

- Cosmic Microwave Background (CMB) Task Force (which is conducted jointly with NASA and the AAAC): its final report is delayed; Rainer Weiss is reporting on its progress at this meeting via telephone.
- Dark Energy Task Force (which is conducted jointly with AAAC): it was reported on the previous day; it is preparing the battlefield; its report is due by December.
- Rare Symmetry-Violating Processes (RSVP): its final report is coming soon.

- ILC/LHC Synergy: its activities will be reported on at this meeting; a report to EPP 2010 is due in August.
- Neutrino Scientific Assessment Group (NuSAG) (which is conducted jointly with NSAC): its first meeting will be held in a couple of weeks; its first report is due this summer; others will follow.
- P5 (which is to be reformed shortly): its charge was revealed yesterday; its chair is Abraham Seiden.

HEPAP also has a new subpanel on accelerator R&D; its charge was revealed yesterday but has no chair or members yet.

Gilman asked **Robert Cahn** to report on the RSVP Subpanel. The charge to the Subpanel is to

- Evaluate the science value of the Muon to Electron Conversion Experiment (MECO) in the context of the U.S. investment in elementary particle physics, assuming three cases: achieving sensitivities of $10^{-17}$, $10^{-16}$, and $10^{-15}$.
- Evaluate the science value of the Kaon Decaying to a Neutral Pion Experiment (KOPIO) in the context of the U.S. investment in elementary particle physics, assuming two cases: observation of 10 events and 100 events (at the rate predicted by the Standard Model).
- Place the science value of each in the context of the U.S. elementary particle physics program, broadly defined, recognizing the fiscal environment and the impact on other potential investments at NSF and DOE. How has this context changed since 1999, when the proposal was submitted?
- Place the scientific value of each in the context of the international elementary particle physics program and assess any potential overlap or complementarity with work being planned elsewhere. How has this context changed since 1999?

In the MECO apparatus, an incoming proton beam hits a target within a solenoid, producing muons. Those of the right momentum are transported to a target that captures the muons. There, some of the muons decay into electrons and neutrinos. The neutrinos resulting from the decay can have a variety of energies, and the momentum of those neutrinos is measured by a straw tracker and a crystal calorimeter.

KOPIO is characterized by nothing in and nothing out. At its heart is an important innovation that uses the beam structure to give time-of-flight information about the momentum of the kaon, which allows discrimination of the signal from the background.

The strength of both RSVP experiments is their ability to find new physics by detecting a signal differing significantly from Standard Model expectations. Such a discovery would be revolutionary. This scientific value is unchanged since the RSVP major research equipment was approved in 1999.

MECO is sensitive to lepton-flavor violation in both the $\mu \rightarrow e\gamma$ interaction and in more exotic interactions, such as those directly mediated by leptoquarks.

KOPIO's measurement of $K_L^0 \rightarrow \pi^0 \nu\bar{\nu}$ is sensitive to new charge-parity- (CP) violating interactions. While KOPIO would not add much to our knowledge of the CKM [Cabibbo-Kobayashi-Maskawa] parameters ($\eta$ will be known to better than 5% by 2015 from $B_d$ and $B_s$ mixing, relying on lattice calculations for a ratio of hadronic matrix elements), it will probe new physics at the TeV scale in models with minimal flavor violation. In the context of other models, KOPIO is sensitive to even higher mass scales.

MECO needs to make a substantial improvement over the current $\mu$-to-e conversion limit of $6 \times 10^{-13}$ in titanium, equivalent to $3 \times 10^{-13}$ in aluminum. It also needs to be able to cover the domain of the planned $\mu \rightarrow e\gamma$ experiment MEG [muegamma], which would reach the equivalent of $2.6 \times 10^{-16}$ if $\mu$-to-e conversion occurs through the same mechanism as $\mu \rightarrow e\gamma$. (There is a conversion factor that depends on the $z$ of the experiment.) A minimum single-event sensitivity of $10^{-16}$ is required, consistent with the MECO goal of $2 \times 10^{-17}$, allowing for somewhat larger backgrounds and/or less-than-perfect detector performance. A sensitivity of $10^{-15}$ is not an adequate level for MECO.

KOPIO's goal of 100 events for the Standard Model rate is appropriate. With a signal-to-background ratio of 2, this would give a $5\sigma$ statistical effect for an intrinsic rate 75% greater than the Standard Model prediction. The 100-event level would be achieved with 6000 hours of running at the expected performance. A sensitivity of 10 events for KOPIO with the Standard Model rate is not an adequate goal.

Proposals for other experiments with similar goals exist for both KOPIO and MECO, but these proposals are not as well developed, and in any case the timescale for RSVP would allow it to reach its goals first. There are also proposals for related experiments, which, in general, are complementary. A letter of intent has been submitted for an experiment [PRISM Mu E (PRIME)] at the Phase-Rotated Intense Slow Muon Source (PRISM) muon facility at the Japan Proton Accelerator Research Complex (JPARC) to measure $\mu$-to-e conversion with a sensitivity goal of $10^{-18}$. Another letter of intent calls for an experiment to measure $K_L^0 \rightarrow \pi^0 \nu\bar{\nu}$ at JPARC with a goal of 100 events. There is also a pilot experiment, E931a, at 12 GeV PS at KEK, with a sensitivity of about $10^{-10}$. The MEG experiment at the Paul Scherrer Institut (PSI) could find evidence for a $\mu e\gamma$ coupling before MECO does. Such a discovery would provide additional motivation for MECO. There are proposed $K^+ \rightarrow \pi^+ \nu\bar{\nu}$ experiments at CERN and KEK. This channel is sensitive to new contributions from both CP-violating and CP-conserving interactions, whereas the $K_L^0$ decay mode is sensitive only to CP-violating interactions. However, the neutral decay mode has the advantages of smaller theoretical uncertainties and greater sensitivity to new CP-violating physics because of the smaller Standard Model amplitude. If the charged K experiments were to find evidence for new physics, it would increase interest in the KOPIO result.

To put this into context, in recent years, it has been learned that neutrinos have mass and large mixing angles, violating (neutral-)lepton-flavor conservation. B-meson decays violate CP, and the best-measured modes conform to the predictions of the CKM model. No direct sign of a Higgs boson has been seen, but electroweak measurements indicate it should have a low mass. The nature of dark matter remains a mystery, as does that of the newly discovered dark energy.

The primary consequences for RSVP of these developments are an increased interest in lepton-flavor violation and a decreased opportunity for KOPIO to contribute to the determination of CKM parameters.

RSVP is complementary to the LHC. Discoveries at the LHC would likely increase interest in RSVP. If the LHC sees only a single Standard Model Higgs, there would still be interest in RSVP experiments because their sensitivity extends beyond the reach of the LHC.

The U.S. domestic experimental program in high energy physics is shrinking dramatically with the cancellation of the Charged Kaons at the Main Injector (CKM) experiment and BTeV and the scheduled completions for BaBar (2008), CESR (2008), and the Tevatron collider (2009). RSVP represents a major fraction of the anticipated accelerator-based program in the United States. With resources after 2009 increasingly concentrated in the LHC and (it is hoped) ILC, there is need for more modest-sized experiments for a balanced program and for increased opportunities for students.

While the B factories and LHCb are positioned to cover B physics thoroughly, the completion of the search for new phenomena in flavor physics requires that both the charged and neutral rare K-decay experiments be completed to the level expected in the Standard Model.

To characterize the importance of MECO and KOPIO, they can be compared to three existing or proposed experiments of generally comparable cost ($100 to 300 million): reactor or accelerator experiments designed to measure $\theta_{13}$ in neutrino oscillations, the search for neutrinoless double-beta decay, and a future cold-dark-matter search.

The angle $\theta_{13}$ in neutrino oscillations is both a fundamental parameter of the Standard Model and a crucial input for future neutrino experiments. It could be beyond the reach of the proposed experiments, resulting only in upper limits.

The cosmological evidence for dark matter is overwhelming, but it is not known if future cryogenic-dark-matter searches will be sensitive enough to detect it. These experiments are complementary to the LHC, which may find particle candidates for the dark matter.

Whether neutrinos are their own antiparticles is an important fundamental question, with implications for both cosmology and particle physics, but answering it may be beyond the scope of the proposed neutrinoless double-beta decay experiments.

KOPIO and MECO share with the three comparison experiments the capability to affect dramatically our understanding of fundamental interactions. The three comparison experiments are responses to specific discoveries: dark matter, neutrino masses, and neutrino mixing. KOPIO and MECO are well-motivated searches for physics beyond the Standard Model, long shots with potentially high payoffs.

We assume that NSF will bear only the incremental cost of running the Alternating Gradient Synchrotron (AGS) for RSVP. The opportunities provided by RSVP would not justify the full cost of running the AGS.

Ferbel asked what fraction of the RSVP's cost would be included in funding for the AGS. Cahn replied that the cost is on the same scale as the EPP budget.

Dragt asked if these experiments can meet the requirements. Cahn said that the Subpanel did not look at operations; it was asked whether the experiments could deal with the issues. The Subpanel was extremely impressed with what the two experiments have done. They seem to be meeting the challenges.

Diebold asked whether the Subpanel had found that the experiments were able to deal with the backgrounds. Cahn said that they had not seen any showstoppers; the experiments are doing a good job dealing with the backgrounds.

Baker asked if there were any comparisons to the underground laboratory (UGL). Cahn said that they had tried to characterize the competition on the basis of science. They did not worry about money, so they did not worry about competition for dollars by the UGL.

Diebold asked if the results of the Wojcicki Review Committee were available. Turner reiterated that this Subpanel was asked to look at the science. Wojcicki's Committee looked at cost and schedule feasibility. They found it to be complete, and it has a known cost. They found no showstoppers, either. Achieving the ultimate goal will probably take some modifications, but that is to be expected. These are parasitic experiments, and the host (RHIC) has to be healthy. The Committee is on track to report to the Science Board in August. This is fabulous physics, and it is currently on schedule. If it is not going to go on, that needs to be known as soon as possible.

Dehmer noted that the discussions of this Subpanel were sophisticated and fair.

Gilman commented that the Subpanel's report will be forwarded to the HEPAP members as soon said is available for review and possible vote. He declared a break at 9:43 a.m. He called the meeting back into session at 10:15 a.m., saying that he hoped to schedule the next meeting in mid- to late July; several subpanel reports will have been submitted, and the charter of the Panel expires shortly thereafter. He asked **Joe Lykken** to report on the activities of the LHC-ILC Subpanel.

The charge was "to explain clearly to the broad nonscientific community the need for a second large particle accelerator in addition to the Large Hadron Collider (LHC). ... what crucial scientific discoveries might not be made without the LC ... into a crisp, accessible, and persuasive case ... (10 pages) no later than summer 2005." The Subpanel's goal is to deliver a printed report to the EPP 2010 Committee at their Cornell meeting on August 2-3, 2005.

He listed the committee members. This is not an advising subpanel; its job is to explain the physics case. But it does not make sense to do that in isolation from the ILC and LHC communities. The Subpanel has been in contact with ILC/LHC groups, especially those who are also preparing/sending input to the EPP 2010 Committee. The group has links and is making outreach to the broad physics community.

What it heard from its customers was to keep it simple; avoid jargon in the introduction; take advantage of and credit for the accomplishments of the Standard Model; connect the report to the Quantum Universe brand; do not make a case for the ILC by selling the LHC short; instead of citing "new physics," state the puzzle that is being solved (it is not, however, an immediately engaging topic); do not lead with concurrency, but let the reader draw the conclusion from examples; and readers are interested in dark matter and do not engage with "unification."

The Subpanel believes it is producing a document with four layers of about 10 pages of text plus sidebars plus illustrations. The document is to have four layers because of the need to reach multiple audiences who have different backgrounds, different levels of awareness of LHC/ILC, and different amounts of time to devote to reading this stuff and because "crisp" implies short, but "compelling" implies enough detail to back it up. The four layers are

1. A gem-like, self-contained executive summary (at the technical level of *USA Today*).
2. A crisp, engaging, jargon-free description of the three outstanding discovery opportunities of the ILC, building from the new era launched by LHC (at a technical level of the *New York Times*).
3. A compelling narrative for each of the three discovery themes, explaining in more detail what ILC experiments could actually do (at the technical level of *Scientific American*).
4. A white paper for the scientific community, the agencies, and the EPP 2010 Committee, answering questions they have posed plus questions in our charge;

this layer includes specific, robust scenarios for what LHC and ILC may actually do, identifying who discovers what with enough detail to back up claims made in the upper layers and acting as an interface to already existing LHC/ILC studies (at a technical level of a physics department colloquium).

The timeline that the Subpanel is shooting for is

August 2-3: unveiling to EPP 2010
July 5: send to printer
June 23–July 4: polish, complete graphics, circulate for comments
June 22: complete layers 1 and 4 and the sidebars
mid-June: meeting to check with customers
June 8: complete layers 2 and 3
May 26: meeting at Fermilab
May 19: HEPAP
April 23: serious writing begins
April 22: meeting in Washington with J. Marburger, M. Turner, R. Staffin, P. Looney, M. Holland, J. Parriott, K. Carroll
March 30: first weekly telecon
March 25: first meeting at the Linear Collider Workshop in Palo Alto

The Subpanel has also been sent a list of questions by EPP 2010.

The Subpanel is not going to downplay the capabilities of the LHC, guarantee a specific roster of ILC discoveries, or explain why the ILC should be built in the United States.

In addressing the science goals of the ILC, the Subpanel is going to focus on discovery opportunities, not just measurements; use the Quantum Universe big questions as a guide and motivation; and make the relationship to LHC clear.

Turner asked Lykken to read additional material from the charge to the Panel. He read: "In the context of already known physics (i.e., our current understanding of the electroweak symmetry breaking sector) what are the synergies and complementarities of these two machines? How would an LC be utilized in understanding a Standard Model Higgs, or whatever fulfills its role in the electroweak interaction? In the context of physics discoveries beyond the Standard Model (supersymmetry, extra dimensions, or other new physics) that are assumed to be made at the Tevatron or early at the LHC, what would be the role of a TeV Linear Collider in making additional and unique contributions to these discoveries, in distinguishing between models, and in establishing connections to cosmological observations?"

The science goals of the ILC are to discover the identity of dark matter, resolve the mysteries of the Higgs, and connect the laws of the large to the laws of the small.

Discovering the identity of dark matter means to

- determine what dark matter particles can be produced in the laboratory,
- discover their identity, and
- connect the dark side to the bright side.

ILC makes the critical contribution to the second and third items.

Resolving the mysteries of the Higgs means to

- reveal the nature of the universal energy field that creates mass,
- determine if Higgs is connected to the dominance of matter over antimatter, and
- discover secrets of the quantum vacuum.

Connecting the laws of the large to the laws of the small means to

- discover how supersymmetry is hidden,
- bring into focus Einstein's vision of an ultimate unified force, and
- detect the quantum signals of extra dimensions.

Three examples of LHC/ILC discovery scenarios that may actually happen:

1. LHC experiments find a new heavy particle Z′ and are able to show that Z′ mediates a new force of nature. This is a great discovery! If the ILC then measures couplings of Z′ to find out what it means, depending on the nature of the data, Z′ might be related to the origin of neutrino masses, related to the origin of the Higgs, or come from an extra dimension of space. These are great discoveries!
2. Another possibility is that the LHC experiments discover several kinds of Higgs particles, and from decays of Higgs, ILC experiments discover a new source of CP violation that solves the mystery of why matter dominates over antimatter. (CPV plays a role; this must happen in $\nu_m$ or Higgs.)
3. A possibility related to dark matter is that CDMS detects weakly interacting massive particles (WIMPs) from the galactic halo, LHC discovers a neutralino, and ILC sparticle [supersymmetric particle] measurements determine the relic density and show that the neutralino really *is* dark matter and discover that it is only 2/3 of the total.

The fact is, communication is hard. It is very challenging to dejargonize the standard arguments of physics, but we, as a community, are improving. It goes back to the saying that "if you can't explain it simply, then you don't understand it." The Subpanel wants the upper layer to be understandable to a general audience but also to build up the case to the point where a physicist will appreciate it.

Turner commented that he hoped that the title is not *Universe II*. A glossy book is not envisioned; that may be premature. In Knoxville the previous night, someone asked how much the ILC will cost, and the answer was $10 billion, and there was total silence. Turner asked what the title would be. Lykken replied that that had not yet been decided. Turner said that they had asked for a white paper, not a sales document. This was to patch up a bad problem that occurred earlier. Lykken responded that he heard the message.

Cahn asked if Turner was asking for the elimination of the two simpler layers. Turner replied, no; he liked the layers. Lykken asked what they wanted to have happen after this paper comes out. Turner responded, to have the physics community adopt it. Gilman stated that, if one does not have sidebars and illustrations, one has a lot of difficulty getting in the statements you really want. Turner stated that what was wanted was not "the next big machine" but "the next big science question."

Staffin said that Isaac Newton had connected the laws of the large to the laws of the small, but in a much richer environment. One needs to define the scientific outcome of the ILC. There is an audience that has broader expectations of what they can use. He wanted to understand better the science case of the synergy of the two machines. He cautioned the Subpanel not to let the simpler layers detract from that. If one could weave in "how to use this," that would be great. This field is very difficult to understand, and the right-sized spoon has to be used to feed it to others.

Turner stated that the report used the term "core arguments." Arguments for or against what? Lykken noted that it is tricky to show the transition, complementarity, and interdependence of the two facilities. We need a good framework for that discussion.

Womersley suggested that the Subpanel also wants a script of what will happen at the ILC if things go a certain way at the LHC. Turner pointed out that the charge is to show the synergy, not to show the current case of the ILC. He extolled the examples as brilliant.

Staffin stated that, at the same time, he could use something to explain the situation to a variety of audiences. Gilman pointed out that the charge says to make a crisp science case for the ILC. Turner added that what was needed primarily was a discourse on the synergy.

Staffin commented that the Subpanel needs to be more specific about what it means by "discovering the Higgs" (as opposed to a "candidate Higgs"). How does one pin down the reality of the discovery?

**Rainer Weiss** joined the meeting by telephone to discuss the status of the CMB Task Force.

The Task Force has had five face-to-face meetings and 16 telephone meetings. In February, it had agreed on an outline but had worries about the content. It gave the report to the NASA Strategic Roadmap committee in March 2005. At the AAAC meeting in February, the Task Force was urged to prioritize the research and to define approximate costs. In April 2005, the finished draft report was sent to external reviewers: 5 theorists and 13 observers. It got a good response: 10 responded.

One reviewer was critical of the report, and the concerns could not be addressed quickly. The first concern was that the scientific case had not been made strongly enough. The Task Force is working on this. NASA said that the difficulty of the measurements is overemphasized. The development time for the space mission could be shorter, and the time for the construction of the space mission was not long enough, it would not be in the proper place in the NASA mission queue. Also, the treatment of foreground emission needs more work. The Task Force made a budget estimate that was controversial; it was told that it needs more justification. It was also told that the judgements about detectors made in the report were premature and need to be reprioritized. There were also some factual errors. The Task Force is grateful to the reviewers for pointing out these shortcomings.

The next steps for the CMB Task Force are to finish the revised report by the end of May 2005. It believes that it should also send the report to reviewers again beginning in June 2005. It hopes to submit the report to the AAAC and HEPAP in July 2005. The suggestion was made by the AAAC to hold a CMB workshop. In retrospect, that sounds like a good idea to pull the field together.

Turner asked if the draft report really needed to be sent back for another review. Weiss replied that the way they responded, he believes that the reviewers would feel better about the report. Maybe they should be called "readers" rather than "reviewers."

Salamon said that his recollection from the AAAC meeting was that the report was *not* going back to the reviewers. Weiss offered to take the issue back to the Subpanel; it certainly would make the members' lives easier if there was not another round of reviewing. Turner noted that all three agencies were represented in the room and all three agreed with Salamon's recollection. Weiss thanked them.

Gilman reviewed the actions taken at the meeting. Questions have been raised about the P5 Committee's makeup; the affected communities need to be consulted with, including foreign collaborators. Staffin asked what would be suggested about consulting with our valuable foreign collaborators. Dragt suggested including them on P5. Staffin commented that NuSAG certainly has foreign members. Cahn pointed out that P5 is different from NuSAG; it advises on the U.S. program; he was not convinced that it should have foreign members. Staffin stated that there is a baseline expectation that advice on the length of running the B Factory at SLAC will come from P5 and wondered how to do that in order to address residual concerns. Gilman commented that specificity and vagueness have to be balanced. Cahn suggested that P5 should consider if there are compelling reasons *to change the baselines* for shutting down the B Factory and Tevatron. Butler suggested that one could invite observers or you could specifically inform them about the meeting and its findings. Staffin suggested that one could also get on the telephone with one's counterparts and explain to them what is going on. Peggs noted that a modest reworking of the charge could broaden the term "international context" to something more inclusive. Hewett pointed out that there is the problem in that there are more than 20 countries, but they all have to feel that they have some voice. Kim added that they have to feel as though they participate in the decision-making process. Hewett asked if it can be done on a charge-by-charge basis. Gilman said, no. Staffin said that he sensed a nervousness about this charge and promised to try to be sensitive to all constituencies. He hoped that it would be workable. Gilman said that he would try to get started on P5 under a specific charge during the next week. Staffin agreed with that.

Gilman asked for comments on the accelerator charge. Peggs reiterated that superconducting magnets need to be looked at no matter whether they are in accelerators or not. The LARP superconducting magnet is vital. The BNL Superconducting Magnet Laboratory will need to be looked at because it is so vital to LARP. Staffin offered that whatever is relevant to the HEP mission should be considered, even if it is funded by another office. Peggs agreed. Staffin cautioned that a joint study with BES or NS is not wanted. Peggs pointed out that this subpanel will also have to distinguish among not only universities and national laboratories but also blends of these entities.

Dragt pointed out that the minutes of the previous meeting said that the Office of High Energy Physics was "moderately effective" and asked why. Crawford said that the highest rating is "effective"; HEP was 1 point (out of 100) short of that. This assessment is the output of a detailed spreadsheet, covering 30 years of performance. This is the mechanism that the Office of Management and Budget (OMB) has set up. HEP will be regraded in FY07. Dragt asked if the rating had budgetary implications. Crawford said that there are supposed to be budgetary implications, but they do not always happen. Turner suggested that Dragt should look at the website; it is amusing. The NSF paid a lot of attention to this assessment, but he did not believe that there is a correlation with budget. The President is very interested in the quality of management. However, when one is very good to begin with, one cannot improve. As a result, one gets marked down for not improving. Staffin commented that NS got an "effective" rating, and look what happened to their budget.

There being no further discussion, Gilman adjourned the meeting at 12:08 p.m.

The minutes of the High Energy Physics Advisory Panel meeting held at the Radisson Hotel, Washington, D.C. on May 18-19, 2005 are certified to be an accurate representation of what occurred.

Corrected by Frederick Gilman, August 11, 2005

Frederick J. Gilman 8/12/05