Minutes
Department of Energy and National Science Foundation
Nuclear Science Advisory Committee
Crystal City Marriott Hotel, Arlington, Va.
June 30–July 1, 2011

Members Participating:

Susan Seestrom, Chair
Robert Atcher
Jeffrey Binder
Jeffery Blackmon
Michael Bronikowski
Gail Dodge
Richard Furnstahl
Alexandra Gade
Carl Gagliardi
Susan Gardner
Peter Jacobs
Joshua Klein
Karlheinz Langanke
Zheng-Tian Lu
Allison Lung
Curtis Meyer
Julia Velkovska
William Zajc*

*Proxy from the American Physical Society in place of Robert Tribble

Members Absent:

David Kaplan
Dmitri Kharzeev

Others Participating:

Muhammad Arif
Dixon Bogert (by telephone)
Martin Cooper
Thomas Gentile
Jehanne Gillo
Timothy Hallman
Eugene Henry
Kevin Lesko
Bradley Keister
Richard Kouzes
James Symons
Steven Vigdor
Fred Wietfeldt
Scott Wilburn
John Wilkerson

Presenters in Order of Appearance:

Timothy Hallman
Bradley Keister
Krishna Kumar
Allena Opper
Bradley Filippone
Mark Reichanadter
William Brinkman

About 40 others were in attendance during the course of the meeting.

Thursday, June 30
Morning Session

The meeting was called to order by the Chair, **Susan Seestrom**, at 8:33 a.m. She reviewed the agenda and introduced **Timothy Hallman** to update the Committee on the activities of the DOE Office of Nuclear Physics (NP).

The vision of the Office is to enable U.S. world leadership in discovery science, illuminating the properties of nuclear matter in all of its manifestations; to provide the tools necessary for scientific and technical advances that will lead to new knowledge, new competencies, and groundbreaking innovation and applications; to make strategic investments in facilities and research to provide the United States with the premier facilities in the world by the end of the decade for research on new states of matter, and the force that binds quarks and gluons in protons and neutrons, the limits of nuclear existence for neutron- and proton-rich nuclei, and reliable isotope research.

The Office has investments in the heavy-ion program at the Large Hadron Collider (LHC); nuclear processes that power stars and supernovae and synthesize the elements; the nature and fundamental properties of neutrinos and their role in the matter-antimatter asymmetry of the universe.

NP got $535 million in FY10 and $540.1 million in FY11. The FY12 request was $605.3 million, which was a 15.9% increase over FY10. The major increases were the 12-GeV Continuous Electron Beam Accelerator Facility (CEBAF) upgrade that is being constructed at the Thomas Jefferson National Accelerator Facility (+$46M) and the Facility for Rare Isotope Beams (FRIB), within the Low Energy subprogram, which is being constructed at Michigan State University (+$18million). Across the rest of the program, the increase is $6.3 million.

The House markup of the FY12 budget request is $4.8 billion for the Office of Science (SC), a $42 million decrease from the FY11 Appropriation. The House Committee recommended $552 million for NP, $11.9 million above FY11 and $53.3 million below the request

There was language in the Committee report concerning irradiation studies of materials: "The Committee encourages the Department to consider ways to strengthen productive cooperation between Nuclear Physics and other programs at the Department of Energy to better understand and develop materials that can withstand high levels of radiation."

The 12-GeV CEBAF upgrade is on schedule and on budget. This upgrade will provide a sea change in what will be done at CEBAF. The 12-Gev magnets are now being installed.

Progress is being made on the FRIB at Michigan State University with the ReAccelerator (ReA) facility, a superconducting linac designed to accelerate rare isotope beams from the Coupled Cyclotron Facility at the National Superconducting Cyclotron Laboratory (NSCL). It will provide a wide variety of exotic isotopes at variable energies. When FRIB is completed, ReA will be part of the new facility and serve as its post accelerator.

The Office operates four facilities: the Relativistic Heavy Ion Collider (RHIC), CEBAF, Holifield, and Argonne. The Holifield will be closed after the FY11 budget year. Talks with Oak Ridge National Laboratory (ORNL) are looking at the future of this facility.

Approximately 1900 U.S. users from 32 states conduct research at NP national user facilities. NP supports a scientific workforce of approximately 2900 full-time-equivalent employees to carry out operations and research at the national user facilities and related programs.

Approximately 1300 foreign users from 50 countries conduct research at NP user facilities. They bring capital and new insights.

Program highlights include:

- A sea change in the understanding of collective flow at RHIC
- The discovery of anti-helium-4 at the Solenoidal Tracker at RHIC (STAR)
- RHIC's identification of a sweet spot for probing behavior changes below a square root of S_{NN} = 39 GeV.
- At LHC, the dijet energy imbalance is seen to be offset by lower-momentum particles opposite the leading jet and outside the away-side jet
- At the Thomas Jefferson National Accelerator Facility (Jefferson Laboratory) lead-radius experiment's indication that the neutron's radius is larger than the proton radius in the nucleus
- The Q-weak is making the first direct measurement of the proton's weak charge; when finished, it will test for new physics out to 2.3 TeV
- Jefferson Lab's continued development of 650-MHz cavities for Project X
- The ORNL, Vanderbilt University, and Dubna team is moving on to look for elements 120 and 119
- The commissioning of the Californium Rare Ion Breeder Upgrade (CARIBU) on May 2, 2011

In underground science, DOE remains committed to the science it has planned within its mission. It is assessing the impacts and viability of underground science using only DOE resources. DOE asked an independent panel to help identify cost-effective options. The Final Report was delivered on June 15, 2011. NSF will provide a $4 million to bridge the funding gap between June and September 2011. SC is requesting $15 million in FY12 to maintain the viability of the Homestake Mine while assessing its options. The Majorana demonstrator remains the big question for NP.

The Office of Nuclear Physics has isotope operations at Brookhaven National Laboratory (BNL), Los Alamos National Laboratory (LANL), and other sites across the country. Demand for strontium-82 has increased; DOE supplies 75% of the domestic market, providing a tremendous impact. It is also the only supplier of Californium in the United States.

Three companies have grown significantly as a result of technology developed at SC laboratories for basic science: Advanced Energy Systems in Medford, New York; Meyer Tool and Manufacturing in Oak Lawn, Illinois; and NIOWAVE in Lansing, Michigan.

Technical staff at LANL continued to identify potential technical options for addressing the problems of the reactors at Fukushima.

A new website at SC tells why NP's research is important.

There are now seven early career awardees at NP.

Four new positions in NP were approved under the FY11 appropriation, and three positions are still on hold. There is a hiring freeze now because of the uncertainty in the FY12 budget.

Zajc asked if plutonium-238 was under the purview of NP's isotope program. Gillo answered that it was not; it is under the Office of Nuclear Energy (NE).

Meyer asked what the impact of the House mark would be on the CEBAF upgrade. Hallman pointed out that the Senate still has to weigh in on this bill, but that the House mark would be bad.

Velkovska noted that the SC fellowship program just started and asked if it would be continued. Hallman responded that SC thinks that this is an important program and is having a dialogue with congressional staff about it. Perhaps it fits in better under NSF. SC is looking at how to staff its facilities in the future. How all this will be supported in the future is unknown.

Lu asked if isotope-detection technologies can fit under the isotope research initiative. Hallman replied that such research could be proposed to the Applications of Nuclear Science and Technology Program.

Furnstahl asked if the research community should provide information to Congress about the SC Graduate Research Fellowship Program. Hallman answered that it might confuse the issue and not be helpful.

Langanke asked if the missing workforce in isotope production is also receiving consideration. Hallman said that it was not just about isotope production but also about educating and growing the community.

Jacobs asked if there were coordination with offshore sources of isotopes. Hallman said that the Office works with international partners. Gillo added that the government can not compete with commercial producers; it can only produce isotopes for which there is no other U.S. source, but there is cooperation with foreign sources.

Bradley Keister was asked to provide an update on NSF activities.

In FY11, authority for support of accelerator-based nuclear astrophysics was transferred to the Nuclear Physics Program. For the NSCL, the National Science Board (NSB) approved a 5-year funding plan that includes a startup of the ReAccelerator (ReA3) and a vigorous research program. The FY11 budget allocation to programs reached the program level about 3 weeks ago and award decisions are underway. Funding of Nuclear Physics and most other programs in the Physics Division are down about 3%. Program management involves multi-year factors, notably into FY12 as awards expire that were funded by the American Recovery and Reinvestment Act.

Highlights of research now go to research.gov (a public website). Also, principal investigators (PIs) must submit a one-page synopsis for this website in addition to their final report.

FY12 budget request: Research and Related Activities (R&RA) is up 12.4% from FY10, and Mathematical and Physical Sciences (MPS) is up 6.0%. If the FY12 budget request is approved, there will be a disproportionate and substantial increase to PI support in the Physics Division.

NSF also provides funding through its Major Research Instrumentation (MRI), Cyber-Enabled Discovery and Innovation (CDI), Petascale Computing Resource Allocations (PRAC), Department of Homeland Security Domestic Nuclear Detection Office, and Physics Frontiers Centers (PFC) programs. These NSF-wide programs can provide additional funding opportunities to the nuclear science community.

There are two NSAC charges outstanding on neutron science and data management. For neutron science, NSF has been a partner with DOE for funding and for the NSAC charge. For data management, the charge is DOE only, but NSF also has policies and requirements for data management, and is therefore interested in the outcome of this study.

Cora Marrett has been confirmed as Deputy Director of the NSF.

Blackmon asked him if he could say more about the changes in the level of funding. Keister said that programs in the Physics Division have been managing the impact of ARRA funding so as to mitigate the expiration of most ARRA awards in FY2012 by distributing the effect over several years.

Gagliardi asked if the transfer of Nuclear Astrophysics was simply an accounting exercise. Keister confirmed this: the transfer of awards with funds represents a better programmatic fit for the research covered by the awards.

Krishna Kumar was asked to present the interim report on fundamental nuclear physics with neutrons. Seestrom recused herself from the discussion, and Peter Jacobs assumed the chair of the Committee pro tem.

The NSAC 2002 Long Range Plan recommended investing in a new neutron beamline at the Spallation Neutron Source (SNS). A 2003 subcommittee recommended the launch of the Neutron Electric Dipole Moment (nEDM) project, and construction of the Fundamental Neutron Physics Beamline (FNPB) at the SNS. nEDM got Critical Decision Zero (CD-0) in December 2005 and CD-1 in December 2006. The NSAC Long-Range Plan of 2007 called for a targeted program of symmetry tests of the new standard model and precision electro-weak physics.

The charge to this Subcommittee was to evaluate the current and proposed research program for the physics potential and specific opportunities, in an international context, and to recommend priorities constrained by a constant level of effort at FY11 funding by identifying the most compelling opportunities, reviewing the infrastructure and effort required, considering the international competition, prioritizing incremental investments beyond the constant level, and assessing the current scientific and technical workforce.

Neutron physics falls into several broad themes. The nEDM experiment has a compelling physics case and would still be a significant fraction of funding and effort. The lifetime of the neutron is a fundamental parameter with current research producing inconsistent results. Correlations comprehensively probe the neutron charged weak current and hadronic parity violation. The Subcommittee examined recent progress of the experimental program, assessing the cost and difficulty versus physics payoff for all the experimental efforts.

The Subcommittee was formed in late December 2010; sought guidance from the agencies; invited members; and held three meetings to provide orientation, make a plan, hold reviews, and discuss priorities and funding. Web resources were set up for Subcommittee members and the public. Findings and recommendations were drawn up.

The physics motivation for nEDM is to search for a nonzero electric dipole moment (EDM). A non-zero EDM significantly larger than the value predicted by the Standard Model is a signature of time reversal symmetry violation, and is sensitive to new physics that influenced the early universe. The community has endorsed such EDM searches. The Subcommittee found the motivation for sensitive EDM searches to be as compelling as ever. The successful completion of an nEDM experiment, the initiative with the highest scientific priority in U.S. neutron science, would represent an impressive scientific and technical achievement for all of nuclear physics with ramifications well beyond the field.

The nEDM conceptual design has a large active volume to gain statistical sensitivity to 1–10 × 10^{-28} e-cm, has several novel techniques to explore unknown new systematics, and is the only concept aimed directly at exploring this new regime of sensitivity. The nEDM reach is nominally estimated at 4 × 10^{-28} e-cm, which would have a profound impact. Even if the reach is about 10 × 10^{-28} e-cm, it is still worth doing at the current scope as long as the final results and publications are produced before 2025. The design has progressed over the past few years. Feasibility studies of physics concepts are driving the experimental design. Several technical challenges have been resolved, and a first-pass engineering design has been developed. The international competition is pursuing intermediate steps about an order of magnitude less sensitive but is estimated to have a faster turnaround time of 5 years; in the same 10-year timeframe they expect to compete at the same level, but details are sketchy.

The Subcommittee found that significant additional R&D is required. Fundamental physics concepts related to measurement techniques have been validated, but important details are still being worked out. The high-voltage breakdown limit, electric field monitoring, electrode coating, and total photoelectric yield per signal event are all significant issues. The Subcommittee believes there should be a singular focus on outstanding R&D issues, the coordination and communication across various teams, improved support for large-scale cryo-engineering, and improved communication and support from ORNL and LANL. After extensive deliberation of the progress and needs, the Subcommittee formulated six recommendations to define a path forward:

1. Successful resolution of R&D items is of paramount importance, and a well-structured and strategically targeted R&D plan are needed.
2. The collaboration has many talented and diverse research groups, but improved coordination and continuous communication are needed among physicists performing R&D, physicists and engineers designing the apparatus, and collaboration leadership. A research effort at the level of 80 to 100% by key PIs is needed on nEDM now.
3. ORNL and LANL should jointly establish an external standing technical review committee (TRC) to review the R&D progress and to report periodically to the management of both institutions. The TRC's primary focus would be to monitor technical progress and to evaluate the mitigation of technical risk, ensuring that resources are properly redeployed, as needed. This is a project of large magnitude and scope, and increased and sustained institutional commitment from ORNL and continued significant support from LANL are needed.
4. Large procurements should be contingent upon resolution of the major outstanding technical issues. The cryogenic system is unusually large in scale and low temperature. Technical expertise and advice from NP accelerator and physics divisions should be sought, and all engineering should be centralized and coordinated.
5. The agencies should provide continued support for a period of two years given implementation of the previous recommendations to provide time and flexibility to evaluate and solve technical problems. In the interim, neither the design configuration nor the cost of the central detector system should be assumed to be stable.

6. There is a 2-year two-year window to initiate construction, given international competition and the evolution of related physics topics. If major outstanding R&D issues remain unresolved after two years, consideration should be given to discontinuing the major item of equipment (MIE) project and re-evaluating the approach to measurement of the neutron EDM.

The other recommendations relate to the rest of neutron physics: neutron beta decay, hadronic parity violation, and neutron interferometry. There has been significant progress during the past decade with several important new results; the field is poised to capitalize on recent investments. The order of presentation of the following recommendations does not reflect a rank ordering.

7. The neutron lifetime is a fundamental parameter that impacts many areas of nuclear and particle physics and cosmology. The current thrust is to improve consistency to a precision of 1 sec. A well-motivated long-term goal is to achieve a precision of 0.1 sec. High priority should be given to acquiring new data with the cold beam-based lifetime measurement at the National Institute of Standards and Technology (NIST), following its planned improvements, because a robust beam-based lifetime measurement at 1-sec precision is very timely. A magneto-gravitational trap that uses ultra cold neutrons aims to reach 0.1 sec in the long-term. The current NIST effort using magnetically trapped ultra cold neutrons is not competitive. All of the research groups working on neutron lifetime would benefit from better communication and collaboration.
8. Neutron beta decay correlation coefficients are fundamentally important. The UCN-A [Ultracold Neutron] experiment at LANL should be supported to improve the measurement precision of the A-coefficient and to develop an expeditious path to the original design sensitivity of 0.2%. Furthermore, there should be parallel R&D to develop the experiment to measure the a-coefficient with the Nab [Neutron a- and b-coefficients] spectrometer with a sensitivity are 0.1%; the A- and a-coefficients can impact many subfields. The B- and b-coefficients in principle are sensitive to beyond-the-Standard Model physics, but current experimental proposals are not yet in the region of interesting sensitivity. R&D for future measurements should be explored, and full-scale UCN-B and abBA/PANDA [Proton Asymmetry in Neutron Decay] project should be revisited in a few years.
9. In hadronic parity violation, nuclear-decay experiments seem to imply that there is a dynamical suppression of long-range nucleon–nucleon weak interactions. The NPDGamma Experiment should be strongly supported as the highest priority measurement in hadronic parity violation, and every effort should be made to reach the design goal, an asymmetry determination of one part in 10^8.

The subfield is vibrant. Many excellent initiatives have been proposed; but there is not the money, people, or time to do everything. The highest priorities in rank order are:

1. nEDM is the highest priority for neutron science.
2. UCN-A should be continued to its logical end, achieving its original design goals.
3. The NPDGamma [Neutron-Proton-Deuteron-Gamma] experiment should be completed, and the design precision should be reached.
4. Nab should be invested in.

5. The modest lifetime effort at NIST should be completed; it is poised for 1-sec precision.

This program fits into a funding scenario of constant effort.

There are ongoing projects that are important but are not part of the charge. The U.S. workforce is insufficient to carry out all the proposed initiatives; the recommendations have singled out the highest priorities. U.S. facilities (NIST, FNPB, and LANL) and capabilities are world class with excellent opportunities.

A tenth recommendation is that consideration should be given to establishing a standing committee to review and prioritize various initiatives in U.S. fundamental neutron science, to provide guidance on scientific prioritization of new initiatives, guidance on allocation of R&D funds, improved collaboration across research groups, optimization of neutron beams, and improved communication to the broader physics community on the role of neutron measurements. It is possible that the optimum strategy is broader: a standing committee to encompass all aspects of fundamental symmetries and neutrinos in nuclear physics.

In summary, this is a very active subfield; the Subcommittee brought a wealth of experience to bear on the physics issues as well as the technical details; the agencies were helpful; and the best science should emerge from this effort.

Zajc asked what the collaboration structure is, given that the nEDM scale is so large. Kumar replied that he would provide a copy of the organization chart. Jacobs noted that there is a lot of backup material.

Lu emphasized that the nEDM uses many of the same techniques and facilities as neutron physics does, so it should be considered to be neutron physics.

A break was declared at 10:23 a.m. The meeting was called back into session at 10:46 a.m. to discuss the neutron charge report, under the leadership of Jacobs. He noted that this is an NSAC report, and a vote could be taken on whether or not to accept it on the next day of the meeting. The current discussion would cover the overall organization, background, and Subcommittee processes; the nEDM Experiment; other experiments; and Recommendation 10.

Klein stated that the report presupposes that the competition will do what they will say they will do. He asked if the possibility that technical barriers may preclude completion of the project had been considered. Jacobs said that these scientific problems are extremely complex and have been investigated for five decades. The Subcommittee considered the progress of other experiments, which are less ambitious than this project. The same technical and systematic issues will confront any extension of these current projects. This project is expected to be very competitive. Lu added that, in 2025, its results would be competitive. Klein noted that there is an error bar on that date; the context may change in the next 2 years. Kumar stated that the Subcommittee is not talking about stopping EDM in 2 years but about re-evaluating the situation at that time. The 2025 date is when information from this project would be important in other areas (e.g., collider physics). Jacobs pointed out that the recommendation is that it would be appropriate to reconsider the project after 2 years to see what fundamental technical issues are still unresolved.

Lung asked to what extent the Subcommittee believes that the 2-year timeframe is adequate to resolve the technical issues. Jacobs added that the collaboration would have to answer that question; the 2-year timeframe is driven to some degree by external issues.

Lung said that, if 2 years is too short and significant technical issues remain, NSAC should give the collaboration a reasonable chance of success. Kumar said that in the current plan the collaboration is aiming to get to the construction phase in 6 months to a year, but the recommendation is to delay this step. Keister stated that the question is whether 2 years are enough to resolve the R&D goals. Martin Cooper (LANL) said that photostatistics issue could be resolved in 6 months. To cost the electrodes, one has to find a manufacturer; 6 months is reasonable. High voltage comes in many stages, and the issues would persist for a long time; 2 years may be enough. Jacobs pointed out that there is always an element of risk in big projects. An evaluation after 2 years would be a positive development and would mitigate some risk.

Meyer noted that an Executive Summary was to be added and asked if the details would be dragged out in this discussion. Jacobs said that the recommendations address a lot of issues; this was an intensive subcommittee. The long report is due in September; this is the interim report with essential recommendations and sketchy details.

Velkovska noted that there had been a series of workshops and asked if the Committee could receive the links to these workshops on the Web. Jacobs answered, yes; the websites are open to the public, and the links will be provided.

Zajc asked if the project were being operated under DOE orders and, if so, where it was. Gillo replied that it was between CD-1 and CD-2.

Blackmon asked if the technical challenges will be stated and evaluated in the final report. Kumar replied that the Subcommittee had documented these details during its discussions, and it would include them in the final report. Jacobs added that the benchmarks setting the sensitivity reach of the project are established by the collaboration. Dodge stated that these technical challenges should be named. Jacobs assured her that the Subcommittee would document the key technical issues it had identified, but that the committee had not conducted a detailed technical review and there may be issues of similar or greater importance that it had not found. Assessing that the risk is manageable has to be a dynamic process between NSAC and the Technical Advisory Committee. Setting benchmarks is outside the purview of the Subcommittee. The interim report does list the identified challenges and calls for a TRC to monitor technical progress and assess technical risk. Recommendation 4 requires technical risks to be overcome before procurement proceeds. Kumar explained that, when one has a large project, one has internal reviews. That process has been missing in this case; it needs to be done as the critical-decision process proceeds. Gagliardi assured the Committee that there have been reviews. Kumar pointed out that the reviews have been focused on project issues; what are needed are reviews of R&D progress and physics benchmarks.

Blackmon asked if there would be an ordered list of recommendations in the final report. Kumar replied that the Subcommittee had placed the highest level of priority on the most important work that would fit in the constant level of funding. Priorities could be assigned to the other work. Gagliardi responded that it would be useful to make that explicit in the report. On one of the slides presented, it said that the current workforce is too small to carry out all projects proposed to the committee. He asked if it were adequate for the current-level priority work. Kumar replied, yes. Jacobs explained that people are involved in multiple projects; it would be good to look at the manpower available for the priority tasks.

Meyer asked what portion of the effort of all proposed projects is nEDM. Kumar said that that number could be determined and supplied to the Committee.

Jacobs noted that the Subcommittee's rank order of priorities would make a good Executive Summary for the report. Gagliardi agreed. Dodge asked if those five points could fit under the constant level of funding. Henry answered that the Office is funding UCN-A now, which, along with others, will be phased out. Nab is on the radar screen; it is being considered for the near future. And nEDM research is in the budget. Although there are tensions about the workforce, this funding is feasible. Gillo noted that it would be feasible only if the MIE profile is placed on top of the current plan. Delays will increase total project costs, and those added costs will have to be made up in the eventual project budget. With the focus on R&D, construction of the project would have to be slowed to manage its total project cost (TPC). Jacobs noted that the statements in the interim report about constant effort are limited to the next few years. Keister said that all five items listed would fit under NSF constant funding. Nab would not. Kumar said that the Subcommittee understood that budgetary discussion but focused only on the physics. Jacobs pointed out that Recommendation 10 would establish a standing subcommittee to assess the field constantly and recommend priorities. Such assessment needs to be done frequently. The current oversight of the field is too infrequent. This subfield is spread over several facilities, making it difficult to understand the costs.

Jacobs asked if anyone wanted to argue against adding an Executive Summary. Gagliardi suggested that the Executive Summary also include Recommendation 10 (establishing a neutron-science standing committee). Blackmon asked if this would be an NSAC subcommittee. Jacobs said that it might be a hybrid; it has to have the buy-in of the facilities. The Subcommittee did not want to be too prescriptive. Kumar commented that the field is hurt by the fact that no national laboratory is vested in this discipline. Klein asked if something like the Neutrino Scientific Assessment Group (NuSAG) was being envisioned. Jacobs's vision was a committee that would give an annual assessment of possible projects. Keister noted that these entities typically act as a subcommittee (sometimes as a joint subcommittee) and get specific charges. Henry pointed out that the community has experience with NuSAG; it sifted through experiments and came up with recommendations for NSAC and the High Energy Physics Advisory Panel (HEPAP).

Jacobs proposed the development of an Executive Summary for consideration by the full Committee. There was consensus on this approach.

Dodge asked if the nEDM collaboration had seen this interim report and agreed with it. Jacobs said that they had seen it for the first time on the morning of this meeting. They would have an opportunity to make comments during the afternoon session.

Velkovska asked if this interim report could be changed after it is approved. Zajc pointed out that this is the interim report; the final report, which may be different, will be voted on in September. Hallman said that the content is set when the final report is accepted by NSAC and transmitted to the agencies. Keister added that what was accepted here would be the broad statements; the writers of the final report will understand that.

Langanke asked what the distinction was between the two lists of priority activities. Jacobs replied that the charge was to recommend activities that would be supported within current funding. Hallman added that there were other activities that did not make that cut, and those are listed as the additional experiments.

Lu pointed out that another consideration in prioritization would be the level of funding needed by each experiment.

Langanke noted that there have been neutron lifetime experiments that have shown different results, disagreeing by up to 6σ. Fred Wietfeldt (Tulane University) explained that there is a controversy about the neutron lifetime. Beam experiments have a precision of 3.5 sec, while ultra-cold bottle experiments claim a lifetime of about 820 sec ± 1 sec and disagree with the beam experiments by about 6 sec. The precision of these experiments has been improving. It would be helpful to do a beam experiment at the 1-sec level. Kumar said that it was the Subcommittee's opinion that the earlier experiments should be cleaned up before going on to higher-precision experiments. Some of these experiments are being run again; the field is very dynamic. Thomas Gentile (NIST) noted that, had this discussion been held 10 years ago, one set of experiments would have been done, but the context has changed with the Russian experiments.

Jacobs stated that the Committee would try to accommodate comments from other experiments. A break for lunch was declared at 12:04 pm.

June 30, 2011
Afternoon Session

The meeting was called back into session by the Chair, Seestrom, at 1:15 p.m. She announced that Eugene Henry was retiring. He has been the Designated Federal Officer of NSAC.

Allena Opper was asked to present the report and recommendations on Public Access to Research Results (PARR).

The America COMPETES Reauthorization Act of 2010 required DOE to identify and assess current practices, policies, and procedures for disseminating research results. The charge from SC asked for a snapshot of what NP currently does but did not request any recommendations. The Subcommittee assumed that it was writing for DOE and members of Congress.

NP researchers typically produce digitized detector signals (raw data), which are processed. Preliminary results are vetted within a collaboration to produce physics results that generally go into a version of record, published research, and final results, where the material is free for all to read and use. Researchers also produce simulated data that results from detector models and physics simulations. The community refers to "green access" when the author's final version is placed in an institutional repository or in a subject repository, such as the arXiv. It refers to "golden access" when someone pays the publishing costs to make the article freely available upon publication. The size of the data sets produced by one experiment over a year may range up to several petabytes.

The Subcommittee polled the community to get information on current practices, policies, and procedures. It also got input from the American Physical Society (APS), the American Institute of Physics, arXiv, and others.

Finding 1: The field of nuclear physics publishes in scholarly journals and uses the publication policies of those journals as well as archives and databases to make its research results available to the public. The results available through these means are the peer-reviewed versions of record (VOR). The VOR represent the ultimate product of the government investment in research and are uniformly available to the public. Most publishers allow authors to post the VOR on their own websites, providing free access. The APS has allowed all public libraries in the United States free access and is now opening that access to high schools; 0.03% of its downloads come from public libraries and high schools.

Comment 1: To assure access policies that are sustainable, close collaboration with publishers and other stakeholders is needed. Publishers are a valuable aspect of access to research results, and their services are not free.

Finding 2: Pre-final data in the form of preliminary data, theses, conference presentations, and reports are generally publicly available on preprint servers.

Comment 2: The availability of these open-access sources to research results should be more widely advertised to the public.

Finding 3: The knowledge and resources required to use these data generally make them useless to persons unfamiliar with the experimental apparatus and the conditions under which the data were collected.

Comment 3: With few exceptions, digitized detector signals, processed detector signals, and associated computer codes are unlikely to be of use beyond the immediate collaborations that produce them. Because the data are in such complex and varied forms, it would be counterproductive to impose a top-down policy regarding the sharing of them. Decisions on the sharing of research at these levels of development should be left to the individual investigators or collaborations. To make this category of data widely available would likely require significant additional resources with little added benefit. Access to raw data is obviously a complicated issue, but the rarity of requests should not guide future policy; the computing-power requirements and the size of data sets have not stopped people from occasionally requesting such data.

Finding 4: A deeper understanding of both experimental and theoretical nuclear science is often enhanced by one-on-one interactions, such as workshops, summer schools, collaboration meetings, and conferences. The dissemination and sharing of pre-final research at these workshops often inspire advances in the field.

Comment 4: Conducting meetings and workshops helps to maximize the potential benefits of research results.

Comment 5: Presenting research results to the public is an important and recognized responsibility of scientists, and the nuclear physics community responds to it through various outreach activities.

Jacobs pointed out that there was a current practice, SCOAP^3 [Sponsoring Consortium for Open-Access Publishing in Particle Physics] centered at CERN that takes global access to its limits. He asked if the Subcommittee had considered it. Opper replied that it had considered SCOAP^3 as part of open access. Jacobs pointed out that all University of California libraries are signatories to the SCOAP^3 agreement. Seestrom stated that that is a bigger recommendation than this Subcommittee considered. Opper offered to discuss it explicitly if the Committee so desired. Seestrom suggested describing SCOAP^3 in a paragraph. Klein asked how SCOAP^3 works. Jacobs said that open-access payment is made to publishers at the level of funding agencies. It is one approach out of many that are being considered. Langanke asked what charges were involved. Opper said that open access charges currently run from \$1500 to \$4700 per publication.

Zajc said that the Subcommittee had done a good job. He noted that the discussion of codes in theory included support and asked if the Subcommittee had considered HepForge through which raw data in astronomy are available exclusively to the team for 10 years and then opened to the public. Klein said that the point here is what constitutes raw data. Opper said that the Subcommittee did not consider HepForge. One issue is that no one asks for raw data, anyway. Raw data are in huge files. Instead, the Subcommittee tried to describe what was done.

Langanke stressed that the astrophysics community also has a platform for collecting data sets and codes and using them for development and teaching. Opper stated that the Subcommittee has to deal with many issues, including the broad discipline and the different data formats.

Gardner pointed out that there is a large mismatch between what one can extract from large (e.g., Tevatron) data sets and what gets published. This issue bears on what should be archived. Opper pointed out that the Jefferson Lab and RHIC archive every bit of data. Archiving data beyond 5 years after publication may not be useful because the codes and people are not available anymore. Gardner stated that occasionally reconsideration of old data leads to new discoveries. She asked what the distinction was between "open access" and "green." Opper said that with open access there was no obstacle to getting the data. Klein said that green access required a sponsoring institution to make the data freely available. To the consumer, there is little difference between the two schemes.

Hugh Montgomery (Jefferson Lab) said that the Subcommittee had put a lot of effort into Recommendation 3; however, different people will view "ownership" differently. Klein stated that this was the right thing to say, but it raised a red flag. Zajc proposed leaving that sentence out, and possibly deleting the last four sentences. Meyer said that that made sense. Jacobs said that one still has to think about who is responsible for the data. He said it should be the laboratory. Klein pointed out that not everything happens in a laboratory, and some laboratories are in other countries.

Keister stated that the statement about the need for a bottom-up data policy is what NSF wants set out in a data-management plan in each proposal submitted. The community will evaluate the proposals and judge the appropriateness of the data-management plan.

Bronikowski asked about data for which there was a lapse in government funding. Seestrom said that the discussion only applied to data that DOE paid for.

It was the consensus of the Committee that the sentence about sharing of research and "likely" should be deleted.

Seestrom said that, to get away from the problem of "ownership," one could put in "or the laboratory hosting the work is best suited to determine the suitability of sharing the data."

Richard Kouzes (PNNL) noted that, in large collaborations, there are data policies. There is no institutional ownership. It is up to the Executive Committee of the collaboration to determine what happens to the data.

Montgomery said that the comment about outreach is weak and should be strengthened to reflect current practices. Keister pointed out that there are highlights going out on public websites plus a final report must have a synopsis in lay terms. Hallman stated that the dissemination of data is a *requirement* of research. Opper asked if a paragraph should be added about that. Klein suggested saying “Outreach happens through … .” It would be worthy to mention some examples. Velkovska said that a mention of outreach examples and the NSF highlights could be added.

Seestrom asked if there were any comments on the findings. Blackmon said that mentioning the U.S. Nuclear Data Program in the findings would be good. Klein noted that it is an example of many databases that are compiled. Seestrom added that it is also funded.

Langanke noted that archiving data is important, but detecting (and proving) fraud should also be mentioned. Opper commented that that is not in the purview of this report.

The consensus of the Committee was that the findings were acceptable. Gagliardi suggested adding “general” before “public” in Finding 2.

Seestrom suggested making a new finding about databases. Blackmon said that there is a difference when old data are re-analyzed to produce a new publication. Opper admitted that the Subcommittee had not discussed that type of VOR.

The discussion was tabled, and a break was declared at 2:28 p.m.

The meeting was called back into session by Jacobs, chair pro tem, at 2:51 p.m. He introduced **Bradley Filippone** (Caltech) to speak about the nEDM TRC.

The collaboration supports the establishment of a TRC. The question of a 2-year window is workable if the TRC works with the collaboration members. The most recent experiment found a large new systematic effect. That problem has been solved, but there may be others. The national laboratories working with the TRC should be able to deal with such problems.

Lung noted that the text of the report does not mention transitioning from R&D to engineering design and asked what the collaboration’s view of the readiness for such a transition was. Filippone said that the collaboration thinks it would be ready in six months; the Subcommittee took a longer stand on that. There will be an overlap. The researchers would work with the engineers. Lung pointed out that the report implies that one goes from R&D to construction. CD-2 is not the same as starting construction. Cooper said that engineering would be ramped up as R&D is ramped down. The engineering decisions and procurements are driven by R&D, but the way it is stated is confusing. Kumar added that the Subcommittee is walking a tightrope. It does not want to tell DOE how to run the project.

Scott Wilburn (LANL) was impressed with the work of the Subcommittee. The recommendations come out nicely. Several neutron research efforts should be pursued. If ultracold neutron research is to be pursued, the Subcommittee should consider mentioning that the unique facility should continue. Kumar said that it is hard for the Subcommittee to make a clear statement on that issue because of the fiscal realities. Jacobs said that there is a scientific aspect, also. The NIST beam lifetime experiment should be run to completion. The Subcommittee deferred on other programs because they were beyond the Subcommittee’s vision. A call for an ongoing assessment of the field would deal with that problem.

Langanke asked what the breakdown of the money would be. Henry said that the charge contained some fiscal information and guidance consistent with the FY11 budget request and out-year costs of living. The base program currently is $6.5 million (including R&D and operations), and EDM is built on top of that. It would handle all of the recommendations. The cost of an nEDM project has not been finalized. It has grown over the years. It would be approximately equal to 4 to 6 years of the base program. The EDM research cost is part of a diverse program and is therefore difficult to specify precisely.

Keister said that the NSF has to do the same thing, and it is difficult to specify. The whole program is funded at $18 million; nEDM would probably be $1.5 million of that. NSF was looking at $7.5 million, but all of that is on hold. Jacobs said that the Subcommittee was given two funding profiles: one if the project could be started today (which is not relevant) and one for FY11 plus cost of living. The question is whether the R&D costs would be covered at today's funding levels. It is too early to answer that question. Lung said that Gillo's earlier comments need to be captured in this discussion in the report. A clarification should be made in the Executive Summary. Kumar promised that the Subcommittee would take that comment to heart in the final report. Lung said that it could be as simple as "nEDM is the highest priority of neutron science."

Dodge stated that the researchers do not know what they need to ask for and asked how the Committee was to know if the report's recommendations are reasonable. Jacobs asked what it means to approve an interim report. Henry said that NSAC gives advice to the agencies and formulates that advice as it sees fit. A quick turnaround was needed to provide guidance during budget formulation. This interim report can form the basis for that guidance. The conversation here does not reflect a consensus of what the final report should say. The Committee might choose not to pass judgment until the final report is ready. This is an unusual circumstance. Keister said that there might be a vote on a sense of the Committee. Jacobs asked if that might have any negative consequences to the agencies. Hallman said that NSAC should take the time to make sure that it has it right. The FY13 budget is coming up in the next few months. Kumar noted that the Subcommittee would like to have consensus on the comments and recommendations as they are. The decision-making is done. The Subcommittee just needs to write the report. A modified document with an Executive Summary would be the best path forward. Blackmon said that some of the Committee members are uncomfortable with endorsing statements that are not understood. Jacobs suggested going through the recommendations one by one and getting a sense of the Committee on each. An Executive Summary will be prepared before the next day's session, and all the text can be considered at the next day's session.

Gagliardi noted that there were efforts at NIST that were swept aside and asked if any of those would be in the group of ten. Kumar replied, no.

Seestrom resumed the chair, and the PARR report was discussed to produce enough information to revise the report so the next draft could be voted up or down in the next week.

Opper noted that the findings were approved. There were not any changes to the first comment. A sentence about databases was added to the second comment. A replacement sentence on leaving the sharing of research to individual investigators or collaborations was offered. Zajc suggested substituting "feasibility" or "practicality" for "suitable" so the sentence would become, "Individual investigators, collaborations, or laboratories are uniquely able to determine the feasibility of sharing digitized detector signals." There was consensus among the Committee members to make this change.

In Comment 5, a sentence was added to mention the fact that NSF now requires PIs to post highlights and synopses of their research in lay language. Examples of outreach activities were also added. Mentions of SCOAP[3], HepForge, and NSF's requirement for a synopsis were also to be added.

Furnstahl suggested adding a discussion of codes that are program deliverables [as in the Scientific Discovery through Advanced Computing (SciDAC) program]. Intellectual property rights must also be considered. Seestrom asked Furnstahl to mark up a copy of the text incorporating those changes.

Gardner suggested stating in Comment 4 that workshops and meetings for scientists and specialists are *essential* for communicating and sharing research data and information.

Steven Vigdor (BNL) suggested that the inserted sentence in Comment 3 be made consistent with the first sentence of that paragraph.

Seestrom asked that any other changes be sent to her. She will incorporate the comments and circulate the resultant text to the full Committee and, upon review, will send it on to the agencies.

The floor was opened to general public comment. Vigdor said that significant funds for R&D should be made available outside of the formal Project Structure, which has detailed oversight mechanisms that are not compatible with the uncertain nature of R&D.

Jacobs said that the Executive Summary of the neutron report will be finished overnight. Constructing the priority list will require some time. It was decided to review the recommendations at this time.

In Recommendation 1, Lung suggested distinguishing among science, engineering, and R&D efforts. Klein suggested defining "restructuring." Kumar replied that that dialogue has begun.

In Recommendation 2, the agencies might be asked to consider redirecting funds. It was suggested to delete "as part of the reorganization."

In Recommendation 3, Lung questioned establishing a joint ORNL/LANL TRC..

In Recommendation 4, the TRC will be limited to technical issues; it will not determine the timing of large purchases. Lung noted that Recommendation 4 is written for "large procurements" that, under DOE orders, are not possible until after CD-2 is granted. Long-lead-time procurements can be initiated before CD-2, however. She asked what the intention of the Subcommittee was, to refer to large or to long-lead. Gillo asked the Subcommittee whether they were referring to long-lead procurements or to the CD-2 process. Kumar responded that the Subcommittee had intended to refer to the CD-2 process. Jacobs said that the reference to large procurements was a mistake; the Subcommittee was referring to long-lead procurements. Lung stated that this recommendation could be written more clearly to reflect that technical issues must be resolved before large procurements are made. Keister noted that the major procurements rely on the resolution of the main R&D issues, and the procurements should not be initiated until those issues are resolved. A re-wording should be made to resolve the ambiguity. The TRC does not have anything to do with procurement. The agencies will provide input on the technical issues and mitigation of technical risk. One would not approach CD-2 until the TRC says that it is convinced that the risk is mitigated.

Klein asked who usually makes the decision to go to CD-2. Gillo responded that the Office of Program Assessments (OPA) makes that decision along with internal (national laboratory) reviews. Jacobs reiterated that the TRC is to assess technical risk only.

Cooper noted that the TRC could come up with one recommendation and that the OPA could come up with another. Jacobs commented that one needs to get the right people on both of those panels. Lung noted that having a laboratory director convene a review panel brings expertise and objectivity to the assessment of a large collaboration.

Wilburn asked what the relationship was between the TRC's reviews and those reviews done by the national laboratories prior to bringing a project to DOE for a CD-2 review. After some discussion, Jacobs said that the message is that, in the short term, the national laboratories need to focus their management on the success of the ongoing R&D and less on developing the baseline for the CD-2 review. Later they can re-focus on the requirements of a CD-2 request. There will be some overlap between the TRC and the national-laboratory reviews; that can be worked out and should not be a problem.

The meeting was adjourned for the day at 4:46 p.m.

Friday, July 1, 2011

The meeting was called back into session at 8:29 a.m. by the chair, Susan Seestrom. Dixon Bogert (FNAL), a member of the Underground Science Committee joined the meeting by telephone. **Mark Reichanadter** was asked to present the report from that Committee.

The charge to the Panel was to help define the cost-effective options for planned underground experiments and strategies for implementing a world-class program of underground science consistent with SC's mission in High Energy and Nuclear Physics. This charge was to be accomplished by (1) reviewing the proposed experiments [the long baseline neutrino experiment (LBNE), third-generation dark matter, and 1-ton-scale neutrinoless double-beta decay]; (2) assessing the cost and schedule estimates for deploying these experiments; and (3) providing the "baseline" needed for budget planning and discussion of strategies going forward.

Specifically, the charge called for the Committee to look at eight scenarios; it also looked at a ninth scenario proposed during the course of the study. This review did *not*

- evaluate the compelling nature of the science or set science priorities;
- review the Deep Underground Science and Engineering Laboratory (DUSEL) project;
- consider strategies for the future of DUSEL;
- pick winners and losers; or
- consider the full range of possible sites, alternate technologies, etc.

The assessment attempted to capture the readiness; technical risks; design, construction, and operational costs; and schedule for each of these scenarios.

The Committee considered input from LBNE, Fermi National Accelerator Laboratory (FNAL), the NSF supported DUSEL Project Team, Sanford Laboratory, and Sudbury Neutrino Observatory (SNOLAB) in an intense three days of meetings. In addition, Committee members visited both Homestake mine and SNOLAB. The Committee assessed each of the scenarios in terms of the readiness; technical risks; design, construction, and operational costs; and schedule. It was not a Lehman-type cost/schedule review. Estimates are in current-year dollars and conditions, and inflation can be a very significant cost risk over the time scale of these experiments. Contingency costs were based on design maturity: pre-conceptual 50%; conceptual 40%.

Scenario 1 is an LBNE that uses water Cherenkov detectors at 4850 ft at Homestake. This option was considered viable and the most cost-effective option for LBNE physics. It has a TPC of $1.2 billion to $1.5 billion, including 50% contingency and infrastructure costs. It was noted that shafts need a full upgrade to support safe/effective construction and operations. The experiment calls for a 700-kW beam, not Project X. The water-Cherenkov-detector technology is mature, detailed design on caverns and detectors can begin immediately. The near detector design could be simplified. The primary areas of risk are general underground construction and the complications to beamline design induced by the FNAL site boundary limitation

Scenario 2 is the LBNE using a liquid-argon detector at 800 ft at Homestake, including an R&D program to prove the scalability of liquid-argon technology. Such liquid-argon technology needs multiyear R&D to prove its viability. The earliest date such a decision could be made would be about 2015. The TPC would range from $1.0 billion to $1.4 billion, including a 50% contingency, infrastructure costs, and costs for liquid-argon R&D. At this time, this scenario is considered less cost effective than Scenario 1 because of the additional escalation during the 4 to 5 years needed to complete R&D and the possibility that liquid argon may be very costly. Its design status is pre-conceptual. The FNAL scope of work is comparable for a water Cherenkov or liquid-argon detector. The primary risks are that liquid-argon technology may not be workable or maybe cost prohibitive; the 800-ft level is not well characterized; and there are liquid-argon cryogenic safety concerns in an underground cavern. Dark matter and/or neutrinoless double-beta decay experiments are not viable at the 800-ft level.

Scenario 3 is a third-generation dark-matter experiment at 4850 ft at Homestake. It is a viable, cost-effective option if the LBNE helps support infrastructure costs; it is not considered cost effective as a stand-alone experiment. The estimated TPC is about $0.3 billion, including a 40 to 50% contingency with LBNE sharing the infrastructure costs. The design status is roughly conceptual. The primary areas of risk are whether the additional background at 4850 ft compared to 7400 ft can be mitigated with additional shielding; there are also risks associated with being underground. The U.S. community consensus is that two complementary dark-matter experiments are needed.

Scenario 4 is a ton-scale neutrinoless double-beta decay experiment at 4850 ft at Homestake. This scenario is similar to the previous one. It is a viable, cost-effective option if the LBNE helps support infrastructure costs. It is not considered cost-effective as a stand-alone experiment. The estimated TPC is about $0.4 billion, including a 40 to 50% contingency with LBNE sharing infrastructure costs. The design status is pre-conceptual. The risks foreseen are that neutrinoless double-beta decay experiments at the ton-scale do not exist today; 3 to 4 years of R&D and operating smaller detectors are needed to confirm a path forward. An extensive R&D program is currently under way to determine whether the additional background at 4850 ft compared to 7400 ft can be mitigated with additional shielding. There are also the generic risks from being underground.

Scenario 5 is a third-generation dark-matter experiment at 7400 ft at Homestake. It is considered viable at 7400 ft. It is not considered a cost-effective option because of substantial infrastructure costs and uncertainties at the 7400-ft level. The estimated TPC is about $0.7 billion, including a 40 to 50% contingency.

Scenario 6 is a ton-scale neutrinoless double-beta decay experiment at 7400 ft at Homestake. The conclusions about this scenario are the same as those for Scenario 5. The estimated TPC is about $0.9 billion, including a 40 to 50% contingency.

Scenario 7 is a third-generation dark-matter experiment at 6800 ft at the SNOLAB. It is considered viable at SNOLAB and appears to be the most cost-effective option for dark matter. The estimated TPC is about $0.2 billion, including a 40 to 50% contingency. However, a careful study of infrastructure costs has not been done, only the rough estimate included in the TPC. The design status is roughly conceptual. SNOLAB is an operating underground science lab and has much experience with constructing underground science facilities (e.g., clean rooms and cryogens). The primary areas of risk are the Canadian cost/liability uncertainties and coordinating with a commercial mining operation (which could be a benefit).

Scenario 8 is a ton-scale neutrinoless double-beta decay experiment at 6800 ft at SNOLAB and is very much the same as Scenario 7. It is considered viable at SNOLAB and appears to be the most cost-effective option for neutrinoless double-beta decay. The estimated TPC is about $0.3 billion, including a 50% contingency. However, a careful study of the infrastructure costs has not been done, only the rough estimate included in the TPC. The design status is pre-conceptual. The primary areas of risk are the same as Scenario 7.

The added Scenario 9 is the LBNE 1 + 1, which starts with a water Cherenkov detector at 4850 ft; advances an liquid-argon R&D program to prove scalability; and adds a liquid-argon detector at 800 ft. This design allows the water Cherenkov detector to move forward today while continuing liquid-argon R&D at a modest cost with the aim of adding a liquid-argon detector or another water Cherenkov detector at a later time. Each detector would be smaller than in the single-technology scenarios 1 and 2. Should liquid argon not prove viable, an additional water Cherenkov detector module can be added to do a full neutrino-physics program. The LBNE collaboration favors this scenario. The total cost of 1+1 has not been assessed but is expected to be dominated by infrastructure costs and could be significantly larger than scenarios 1 or 2. More study is needed. The FNAL scope of work is comparable with scenarios 1 and 2. This design would get physics started at a lower initial cost than scenarios 1 or 2; water Cherenkov detectors and liquid-argon detectors have complementary capabilities with different systematics and sensitivities to different final states; and would allow a decision to be made for doing dark matter and neutrinoless double-beta decay at Homestake. The risks are the same as for scenarios 1 and 2, but smaller detectors mean smaller caverns and a reduction in associated risk.

The major conclusions of the Committee are that:

1. At the current level of maturity, the cost estimates for the third-generation dark matter and ton-scale neutrinoless double-beta decay experiments should be taken as accurate to about one significant figure. The cost estimates for the LBNE and associated infrastructure costs are more mature; however, they are not greater than the conceptual design level.
2. The likely approximate costs of the three experiments are: for LBNE, including detectors, beamline, and infrastructure, $1.2 to 1.5 billion; for each third-generation dark matter experiment, $0.1 billion (infrastructure is not included, and this value is site dependent); and for each ton-scale neutrinoless double-beta

decay experiment, \$0.2 to 0.3 billion (infrastructure is not included, and this value is site dependent). The operating costs for the LBNE detector alone in the Homestake infrastructure are \$18 to 23 million per year (FY15 dollars). If one wanted to do dark matter or neutrinoless double-beta decay without LBNE at Homestake, it would cost about \$20 million per year and about \$2 to 3 million per year in marginal operating costs if the LBNE were already established. The same work at SNOLAB would cost about \$2 to 3 million for marginal operating costs; further work is needed to understand if there would be any shared facility/infrastructure operational costs.

3. With LBNE at the 4850-ft level at Homestake, the additional cost of infrastructure to allow construction of a third-generation dark matter or ton-scale neutrinoless double-beta decay experiments would be about \$0.15 billion for the first experiment and \$15 million for each subsequent experiment if infrastructure for all the experiments is done up front. That means that this approach would exceed the infrastructure costs at SNOLAB for a single dark-matter or neutrinoless double-beta decay experiment by something like \$100 million. Adding a second dark-matter or neutrinoless double-beta decay experiment at the Homestake 4850-ft level brings the infrastructure cost to roughly that of SNOLAB's.
4. It is not cost effective to consider third-generation dark matter or ton-scale neutrinoless double-beta decay experiments as stand-alone experiments at Homestake because of infrastructure costs. If one constructed three or more of these experiments at the same level, the infrastructure costs could be shared.
5. Constructing the third-generation dark matter or ton-scale neutrinoless double-beta decay experiments at the 7400-ft level at Homestake appears to be prohibitively expensive because of infrastructure costs and uncertainties. The dark matter experiments can likely be accomplished at the 4850-ft level with additional shielding. Whether shielding can be sufficient for neutrinoless double-beta decay experiments at the 4850-ft level will not be known for several years.
6. Significant investments in infrastructure will be necessary to safely construct, commission, and operate a modern underground laboratory at Homestake. Modernizing the Yates and Ross shafts at Homestake is a necessary prerequisite and should not be considered an opportunity for "value engineering."
7. Constructing a third-generation dark matter or ton-scale neutrinoless double-beta decay experiment at SNOLAB appears to be the most cost-effective option even if a U.S. investment is needed to dig and outfit a pit and provide utilities and other support. This conclusion should be verified by detailed studies.
8. One must recognize that the time needed to carry out the three experiments (LBNE, ton-scale neutrinoless double-beta decay, and third-generation dark-matter experiments) will extend over two decades or more from now, including about one decade before data taking begins. It is likely that, in each case, there will be upgrades and follow-on experiments that will further extend the time scale of these physics programs.
9. Therefore, given the scale of investment needed to carry out these experiments and the long timescales and likelihood of follow-on experiments in each of these areas of research, there are major advantages to developing a common underground site for these experiments despite increased costs. The advantages

include (1) opportunities to share expensive infrastructure and coordinate design efforts, construction, management, and operations and (2) significant benefits in training of the next and subsequent generations of scientists by having a common facility serve as an intellectual center in these fields of research. Locating the facility in the United States would help to promote U.S. leadership in these fields for the foreseeable future.

10. That opportunity could be realized only if the three experiments are funded and if the LBNE technology choice (water Cherenkov vs. liquid-argon TPC) strongly impacts the strategic options for siting third-generation dark matter and ton-scale neutrinoless double-beta decay experiments. If the LBNE choice is a water Cherenkov detector at the 4850-ft level at Homestake, then the third-generation dark matter and/or ton-scale neutrinoless double-beta decay experiments at the 4850-ft level becomes significantly more cost effective. If the LBNE technology is a liquid-argon detector closer to the surface, then this would not be so. There is a very significant strategic benefit to making the LBNE technology choice as soon as possible.
11. A "1+1 Option" for LBNE may make possible considerable physics advantages because of complementary detectors, but further study is necessary. Implementing a water Cherenkov detector initially, while continuing with liquid-argon R&D for possibly adding this capability later would be consistent with sharing infrastructure among the LBNE, neutrinoless double-beta decay, and dark matter experiments at the Homestake 4850-ft level.

In summary, the Committee believes there are compelling scientific motivations for all three experiments and an important opportunity for the United States to take a leadership position for the foreseeable future. It also believes that there are important advantages and opportunities in developing a common site for these experiments if the needed infrastructure can be shared in a cost-effective manner. A common site only works in the scenario where LBNE has one or more detectors at 4850 ft at Homestake. Either an early technology choice for water Cherenkov or the 1+1 option would support this scenario, but it may be several years before it is known if neutrinoless double-beta decay is feasible at 4850 ft. If LBNE pays for the infrastructure that LBNE needs, there would be additional infrastructure costs for dark matter or neutrinoless double-beta decay experiment. Those costs would exceed those at SNOLAB by something like $100 million. The Committee believes that these added costs are worthwhile given the advantages of a common site and the multi-decade timescale. If there is no LBNE at the Homestake 4850-ft level, neutrinoless double-beta decay and dark matter are not cost effective at Homestake. The lowest-cost option for dark matter or neutrinoless double-beta decay is SNOLAB.

Klein pointed to the $100 million difference in costs between Homestake and SNOLAB and asked why it was so much more expensive to do these experiments in the United States, saying that the United States was pricing itself out of the science. Reichanadter replied that SNOLAB has the caverns, mine safety team, and utilities already. One has to pay for them to be emplaced at Homestake. Infrastructure costs were the driver. Some of those would have to be paid for by the dark-matter and double-beta-decay experiments, whereas they are already available at SNOLAB. Seestrom pointed out that there was not an estimate for the same facilities at SNOLAB as would be constructed at Homestake. The cavity at Homestake would be much larger and accommodate more experiments. Reichanadter agreed that the comparison was not apples to apples. Kevin Lesko (LBNL) said that the studies at Homestake were for high-watermark experimental facilities. The DUSEL team was confident that a 65-m cavern could be built.

Jacobs asked what the international competition for the LBNE was. Klein replied that there is nothing that is ongoing, but Japan is planning a facility with a similar science reach. People do not think that the LBNE will go forward because it is a big project and the United States has a history of not completing big projects.

Lu said that Majorana is a U.S.-led effort and asked if the leadership would be shared by the Canadians if the experiment went to SNOLAB. Reichanadter observed that the Europeans have taken over the leadership of high-energy physics with the LHC. There is a similar risk with SNOLAB. John Wilkerson (University of North Carolina and ORNL) said that, with a 1-ton germanium experiment, significant international and cost-sharing partnerships are expected. The Canadians said their contributions would be proportional to the number of Canadians involved. One would expect a substantial European participation. Blackmon asked whether going to the 4850-ft level was a detriment. Wilkerson replied that, for a 1-ton experiment at 4850 ft, one is exposed to cosmogenic-induced backgrounds that cannot be vetoed. Germanium-77 and xenon are susceptible to cosmogenic isotope production by secondary neutrons. One reduces that flux significantly by going deep. One cannot guarantee that a 1-ton experiment would work at 4850. There is much more confidence at 7400. The dark matter and double beta decay experiments also have concerns about cosmogenic backgrounds.

Lu observed that the host country of an accelerator gets great benefits in education and leadership. However, in astronomy, one goes to where the telescopes are and does not lose leadership. Reichanadter responded that there is a critical mass of personnel at an accelerator. The telescopes do not replace the social interaction that happens at the CERN cafeteria. Klein pointed out that, if the United States goes to SNOLAB and decides to pursue double-beta decay, the Canadians will decide what experiment to employ, not the United States. Seestrom reminded the Committee that the cost-effective judgment was based on one experiment. If three experiments were put in, it would be cost effective with or without the LBNE.

Blackmon pointed out that there were other, smaller facilities planned for DUSEL and asked if the Committee had discussed them. Reichanadter replied, no; the Committee stuck to its charge. Blackmon asked if this was something the NSF should comment on. Keister said that the Physics Division of NSF was interested in several experiments, but they could not drive the decision.

Zajc asked if the cost effectiveness of two dark-matter experiments and a double-beta-decay experiment was mentioned in the report. The report seemed to presume that LBNE was a prerequisite. Seestrom said that that is not stated in the report. Bogert said that the only experiment for which costs were estimated was LBNE. Not explicitly stating that may be a deficiency in the report. Hallman confirmed that the study had not looked at three experiments that did not include LBNE.

Langanke asked why two dark-matter experiments would be conducted at Homestake. James Symons (LBNL) replied that there are a lot of different parameters that are uncertain in dark matter. At least two approaches are needed. One would like two detectors in one room. Lesko added that the menu for pricing options would allow one to cost a Plan B. Even with local disruptions, work continues in South Dakota with the installation of instruments planned for early next year.

Gardner pointed out that there is also an argument for multiple double-beta-decay detectors, as well. A rich non-LBNE environment may be very attractive.

William Brinkman was introduced to make his comments. He thanked the Committee for its hard and excellent work. He pointed out that DOE needs to know where this research is going before constructing the FY13 budget. There is money in the FY12 budget to keep the Homestake mine going. Recent research seems to indicate that θ_{13} for neutrino mixing seems to be nonzero. That will make it more possible to see something. With the LBNE, one could do real physics. Congress wants to keep funding at the constant level rather than the doubling path that the President is requesting. It is desirable to keep basic science alive and productive. It is hoped that this conversation will get sorted out in the next few months. The National Science Board (NSB) asked that the management of DUSEL be rethought, and that needs to be done, also.

Hallman added that, in dark matter and double-beta decay, one may not see a signal, but good science would still come from the effort.

Klein asked when a decision will be made. Brinkman replied that a plan is needed. Options can be kept open in FY13, but one needs to be more specific beyond that. Where the FNAL goes after the shutdown of the Tevatron is important.

Dodge asked if DOE could pick up the infrastructure costs and NSF could pick up more experimental costs. Brinkman said that he did not know the answer to that question.

Lesko said that, if one looks at the collaborations involved in dark matter, they are overwhelmingly supported by NSF. In double-beta decay and the LBNE, more than half the groups are supported by NSF. NSF needs to be re-engaged in the science.

Seestrom thanked Brinkman for participating in the discussion.

A break was declared at 9:46 a.m. The meeting was called back into session at 10:01 a.m. by Chair pro tem, Jacobs, to discuss the neutron report further. He noted that endorsement of this report at this time is not critical to the agencies.

A number of experiments in addition to nEDM had been reviewed:

- Nab: A precise measurement of *a*, the electron-neutrino correlation parameter, and *b*, the Fierz interference term in neutron beta decay, at the Fundamental Neutron Physics Beamline at the SNS with a novel electric/magnetic field spectrometer and detector design to determine *a* to an accuracy level of 3×10^{-3}
- Ultracold neutrons: UCN-A will probe neutron decay with unprecedented precision by measuring the correlation between a neutron's spin and its decay electron's momentum (the "A-correlation") with a factor of 4 improvement in precision
- aCORN ("an electron-antineutrino co-relation in neutron decay" at NIST): Aiming to get to better than 1% precision in the *a* coefficient
- Radiodecay (NIST): Measures the neutron radiative decay; if one gets beyond 1%, one gets some interesting angle correlations
- In-beam method to measure the neutron lifetime (NIST): has made incredible progress in the systematic control of neutron-proton counting it will likely get to the 1-second level

- Cold-beam-based lifetime measurement at NIST: It has hit several technical problems; it is working its way through these snags
- Magneto-gravitational trap: Has potential but will not likely get to the 1-sec level in a finite amount of time
- NPDGamma: The apparatus is nearly done; production runs are scheduled; aiming at a statistical error of 10^{-8} and a systematic error of 10^{-9}
- Neutron–helium-3: Requires new instrumentation; they are doing R&D on that now; the goal is to get to a statistical error of 10^{-8}
- Neutron spin rotation: Now at 10^{-6}; it hopes to get to 10^{-7} in a few years
- emiT: They have completed measurements of the triple-correlation term of the D coefficient, neutron spin direction, and the momenta of the electron and antineutrino in the beta decay of polarized neutrons; there is no plan to go on to the next level

Jacobs pointed out that the experimental prioritization was based on the projected sensitivities' producing new physics. He said that the Executive Summary has been drafted and it was e-mailed to the Committee. Blackmon asked about a discussion of the "constant level of effort." Kumar said that that is in the report and asked if it should be in the Executive Summary. Jacobs offered to mention that part of the funding is MIE. Kumar said that the report also assumes that Nab is funded; it would be possible to consider putting that discussion in the Executive Summary, also.

Lung said that this report implies that nEDM is included in the constant-level effort, and it is not. Only the R&D for nEDM is included. Jacobs offered to put an explicit statement to that effect in the Executive Summary and to include a mention of Nab, also.

Jacobs asked if the establishment of a standing subcommittee to review the neutron physics was a consensus of the Committee. Klein asked if this were a perpetual subcommittee. Kumar said that the view is that it would be reconstituted every 2 or 3 years and meet once a year. Symons asked what was special about neutrons that they should get a separate subcommittee. Kumar said that neutron science is something that does not happen only at one laboratory; as a result, it does not have a laboratory science advisory committee to assess the field. Gardner said that it should be ensured that the membership of the standing subcommittee was rotated when it is reconstituted every 2 to 3 years.

Kumar noted that other changes were made to the report in response to the comments of the Committee during the previous day's discussions.

The consideration of the recommendations, begun the previous day, was resumed with the revised version of Recommendation 4. Lung confirmed that the revision met her concerns of the previous day.

Recommendation 5 was reviewed. Gagliardi said that if recommendations 1 and 2 were removed, the R&D would not appear in the report. Kumar replied that the assumption is that there will be enough text before Recommendation 4 to make it stand. Jacobs said that recommendations 1 and 2 will not disappear but will be re-expressed.

Recommendation 6 was reviewed. Klein requested consideration of the case where R&D problems delay the US program *and* the foreign competition beyond the 2 years allocated for that research. Kumar said that the assumption is that if R&D issues are not solved in 2 years, a new direction should be undertaken, and the situation should be reconsidered. Lung pointed out that the Subcommittee did not mean to refer to the "start of construction," which is CD-3. Kumar agreed; that reference needs to be redefined in terms of CD-2.

Recommendation 7 was reviewed; it is the first of three recommendations on physics considerations. Langanke said that he was of the understanding that Recommendation 9 was the recommendation of highest priority. Kumar replied that the Subcommittee may reorganize these recommendations as 9, 8, and 7. Jacobs said that the phrasing on magneto-gravitational traps needs to be reworked. Muhammad Arif (NIST) said that the magneto-gravitational-trap experiment was a Harvard effort that was hosted at NIST; it is not a NIST experiment.

Recommendation 8 was reviewed; it garnered no comments.

Recommendation 9 was reviewed; it garnered no comments.

Kumar said that he would ask the Subcommittee about the priorities for incremental funding and reflect the answer in the recommendations.

Wilkerson noted that the Subcommittee had asked experimenters their level of commitment to each experiment and he asked if the report would reflect that level of manpower commitment in the readiness assessment. Kumar replied that there was one sentence in the report to that effect. Jacobs inserted that there is a sufficiently robust workforce to support the recommendations. Wilkerson asked if the Subcommittee had checked for overlap. Jacobs said that it had done a cursory check.

Arif asked if the Subcommittee would recommend that experiments that are interesting but not ready yet should receive support. Kumar said that the Subcommittee had already made statements on Nab. It would rely on the standing subcommittee to make assessments and recommendations in the longer term. Jacobs noted that there was an extant prioritization. Perhaps it should be brought to the table. Arif said that backup experiments should be supported in case something goes wrong.

Jacobs observed that the Subcommittee has more work to do to finalize the report by September. Gagliardi asked whether the approval of the final report would be done by e-mail or at another meeting of NSAC. Keister said that an e-mail vote could be conducted if there were not a lot of discussion. Gagliardi said that it would be a big help if the revised recommendations 1 and 2 could be circulated as soon as possible. Kumar said that he would get the modified recommendations 1 and 2 and the Executive Summary out to the Committee members as soon as possible. He asked if September 1 was the date for sending the final report to NSAC or the date that NSAC was to send the final report to the agencies. Henry replied that the agencies want a high-quality product. If that requires some extra time, that would be acceptable.

Jacobs asked if NSAC wanted to take a vote and indicate that the Subcommittee was headed in the right direction. The consensus was that the Subcommittee was heading in the right direction, that there was no need to accept the preliminary report, and that the vote to accept the final report would be sufficient.

Keister, Henry, and Hallman noted that the Subcommittee had handled a complex and difficult charge with great diligence and expertise and they thanked the members for their extraordinary efforts.

Seestrom resumed the chair and said that the date of the next meeting may be moved up from November/December to September if a review and vote of acceptance of the neutron report were needed.

The floor was opened to new business and public comment. There being none, the meeting was adjourned at 10:56 a.m.

These minutes of the Nuclear Science Advisory Committee meeting held at the Crystal City Marriott at Reagan National Airport, Arlington, Virginia, on June 30-July 1, 2011, are certified to be an accurate representation of what occurred.

Susan J. Seestrom
Chair, Nuclear Science Advisory Committee