NSAC Activities—2003-2005

CHARGES (short titles)	CHAIRS	STATUS
acilities	Charlie Glashausser Rutgers	Completed
undamental NP with eutrons	Bob Tribble TAMU	Completed
uclear Theory	Berndt Mueller Duke	Completed
ducation	Joe Cerny U.C. Berkeley	Completed
erformance Measures/ ilestones	Don Geesaman ANL	Completed
ommittee of Visitors	John Cameron Indiana	Completed
SI/RIA	Peter Bond BNL	Completed
HI	Peter Barnes LANL	Completed
nplementing the 002 LRP	Bob Tribble TAMU	Completed
luSAG – Neutrinoless ouble-beta decay	Gene Beier (U. Penn) Peter Meyers (Princeton)	Completed

U.S. Department of Energy and the National Science Foundation

March 7, 2005

Professor Frederick Gilman
Chair, HEPAP
Carnegie-Mellon University
5000 Forbes Avenue
Pittsburgh, PA 15213

Professor Richard F. Casten
Chairman, NSAC
Wright Nuclear Structure Laboratory
Yale University
New Haven, CT 06520

Dear Professors Gilman and Casten:

This letter is to request that, in response to the Office of Science & Technology Policy led interagency working group report on a federal strategy for the Physics of the Universe, you form a subcommittee to address issues involving neutrinos that cross disciplinary and agency boundaries. Specifically, we ask that the High Energy Physics Advisory Panel (HEPAP) and the Nuclear Science Advisory Committee (NSAC) establish a Neutrino Scientific Assessment Group (NuSAG) as a joint sub-committee to advise the Department of Energy (DOE) Offices of Nuclear and High Energy Physics and National Science Foundation Programs of Nuclear Physics and Elementary Particle Physics on specific questions concerning the U.S. neutrino physics program.

There has been a growing recognition of the important role played by neutrinos in answering some of the most compelling questions in subatomic physics. Two National Research Council studies (*Quarks to the Cosmos, Neutrinos and Beyond*), two long range planning exercises (HEPAP and NSAC), and most recently a multi-divisional year-long American Physical Society (APS) study have all identified compelling discovery opportunities involving neutrinos. These studies laid the scientific groundwork for the choices that must be made during the next few years. They did an excellent job of explaining the new paradigm of neutrino science, why this science is filled with important and interesting questions, and why the time is right to address these questions.

It is clear that a number of experimental directions should be pursued, but none of the studies mentioned made recommendations on particular projects. For those directions where the timescale is long-term, we will wait to take advantage of additional input, such as from the National Academy Sciences study on Elementary Particle Physics (EPP2010). However, for those directions where expeditious action is appropriate, we ask the NuSAG to make recommendations on the specific experiments that should form part of the broad U.S. neutrino science program. In addition, on a similar time line to NuSAG, the NSAC will be reviewing the full DOE Nuclear Physics program. Timely recommendations from NuSAG will be important input for this review.

NuSAG will be constituted for a fixed period of two years as a joint subpanel of HEPAP and NSAC. It will report to the agencies though HEPAP and NSAC who will consider its recommendations for approval and transmittal to the agencies.

The recommendations of the APS Neutrino Study form the basis for the first three charges for NuSAG listed below.

Charge 1

We request that NuSAG address the APS Study's suggestion that the U.S. participate in *"An expeditiously deployed multidetector reactor experiment with sensitivity to ν_e disappearance down to $\sin^2 2\theta_{13}$=0.01, an order of magnitude below present limits."*

The options to be considered should include, but need not be limited to:

- A U.S. experiment (in Diablo Canyon, CA, Braidwood, IL, or elsewhere)
- U.S. participation in a European reactor experiment (Double Chooz or elsewhere)
- U.S. participation in a Japanese reactor experiment
- U.S. participation in a reactor experiment at Daya Bay, China.

Charge 2

NuSAG is requested to address the APS Study's recommendation of a phased program of sensitive searches for neutrino-less nuclear double beta decay. In particular, a timely assessment of the scientific opportunities and resources needed should be performed of the initiatives that are presently under discussion in the research community. These include, but should not be limited to:

- U.S. experiments (Majorana, EXO, others)
- U.S. participation in an Italian experiment (Cuoricino/Cuore)
- U.S. participation in a Japanese experiment (Moon).

Charge 3

We request that NuSAG address the APS Study's suggestion that the U.S. participate in *"A timely accelerator experiment with comparable $\sin^2 2\theta_{13}$ sensitivity [to the recommended reactor experiment, i.e. $\sin^2 2\theta_{13}$=0.01]* and sensitivity to the mass-hierarchy through matter effects."

The options to be considered should include, but not be limited to:

- U.S. participation in the T2K experiment in Japan
- Construction of a new off-axis detector to exploit the existing NUMI beamline from Fermilab to Soudan, as proposed by the Nova collaboration
- As above but using a large liquid argon detector.

Within each of these three charges, NuSAG should consider the various initiatives that have been proposed. NuSAG should look at the scientific potential of each initiative, the timeliness of its scientific output together with the likely costs to the U.S., and its place in the broad international context. In addition, for the off-axis initiatives (charge 3), the context should include a consideration of what is likely to be learned from other experiments, and the likely future extensibility of each option as part of an evolving U.S. neutrino program. For all three charges NuSAG should then recommend a strategy of one (or perhaps more than one) experiment in that direction, which in its opinion should be pursued as part of the U.S. program.

It is requested that the NuSAG Report be sent to HEPAP and NSAC by no later than the end of June 2005.

We thank you for your help in establishing this advisory group; its input is very important. We look forward to working with you in this endeavor.

Sincerely,

Dennis Kovar
Associate Director
Office of Nuclear Physics
Department of Energy

Robin Staffin
Associate Director
Office of High Energy Physics
Department of Energy

Michael S. Turner
Assistant Director
Mathematical and
Physical Sciences
National Science Foundation

Recommendations for a United States Program

The Neutrino Scientific Assessment Group recommendation below provides guidance for both the near-term activities in neutrino-less double beta decay and for the mid-term goals of the discipline. The panel finds that it is important for the program in neutrino-less double beta decay to develop detector technology to explore the inverted neutrino mass hierarchy region. At the present time, the most promising isotope and technology for a detector at the one ton scale cannot be identified.

Recommendation: The Neutrino Scientific Assessment Group recommends that the highest priority for the first phase of a neutrino-less double beta decay program is to support research in two or more neutrino-less double beta decay experiments to explore the region of degenerate neutrino masses ($\langle m_{\beta\beta} \rangle > 100$ meV). The knowledge gained and the technology developed in the first phase should then be used in a second phase to extend the exploration into the inverted hierarchy region of neutrino masses ($\langle m_{\beta\beta} \rangle > 10-20$ meV) with a single experiment.

For the region of degenerate neutrino masses, NuSAG recommends the following implementation strategy for the specific experiments. The following three experiments, listed in alphabetical order, have the highest priority for funding.

- **CUORE:** The CUORE ^{130}Te experiment has potential for good energy resolution and low background, provided the technology develops as planned. The high natural abundance of ^{130}Te results in a relatively low cost for a detector sensitive to the degenerate neutrino mass region. The cost of enriched ^{130}Te needed to extend the sensitivity is lower than for some other isotopes. The schedule presented by CUORE is timely. The panel is concerned that the requested budget share is not commensurate with the U.S. involvement in the project.
- **EXO:** The EXO-200 ^{136}Xe experiment is presently under construction and should continue to be supported. R&D for barium tagging is a priority as a step to a one ton scale $0\nu\beta\beta$ experiment. If barium tagging is successful, EXO may offer a unique and cost effective approach to a one ton or larger experiment.
- **Majorana:** The excellent background rejection achieved from superior energy resolution in past ^{76}Ge experiments must be extended using new techniques. The panel notes with interest the communication between the Majorana and GERDA ^{76}Ge experiments which are pursuing different background suppression strategies. The panel supports an experiment of smaller scope than Majorana-180 that will allow verification of the projected performance and achieve scientifically interesting physics sensitivity, including confirmation or refutation of the claimed ^{76}Ge signal. A larger ^{76}Ge experiment is a good candidate for a larger international collaboration due to the high cost of the enriched isotope.

The following two experiments, listed in alphabetical order, have a lower priority for funding.

MOON: The MOON ^{100}Mo detector is in a state of R&D and expects to have a proposal for a 200 kg detector in 2007. Support beyond the R&D phase is not a priority at this time.

Super-NEMO: The Super-NEMO ^{82}Se experiment is entering an R&D phase to prepare for a 100 kg detector. Super-NEMO does not have a convincing path to explore the inverted hierarchy neutrino mass region at present. Support is not a priority.

To include some financial realism in the NuSAG recommendations, only a few approaches, those that in our opinion have the best chances of success, have been given the highest priority for the US program. It should be evident that other national programs may decide to prioritize different projects. This would be beneficial as it would broaden the range of techniques explored which, in turn, would result in a more objective selection of the optimal technique to be pursued to the next stage. We also recommend a concerted effort to improve calculation of the $0\nu\beta\beta$ decay nuclear matrix elements in relevant nuclei. This would require sustained and targeted investment in low-energy many-body theory and would lead to a better constrained experimental value of the effective mass if double-beta decay is seen in the first or later phase experiments.

U.S. Department of Energy
and the
National Science Foundation

March 14, 2005

Professor Richard F. Casten
Chairman
DOE/NSF Nuclear Science Advisory Committee
A.W. Wright Nuclear Structure Laboratory
Yale University
New Haven, CT 06520

Dear Professor Casten:

In 2002, the Nuclear Science Advisory Committee (NSAC) completed work on a Long Range Plan for nuclear science for the decade. This plan recommended, with highest priority, the exploitation of the opportunities for scientific discoveries made possible by recent investments – especially at the new facilities, the Relativistic Heavy Ion Collider (RHIC), Continuous Electron Beam Facility (CEBAF) and National Superconducting Cyclotron Laboratory (NSCL). Funding above the FY 2001 constant-effort level (+15%) was identified as needed to effectively utilize the program's facilities and mount strong university and theory programs. In addition, the plan recommended the development of new research capabilities that required funding above this funding level. These included construction of a world-class Rare Isotope Accelerator (RIA) facility, construction of the world's deepest underground laboratory and the upgrade of CEBAF to 12 GeV.

Since the issuance of the Long Range Plan, the resources needed to implement the recommended program have not been identified by the agencies. In the FY 2002-2005 period, funding for the Department of Energy (DOE) Nuclear Physics program has been at a near constant-effort level. The FY 2006 President's Budget Request for Nuclear Physics of $370.4 million is an 8.4% reduction from FY 2005 Appropriations ($404.8 million). At this funding level, the Nuclear Physics user facilities will operate at ~65% of optimum operations and there will be a ~10% reduction in the number of researchers and graduate students supported by the program. This funding level, projected into the outyears, is not sufficient to maintain the scope of the present Nuclear Physics program and, in particular, to continue operations of the program's two major facilities, RHIC and CEBAF, as they are presently conducted. The major initiatives recommended in the Long Range Plan, such as RIA, are not accommodated. In light of these projected budgetary stringencies and their implications for the U.S. Nuclear Physics program, the priorities and recommendations of the 2002 Long Range Plan need to be revisited. A strategic plan on how to implement the highest priority science in the context of available funding and world-wide capabilities needs to be developed.

In FY 2005 the DOE Nuclear Physics program has world-leading research efforts in the major areas of nuclear science. NSAC should examine the existing research capabilities and scientific efforts, assess their role and potential for scientific advancements in the context of international efforts and determine the time and resources (the facilities, researchers, R&D and capital investments) needed to achieve the planned programs. NSAC should then identify and evaluate the scientific opportunities and options that can be pursued at different funding levels for mounting a world-class, productive national nuclear science program.

Your report should provide recommendations on the priorities for an optimized DOE nuclear science program over the next five years (FY 2007-2011), under the following scenarios:

- Flat-flat funding at $370.4 million, actual dollars
- Constant effort funding (starting with $370.4 million in FY 2006), inflated dollars
- Funding levels needed to restore research capabilities and scientific programs to mount an optimized program and to address the scientific opportunities identified in the 2002 Long Range Plan in order of their priority.

The report should discuss what scientific opportunities will be addressed, and what facilities and instrumentation capabilities will be used and developed by the DOE Nuclear Physics program, including those supported by the National Science Foundation and outside the United States, in mounting a productive, forefront program at each of the funding scenarios. For each funding scenario, the report should articulate what scientific opportunities and capabilities can and cannot be pursued, the impacts on training nuclear scientists, and how major initiatives such as RIA should be viewed.

NSAC should submit the report by the end of June 2005. We are aware that this is a difficult task. However, the involvement and input of the research community is essential for decisions that would restructure the nuclear physics portfolio in times of fiscal constraint. Your report will provide critical guidance as we go forward.

Sincerely,

Raymond L. Orbach
Director
Office of Science

Michael S. Turner
Assistant Director
Directorate for Mathematical
and Physical Sciences

cc:
Bradley D. Keister, NSF
Joseph Dehmer, NSF
Dennis Kovar, DOE

Membership List of the NSAC Subcommittee Implementation of 2002 Long Range Plan

Ani Aprahamian (Notre Dame)
Peter Barnes (LANL)
Gordon Cates (Virginia)
Don Geesaman (ANL)
Charles Glashausser (Rutgers)
Edward Hartouni (LLNL)
David Hertzog (Illinois)
Xiangdong Ji (Maryland)
Gail McLaughlin (North Carolina State)
Curtis Meyer (Carnegie-Mellon)
Alice Mignerey (Maryland)
Richard Milner (MIT)
Berndt Mueller (Duke)
Witek Nazarewicz (Tennessee)
Michael Ramsey-Musolf (Cal Tech)
Hamish Robertson (Washington)
Brad Sherrill (Michigan State)
Michael Smith (ORNL)
James Symons (LBNL)
Bob Tribble (Texas A&M), Chair
Steve Vigdor (Indiana)
Bill Zajc (Columbia)
Rick Casten (Yale), ex-officio

Decades of careful planning and domestic and foreign investment into unique facilities have resulted in many important discoveries and remarkable payoffs. The subcommittee recognizes that under either scenario, the nation and its foreign partners will suffer a tremendous loss in science and the U.S. will no longer be able to maintain international leadership in at least one of the subfields of nuclear science. Because of the superb science lost in both scenarios, the committee was not able to make a choice based on scientific merit alone. The present budget scenario, however, represents a crisis that would preclude running both large facilities simultaneously and force an immediate choice while RHIC is still in its initial discovery phase. Based on this additional consideration, the subcommittee, while split in its decision, has a slight preference for the choice that maintains operations at RHIC. If such a budget exercise were to occur in the future, for instance, with the Jefferson Lab 12-GeV Upgrade well underway, a different choice might well be made.