REPORT TO THE
NUCLEAR SCIENCE ADVISORY COMMITTEE

Neutrinoless Double Beta Decay

NOVEMBER 18, 2015

EXECUTIVE SUMMARY

In March 2015, DOE and NSF charged NSAC Subcommittee on neutrinoless double beta decay (NLDBD) to provide additional guidance related to the development of next generation experimentation for this field. The new charge (Appendix A) requests a status report on the existing efforts in this subfield, along with an assessment of the necessary R&D required for each candidate technology before a future downselect. The Subcommittee membership was augmented to replace several members who were not able to continue in this phase (the present Subcommittee membership is attached as Appendix B). The Subcommittee solicited additional written input from the present worldwide collaborative efforts on double beta decay projects in order to collect the information necessary to address the new charge. An open meeting was held where these collaborations were invited to present material related to their current projects, conceptual designs for next generation experiments, and the critical R&D required before a potential down-select. We also heard presentations related to nuclear theory and the impact of future cosmological data on the subject of NLDBD. The Subcommittee presented its principal findings and comments in response to the March 2015 charge at the NSAC meeting in October 2015.

The March 2015 charge requested the Subcommittee to:

- Assess the status of ongoing R&D for NLDBD candidate technology demonstrations for a possible future ton-scale NLDBD experiment.
- For each candidate technology demonstration, identify the major remaining R&D tasks needed ONLY to demonstrate downselect criteria, including the sensitivity goals, outlined in the NSAC report of May 2014. R&D needs for candidate technology demonstrations should be sufficiently documented beyond assertion to allow critical examination by the panel and future assessments.
- Identify the time durations needed to accomplish these activities and the corresponding estimated resources, as reported by the candidate technology demonstration groups.

Guided by the R&D assessments provided by this NSAC subcommittee and within funding availability, NSF and DOE plan to move forward in a coordinated, unified approach to address these R&D needs, similar to the process used in the recent joint effort on the second generation dark matter experiments. That process included independent calls for proposals with coordinated requirements, and a joint review. The role of this NSAC subcommittee is to assess the proposed R&D plans for each technology, but not to review the relative value of the plans for one technique vs. another.

In general, the suite of mid-scale experiments and demonstration projects are making good progress in setting new $0\nu\beta\beta$ limits and in testing out techniques that can be extrapolated to ton-scale installations. During the next 1-2 years many techniques will be acquiring data and producing a body of information that will inform the future plans. However, it is already clear that additional R&D issues must be resolved in preparation for a future downselect decision.

Therefore the subcommittee strongly recommends that R&D efforts aimed at solving specific technical issues relevant to the downselect decision be supported.

The additional R&D required for each technology was presented to the subcommittee and discussed with the collaborations. For several methods, it is clear that a few well-defined issues need attention, and a 2-3 year R&D program offers good prospects for resolution. For the most part, the cases identified in this report will be straightforward to address through further review. Other technical issues have more open-ended R&D requirements to address. In these cases the allocation of resources will be more difficult to assess. In any case, the longer term future of NLDBD will require continued R&D effort. **The subcommittee strongly urges continuation of longer term R&D necessary for the future development of the subject in addition to the support of shorter term R&D aimed at a near future downselect.**

It was noted by the subcommittee that there are several common R&D topics that would benefit several different techniques. It seems in these cases that a coordinated approach could be a more efficient use of resources. **The subcommittee suggests that the funding agencies consider an approach that would encourage several groups to work together on these common goals.**

There is clearly substantial, and growing, international interest in NLDBD. The decision by the US community on its strategy for the next generation experiment will necessarily involve consideration of the international context. Coordination with the international community will clearly be a necessary component in future decisions on technology selection.

The subcommittee also reviewed the scientific context and motivation for NLDBD experiments. There are many developments in cosmology and in accelerator-based neutrino oscillation experiments that bear on the issues of mass hierarchy and absolute neutrino mass which are highly relevant to NLDBD. One can anticipate further developments that impact the interpretation of NLDBD experiments in the next few years. For example, potential new physics that may emerge from LHC experiments or results from sterile neutrino experiments can have substantial impact on NLDBD, perhaps increasing the already strong motivation for future NLDBD experiments. Furthermore, it is important to remember that NLDBD has a unique role in potentially addressing the issue of Dirac vs. Majorana nature of neutrinos. The Subcommittee remains convinced that the scientific case for pursuing NLDBD experiments at the ton-scale is very compelling.

The subcommittee also considered the progress in nuclear theory of NLDBD, which is important in providing guidance to the experiments and is crucial to the interpretation of the experiments in the context of models of lepton-number violation. There appears to be a trend towards increasing and broadening the community of nuclear theorists working on this problem and towards employing the most modern theoretical techniques. Indeed a proposed topical theory collaboration to address the issue of calculation of the nuclear matrix elements, including quantification of the uncertainties, has been approved for funding by the DOE Office of Nuclear

Physics. Given these developments, it is reasonable to expect that the uncertainty in the nuclear matrix elements will be significantly reduced in the near future.

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1.0 OVERVIEW

Introduction

The experimental study of neutrinoless double beta decay (NLDBD) is an important component of an international effort to study the properties of neutrinos. The subject received tremendous impetus from the 1998 discovery of neutrino oscillations by the SuperKamioande experiment and the 2002 discovery of neutrino flavor transformation as the solution to the solar neutrino problem by the SNO experiment. These two experiments were recoqnized by the award of the 2015 Nobel prize in physics to Arthur B. McDonald and Takaaki Kajita. The study of NLDB is viewed as addressing some of the key remaining questions in neutrino physics today.

The previous report [1] of this subcommittee, received by NSAC in April 2014, described in some detail the status of experimental efforts in neutrinoless double beta decay (NLDBD), some guidelines for consideration of future next generation experiments, and the status of nuclear theory calculations. In this report we address the new charge (Appendix A) from DOE/NSF to update the status of existing efforts and to assess the necessary R&D required for each candidate technology before a potential future downselect. In this overview, we address some overall themes and issues relevant to the whole field and related areas of neutrino physics and cosmology. The following section 2 discusses the recent progress of the various NLDBD projects since our last report and the R&D proposed for each technology by the proponents. Section 3 presents an update on the status of nuclear theory relevant to the evaluation of nuclear matrix elements involved in the interpretation of NLDBD experiments.

General Considerations

Except for the SuperNEMO approach, all other proposed NLDBD techniques involve calorimetry with the decaying isotope embedded in the calorimeter material. In addition to selection of events where the observed energy deposited is at the unique expected Q-value, various other methods are used to identify real NLDBD events from potential background events.

As we stressed in our previous report, the reduction of background rates and efficient rejection of residual background events are essential features in realizing a technology that can be scaled up to multi-ton scale. However, it is useful to note that there are two approaches to the scaling problem. One approach involves modular crystal calorimeters that can, in principle, be shown to perform to the necessary specifications in small quantities. Then one can propose to just duplicate these modules many times to achieve the larger masses necessary for the ultimate desired sensitivity. If one can sufficiently demonstrate the performance in the individual modules, then the performance of the complete system is, in principle, fully determined. In addition, it is possible to improve the performance of additional modules based on measurements with the previously deployed modules or other new information.

The other approach is to achieve the scaling to large masses via an increase in the scale of a single monolithic detector system. A feature of this approach is that externally generated backgrounds only produce events near the surface of the detector and selection of an inner fiducial volume, perhaps coupled with an analysis of the spatial distribution of remaining events, is used to mitigate the external backgrounds.

The modular and monolithic approaches both offer advantages and disadvantages. However, it is not possible to firmly conclude which approach will be optimal at this point and it is certainly prudent to pursue both approaches in this R&D phase of the subject. This is certainly the case at present, and will likely continue to be the situation for at least a few more years.

In our first report, we also stressed a conservative approach to the characterization of the sensitivity of proposed next generation experiments. In particular, we advocated using the smallest of the available nuclear matrix elements for each isotope and quoting a lifetime for a 3σ discovery in the complete inverted hierarchy region. We were pleased to see that many of the groups have now adopted this uniform standard for their ultimate sensitivity goals, which provides a useful basis for comparison. In addition, it seems that at least for several of the methods this goal translates into a roughly common goal for background in the range of 0.1 counts per ton of isotope per year in the region of interest (ROI). The subcommittee generally agrees with this approach to projecting the goals of future experiments and would like to see this trend continue. Indeed, achievement of this rate of background in the next generation experiment seems like a desirable goal for the near-term R&D program.

Table 1 summarizes the currently active projects along with some of their attributes. As one can see from the next section of this report, the present efforts are making good progress towards their goals. We also update the timeline we previously presented to reflect the latest information from the collaborations (see Figure 2.2). It is still quite clear that much more information will be available in 2-3 years as these efforts continue. In addition, the next few years will potentially provide new information from other sources on the values of neutrino masses and the neutrino mass hierarchy that have potential impact on the scientific case for future NLDBD experiments.

We also discussed the issue of the availability of isotopes in sufficient quantities for ton-scale NLDBD experiments. Some of the collaborations provided technical information on the feasibility of enrichment and also costs. The cost of enriched isotope varies widely, from \$9/g of ^{136}Xe to \$90/g for ^{76}Ge. So the cost of enriched material could certainly be a factor in a future downselect decision. Beyond this there is the question of availability. The DOE isotope program, which is stewarded by the Office of Nuclear Physics, has taken an interest in this question and started collecting some information. This is a commendable first step, but the subcommittee remains concerned that this issue will be problematic for scaling up some (or all) of the candidate techniques.

Project	Isotope	Isotope Mass (kg fiducial)	Currently Achieved (10^{26} yr)	Location	US Funding Level
CUORE	^{130}Te	206	>0.028	Gran Sasso	Major
MAJORANA	^{76}Ge	24.8		SURF	Major
GERDA	^{76}Ge	31	>0.21	Gran Sasso	None
EXO-200	^{136}Xe	79	>0.11	WIPP	Major
NEXT	^{136}Xe	10→100		Canfranc	Minor
SuperNEMO	^{82}Se+	7	>0.001	Frejus	Minor
KamLAND-Zen	^{136}Xe	434	>0.19	Kamioka	Minor
SNO+	^{130}Te	160		SNOLAB	Minor
PANDAX-III	^{136}Xe	200		Jinping	Minor

Table 1 Currently active NLDBD projects. The timelines are shown in Figure 2.2.

Impact of Other Future Neutrino Oscillation Experiments

As we discussed in our earlier report, "Determination of the **hierarchy** (NH vs IH) and **absolute mass scale** would sharpen the interpretation of both positive and null results in the next generation of 0νββ experiments"...and "There is a reasonable chance that, within the next decade, a combined analysis of several new experiments may well determine the neutrino mass hierarchy…". These anticipated developments, and other possibilities as discussed below, could have important consequences for the future NLDBD experimental strategy.

Recent new results from Super-K atmospheric neutrinos [2], the T2K long-baseline experiment[3], and first results from NOvA [4] all seem to favor a maximal δ_{CP} of $3\pi/2$ and a normal hierarchy. At this point, the indications from each experiment are marginal at the 1-2 sigma level, but future global analyses may provide more convincing evidence. Furthermore, a large value of δ_{CP} means that the NOvA experiment alone could make an unambiguous determination of the hierarchy with continued running. In fact NOvA expects to double their exposure within one year, which should improve their determination of the mass hierarchy.

Sterile Neutrinos

Even if the neutrino mass hierarchy is established to be normal, there is potential for new physics to impact the science case for NLDBD experiments. For example, another potential source of new information relevant to NLDBD is experimental investigation of the possible existence of a sterile neutrino with mass ~ 1 eV. The reactor antineutrino anomaly [5] and the Gallium neutrino anomaly [6] have been interpreted as evidence for mixing with a sterile neutrino in the eV mass range. New experiments are being proposed to resolve this issue over the next few years. It has been shown [7] that the existence of such a sterile neutrino has a substantial effect on the neutrino mass scale and neutrino mass hierarchy dependence of NLDBD experiments. In fact, the ~1 eV sterile neutrino scenario could enable the ton-scale NLDBD experiment to discover lepton number violation even if the neutrino mass hierarchy is normal.

Relation of NLDBD to LHC

The Large Hadron Collider (LHC) at CERN is now beginning a new running phase with higher energy and has plans for increasing the luminosity in the near future. Discovery of new physics at LHC could also impact the interpretation of future NLDBD experiments. For example, it has been shown [9] that a heavy right-handed W' boson could have substantial impact on the expected signals from NLDBD experiments, again enabling the discovery potential of ton-scale experiments even in the normal hierarchy scenario. In addition, recent studies [10] show that NLDBD experiments are complementary to future LHC capability to probe possible TeV scale lepton number violating interactions.

Impact of Cosmological Data

It is important in the planning of NLDBD experiments to understand the implications for neutrino masses from cosmological measurements.

A very successful model for the origin of the primordial mass inhomogeneities that later gave rise to the growth of cosmic structures, like galaxies, clusters of galaxies, and even larger-scale structure is now in place. The model postulates a nearly scale-invariant spectrum of primordial inhomogeneities and is parametrized by the densities of the primary constituents (baryons, photons, dark matter, and dark energy) of matter in the Universe. Data from measurements of the cosmic microwave background (CMB) and galaxies accrued over the past decade and a half are now so precise that the small contribution of neutrinos to the primordial energy budget must be taken into account to match theory to measurement. The current state-of-the-art data now comes from temperature data from the Planck satellite combined with data from galaxy surveys and constrains the sum of the three neutrino masses to be less than 0.23 eV at the 95%

C.L. [8]. (There is additional statistical constraining power in Planck's CMB polarization data, but that data is not yet well enough understood to be employed in a robust bound.) This analysis then implies that the lightest neutrino mass is less than 0.04 eV for the inverted neutrino-mass hierarchy and 0.06 eV for the normal hierarchy and thus that an NLDBD experiment must have sensitivity to values of $\langle m_{\beta\beta} \rangle$ below 0.05 eV before there is a prospect to detect NLDBD. While there may be some wiggle room in the cosmological upper limit, any significantly larger sum of neutrino masses would require modified assumptions or the introduction of some additional physics into the cosmological model.

There are prospects with improvements to CMB measurements and galaxy surveys for an order-of-magnitude improvement to the sensitivity of a cosmological measurement of the neutrino mass. Such measurements could provide valuable information on the neutrino-mass hierarchy and/or absolute values of neutrino masses, and thus further guide the planning of NLDBD experiments.

It is important to note, in the consideration of cosmological constraints to neutrino properties, that there is nothing in these cosmological measurements that distinguishes Majorana from Dirac masses, or put another way, nothing that can test lepton-number violation. Thus, cosmic measurements can be used to determine the NLDBD sensitivities required to test for lepton-number violation, but they cannot address the questions that NLDBD seeks to address.

Common R&D Topics

It appears to the subcommittee that the R&D plans proposed by the various collaborations contain several common elements. Particular areas to note are higher radiopurity materials, low-activity electrical connectors, scintillator balloon development, and improved photosensors. Our perception is that some of these activities could be carried out jointly by two or more of the collaborations. This would bring more expertise to bear on the problem and reduce the level of potential duplication of effort. We suggest that the agencies consider encouraging proposals of this joint nature in calling for R&D proposals.

International Context and US Strategy

As we noted in our previous report, the subject of NLDBD is a global effort with strong groups and facilities both inside and outside the US. While various countries seem committed to particular technologies at present, the perception of the relative value may change with time as new information becomes available from the existing projects and R&D efforts. We continue to advocate that the US should plan for a leadership role in (at least) one experiment, while perhaps maintaining options to participate in one or more internationally led projects. This will require timely and astute assessment of both the technological opportunities as well as the inherent strengths of research groups in various countries. At this point, the best one can say is that it is advisable to maintain a nimble posture, with an eye towards a timely decision in the near future (perhaps as short as 2 years).

2.0 STATUS OF CURRENT PROJECTS AND R&D PLANS

The material in this section is based on documentation submitted by the various experiment collaborations and the presentations at our meeting in Washington, DC on August 17-19, 2015.

SNO+

Introduction: The existing SNO detector has been re-outfitted to operate with liquid scintillator instead of heavy water in the central acrylic vessel. The new experiment SNO+ will search for neutrinoless double beta decay by loading the scintillator with a compound containing ^{130}Te and searching for an enhanced signal near the two neutrino double beta decay endpoint. The expectation is that external backgrounds can be greatly reduced by self-shielding and that the liquid itself can be made very radioactively clean by redundant purification.

Status and Future Plans: The SNO+ collaboration has made significant progress in processing the scintillator and developing loading techniques since the last review. The scintillator plant is complete and commissioning has begun. They have received funding for the Te from Canada sufficient for a 0.5% loading. The first 30% has been underground for 8 months and the purification process is now also planned to be done underground. This relaxes the purification requirements by two orders of magnitude. Unfortunately, a water leak in the SNO cavern has developed and the experiment has experienced a delay associated with diagnosing and repairing this leak.

Plans for R&D: The collaboration has identified a number of R&D priorities in order to be ready for a “down select”.

1. Technical Issue: The collaboration must demonstrate that they understand their backgrounds and their detector model.

Proposed R&D: They propose to run the detector in what they call Phase I with ~0.3% loading.

Comments: The data from this run can be used to compare with the backgrounds that they deduce from their simulations, thus validating their detector model and giving some confidence that backgrounds with larger Te loading can be reasonably well predicted.
Progress in first filling with unloaded scintillator has been slowed by a water leak in the mineguard lining of the rock cavity. There is great effort being put into finding and then repairing the leak. Water loss in the cavity could compromise the mechanical support of the acrylic vessel.
All the work necessary to accomplish the phase I loading is complete and only the water leak is preventing further progress.

2. **Technical Issue:** A path must be found to achieve the Phase II 3% loading.

Proposed R&D: Work is continuing on new surfactants and additional loading techniques. Support for postdocs and students is requested for this effort.

Comments: They have now demonstrated a 5% Te loading with the conventional Water-based Liquid Scintillator method. The surfactant that is necessary for this process is colored and since this affects the total light collection, purification (also underground) is planned. More recently, a promising new surfactant with lower quenching has been identified and testing is on-going.
An additional loading technique using diols ((hydrocarbon with two OH molecules) has been tested and they have shown Te loading as large as 15%. The quenching of the light with this technique is being studied.

3. Technical Issue: They have established that in order to achieve a sensitivity that completely covers the inverted hierarchy with the 3% Phase II loading, they must collect 300 photoelectrons per MeV. Their reference design with bis-MSB as secondary fluor shifts the emitted light further from the absorption region, but still would result in about a factor of 4 shortfall in light yield.

Proposed R&D: The R&D for potential solutions includes:

- Improving the intrinsic optics of the solution with new, less quenching and absorbing cocktails.
- Increasing the PMT coverage and or the efficiency thus Increasing detected light yield.
- Isolating the fiducial volume in a containment bag to reduce the amount of cocktail that the light has to pass through.

Comments: The resulting light yield is a product of a number of factors; these include intrinsic emission, quenching, absorption in the liquid and the vessel, and PMT collection. The eventual technique that is used to achieve the required loading will determine the light collection enhancement that will be necessary.

All of the listed options are being explored and some already show great promise.
A straight-forward solution, if all else fails, is simply to buy new high quantum efficiency PMT's outfitted with light collectors. This would have the added advantage of greatly improving the timing resolution of the currently installed SNO tubes. This could have a beneficial effect on identifying and reducing the ^{8}B intrinsic background. The cost of this solution is modest, ~$10-20M.

Gaseous Xenon TPC

<u>General description</u>: The key advantage of a high-pressure gaseous (HPG) Xenon TPC is the promise to uniquely image the tracks of the two emitted electrons in a NLDBD event. Together with high resolution, which evades the $2\nu\beta\beta$ decay mode, the background should, in principle, be reduced to negligible levels. The two emitted electrons in a NLDBD event have total energy equal to $Q_{\beta\beta}$; the measure corresponds to the usual calorimetry signature of other methods. The HPG TPC is designed to actually image the tracks and measure their ionization along their paths through the gas until the electrons have stopped, typically some cm in distance for each. The greatly increased ionization deposited per unit path length toward the endpoint of each track (the Bragg peaks) provides a signature which further characterizes the event signature. As demonstrated in studies by NEXT, gaseous xenon with electroluminescent detection of the ionization has excellent energy resolution. As a further possible handle on the decay, it might become possible to tag the daughter Ba++ ion following Xe-136 NLDBD decay, as the nEXO Collaboration has been investigating, and which NEXT proposes with a new single ion detection method.

The use of source = detector is further enhanced because the xenon gas can be enriched to consist of 90% ^{136}Xe, and at a relatively inexpensive cost. Both gaseous and liquid Xe TPC detectors can purify the materials to eliminate impurities by recirculation and filtering methods. A challenge for HPG-Xe based experiments is how to evolve to the ton scale given the comparatively lower density of the target. The PandaX-III Collaboration assumes 200 kg scale individual TPCs modules, replicated to reach the ton scale. The NEXT Collaboration, while not yet presenting plans of a ton-scale concept, is evaluating a 300-kg detector while working on smaller prototype vessels to optimize the design parameters. A challenge in tracking TPC detectors is the fact that the overall reconstruction efficiency is in the range of 30 – 40%, owing to the topology of the identification of the events and the fiducial volume cut that must be made to avoid tracks near vessel surfaces

While the concept of a unique event signature suggests a ‘zero background’ experiment, in practice there remain many challenges owing to the radio-purity of the internal TPC components, and the cosmogenic backgrounds that create gamma events that can fake the topology signature. Therefore there is a demand here – as much so as in other efforts – for careful material selection and detector preparation methods to minimize these backgrounds. Further, the involved groups must introduce components to the xenon gas to avoid the natural diffusion that can spoil track resolution (and thus lead to false events). The overall optimization of parameters – high-voltage grids, high-purity pressure vessels, readout techniques, dimensions of the TPC, etc. – remains an important R&D exercise and authentic simulation efforts are required going forward.

The NEXT and PandaX efforts are discussed in turn below. We note that the Japanese AXEL Collaboration is also becoming active employing this technology in a small prototype module. They were not represented at the Subcommittee meeting.

NEXT

Status and Future Plans: The NEXT collaboration – pioneers of the HPG Xe TPC approach – is evolving their design through a continuing series of studies with small-scale detectors to establish the technology to be implemented in NEXT-100, a 100kg detector. The present status of tests carried out with the NEXT-DEMO and NEXT-DBDM prototype detectors is as follows:

- NEXT-DEMO
  - Observed the first two-track evidence for topological structure in high pressure xenon gas [11].
  - Achieved an energy resolution of 0.7% at $Q_{\beta\beta}$, based on extrapolation from 511 keV gamma rays, surpassing the design requirement of 1%.
  - Achieved gamma rejection factor of 24.3 +/- 1.4, with efficiency of 66.7 +/- 0.6 % for detecting electrons.
- NEXT-DBDM
  - Achieved energy resolution of 1% at 662 keV, which extrapolates to 0.5% at $Q_{\beta\beta}$ in NEXT-100.
  - Developed background model based on measured radioactivity of detector components.

Future plans are focused on small scale studies outlined in the R&D section below, and on completing the construction of the NEXT-100 detector. The NEXT-100 detector has a predicted background of $5\text{x}10^{-4}$ counts/kg/keV/yr in the ROI. The projected background is based on the measured rejection of gamma rays obtained in studies of track topology. The background estimate is deemed to be conservative because the largest background sources are based on upper limits. Adding gas to reduce diffusion of electrons could reduce the gamma background to $1.5\text{x}10^{-4}$ counts/kg/keV/yr.

In addition, NEXT is investigating the possibility of detecting Ba++ directly in the gas using single molecule fluorescent imaging, a new concept developed in biology for selective detection of Ca++. If successful, the background could be reduced to less than $1 \text{ x } 10^{-5}$ c/kg/keV/y, and the efficiency of cuts could be increased from 30% to 42%.

Plans for R&D: NEXT-10 is a significant scale up in size compared to previous small-scale detectors. It will start data taking in 2016 with the goal of demonstrating overall backgrounds, energy resolution, and topology from direct measurements with a detector of size similar to the full-scale 100-kg detector. In general, gas phase detectors have flexibility for improving performance. Possible enhancements considered in the NEXT program include adding other trace gasses, such as CO_2, to reduce diffusion of the electrons, improve the topology with external magnetic fields, and detecting the Ba++ daughter ion.

1. ***Technical Issue:*** The expected background in the ROI for NEXT-100 is ~6 counts per ton-

year. This scales to ~3 counts per ton-year for a 1-ton detector, which implies a prohibitively long exposure to probe the inverted hierarchy. This is a conservative estimate of the background in a 1-ton detector, and a better background estimate should be provided by NEXT-10. Nevertheless, this does illustrate the importance of on-going research outlined below to reduce the background. An example of a potentially serious background problem is the 2447.8 keV gamma ray of ^{214}Bi, a daughter of ^{222}Rn that may end up on the anode. This gamma ray is only ~10 keV below $Q_{\beta\beta}$ = 2457.5 keV. Even with the good resolution of 0.5% expected in NEXT, ~12 keV at $Q_{\beta\beta}$, the resolution may show some evidence for background from ^{214}Bi but is not sufficient to separate this dangerous background. Sources of radon emanation and deposition of the radon daughters on surfaces by the electric field need to be evaluated.

Proposed R&D: There are a number of studies with small prototype detectors that are designed to investigate methods that will reduce the background.

- NEXT-10+: incorporate SiPMs only readout with a symmetric TPC design, which will eliminate the large PMT background.
- DEMO: will explore low diffusion gas admixtures and demonstrate possible benefits of better track resolution for reducing multi-hit gamma background.
- DEMO+: will add external magnetic field to determine impact for 1 or 2-electron identfication
- NEXT-BaT: introduce molecular fluorescence barium tagging if R&D warrants.

Comments: Reducing the background below 1 count per ton year is very important to achieve a sensitive search for NLDBD with a reasonable exposure. The research plan identifies improvements in the current detector design that could have a significant impact on lowering the background. Apart from the challenging goal of Ba++ tagging, the improvements appear to have a reasonable chance of success to reduce the background significantly and should be undertaken.

Single molecule fluorescence is a new technology proposed for detecting Ba++ in gaseous xenon. Developing this technology for NLDBD is both high-risk and high-merit research. If successful, it would provide a method to reduce background to a level well below 1 count per ton-year.

PandaX-III

Status and Future Plans: The largely Chinese-based PandaX-III Collaboration is new to NLDBD, but builds on two generations of successful xenon-based dark matter detectors. They enjoy the enviable siting of the Jinping Underground Laboratory (CJPL-II) with its world-deepest overburden, a horizontal access, and a custom 16 x 14 x 65 m^3 laboratory with a planned Class-2000 clean room environment. The laboratory will include a 13 x 13 x 25 m^3 water shielding tank large enough to host more than five 200-kg-sized detectors. This cavern is already under construction. The Collaboration plans to build by 2017 a 200-kg enriched 10-bar xenon TPC with dimensions 1.4 m diameter and 1.9 m length (3 m^3 overall). It is a symmetric device with a cathode plane in the middle and two anode planes at the corresponding endcaps. Unlike NEXT where charge is detected by electroluminescence, the PandaX Phase 1 readout will employ charge collection using the modest-resolution micro-mega readout, which does not. Their initial physics program will focus on observation of the $2\nu\beta\beta$ decay. For $0\nu\beta\beta$ in Phase II, they are exploring the novel Topmetal technology, which aims for significantly improved resolution. That technology is in an R&D phase.

The PandaX-III Collaboration plans include construction of a 200 kg prototype module by 2017 and thereafter additional 200 kg TPCs roughly yearly, with improvements based on continuous R&D. The funding for the main high-cost items appears to be adequate and supported by China. Funding for smaller and critical R&D activities might – ironically – be harder to obtain and yet quite critical to the success. Funding for U.S. participation could represent a strategic investment if allocated to the high-priority items discussed below. PandaX will procure 200kg of ^{136}Xe by the end of 2016 with 90% enrichment.

Plans for R&D: The Collaboration presented three major R&D issues, where selective U.S. funding could contribute in two cases.

1. Technical Issue: The first 200 kg vessel, with a modest energy resolution goal of 3% is based on a micromega readout scheme requiring a uniform coverage of the 1.5 m^2 endcaps. An important issue remains to develop an optimized mosaic of single modules and to ensure the radio-purity of the components.

Proposed R&D: They will explore minimizing electronic channels with x-y strip installation, and determine the impact on tracking and energy resolution. A triggering plan must be developed and the identification of low-radioactivity electronics.

Comments: A satisfactory installation here for the 1st module is important to evaluate the overall concept going forward. The University of Zaragoza group and Saclay in France will largely lead this effort. They developed the micromega technology. No U.S. support is required for this project.

2. Technical Issue: As is common in other TPC approaches – gas or liquid – the high-pressure vessel design and insurance of its radio purity construction requires significant R&D and design work. The HV grid and high-purity pressure vessel are particularly challenging.

Proposed R&D: PandaX-III proposes to construct the 8 ton copper vessel using OFHC with U and Th contamination less than 2-3 ppt. They aim for a 100 kV field cage cathode with super-low radioactivity. The HV feedthrough is particularly challenging (here, as in other projects). They outlined a plan to study the above issues. For example, considerations for the field cage include low-radioactivity Cu, acrylic, and Teflon resistors; HV design with a shaping ring and resistors, and an acrylic barrel; a Cu mesh grid cathode.

Comments: They are working with vendors to develop the radio-pure, high-pressure vessel. The critical field cage has plagued other efforts and must be a priority. Obviously a state-of-the art materials assay is critical as these elements are in contact with the xenon target. The field cage design includes materials used for field shaping and high-voltage and signal feedthroughs. The background level established in the vessel overall will ultimately determine the physics reach of the experiment. They are requesting ~$1M U.S. funds for material, equipment and personnel support through U. Maryland.

3. Technical Issue: The required resolution for their ton scale installation is 1% FWHM or better, a factor of at least 3 improvement beyond the prototype based on micromegas. The readout must also be uniform in coverage and naturally of radio-pure materials.

Proposed R&D: The final energy readout design is intended to use a novel charge readout scheme based on Topmetal. This is a direct charge sensor, without gas amplification, that is enabled in a new CMOS chip design that allows for bare charge to be received on an exposed plate at an exposed top of the chip, where it is then directly amplified and digitized in the same device.

Comments: Resolution in this design, much like others, is crucial and the goal must be met to achieve the desired sensitivity. The Topmetal concept is promising and should be explored, not only for PandaX but also for its potential broader impact. The work is ongoing at UC Berkeley and LBNL, with LDRD partial support. The potential for a breakthrough here is worth investment. The U.S. requested funding is ~$1.5 M, which would support material, equipment and personnel.

Additional Comments: Discussion of calibrations, background budgets, triggering, and detailed event simulation are not at a mature state at this time. The event reconstruction without a conventional t0 (light) signal was not discussed. Omitting the t0 start signal – the *z* coordinate along the drift direction -- could have significant impact on the overall detector performance and should be evaluated. We encourage additional effort in these lower-cost, but high-impact activities. While the committee appreciates the aggressive approach toward realization of a ton-scale installation by 2022, many questions arose owing to the relatively immature nature of the presented plans. These include questions the Collaboration is very well aware of and that they also recognize must be addressed soon. In no particular order we suggest to:

- Present a detailed background budget based on the selected materials and performance measurements.
- Discuss calibration procedures in detail.
- Explore detailed event reconstruction issues to optimize design parameters (resolution, HV, diffusion, dimensions, and the importance of *z*-coordinate information)

MAJORANA/GERDA

Introduction: GERDA and MAJORANA represent the state of the art in high purity germanium detector (HPGe) arrays for $0\nu\beta\beta$, but are, by design, pursuing complementary approaches and sharing their results within a loose collaboration. While GERDA is reducing backgrounds by using low-mass holders and immersing the detectors in liquid argon as cooling and shielding, MAJORANA is using electroformed copper cryostats and continuing to improve the radiopurity of all small parts near their detectors. Both approaches have demonstrated advances in the last year.

GERDA Status and Future Plans: Data from Phase 1 (21 kg-y exposure) achieved ~ 10 cts/(keV-t-y). They are now preparing Phase II with a goal of ~ 1 ct/(keV-t-y). Phase II will use the same argon cryostat and water shield, but the payload will be larger (35 kg), the detector holders more radiopure, and the liquid argon instrumented to create an active veto shield. The 128 nm scintillation light from argon is read out using PMTs at the top and bottom, and from fibers with silicon photomultipliers around the central column. Calibration data show that 99% of the ^{208}Tl and 80% of the ^{214}Bi decays that fall within the ROI can be rejected by this veto.

Phase II will deploy Broad Energy Germanium, BEGe, detectors with superior energy resolution and pulse shape discrimination. The resulting analysis cuts, together with the liquid argon veto, is expected to reject more than 95% of the ^{214}Bi events. Work on improved low capacitance electronics with the JFET and feedback resistors located directly on the detectors has not been achieved due to the difficulty in identifying radiopure JFETs with low failure rate during handling in glove boxes. They are continuing to investigate solutions, but will start Phase II with electronics similar to Phase 1. Their largest background contribution at the endpoint is expected to be ^{42}K (from ^{42}Ar in the shield). A nylon cylinder coated with TPB wavelength shifter will limit the collection of ^{42}K ions on the detectors and pulse shape analysis will reduce this background.

MAJORANA Demonstrator Status and Future Plans: The goal is to demonstrate, using a 44 kg array, that scaling up to a ton-scale ^{76}Ge experiment will be able to achieve <1 cts/(ROI-t-y) in a 4 keV ROI at the 2039 keV endpoint. This can be achieved in the ton-scale experiment with a thicker electroformed-copper layer in the shield, greater depth (if needed), and increased self-shielding, if the MAJORANA experiment can achieve 3 cts/(ROI-t-y). A large part of the ongoing work involves improvements to assay methods, screening of all parts, and appropriate simulation to create a well-founded background budget. They have produced a very thorough table of screened items, and based on these measurements they expect to achieve a background level < 3.5 cts/(ROI-t-y). Data were collected from July 2014 – July 2015 with three strings of natural-abundance HPGe detectors inside a prototype cryostat made from commercial copper. Data from this run is required to demonstrate background rejection using analysis methods such as pulse shape discrimination (PSD). The preliminary data shows that the PSD cut selects single-site events with a >90% efficiency.

Based on expertise gleaned in the prototype module, MAJORANA has produced two new cryostats using electroformed copper instrumented with 7 strings of 3-5 detectors. Module 1 is loaded with 16.8 kg of 87% ^{76}Ge enriched HPGe and 5.7 kg of natural HPGe and Module 2 is currently being assembled with 12.8 kg of enriched HPGe and 9.4 kg of natural HPGe. Module 1 is now within the shield and taking preliminary data, while Module 2 is due to start commissioning by the end of 2015.

Plans for GERDA R&D:

GERDA is primarily a European experiment sited at Gran Sasso. The R&D which they plan to pursue in the next couple years will not be funded by US agencies. However, it is commonly recognized that there will be only one ton-scale germanium $0\nu\beta\beta$ experiment moving forward, and that GERDA and MAJORANA collaborations are committed to working together on the best technology. The MAJORANA R&D request must therefore be considered in light of the overall germanium program including probable GERDA advances. The plans for Phase II have already been discussed. In addition, GERDA has the following initiatives that will continue over the next several years

- Screen and procure cleaner materials (PTFE, Cables) near the detector
- Study μ-induced backgrounds from delayed coincidences (^{77}Ge, ^{77m}Ge)
- R&D on ^{42}Ar backgrounds: thicker n+ dead layer on HPGe, optimized front-end electronics, using depleted argon…
- Reduce surface contamination from radon plate-out
- Improve liquid argon veto with better light collection
- Improve detector stability (HV issues) in liquid cryogens
- Engineering required to scale up to many channels (feedthrus, cables, etc)

Plans for Majorana R&D:

The technical issues identified by the MAJORANA collaboration associated with achieving the goal of a background rate of 0.1 cts/(ROI-t-y) are:

1. Technical Issue: Connectors have to simultaneously carry analog signals or high voltage, be radiopure, and be robust under multiple connections and cryogenic cycling. The commercially available, robust connectors are made with BeCu alloy which is not radiopure. Alternative solutions do not yet have the required reliability but are improving.

Proposed R&D: They propose to begin design and early testing of connectors with different gauge wire and feed-through designs. All components need to be tested for radiopurity, as well as thermal and vacuum performance. They would work with companies to develop clean production methods and facilities to produce micro-sized signal connectors and clean copper wire. The collaboration estimates $350K required for this effort.

Comments: This work is crucial for both GERDA and MAJORANA and is especially important for modular experiments which scale up by replication of multiple signals, and will have a very high cable (and connector) density. It is very high priority because currently there is simply no radiopure method for coupling signals and high voltage through cryostats in a reliable way. The G2 dark matter experiments are in exactly the same boat. Advancements in connector design will greatly help all the $0\nu\beta\beta$ experiments, but will have a greater financial impact on the modular experiments with higher densities and numbers of connectors required. It will thus influence the cost comparison between technologies in any down-select scenario.

They have already identified improvements which are being implemented in Module 2 and can be retrofitted to Module 1. But if they are successful with a new miniaturized connector design, then they can replace signal connectors in both modules. They are also investigating new spring-loaded HV cable connectors in the search to make them both radiopure and reliable. This work should be supported and they should be encouraged to work closely with GERDA to make this successful and as generalizable as possible.

It appears that significant progress on this issue is possible during the next 1-2 years with the existing available infrastructure and proposed resources.

2. Technical Issue: Radiopurity of small parts close to the detectors can introduce unacceptable backgrounds. Contamination during processing and storing has occurred in these small parts, even though they are made of electroformed copper. This is not understood. Contamination may enter the electroformed copper during machining. For larger parts and in-situ jobs, conventional welding introduces contamination, but laser and electron-beam welding are limited in the size they can handle, so it would be useful to explore new welding techniques, too.

Proposed R&D: They would explore ways to improve their processes of etching and chemical cleaning. This includes a study of the chemical effects of the glove box environment on the detector components. They need to improve and scale up the electroforming process. A promising avenue is electroforming new radiopure copper alloys with better machining properties. This will also necessitate finding new welding techniques using the new copper alloys. As part of this process, they will need to assay the alloys and welding techniques using ICPMS. An estimated $450K is required for these activities.

Comments: This aspect of the R&D program is extensive and diverse. Certainly it is important to understand why they suffered unexpected contamination in their small parts, however, it is hard to separate more general R&D in this area from the incremental progress each project must necessarily advance for their own specific realization. On the other hand, finding new alloys with better mechanical properties is very important for all the small parts that are needed close to detectors. This is unlikely to be a driver for the choice of technology in a downselect, but would be a nice advance in the technology of electroforming and clean copper fabrication. It would result in a much more flexibility in the design of mounts and connectors close to detectors. The end result would be reduction in background and more reliable fixtures.

Some progress is possible during the next 1-2 years with the proposed resources. However, a more robust R&D plan to address this issue will likely take too long to be a major criterion in deciding between projects for a down-select. On the other hand, long term R&D like this needs to be started as soon as possible if it is to benefit the project.

3. ***<u>Other technical issues</u>***: Several other technical issues were presented which do not appear to be critical for a near term downselect. These include development of larger detectors with thin contacts, development of radiopure front-end electronics FEE on flexible cables, new vacuum flange profiles and seal materials for vacuum tightness and radiopurity, and development of hybrid MAJORANA/GERDA cryostat system. These are all important, but likely would be candidates for longer term R&D associated with the next generation experiment.

nEXO

Introduction: The nEXO Collaboration proposes to search for neutrinoless double beta decay of ^{136}Xe with a 5-ton homogeneous liquid xenon TPC. This next-generation experiment would represent a scaling up of EXO-200 by a factor of about 25 and would apply the same basic techniques that have been used in the current experiment: measurement of both scintillation and ionization density in the xenon; suppression of backgrounds by distinguishing single-site (SS) from multi-site (MS) events and electron from α-particle tracks; constraints on both the background model and the $\beta\beta$ spectrum from a simultaneous fit as a function of position to both SS and MS events detected within the full TPC volume.

Status and Future Plans: After three years of operation, EXO-200 has achieved a 90% CL sensitivity on the $0\nu\beta\beta$ decay half-life of ^{136}Xe at 1.9×10^{25} years. This sensitivity is limited by the currently achieved background rate of 151 events per ton-year within the EXO-200 region of interest (ROI), with an important contribution from a ^{214}Bi peak unresolved from the $0\nu\beta\beta$ ROI. A factor of 3 improvement in the background is anticipated in the second phase of EXO-200, some of which arises from improved energy resolution. The start of the second phase, which had been projected to complete data-taking by the end of calendar year 2016, has been delayed by the suspension of operations at the Waste Isolation Pilot Plant, following an underground truck fire and radiological release during 2014.

Reduction of backgrounds by an additional two orders of magnitude (reaching, for example, 0.17 events/ton-year within a FWHM ROI for the innermost ton of xenon) is projected for nEXO under the assumption that the same levels of radiopurity achieved for EXO-200 construction materials and the same energy resolution projected for the second phase of EXO-200 can be attained for the much larger nEXO detector. The projected improvement comes primarily from the much larger ratio of TPC linear dimensions to photon interaction lengths, facilitating better rejection of surface background photons that undergo multiple Compton scatterings within the TPC liquid. This projected background level is sufficient to fuel a 3σ discovery within ten years of nEXO operation for $\langle m_{\beta\beta} \rangle$ at the bottom of the inverted hierarchy for all currently viable nuclear matrix elements (NME) other than that from the Shell Model; in order to extend the discovery potential to include the Shell Model NME as well, it would be necessary to improve the radio-assay sensitivity of the copper used for the TPC vessel by an order of magnitude from that achieved in EXO-200. R&D studies of high-purity copper supplies are currently under way.

Plans for R&D: The technical R&D issues highlighted by the collaboration for this project are necessary for attaining comparable or better radiopurity, reliability and resolution as for EXO-200 in a TPC of much larger scale.

1. Technical issue: A number of liquid xenon detectors have experienced unexpected and poorly understood high voltage breakdown problems. The EXO-200 TPC was designed to operate at a cathode voltage of 50 kV, but has been able to operate reliably only at ~10 kV.

Attaining the same electric field (~400 V/cm) as in EXO-200 operations would require a cathode voltage of ~50 kV in nEXO.

Proposed R&D: The collaboration proposes a high-priority R&D program to investigate high voltage breakdown locations and sensitivity to materials in, first, a "miniEXO-200" setup and, subsequently, a full-scale nEXO segment. In an already completed first-phase investigation of a small liquid xenon sample, they have not observed any unexpected high-voltage breakdowns. The miniEXO-200 studies are under way, and suggest that problems in EXO-200 may be associated with particular dielectric material choices, perhaps of acrylic standoffs, or the inadvertent introduction of some form of dirt.

Comments: There is no question that reliable operation of the nEXO TPC would hinge on an improved understanding of the high voltage issues. The R&D is thus essential and the phased approach is reasonable and capable of being completed in about two years, at a projected cost (U.S. share) of $1.1M. However, the impact on detector performance of a potential shortfall in the achievable electric field will need to be clarified in a subsequent R&D proposal. The graph supplied by the collaboration shows the energy resolution to vary rather slowly with electric field, and the impact of a small deterioration in energy resolution in a situation where the background contains a sharp peak under the ROI is not obvious. Other implications of reduced electric field for background suppression should be spelled out.

2. Technical issue: Optimizing nEXO energy resolution requires detection of a large fraction of the 175 nm scintillation photons from xenon. Good coverage within the nEXO design requires mounting the photodetectors in the barrel region, just outside the field shaping rings.

Proposed R&D: The collaboration's other highest-priority R&D program involves the identification and development of suitable photodetectors with high quantum efficiency for VUV photons, low noise, high radiopurity, reliability of operation in high-field regions in liquid xenon, and availability at reasonable cost to cover ~4 m^2 of surface area. They are working with a few companies to develop SiPMs with suitable performance.

Comments: The recent SiPM developments at FBK, in collaboration with nEXO, appear promising and of likely interest as well for high-pressure gaseous xenon TPC approaches. The use of SiPMs offers, in particular, the promise to attain energy resolution better than the anticipated EXO-200 performance on which nEXO projections have so far been based. The Subcommittee concurs that identification and testing of suitable photodetectors is essential for a competitive nEXO down-select proposal. Given the good progress so far, it seems reasonable that the needed R&D could be completed within two years at a projected cost of $425K.

3. Technical issue: nEXO needs to attain equal or better levels of radiopurity than EXO-200 in a detector of much larger scale. Of particular concern is the readout electronics: approximately 8000 channels of readout will be needed for each of the charge and scintillation light processing. The collaboration proposes to operate the electronics in the liquid xenon environment in order to minimize intrinsic noise, pickup, and cable and feedthrough complexity.

Proposed R&D: The collaboration proposes, at slightly lower urgency than the first two issues, intertwined R&D programs to develop very low-background cryogenic electronics and TPC construction materials that can provide the performance needed over the large proposed size of the nEXO TPC. A proof-of-principle readout chip for a 10 x 10 cm^2 charge readout tile is to be developed and tested for radiopurity and performance in liquid xenon. Sapphire and other inorganic dielectric materials will be tested as non-plastic alternatives for spacers, readout tile support, and other TPC components.

Comments: R&D to identify suitably high radiopurity of cryogenically compatible readout electronics and cabling, as well as of TPC construction materials and approaches, are also important for developing a detailed, compelling nEXO down-select proposal. The projected U.S. cost for these R&D programs is significant, about $2.2M over 2-3 years. Radiopurity investigations are critical for all of the proposed next-generation $0\nu\beta\beta$ technical approaches, and some of the generic material testing might be carried out collaboratively with other groups. Some material testing may also be appropriate for project R&D following a down-select.

<u>Other technical issues:</u> The collaboration identifies additional R&D needed to develop pre-conceptual designs for the nEXO calibration systems and for the TPC and cryostat vessels as important, but probably not essential to be completed prior to a down-select process. Development of a full nEXO simulation package and work with other groups on radioassay capabilities support the above R&D efforts, but the collaboration does not plan to propose dedicated funding for these efforts prior to a down-select. On a longer time scale, ongoing work to develop an efficient means of tagging the barium daughter nuclei from double beta decay offers the hope to extend nEXO sensitivity eventually to $0\nu\beta\beta$ half-life values well beyond the inverted hierarchy region.

Comments: The Subcommittee concurs that work toward a conceptual design of the calibration system and cryostat vessel would be suitable topics for project R&D, once a down-select of basic technological approach has been made. However, the importance of attaining the radiopurity of the copper TPC vessel needed to reach the desired discovery potential even for the Shell Model NME argues that appropriate R&D could have significant impact on a down-select decision. R&D on barium tagging is a suitable long-term project that could benefit all xenon TPC approaches, but is not likely to reach fruition on the desired timescale for a down-select.

SuperNEMO

Introduction: SuperNEMO is intended to comprise 20 rectangular modules, each with a 2.8 × 5 m isotopic foil sandwiched between a pair of wire-chamber 3D trackers, which are backed by large scintillator blocks to measure electron energy and radioactivity. This technique enables reconstruction of topology, kinematics and timing of the two electrons emitted in any double-beta decay. An earlier NEMO-3 experiment that operated from 2002 to 2011 successfully employed the same technique in a different configuration to simultaneously measure $2\nu\beta\beta$ half-lives for 7 different emitters. The SuperNEMO collaboration is predominantly made up of European Institutions and is primarily funded by European agencies.

Status and Future Plans: The first SuperNEMO module – the "demonstrator module" – is currently being manufactured and assembled in the Laboratoire Souterrain de Modane in the Fréjus Tunnel at a depth of 4800 m.w.e. under the Alps. Installation will continue into 2016, with some commissioning to begin in summer, and data taking on ^{82}Se decay starting around the end of that year. The main objective is to quantify the background level for 100 kg. This will require 1 year of data taking and analysis to get results around the end of 2017 or beginning 2018. It is planned to measure the source foil for a period of 2.5 years, until at least mid-2019. The energy resolution of their scintillator system, or calorimeter, is 8% FWHM and the $Q_{\beta\beta}$ for ^{82}Se is 3.0 MeV so the ROI for the measurement is between 2.8 and 3.2 MeV. Their background level is projected to be 13 cts/(ROI-t-y), which corresponds to a half-life sensitivity of about 6×10^{24} y.

Plans for R&D beyond the demonstrator module: Given the current make-up of the SuperNEMO collaboration, very little of the R&D that they plan to pursue in the next couple of years will require funding by US agencies. The main short-term (2-3 years) R&D areas identified by the collaboration are:

- Isotopic foils – enrichment and purification of the material; foil production
- Calorimeter – scintillators and PMTs
- Tracker – charge-division readout to replace Geiger mode

1. Technical Issue: Purity of the isotopic material – ^{82}Se in the case of the demonstrator experiment, but ^{150}Nd for later measurements – is required to reduce backgrounds, while thinning of the isotopic foil would minimize electron scattering.

Proposed R&D: They propose to seek improvements in the radio-purification processes currently used for ^{82}Se, and to resurrect a ^{150}Nd purification method used previously. The group anticipates support in the latter endeavor from collaborators at Idaho National Laboratory. As to improvements in the foil properties, work has already started on exploring the use of meshes or filaments to provide structural integrity to the isotopic foil in place of the Mylar film currently used. Since the ^{82}Se is used in powder form, R&D is also required to allow better control of the grain size.

Comments: This work is critical to the full 20-module SuperNEMO project achieving the sensitivity required for a meaningful measurement.

2. Technical Issue: The calorimeter components must have low radioactivity, particularly low counting rate due to radon emanation. The scintillators must also have sufficient light-yield and be of sufficient granularity for efficient track reconstruction.

Proposed R&D: The collaboration proposes to work with manufacturers to obtain PMTs with lower radioactivity than the ones being used in the demonstrator module. More work also needs to be done to further optimize the scintillator block size, shape and composition.

Comments: Improvements in the PMT radioactivity level are important but not critical to the successful operation of the full SuperNEMO project. Optimization of the scintillator properties is also important.

3. Technical Issue: If the tracking detector could be operated in proportional mode, then the energy loss along each individual track could be measured and the particle responsible more reliably determined. In addition, under these conditions it would be possible to use thinner wires and fewer tracking layers, resulting in less multiple-Coulomb scattering and reduced radioactivity emanating from the wires.

Proposed R&D: The collaboration proposes to build a test stand with which appropriate tests could be performed.

Comments: The collaboration itself assesses this to be a lower priority research area because it considers that the current Geiger-mode technique works well.

CUORE/CUPID

Introduction: CUORE is a detector array of 988 TeO_2 crystals that will operate as cryogenic bolometers. The crystals have a mass of 741 kg, with 206 kg of ^{130}Te in natural abundance of 34.2%. The CUORE concept of a bolometric 0νββ detector has already been successfully demonstrated through the operation of two medium size prototypes: Cuoricino and CUORE-0.

CUORE-0, the first tower assembled using these CUORE techniques, has operated in the Cuoricino cryostat for two years since Spring 2013. The background rates have been measured to be 0.058±0.004 counts/(keV·kg·year) in the ROI and 0.016±0.001 counts/(keV·kg·year) in the α-continuum region [12]. Dominant CUORE-0 background in the ROI is associated with ^{232}Th contamination in the old cryostat, and it is expected to be reduced by an order of magnitude in CUORE. The CUORE-0 data also show that the energy resolution has improved from 5.8 keV down to 4.9 keV FWHM at the ^{130}Te endpoint.

The sensitive detector mass must be in the range of several hundred kg to a ton of the isotope, and the background must be close to zero at the ton·year exposure scale in the ROI of a few keV around 0ν ββ transition energy.

CUORE-CUPID Status and Future Plans *Recent Progress*: The full-scale detector is expected to begin commissioning in the LNGS laboratory in Italy in early 2016. Based on operations with CUORE-0, the CUORE detector should have a background of 10 c/keV/ton/yr in the region of interest (ROI) and have FWHM energy resolution of 5 keV.

Future Plans: The collaboration plans is to operate CUORE for 5 years with the present TeO_2 crystals. It will be the first operation of a ton-scale cryogenic detector and as such, an important technical achievement for this technology. The initial experience will be valuable for planning the future CUPID experiment. Alpha particle and gamma backgrounds in CUORE, however, are expected to be too high by a factor of ~100 for detecting neutrino-less double beta decay in the parameter space of the inverted hierarchy.

Plans for R&D

1. Technical Issue - Alpha backgrounds: Alpha particle backgrounds in the ROI are too high for exploring the full inverted hierarchy. The collaboration is attempting to separate alpha particle background from beta signal by particle identification that uses a combination of measurements of heat and photons. The future program of CUORE is named CUPID: CUORE Upgrade with Particle IDentification. Detecting photons from Cherenkov radiation or from scintillating bolometers are in development.

Proposed R&D for Particle Identification using Cherenkov radiation: Particle identification that should provide alpha/beta separation for the TeO_2 crystals is in development based on measuring heat deposited and Cherenkov photons emitted. The ongoing challenge is that detecting the Cherenkov radiation requires an additional sensitive light sensor for each crystal that must have a very low-threshold energy (< 20 eV).

Existing technology employed so far for detecting light from the scintillating bolometers uses germanium disks operated as bolometers and read out by conventional neutron transmutation-doped thermistors, NTDs. These devices have an average noise of 100 eV RMS, unsatisfactory to read the tiny Cherenkov signal.

An appealing alternative to NTDs is a high-resolution Transition Edge Sensor (TES) – a thin-film superconducting device that operates at the critical temperature Tc of the superconductor. Scaling and reproducibility of the existing technology to a thousand detectors requires extra R&D.

Proposed R&D for Particle Identification using Scintillation: Since scintillation produces more photon than the Cherenkov process, the collaboration is developing bolometric crystals that scintillate. TeO_2 crystals do not scintillate so this solution cannot be applied to the existing detectors. Research on scintillating crystal bolometers made of $ZnMoO_4$, ZnSe, and $CdWO_4$ has produced results with good alpha/beta separation. However, a reliable source of crystals has been problematic. The collaboration is investigating alternate crystal growers in Europe and the U.S.

Comments: Except for using the same large cryostat, the switch to using scintillating bolometers is a major upgrade, nearly equivalent to starting a new experiment. This is a multi-institutional effort, including Europe, Asia, and the US. The anticipated US effort is about 2-3 FTE-years.

2. <u>Technical Issue - Gamma Background:</u> Estimates of gamma background from internal radioactivity in internal parts of the detector (lead, copper, and crystals, etc.) are relatively high (> 1 ev/year in ROI). In some cases the predicted background rates are based on upper limits because of insufficient sensitivity of the assay methods.

Proposed R&D: Data taken with CUORE will be valuable to understand the predicted backgrounds by a direct measurement of the overall background. Research on methods to improve the sensitivity of assay methods, as well as use of materials with lower radioactivity, such as electro-formed copper are also planned toward lowering the gamma background.

Comments: Reducing the gamma background may be difficult. Determining the sources of background that could come from multiple parts of the detector from detailed background studies in CUORE will likely be a difficult and slow process. Since the assay methods are already highly developed, further improvement is likely to be slow. The US effort is anticipated to be 0.5 FTE years.

3. ***Technical Issue - Cosmogenic background:*** Estimates of cosmogenic background are high with a rate of > 1 ev/year in ROI.

Proposed R&D: Improved simulations are planned and a scintillator-based muon tagger is planned to be installed around the CUORE cryostat. This is anticipated to be a 2-3 FTE-year effort by US collaborators. They will operate it during normal data-taking periods to try and tag the muon-induced backgrounds. If needed, the experiment will move to a deeper site.

Comments: The massive amount of lead and copper passive shielding could be a serious source of muon-induced gamma and neutron background. It is a little surprising that this background has not been thoroughly studied, since there is good simulation software and much is already known about this potential problem at LNGS. The prompt muon tagger may not be able to evaluate long-lived radiogenic isotopes.

4. ***Other Technical Issues.***
   a) The isotopically enriched TeO_2 has a high level of bulk radioactivity. They plan to develop chemical and zone refining method to reduce background before growing crystals.
   b) If particle ID is employed, there is possibility that the light sensors will introduce radioactivity, or interfere with the bolometer. A demonstrator detector with 8 to 20 crystals is planned for tests.
   c) Growing crystals suitable for scintillating bolometers has been challenging in Russia and the Ukraine. Alternate sources of crystals are being considered in Europe and the U.S.

Comments: These technical issues are considered to have a medium risk. However, they all require a significant effort to overcome, and any one of them could compromise the sensitivity significantly or even be a showstopper.

KamLAND-Zen

Introduction: KamLAND-Zen uses the highly developed infrastructure of the Kamland detector to search for neutrinoless double beta decay in ^{136}Xe. KamLAND's large size allows ton-scale quantities of Xe gas to be directly dissolved in liquid scintillator while also accommodating a large volume of pure liquid scintillator to provide substantial active shielding.

KamLAND-Zen Status and Future Plans: An initial set of measurements has established a limit on $T_{½}$ of $2.6x10^{25}$ years at 90% confidence. KamLAND-Zen's recent operation represents the largest sample sizes (365 kg) of a single isotope yet studied in any NLDBD experiment. KamLAND-Zen has since acquired a total of 600 kg of enriched isotope and has an additional 200 kg underground awaiting purchase.

As part of a major refurbishment planned for 2015, the KamLAND-Zen project will dissolve this sample in liquid scintillator contained in ~3-m diameter "inner balloon." This "inner balloon" resides within a larger balloon filled with ~ 1 kiloton of un-doped liquid scintillator. This larger balloon is surrounded by pure mineral oil. This active and passive shielding offers significant advantages in background reduction; however, the addition of the balloon material (and possible support meshes) do provide some challenges with respect to mechanical design and additional background.

Plans for R&D: The collaboration has identified two R&D priorities in order to be ready for a "down select".

1. Increasing light yield. An improvement of the current ~4% resolution to ~2.5% will require an increase in light production and collection. The KamLAND-Zen R&D effort will be directed towards meeting this goal in three ways:
    a. The use of larger and more efficient photomultipliers as well as the possible inclusion of additional photomultipliers to increase areal coverage.
    b. The use of Winston cones to increase light collection efficiency.
    c. Replacement of the current pseudocumene scintillator with Linear Alkylbenzene.

2. Improved ^{10}C tagging. Decay of muon-induced ^{10}C remains a significant source of background. The ~20-s half-life poses challenges to delayed coincidence rejection. The KamLAND-Zen R&D effort includes the development of new electronics which, it is hoped, will reduce the effective ^{10}C background by nearly one order of magnitude.

Notional Timeline of Presented Projects

Based on the information supplied to the Subcommittee by the collaborations, we have updated our timeline for these projects in Figure 2.2. One can see that there is about 1 more year of construction and assembly before all the projects are in an operational phase taking data. Therefore, over the next 2-3 years one can expect to have valuable information based on real data combined with results from additional R&D for these different techniques. At that point, one would expect that an assessment of the relative merits would be more reliable than at the present time.

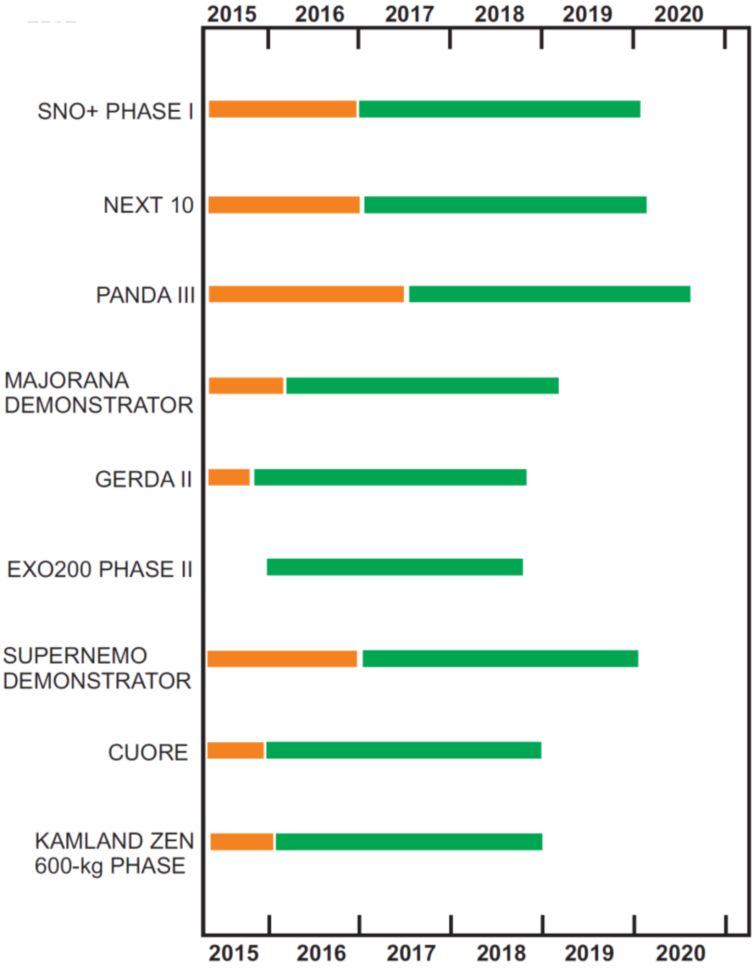

Figure 2.2. Approximate timelines for the presently funded mid-scale experiments and demonstration projects. The orange bars represent nominal construction periods and green illustrates actual or intended running. The running periods are aimed at achieving higher sensitivity to $0\nu\beta\beta$ in addition to demonstrating the capability of the technology (which in some cases may not require the full running time).

3.0 NUCLEAR THEORY UPDATE

Status and prospect of nuclear matrix element calculations

A detailed account of the theoretical aspects of neutrinoless double beta decay has been provided in the first NSAC Subcommittee Report [1]. Here we briefly summarize the main issues related to the nuclear matrix element calculations and describe the prospects for improvement that have emerged since the first report.

While the observation of neutrinioless double beta decay would unambiguously demonstrate the Majorana nature of neutrinos, the connection to the underlying Lepton-Number-Violating parameters (e.g. the neutrino masses in the "standard" light Majorana neutrino exchange mechanism) requires knowledge of appropriate matrix elements between the initial and final nuclear states. At present, matrix elements are known rather poorly, with different approaches leading to results that differ by factors up to two or three, and the current sensitivity goals for experiments are set using "pessimistic" values. Improvement in this area is highly desirable, because matrix elements are essential for interpretation of a positive or null result in the next generation searches.

As discussed in [1], current approaches to matrix elements are based on models like the shell model, the Quasi-Random Phase Approximation, QRPA, and others that employ truncated bases and depend on empirical methods to determine the effective interaction and effective operators. It is difficult to assign uncertainties to the resulting matrix elements because the methods are not based on systematic approximation schemes.

This state of affairs need not continue, however. In fact, the nuclear theory community is gearing towards a systematic re-assessment of matrix element calculations. As emphasized in the presentation to the Subcommittee by Prof. J. Engel, nuclear-structure theory and effective field theory (EFT) are both mature enough to produce significantly more accurate matrix elements, especially when their methods are used together and/or compared with one another. Ab-initio nuclear structure theory, while it currently cannot be directly applied to open-shell nuclei as heavy as those used in experiment, can produce improved effective interactions and decay operators for shell model calculations in those nuclei. It can also test the adequacy of other methods in lighter nuclei, for which matrix elements can be computed even though the decay may be energetically forbidden. Those other methods, based on the shell model, density functional theory (DFT), and the interacting boson model (IBM), can themselves be improved dramatically, by working in larger model spaces or including all possible collective degrees of freedom and appropriate non-collective ones in the DFT-based generator coordinate method and the IBM. And at the hadronic level, careful application of EFT should allow us to construct effective nucleon-level decay operators corresponding to the action of new physics at the elementary-particle level and construct and assess many-body corrections to bare one-body weak currents.

There are also prospects to resolve one of the largest sources of uncertainty in matrix-element calculations, the so-called “quenching of g_A“. This uncertainty relates to the over-prediction of calculated single beta-decay and two-neutrino double-beta decay rates if compared to data. In shell model calculations, performed in model spaces spanning a complete major oscillator shell and hence expected to be adequate for transitions mediated by the Gamow-Teller operator, this over-production reduces approximately to a constant factor which is then conventionally handled by usage of an effective (“quenched”) value of g_A. For other nuclear models, which do not consider all correlations within a complete major shell, the assumption of a constant over-production factor is prohibited. The underlying reason for the over-production is not definitely known, but there is growing evidence that it is related to either neglected correlations or many-body currents. The big question is then: once the source of “quenching” is identified, does that also quench neutrinoless double beta decay transitions, which operate at a vastly different scale of momentum transfer than single beta decay or two-neutrino double-beta decay? Ab-initio nuclear theory and chiral EFT now provide the framework to obtain a definitive answer to this question, at least in light nuclei, and there are excellent prospects for significant progress in the two- to three-year time scale.

Based on the level of international and US community engagement (e.g. within the NUCLEI SciDAC collaboration), it is realistic to expect a new generation of matrix elements calculations in the next few years. In this context, the nuclear theory community is also aiming to perform benchmarking and uncertainty quantification. Besides a statistical “error propagation” on the parameters that enter the nuclear interactions, the major component of this program will involve an assessment of the theoretical systematic error, through: (1) Calculation and benchmarking of spectra and transition rates (including β decay), starting in sd shell with all modern methods; (2) Calculation of 2νββ and 0νββ matrix elements in, e.g., ^{22}O and ^{24}O, and ^{48}Ca, accessible to all methods. (3) Benchmark methods against spectra and electromagnetic transitions in A = 76, 82, 100, 130, 136, 150. Based on this, it is reasonable to expect that the uncertainty in the nuclear matrix elements will be significantly reduced in the near future.

We wish to conclude by noting that the theory community has responded very promptly to the input and recommendation from the first Subcommittee Report. In fact, a first workshop on the topic of nuclear matrix elements has been hosted in November 2014 at the Extreme Matter Institute in Darmstadt, which will lead to a white paper. Meanwhile, the US community is devoting more attention to this problem within the NUCLEI SciDAC collaboration, and a DOE topical collaboration in Nuclear Theory centered on the subject of neutrinoless double beta decay has been recommended for funding. Finally, the INT will host in the Summer of 2017 a five-week program on “Neutrinoless double beta decay”, co-organized by J. Carlson, V. Cirigliano, and J. Engel. It is the assessment of the subcommittee that a relatively modest investment in nuclear theory focused on neutrinoless double beta decay is likely to have a high impact on the field.

4.0 REFERENCES

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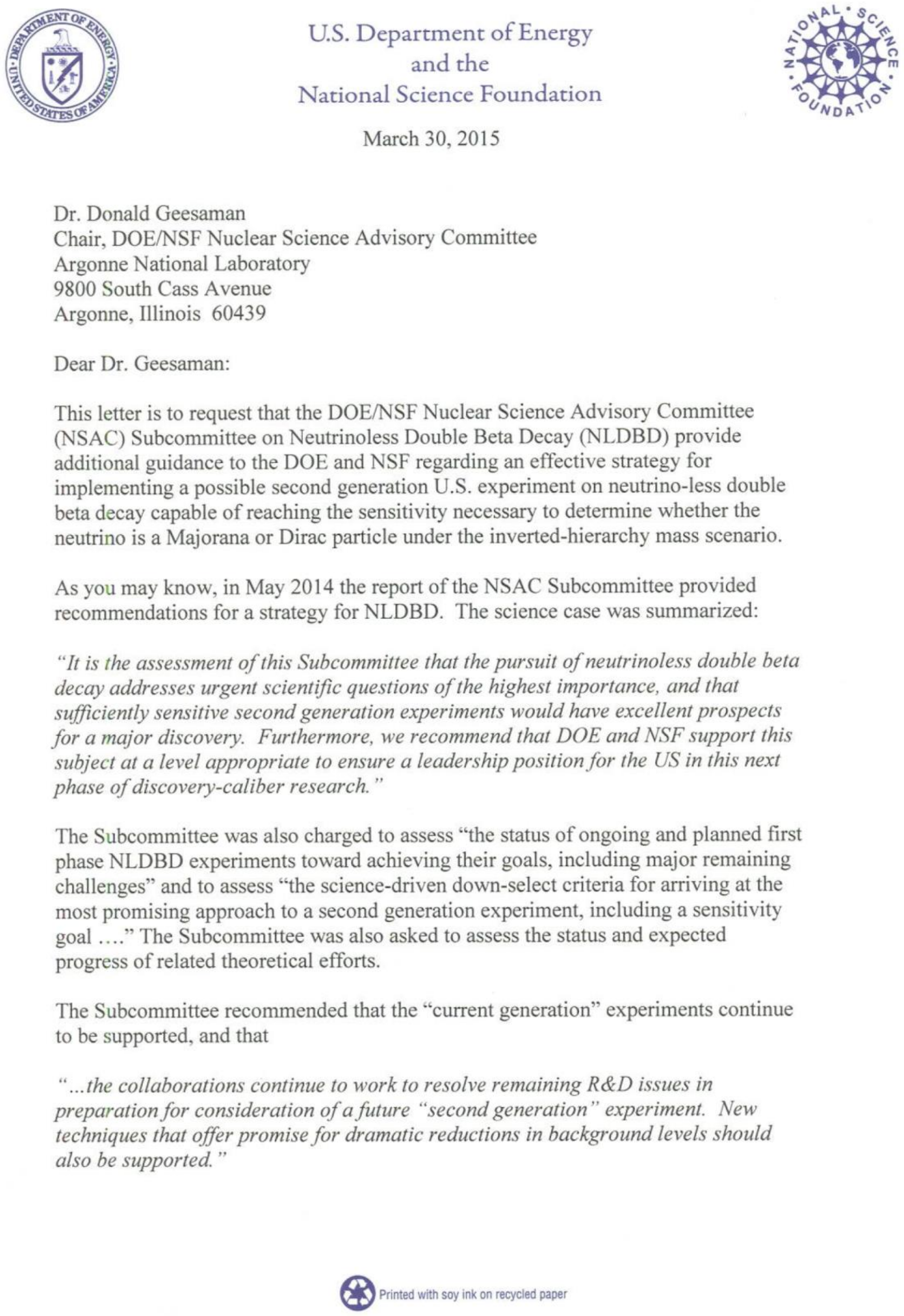
APPENDIX A: Charge to NSAC



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

March 30, 2015

Dr. Donald Geesaman
Chair, DOE/NSF Nuclear Science Advisory Committee
Argonne National Laboratory
9800 South Cass Avenue
Argonne, Illinois 60439

Dear Dr. Geesaman:

This letter is to request that the DOE/NSF Nuclear Science Advisory Committee (NSAC) Subcommittee on Neutrinoless Double Beta Decay (NLDBD) provide additional guidance to the DOE and NSF regarding an effective strategy for implementing a possible second generation U.S. experiment on neutrino-less double beta decay capable of reaching the sensitivity necessary to determine whether the neutrino is a Majorana or Dirac particle under the inverted-hierarchy mass scenario.

As you may know, in May 2014 the report of the NSAC Subcommittee provided recommendations for a strategy for NLDBD. The science case was summarized:

"It is the assessment of this Subcommittee that the pursuit of neutrinoless double beta decay addresses urgent scientific questions of the highest importance, and that sufficiently sensitive second generation experiments would have excellent prospects for a major discovery. Furthermore, we recommend that DOE and NSF support this subject at a level appropriate to ensure a leadership position for the US in this next phase of discovery-caliber research."

The Subcommittee was also charged to assess "the status of ongoing and planned first phase NLDBD experiments toward achieving their goals, including major remaining challenges" and to assess "the science-driven down-select criteria for arriving at the most promising approach to a second generation experiment, including a sensitivity goal …." The Subcommittee was also asked to assess the status and expected progress of related theoretical efforts.

The Subcommittee recommended that the "current generation" experiments continue to be supported, and that

"…the collaborations continue to work to resolve remaining R&D issues in preparation for consideration of a future "second generation" experiment. New techniques that offer promise for dramatic reductions in background levels should also be supported."

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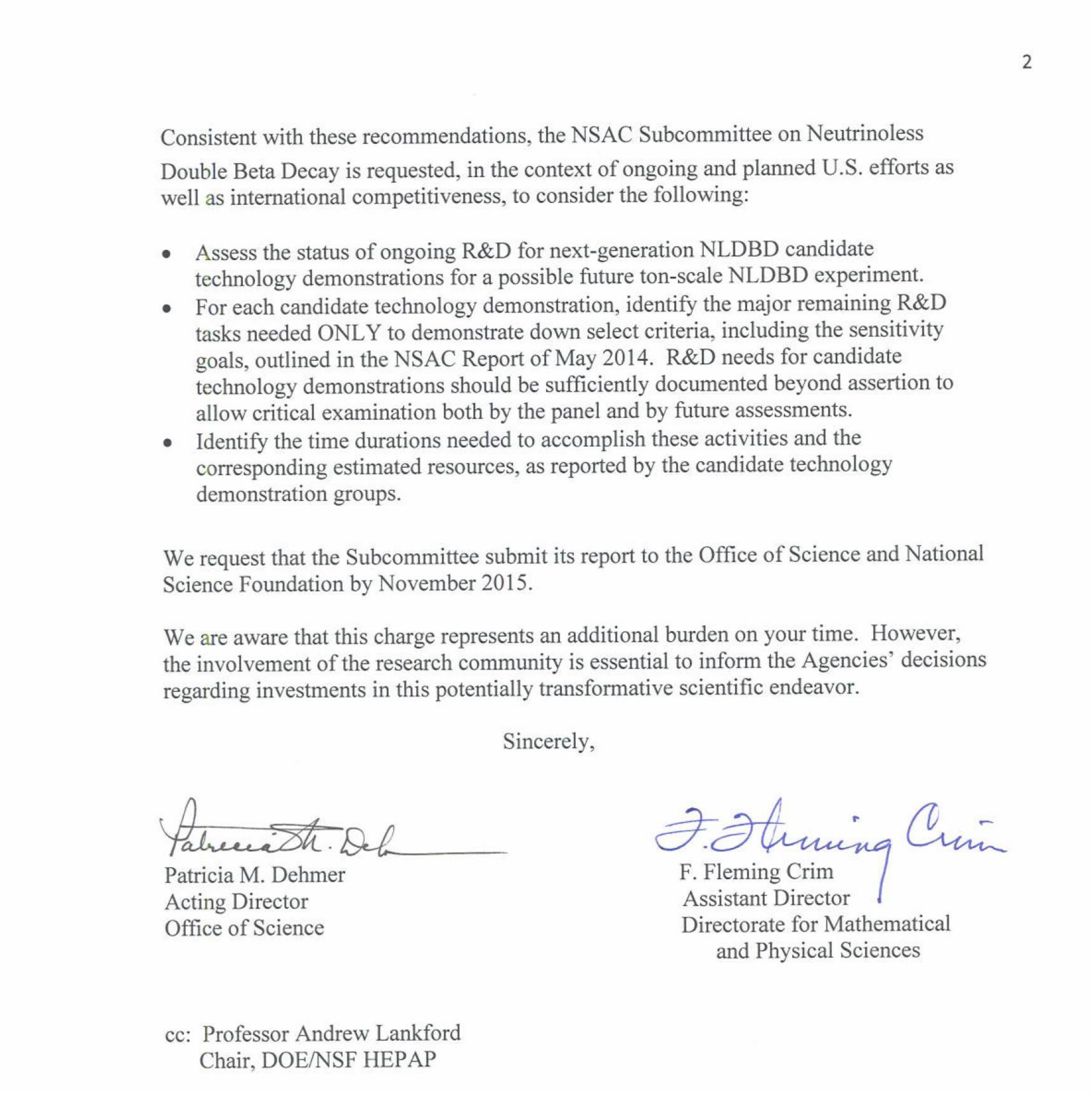

Consistent with these recommendations, the NSAC Subcommittee on Neutrinoless Double Beta Decay is requested, in the context of ongoing and planned U.S. efforts as well as international competitiveness, to consider the following:

- Assess the status of ongoing R&D for next-generation NLDBD candidate technology demonstrations for a possible future ton-scale NLDBD experiment.
- For each candidate technology demonstration, identify the major remaining R&D tasks needed ONLY to demonstrate down select criteria, including the sensitivity goals, outlined in the NSAC Report of May 2014. R&D needs for candidate technology demonstrations should be sufficiently documented beyond assertion to allow critical examination both by the panel and by future assessments.
- Identify the time durations needed to accomplish these activities and the corresponding estimated resources, as reported by the candidate technology demonstration groups.

We request that the Subcommittee submit its report to the Office of Science and National Science Foundation by November 2015.

We are aware that this charge represents an additional burden on your time. However, the involvement of the research community is essential to inform the Agencies' decisions regarding investments in this potentially transformative scientific endeavor.

Sincerely,

Patricia M. Deh

Patricia M. Dehmer
Acting Director
Office of Science

F. Fleming Crim

F. Fleming Crim
Assistant Director
Directorate for Mathematical
and Physical Sciences

cc: Professor Andrew Lankford
Chair, DOE/NSF HEPAP

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APPENDIX C: Meeting Agenda

Agenda
NLDBD Meeting, 8/17-19/2015

Monday Aug. 17

8:30 am	Executive Session (closed)	60 min
9:30 am	SNO+	45 min
10:15 am	Coffee Break	
10:30 am	Discussion	30 min
11:00 am	NEXT	45 min
11:45 am	Discussion	30 min
12:15 pm	Working lunch for Subcommittee	60 min
1:30 pm	Large Ge Experiment (GERDA/Majorana)	45 min
2:15 pm	Discussion	30 min
2:45 pm	nEXO	45 min
3:30 pm	Coffee Break	
3:45 pm	Discussion	30 min
4:15 pm	Cosmological Data Impacts (K. Abazajian)	45 min
5:00 pm	Discussion	30 min

Tuesday Aug. 18

9:00 am	Executive session (closed)	90 min
10:30 am	Coffee Break	
10:45 am	SuperNEMO	45 min
11:30 am	Discussion	30 min
12:00 pm	Working Lunch	60 min
1:00 pm	Nuclear Theory (J. Engel)	45 min
1:45 pm	Discussion	
2:15 pm	CUORE/CUPID	45 min
3:00 pm	Coffee Break	
3:15 pm	Discussion	30 min
3:45 pm	KamLAND-Zen (remote)	45 min
4:30 pm	Discussion	30 min
5:00 pm	Executive session (closed)	60 min

Wednesday Aug. 19

9:00am	Chinese experiment	45 min
9:45 pm	Discussion	30 min
10:15am	Coffee Break	
10:30am	Executive session - Discussion of Report	
12N	Lunch	
1PM	Adjourn	

APPENDIX D: Glossary of Acronyms

CERN	European Organization for Nuclear Research
CMB	Cosmic Microwave Background
EFT	Effective Field Theory
FEE	Front End Electronics
HPGe	High Purity Germanium
IH	Inverted Hierarchy
JFET	Junction-gate Field-Effect Transistor
LHC	Large Hadron Collider
LNGS	Gran Sasso National Laboratory
NH	Normal Hierarchy
NME	Nuclear Matrix Element
NTD	Neutron transmutation doped Thermistor Detector
PMT	Photomultiplier Tube
ROI	Region of Interest
SiPM	Silicon Photomultiplier
SURF	Sanford Underground Research Facility
TPB	Tetraphenyl butadiene
TPC	Time Projection Chamber
VUV	Vacuum Ultraviolet
WIPP	Waste Isolation Pilot Plant