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Report from the US Roadmap

Personal perspective with a focus on liquid xenon double-beta decay

Krishna Kumar (University of Massachusetts, Amherst), September 16, 2026

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Report from the US Roadmap

Personal perspective with a focus on liquid xenon double-beta decay

This is a very dry talk, no eye candy, basically no physics, I blame Thomas :)

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Outline

- EXO history and US Double-Beta Decay ($0\nu\beta\beta$) buildup since 2000
- Events during this period in the US planning process that led to the tonne-scale $0\nu\beta\beta$ “campaign”
- Observations on the current US landscape relevant to future planning for xenon $0\nu\beta\beta$ experiments
- Recent changes to the Office of Science and the new DOE long-term planning exercise
- A possible strategy to regain US support for a large-scale xenon underground science program

EXO-200 to nEXO

US liquid Xe-136 $0\nu\beta\beta$ Development Early History

- 2000's: EXO-200 was launched as an R&D project; demonstrated scintillation/ionization anti-correlation; a 100 kg class detector was constructed; contributions from Canada, Russia and Switzerland. (Political clout was required to get construction funding quickly)
- Early 2010's: Xe-136 $2\nu\beta\beta$ decay discovered: rate a "surprise" due to previous wrong limit
- Mid 2010's: The first 100 kg class detector to set world-leading limits on $0\nu\beta\beta$ decay
- The success of EXO-200 led to the development of the nEXO concept, armed with **demonstrated** ability to reliably predict energy resolution and background rejection
- By 2018, the nEXO collaboration had achieved critical mass and converged on a conceptual design, with a mix of universities, national labs and international partners

2010's: $0\nu\beta\beta$ Becomes a US NP Priority

US NP Community becomes strongly supportive of “campaign”

- The 2003 20-year DOE Facilities Outlook Report listed a $0\nu\beta\beta$ Underground Detector mentioned as a mid-term priority, and the 2007 Long Range Plan (LRP) is where DOE Nuclear Physics took intellectual possession of this in the US (transitioning from HEP)
- The 2012 “Implementation of the 2007 LRP” report and the DOE NP 2013 Facilities report both reiterated the need to give high priority to tonne-scale $0\nu\beta\beta$ experiment development
- The 2015 Long Range Plan's second recommendation: develop and deploy a tonne-scale $0\nu\beta\beta$ experiment (multiple tonne-scale experiments not yet proven); higher priority than EIC
- The success of several 100 kg class experiments led to the emergence of LEGEND, CUPID and nEXO with significant US participation in two and US lead in the third; NP Office finally scheduled the Portfolio Review. The notion of a “campaign” with multiple-isotopes born.
 - Report prioritized LEGEND but assessed that ***all 3 projects are technically viable***

The Recent Past

The 2023 Long Range Plan and the Facilities Subcommittee

- Early 2020's: insufficient momentum to form the international funding partnerships to launch experiments "quickly". (EXO-200 had political clout; circumvented DOE advocacy)
- 2023 LRP Long Range Plan reinforced the 2015 recommendation, formally expanding the Ov $\beta\beta$ plan into a "program", recognizing the multiple isotopes must launch concurrently
- 2024: The 2023 Report's assumed rosy funding scenario deemed unviable. A Facilities Subcommittee for NP project prioritization launched. ***The report quite important: EIC and a Ov $\beta\beta$ campaign of 3 isotopes were prioritized above all other projects***
- Over the last few years, there has been a significant evolution in annual funding allocations at the DOE: running facilities RHIC, FRIB and CEBAF, the construction of the EIC, and Genesis prioritized over research budgets and other new projects

Recent Evolution in the Office of Science

Particularly the NP Office (this is a very personal view)

- Over the last decade, the way Office of Science decisions involved the scientific community has steadily eroded: political pressure “from above” and the National Labs. This may well be correlated to when EIC was launched as a construction project (late 2010’s)
- Part of the reason is that folks with significant **scientific gravitas** (folks with great personal research accomplishments) are no longer in a position to influence decisions and speak to important “big” science at the highest DOE levels. Also, US political climate now less supportive of basic research purely for the sake of fundamental knowledge; NP and HEP more affected more than other subfields of the Office of Science
- The recent AI initiatives such as Genesis have insufficient Congressional support and therefore executed by reorganizing funding internally: exacerbates basic research funding problems
- The NP office struggles to simultaneously respond to political pressure to run all User Facilities at near 100% capability while maintaining basic university research and new project funding

The US Xenon $0\nu\beta\beta$ Community

What should we do now? First, some observations (some personal):

- nEXO is officially “paused”. Official line: there is unlikely to be funding to launch nEXO in the next few years. The full $0\nu\beta\beta$ “campaign” achieved CD-0: nEXO project not canceled.
- Due to the pause, funding for Xe $0\nu\beta\beta$ R&D in 2026 has received very low priority and many nEXO groups are slowly pivoting to other things; it is hoped that this is temporary.
- Similar situation for US XLZD groups: no further funding for pre-project development. Some R&D continues at the discretion of specific groups negotiating with DOE program managers
- “Reading tea leaves”: it is UNlikely that DOE wants US xenon R&D to disappear: 1) past investments and 2) synergies between $0\nu\beta\beta$ and dark matter projects
- ***In the larger nuclear and high energy physics community (folks not involved in underground science): more excitement for getting $0\nu\beta\beta$ searches to the normal hierarchy than to close the dark matter parameter space to the neutrino fog***

Historical Federal Advisory Committees

6 US FACA (Federal Advisory Committees Act) Committees disbanded

Office of Science Programs

Accelerator R&D and Production

ASCR

Advanced Scientific Computing
Research

ASCAC

BES

Basic Energy Sciences

BESAC

BER

Biological and Environmental
Research

BERAC

FES

Fusion Energy Sciences

FESAC

HEP

High Energy Physics

HEPAP

Isotope R&D and Production
(DOE IP)

NP

Nuclear Physics

NSAC

Workforce Development for
Teachers and Scientists

DOE HENP and SCAC

Evolution at DOE and the new Facilities Charge

- There has been significant reorganization within the Office of Science, **most notably the merger of nuclear and high energy physics into HENP**
- Equally important: termination of the six FACA Committees & emergence of a single Office of Science Advisory Committee (SCAC): oversees HENP, ASCR, BES, BER and Accelerator R&D. The inaugural committee membership is heavily weighted towards ASCR and BES.
- SCAC Chair: Persis Drell, formerly a high energy physicist who held the Research Director, then Director position at SLAC before moving into university leadership
- **SCAC new Charge: select a prioritized list of big projects for DOE in the next 10 years in 2B\$, 4B\$ and 6B\$ scenarios.** Previous such charge was 2003-8 mentioned earlier.
- Two facts in the public list of considered projects: 1) *LEGEND* and “Tonne Scale Onbb” (including Xe-136) **listed separately**, and 2) **no dark matter project listed**

How to Approach the Future

Some facts

- Two important documents relevant for the SCAC charge for $0\nu\beta\beta$: the NP Facilities Report mentioned earlier and the SCAC list of projects under consideration (on the website).
- SCAC solicited community input. There are now two relevant submissions:
 - $0\nu\beta\beta$ -focused (G. Gratta, M. Heffner, K. Kumar, B. Lenardo): next slide
 - Dark Matter focused (US XLZD IB): speak to gap in the DM space; target the LRP $0\nu\beta\beta$ portfolio (strive to approach nEXO reach); timely competition with Chinese DM program
- These submissions are necessary but BY NO MEANS SUFFICIENT to gain support!
- Any large project requires 20 years of constant pushing to get to start of physics in the US (there are plenty of examples big and small for this canonical number)
- With international partners and unification among the international proponents, maybe you can push it down to 15 years, especially given all the work that has already been done.

The $0\nu\beta\beta$ -focused SCAC Submission

Reacting to the scenario where both US nEXO and US XLZD are stalled

- **The Gap:** The United States has no identified path to a U.S.-led $0\nu\beta\beta$ facility
- **Why Xenon:** contrast with segmented detectors; scalability
- **The Proposal:** The roadmap must recognize the need for a many-ton liquid-xenon TPC, designed first for $0\nu\beta\beta$, with a major dark-matter and rare-event physics capability as a secondary benefit of the same target mass and depth
- **Funding:** To maintain US leadership, 500-750M\$ is required eventually; 20-40M\$ in pre-R&D, as well as access to suitable space at SURF
- **Recommendation:** We ask the Subcommittee to not only follow the second NP LRP recommendation to expeditiously fund the tonne-scale experiments, but also identify a U.S.-led, post-ton-scale $0\nu\beta\beta$ facility

A Possible Strategy to gain US Support

My concluding (very personal) view, and the main points for discussion

- What I presented in the previous slide is a serious path to consider for two reasons:
 - “Slowdown” of US XLZD: one needs a long term plan to get beyond $0\nu\beta\beta$ half-life of 10^{28} years before $0\nu\beta\beta$ community support can be secured
 - More resonance in the US, OUTSIDE the underground science community, for $0\nu\beta\beta$ vs final decade of WIMP dark matter searches. We are trying to get the funding agencies excited over the long-term
- Independent of what SCAC does, helpful if the US xenon community coalesced around one strategy. Can the differences be reconciled along with buy-in from international partners?
- **To summarize:** to get the US xenon $0\nu\beta\beta$ folks who signed on to nEXO motivated again, we need:
 - An R&D plan consistent with the new long term strategy to get beyond the tonne-scale
 - A realistic path to an intermediate step similar to the original nEXO sensitivity (or maybe nEXO itself if we get international partners)