# Multi-ton Double-Beta Decay with LiquidO

## CNRS/IN2P3 2020 Prospect on Neutrino Physics

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The research on rare decay searches, such as $\beta\beta$, is key in the fundamental physics research due to the groundbreaking consequences of a possible discovery. Establishing whether a neutrino is its own anti-particle and its mass scale have major phenomenological implications to our understanding of our Universe. Here, one of the most important design criteria for any $\beta\beta$-experiment technology is its potential to yield ever larger isotopic mass exposures in order to reach the lowest possible Majorana mass ($m_{\beta\beta}$) sensitivity. The 2020 decade is expected to witness the preparation of the experimental program to yield $m_{\beta\beta}$ <10 meV for the first time. The LiquidO-$\beta\beta$ R&D programme is here presented with such a goal for design. Its breakthrough potential relies on the unique combination of many features hardly ever found in one single experimental framework such as multi-isotope capability, large multi-ton isotopic mass at low cost (i.e. no enrichment) via high detector loading, high detection efficiency, low background and possible particle and event identification for active background rejection and a data driven background-model construction. The CNRS/IN2P3 teams hold pioneering leadership in the LiquidO technique and its conceptual articulation for $\beta\beta$ physics. Besides, the LiquidO-$\beta\beta$ program uniquely fusions, for the first time, otherwise independent groups specialised in MeV neutrino detection and $\beta\beta$ scientists in France thus benefiting from unique synergy in terms of logistics, technology and decades of expertise, thus reinforcing the CNRS/IN2P3 team position to an unique level.

The research on rare decay searches, such as $\beta\beta$ and *proton decay*, have long been in the prioritised focus of fundamental physics research due to the groundbreaking consequences of a possible discovery [1]. Both subjects have historically been closely linked to neutrino physics due to shared detection framework and, often, even in direct relation. The common ground encompasses detection, techniques for ultra-pure background (**BG**) control for both radiogenic[1] and cosmogenic[2] contributions, where deep underground laboratory articulation is mandatory. The manifestation of both processes do not imply the direct detection of neutrinos, whereas such an expertise has key complementary impact for BG control.

In the case of $\beta\beta$ decay the detection consists in the observation of the spontaneous emission of two $\beta^-\beta^-$, upon the decay of a handful of $\beta\beta$-*isotopes* allowing this rare transition. The $\beta^+\beta^+$ is also possible and, indeed, pursued in today research. However, in this document, we shall refer mainly to $\beta^-\beta^-$ decay (or $\beta\beta$) processes. At least, two main processes can lead to the $\beta\beta$ decay manifestation. The first process is predicted by the *Standard Model of Particle Physics* (**SM**) and labelled $\beta\beta(2\nu)$ – referred as "$2\nu$" – whose main feature is the emission of two $\nu$'s upon decay, whose detection is impractical. This process leads to a $\beta$-like continuum spectrum whose maximal value, called *q-value*, is well known. Each $\beta\beta$-isotope has a characteristic q-value. Isotopes whose q-value is <2 MeV are typically ignored since the radiogenic BG overwhelms those energies. Isotopes with large phase-factor and high q-value ($\sim$3 MeV) are particularly precious since most radiogenic BG dies off above the 2.6 MeV $^{208}$Tl line. The second process has never been observed and its discovery would imply a major breakthrough to our understanding of the nature of the neutrino. This process is characterised by the absence of $\nu$ emission, hence labelled $\beta\beta(0\nu)$ – referred as "$0\nu$". Unlike the SM process (i.e. the $2\nu$), the $0\nu$ has a characteristical mono-energetic spectrum centred at the q-value. The sensitivity to $0\nu$ can be characterised as its lifetime or the sensitivity in the derived Majorana mass [2], referred as $m_{\beta\beta}$. The $m_{\beta\beta}$ metric allows the convenient inter-comparison among different isotopes, so it is adopted here as reference.

The discovery of the $\beta\beta(0\nu)$ would imply a major breakthrough in our understanding of the Universe since this would established the unique Majorana nature [2] of neutrinos. A hypothetically Majorana $\nu$ opens for new phenomenology that could link the neutrino to fundamental questions such as *the matter to anti-matter asymmetry* in the Universe. Such an asymmetry, generated soon after the Big Bang, needs for CP-violation processes to yield the existence of the cosmos we see. Today's quark sector CP-violation – the only known and measured today – is far too small, by many orders of magnitude, to justify observations. This implies the compelling

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[1]This refers to BG originating from stable radioactivity, dominated by the U and Th chains, where $\beta^-$ go as high as $\sim$5 MeV.

[2]This refers to excited radioactive contributions upon cosmic-$\mu$ interaction with $\beta^-$ up to 15 MeV, excluding the Michel-e$^\pm$ upon stopped-$\mu$'s.

necessity for phenomenology beyond the SM. A neutrino related mechanism, called Leptogenesis, is proposed and widely discussed in the literature [3] to generate such a matter asymmetry. The answer remains, of course, unknown awaiting for discoveries that shed further light. Hence, the observation of $\beta\beta(0\nu)$ would be the kick-off in this thread of research, where the neutrino may well play a key role behind our existence.

# The KLZ Breakthrough: Large Mass

Today's best limit on the $0\nu$ searches is led by the KamLAND-ZEN [4] (**KLZ**) experiment, based in Japan, whose experimental approach relies on the reuse of successful KamLAND 1 kiloton detector, designed for reactor neutrino detection [5]. Despite some important limitations, the KLZ approach changed somewhat the driving trend in the $\beta\beta$ community. And this has inspired the first steps behind the designed strategy for the employ of LiquidO technology [6] in $\beta\beta$ decay searches. Indeed, up to KLZ, most experimental techniques had focused on an exquisite characterisation of the signal, including $\leq$1% energy resolution, and/or background rejection. However, their ability to scale to large masses was limited. For long, the possible isotopic masses were small; i.e. order 10 kg at best. Hence, enrichment had been resourced as the only way to yield the maximal sensitivity since, other than Te, the natural abundance of most relevant $\beta\beta$ isotopes is typically order 10% at most. KLZ strategy, instead, relies on huge volume to self-shield external background and a comparable enormous mass scaling potential from the 100 kg range up to the 1 ton. The main disadvantage is a poor energy resolution ($\sim$10 % at q-value). A good energy resolution not only minimises the otherwise irreducible $2\nu$ contamination but it is also exploits better a possible signal understanding, should a positive observation occurred. Its best limit relies on 400 kg, which are fiducialised at analysis reducing to order 100 kg effective mass. KLZ also benefited from two important features: a) large volume to surface ratio, since many BG's scale with surface, and b) the scintillator typical self-quenching for $\alpha$ signals, which de-promote their energy deposition well below the q-value energy range, often referred as *Region of Interest* (**RoI**). However, those $\alpha$'s are still detectable for tagging. The $\alpha$ BG is a dominant concern in semiconductors and bolometers techniques, unless dedicated actions are taken. Thus KLZ has achieved a remarkable order $10^{-4}$ *BG index*[3]. Hence, KLZ has succeeded to demonstrate that their rationale (i.e. large mass driven) was an important necessary condition – while not sufficient a priori. This has embodied a breakthrough trend in the field allowing larger phase-space explorations. The **SNO+** [7] pursues a similar vision despite some different experimental choices. Many experts often argue that this approach is most suitable as *exclusion experiment*, since there is limited insight in case of an excess to be found in the RoI. Arguably though, that is true for most experiments one way another, unless robust signal redundancy was articulated. As of today, there is very limited (or no) practical experimental redundancy upon the observation of the $0\nu$ signal. LiquidO is however designed as the next generation along the KLZ trend. Its goal is to address, if the R&D succeeded, the limitations in today's technology.

In fact, upon KLZ energising the high mass requirement in the field, this is now pursued aggressively by most experiments aiming to yield the world best sensitivity in $\beta\beta(0\nu)$. However, today's best world neutrino oscillation data [9] suggest that the atmospheric mass ordering (or hierarchy) is favoured to be "normal" – as opposed to "inverted" – at $\sim 3\sigma$. This is important to $\beta\beta(0\nu)$ since the inverted solution would have implied an effective bound in the $m_{\beta\beta}$. This bound is today disregarded for design of $\beta\beta(0\nu)$ experiments. So, the experimental design goal today is to reach the best sensitivity to the lowest possible $m_{\beta\beta}$, hence ever larger isotopic mass experiments is crucial must for all future experiments. Hence, LiquidO's design goal – further described below – aims for multi-ton (>10 ton) articulation at the highest priority. With this huge mass, we are to exclude the largest fraction of $m_{\beta\beta}$ phase-space, if no signal was observed. There is consensus that only experiments like SuperNEMO [8], with stunning MeV $e^-$ tracking, can provide the deepest insight on the process, once discovered, exploiting its unique angular distribution handle. Such capability is however today considered impractical in LiquidO. Hence, if observed, LiquidO's goal would be to yield the highest possible statistics and maximal characterisation, including some degree of redundancy, before next generation of tracking-based experiments may take over.

With such huge isotopic masses, three practical limitations are foreseen to be encountered. The first is cost. Enrichment, still today exploited by most experiments, including KLZ (enriched Xe), becomes prohibitively expensive for large masses. At the ton scale, this might imply a mandatory change in paradigm, unless new inexpensive enrichment techniques are found. The cost is roughly between [10,100] M€ per ton, where the lowest case is for Xe. However, most isotopes are typically >50 M€ per ton. Besides, enrichment is not always possible. Isotopes, such as Nd, suffer from severe practical limitations. The second is isotope purity upon enrichment. This was reported by CUORE R&D [10], whereby enriched Te exhibited exotic radiogenic contamination otherwise unobserved in natural Te. Those exotic impurities are particularly dangerous since any unknown BG contributions must be avoided and, worse, such exotic decays are likely to yield higher energies thus unacceptably polluting the RoI. The third is the poor experimental knowledge on the impact of cosmogenic BG. As of today, cosmogenics are mitigated by deep underground laboratories and the low target masses. However, their impact can only increase with larger exposed masses. The problem is that those BG are typically poorly known, as they are very hard to predict accurately (nor spectra or production cross-section). Some contributions may well be fully unknown since there is little experimental data. Again, the main concern is the likely pollution of the RoI due to high energy contributions. Today most experiments effectively assume – to a lesser or larger extent – that those BG's are negligible since no experimental knowledge has been derived to demonstrate. This is a critical additional challenge for the next generation of experiments with large regardless of the isotope or technique.

[3] This convenient metric, in events per keV per kg per year in the RoI, or (keV kg year)$^{-1}$, allows direct comparability across experiments.

## The LiquidO Double-Beta Strategy

The LiquidO $\beta\beta(0\nu)$ searches relies on the features listed below. Further details on the LiquidO can be found here [6].

**High Isotopic Mass.** The large loading capacity of LiquidO is expected to enable an unprecedented isotopic mass. This increase in signal rate per unit of detector volume is important for some types BG, such as the irreducible solar neutrino BG expected in large detectors, as reported by SNO+. LiquidO aims for loading in the range [5,30]%, which is as of today impractical in transparent detectors, typically limited to order 1%, up to 3%. This implies LiquidO can reach >10 ton (isotopic) in 1 ton detector using natural Te. In order to yield those level, dedicated R&D is needed and planned starting from many years of expertise by SNO+ on Te and Nd loading. Indeed, SNO+ and others, in different physics cases, suggest that such level of loading are not impossible, although dedicated effort and further tuning is needed to ensure minimal light yield quenching. The dominant transparency concern is largely relaxed in LiquidO's unique opaque articulation.

**Non-Enriched Multi-Isotope Loading.** With such a large loading fraction, loading compensate for the lack of enrichment, providing natural abundance is >5%. The costing per ton is expected to reduce by one or two orders of magnitude, as suggested by the SNO+ R&D. In addition, LiquidO is capable a priori to accommodate any isotope loading. Although dedicated R&D is needed for each element to be loaded, no showstopper exist a priori for any element. The main elements being considered in LiquidO are i) Te, due to its highest natural abundance, and/or ii) high q-value elements such as Mo, Nd and Se. The latter cases are important to increase the BG resilience of the experimental design, as LiquidO poor energy resolution may play somewhat against. This is good news to the CNRS/IN2P3 $\beta\beta(0\nu)$ teams holding unique leading expertise specialised in non-Xe isotopes, following their past efforts in NEMO3 [11], the SuperNEMO demonstrator and also CUPID [12]. Strategically, the LiquidO collaboration has so far particular favoured the articulation of isotopes different from Xe, since most leading experiments so far rely on Xe. This grants a unique complementary role for LiquidO compared to other Xe-based experiments. The lower q-value of Xe (2.4 MeV) might also disfavour its use in LiquidO. Regardless, Xe loading is expected to easy following the long expertise and demonstration from KLZ, which is expected of direct application to LiquidO, if needed.

**Active BG Rejection.** LiquidO powerful particle ID (**PID**) is particularly well suited for $\beta\beta$ experiments where BG rejection and control (i.e. a robust BG model construction) are most vital. This is major advantage compared to KLZ and SNO+. For example, major rejection of $\gamma$'s is possible as compared to point-like energy depositions ($e^-$ like), as illustrated in FIG. 1. More, the rejected $\gamma$ spectra allows the construction of an independent date-driven BG model, as done in EXO [13] and NEMO3 [11]. The native PID of LiquidO deteriorates somewhat with loading due to the change of the radiation properties of the medium. However, fortunately, the manifestation of radiogenic and cosmogenic BG is often richer than the single particle case, thus tagging and/or rejection is possible via coincidences, richer multi-particles topology, etc. Indeed, the more complex the event topology is, the better the LiquidO's aggregated event ID (**EID**) for rejection is, as compared to the simplest point-like $e^-$ like signature, show in FIG. 1. The overall LiquidO EID rejection power performance awaits for further study including some degree of experimental validation, including the impact of loading. LiquidO reconstruction is expected to improve much with the adoption of machine learning and artificial intelligence techniques. Last, LiquidO self-segmentation makes any segmentation unnecessary or even futile, preventing the need for extra materials (i.e. a contamination risk) in the detector. All that is envisaged and ongoing within our R&D.

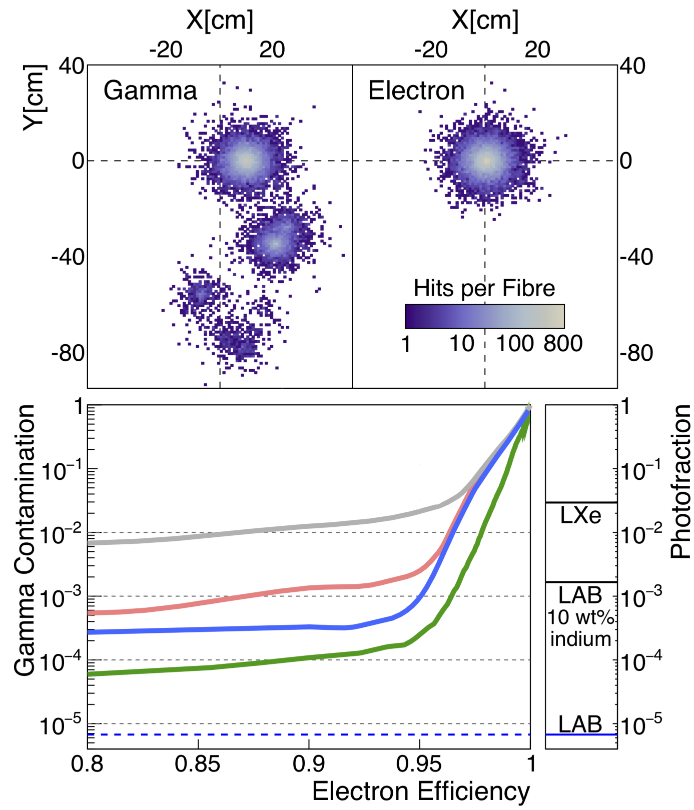


Figure 1: **LiquidO Particle Identification.** Unlike KLZ and SNO+, LiquidO has a imaging and PID capability to actively reject BGs. This is illustrated by an optimised LiquidO (red and blue curves) detector where a native rejection of $\gamma$'s from a $e^-$ sample is possible at $\sim 5\times10^{-4}$ with an efficiency of >85%. The green and grey curves illustrate, respectively, the cases of possible improvement (10× more light) and lack of optimisation due undersampling. The impact of loading issues important, as illustrated in the photofraction (i.e. $\gamma$ photo-electric to total cross-section) scale. Considering loading of ∼10% some PID performance is expected to reduce to the range $10^{-2}$ to be demonstrated experimentally. Fortunately, both radiogenic and cosmogenic BG often manifest as complex event energy deposition (i.e. several particles), thus making LiquidO ability to reject even stronger. This depends on the BG topology, hence it cannot be here generalised.

**High Detection Efficiency.** Like KLZ and SNO+, LiquidO relies on events in the volume bulk with minimal surface effects. Hence, high detection efficiency is possible as compared to foil-targets experiments, like SuperNEMO. Also, the detector is large enough to fiducialise with mm precision. This is important for high quality calorimetry, energy containment and/or BG rejection, as opposed to highly sub-divided or even segmented detectors.

**Radiopurity.** LiquidO benefits from the known scintillator ultra purity articulation and techniques legacy developed and implemented in past experiments such as Borexino [14]. The purity of scintillator is possible in the range $10^{-16}$ g/g contamination or better. So, the radiogenic BG is expected to be dominated by the fibres and the $\beta\beta$-element compound to be loaded. Fibres account for <1% of the total of the scintillator mass. The fibres used in GERDA [15] exhibit a radio-activity is 50 $\mu$Bq/kg. However, fibres being made of plastic are expected to be able to yield better radiopurity, should the industry use clean-room control. This is a R&D envisaged. The $\beta\beta$-element depends on the compound. Using today's SNO+ knowledge, we expected $10^{-16}$ g/g, upon dedicated purification procedures. Hence LiquidO's potential for ultra radiopurity seems possible while the ongoing R&D is needed to demonstrate and quantify performance. Last, LiquidO's scintillator solidification might proof an extra key asset to prevent convection effects aiding the transport of radon and other contaminants within the innermost central volume.

**Energy Resolution.** The main weakness of LiquidO is expected to inherit from its humble light yield. Light level is expected to be close to KLZ and SNO+; i.e. order <400 PE/MeV with today's standard technology. This limitation is typical in scintillation detection. The main impact of the poorer energy resolution is the unavoidable contamination of the $2\nu$ irreducible BG. This reduces the gain in statistical sensitivity (due to BG subtraction) without saturating the sensitivity in $m_{\beta\beta}$ for long. However, at some BG dominates yielding an effective saturation. This effect is illustrated in the next section. The RoI optimisation (width and location at higher energy) aids to minimise this unavailable effect.

A hypothetical increase in light yield in LiquidO is under exploration. LiquidO, unlike transparent based scintillator detectors, could exploit different scintillator composition today unexplored due to its non-transparent condition. Some such scenarios are known and under preliminary conceptual exploration. While the potential exist this is considered an explorative R&D with no evident outcome. Should this be possible, this will embody a major breakthrough in liquid scintillator technology, beyond LiquidO exploitation.

**Liquid Scintillator Heritage.** LiquidO inherits many of its features from its scintillator medium. This implies that many well known features typical in such detectors (even if transparent) are expected to be possible. One key example is pulse-shape discrimination (**PSD**), which along with quenching, can provide key handles for $\alpha$ BG rejection. The final performance on those effects cannot be generalised a priori. For example, PSD depends on the scintillator formulation, which might be impacted by the loading. However, there is no a priori showstopper on such capabilities for them to become available for experimental use, should they be needed.

**Signal Signature Tagging.** LiquidO could allow for $\beta\beta$ signal tagging. Up to now, this critical capability has proved impractical. Three examples are provided. First, recent developments towards the tagging of the $\beta\beta$ decay isotope product appears as a possible breakthrough possible. The goal is transform the $\beta\beta$ manifestation into some degree of coincidence signature for major BG rejection. This powerful possibility might benefit from LiquidO's ability to doped, but the full range of consequences is under consideration. This first step is for now restricted to barium tagging [16] upon Xe $\beta\beta$ decay in the context of the NEXT TPC detector [17]. As of today, there is no specific LiquidO R&D effort here. Second, the experimental tagging of sub-fraction of $\beta\beta$ decay due to excited states, as oppose to ground-level decay (i.e. only $\beta^-\beta^-$ observation), could also be very powerful in LiquidO. Such excited states lead to unique topologies (multi-$\gamma$ emission together with $\beta^-\beta^-$), where LiquidO might yield even a background-less regime. Excited states are expected in all or most isotopes, although their branching ratios for the $0\nu$ are unknown, even if they were fully measured in the $2\nu$ case, like for Nd [18] and other isotopes. Branching ratios are not expected to be identical. However, the $2\nu$ sample provide experimental validation and characterisation of this unique signature. Despite potential, today's unknown branching ratios unfortunately prevent this strategy to become part of the leading criterion for the experimental design. Regardless, this is a factual possibility exploitable in LiquidO with unique high detection efficiency, should branches ratios happen to be large enough; i.e. hypothetically >10%. Third, LiquidO $e^+$ PID is very powerful [6], thus making it an ideal framework for $\beta^+\beta^+$ decay tagging with low or no BG, for example, using $^{106}$Cd [19]. Currently, the $\beta^+\beta^+$ is not considered top priority for $0\nu$ discovery due to several diminishing arguments. However, it might be important, if the $0\nu$ was discovered [20]. No $\beta^+\beta^+$, even the SM allowed case, has ever been observed.

# High Mass $\beta\beta$ Decay Projections

The FIG. 2 illustrates the possible phase-space region explored by a hypothetical LiquidO as compared to past (black points) and envisaged (grey points) experiments. The diagonal line describes the statistics only sensitivity per isotope, considering the impact of phase-space factor – only shown for Te. From this viewpoint alone, Nd, Mo and Te are particularly favourable. Today's exclusion is dominated by the KLZ data (pink shared region) considering the range in matrix elements calculations, as indicated in the legend. The excluded region is also shown (red shaded region). Today's experiments has barely reach the 100 kg range, driven by KLZ upon fidualisation. Forthcoming experiments within 2020-2030, including the upgrade of existing cases such as KLZ-800[21], will start exploring the range up to 1 ton isotopic mass. The most ambitious nEXO [22] proposal aims for a target mass of up to 5 ton enriched. Despite almost one order of magnitude less mass, the sensitivity of CUPID exhibits a remarkable potential as one of the best experiments in the next generation. Experiments beyond 2030 are not shown, where a pertinent highlight is the hypothetical JUNO-$\beta\beta$ [23].

The LiquidO potential is illustrated by two sets of points (blue) with masses beyond 10 ton of natural Te – no enrichment. The detector size assumes a 1 kton detector like KLZ or SNO+, as goal. However, the LiquidO technique not needing a buffer might go even larger volume (up to 3×) within the same site, if reused. The impact loading can be best ap-

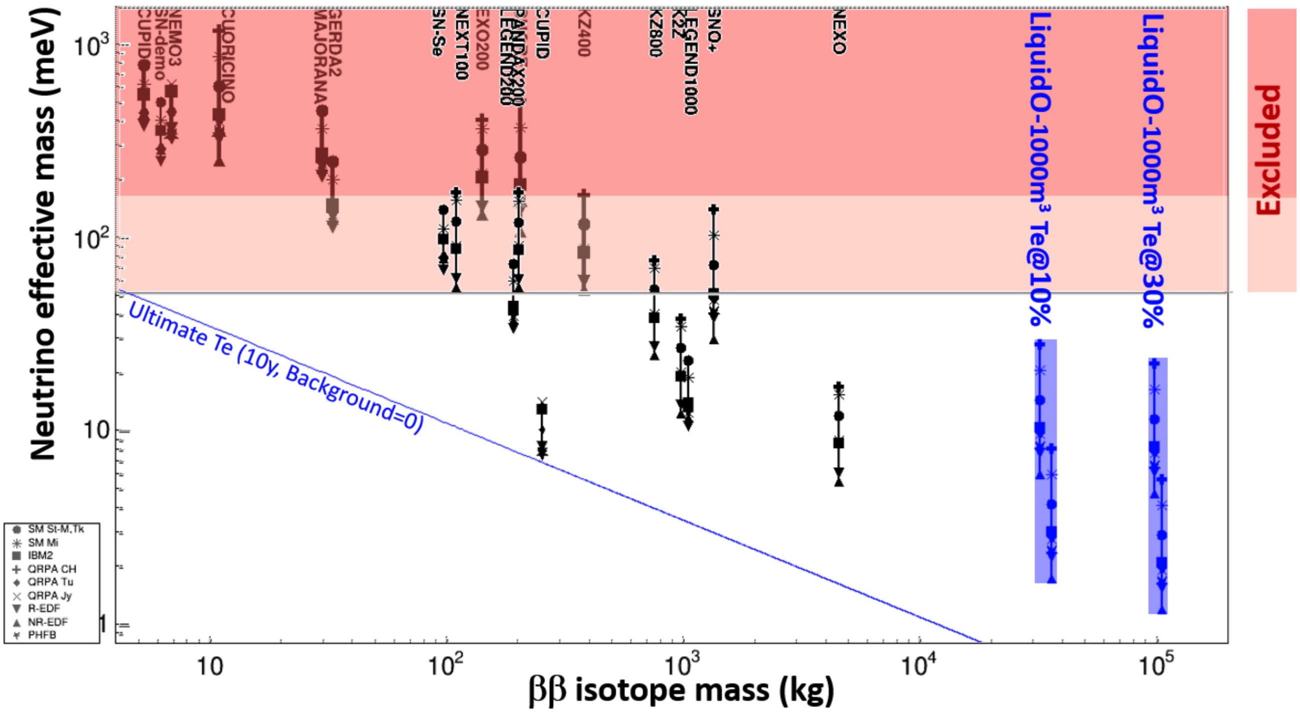


Figure 2: $\beta\beta$ **decay Sensitivity Evolution.** Current (black) and next (grey) generation of $\beta\beta$ experiments are shown in comparison to a hypothetical LiquidO potential (blue) upon the completion of the ongoing R&D. Detailed description is in the main text.

preciated when comparing the mass range difference between SNO+ and LiquidO. LiquidO here considers 10% and the most ambitious 30% loading cases with a 10 years exposure. Higher masses are necessary to increase the statistical sensitivity dramatically, but the control of BG remains critical to avoid an effective saturation. Despite the limited energy resolution, LiquidO's sensitivity improves significantly and grows with exposure. The inclusive loss of sensitivity relative to statistical limit is <4×. The BG indeces considered here are $10^{-4}$ (top point) and $10^{-6}$ (bottom point). This assumes a KLZ control (order$10^{-4}$) enhanced by the active BG rejection power provided by LiquidO's PID (up to $10^{-2}$). The $2\nu$ BG is about $10^{-6}$ BG index equivalent. The final feasibility depends on the ongoing R&D, where the LiquidO's performance to be accurately quantified. The LiquidO-$\beta\beta$ program remains in R&D demonstrator phase, hence not ready for large scale experiment articulation yet. However, LiquidO potential is evidently large leading to a high mass at low cost breakthrough in the field with unique ability to articulate different isotopes, including Xe, if necessary. Hence, LiquidO is expected to play a key role in $\beta\beta$, whose flexibility adds unique complementary to the world strategy.

# Double-Beta Synergies in CNRS

One of the most interesting features of LiquidO preliminary studies and potential is that it naturally exploits much of today's state of the art existing knowledge in CNRS/IN2P3 on both MeV neutrino detection and $\beta\beta$ decay. This implies decades of expertise in physics and leading knowledge on the most important isotopes (Mo, Nd, Se, Te), ultra pure BG control and facilities [24]. For example, CUPID expertise has a large synergy potential in terms of the possible exploitation of Te and Mo complementary to the NEMO3 and SuperNEMO framework with additional expertise in Se and Nd. In LiquidO, already two independent communities – MeV neutrino detection and $\beta\beta$ – push commonly towards the exploitation of state of the art scintillator detection technology, including the LiquidO technique pioneering teams in CNRS/IN2P3. So, our approach (skipping further details) involves a tight synergy strategy in terms of expertise, logistics (common facilities, etc) and technology for the first time in France, leaving CNRS/IN2P3 laboratories in leading position.

# Prospect & Conclusions

The LiquidO-$\beta\beta$ R&D program is here briefly described for the first time in terms of its technological breakthrough potential to attain an improvement of one of order of magnitude in $m_{\beta\beta}$ sensitivity relative to today's KLZ limit. If successful, this might open for an explorations <10 meV with multiple isotopes potential. However, LiquidO approach must complete the compelling R&D demonstration before it can be considered ready for any experiment. In brief, LiquidO potential relies on a unique combination of experimental features ideal for $\beta\beta$ searches such as multi-isotope capability, large multi-ton isotopic mass at low cost (i.e. no enrichment) via unprecedented large loading, low background framework and unique sub-atomic imaging enabling PID for active background rejection. In addition, LiquidO detection framework flexibility might have the potential to adopt further powerful experimental solutions being under active R&D exploration elsewhere in the field. We expect LiquidO's R&D phase to last about 5 years from now, thus engaging some scientific activities within CNRS/IN2P3 within the 2020 decade in partnership with the LiquidO international collaboration.

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