Dear Editors,

Thanks for the review of our article. Please see attached the revised version of the paper, modified in accordance to referees recommendations. The changes are highlighted using revision command, as recommended. See below the list of changes and replies to referee comments.

With best regards,
Alexei Buzulutskov
Corresponding author

List of changes and replies to the comments.

REPORT A

REFEREE A - Second Report

Comment 1. I will ignore the ramblings of the author regarding lobbies etc in their reply and concentrate on their manuscript. I acknowledge their additions to clarify the gamma background, which they themselves agreed was a weakness. This has been resolved to a reasonable extent. It is not clear why a full Monte Carlo of the actual experiment could not be performed, but this at least sets the scale for the gamma contamination that could otherwise be perceived to bias their result.

Reply: thanks.

Major comments

Comment 2. I maintain my comments regarding the introduction. The suggestion that the discrepancy between the null result experiments and those who claimed to observe a signal cannot be explained by any reasonable systematic uncertainty associated with the calibration of the noble liquid detectors. In any case the authors go on to recognize that that there is ample experimental data on such yields [for LXe]. So are they suggesting that all those measurements are wrong?

My comment on the relevance of this measurement for the detection of coherent neutrino scattering was ignored. None of these things take merit away from the work, as I tried to explain in my previous review. Rather, its value is to elucidate on a microscopic understanding of the ionization yield in liquid argon which complements other measurements conducted at lower energies. Those, e.g. that from Joshi et al, are indeed relevant for coherent neutrino scattering. I urge the authors to reconsider those introductory paragraphs if they want their paper to be taken seriously.

Reply. We finally agreed with this comment. The introduction has been accordingly modified: namely the motivation based on confusion situation in dark matter search, as well as two references trying to explain that by the calibration problem, have been removed. Coherent scattering is not anymore used as a basic motivation for current study. Moreover, we mention that our results complement those at lower energies. Now the first two paragraphs of the introduction are as follows.

The energy calibration of nuclear recoil detectors is of primary importance to rare-event experiments [**?**] such as those of direct dark matter search [**?**, **?**, **?**, **?**, **?**, **?**, **?**, **?**, **?**, **?**, **?**, **?**] and coherent neutrino-nucleus scattering [**?**, **?**]. Such a calibration, in particular in liquid Ar and Xe detection media, is usually performed by measuring the ionization yield and scintillation

efficiency of nuclear recoils, using neutron elastic scattering off nuclei (the latter imitating the interaction with dark matter particle or coherently scattered neutrino). While for liquid Xe there is an ample of experimental data on such yields [**?**, **?**, **?**], little is known about the ionization yield [**?**, **?**] and scintillation efficiency [**?**] in liquid Ar.

Recently the first results on the ionization yield of nuclear recoils in liquid Ar have been presented, in the lower energy range: at 6.7 keV [?] and 17-57 keV [?]. In the present work, the ionization yield of nuclear recoils in liquid Ar has for the first time been measured at higher energies, at 80 and 233 keV. These results complement those measurements conducted at lower energies and thus might be relevant to the future dark matter search experiments [?, ?] and to thorough understanding of the ionization yield in liquid Ar. ...

Comment 2. My only remaining major comment is that regarding the energy dependence of the yield: although I agree that it is worth pointing out that it appears similar to that predicted by the Jaffe model, it is not reasonable not to point out (in the abstract) that there is a significant difference in absolute value. In the conclusions this disagreement is described as slight, which is inappropriate. Figure 6 shows this to be of the order of 60% - this is not slight, it is significant. I propose that they amend their new sentence in the abstract to include this: The energy dependence of + is similar to that predicted by the Jaffe model, although the absolute value is substantially lower or words to that effect. This is, after all, a significant result of their study.

Reply. In fact the Jaffe model perfectly describes even the absolute values of the ionization yield if the ratio Nex/Ni is taken as a free parameter, resulting in Nex/Ni=2.3. Accordingly we don't need to modify the conclusions and abstract. However, such a large ratio value needs the justification. To clarify the situation with Jaffe model, we changed the Fig.6 adding to it the Jaffe model curve at Nex/Ni=2 and modified the last paragraph in theoretical model section as follows.

In the lack of theoretical and experimental data for liquid Ar, the ratio N_{ex}/N_i can be taken here either equal to that measured for nuclear recoils in liquid Xe [**?**], namely N_{ex}/N_i=1, or 10 times greater than the ratio for electron recoils in liquid Ar (in the same way as it was for Xe - see table 2 in [?]), namely N_{ex}/N_i=2. One can see that the Jaffe model can probably consistently describe the experimental data in terms of the energy dependence, and even in terms of the absolute values in the latter case.

Minor comments

Comment 3. Line 43: parameterization rather than parameterizations

Reply: corrected.

Comment 4 Line 46. The terminology electron-equivalent recoils is misleading here. The authors are probably referring to electron recoils, the calibration of which is often characterized by an electron equivalent energy.

Reply: agree. Corrected as follows.

Equations 1 and 2 are valid for both electron recoils, induced by electron or gamma-ray irradiation, and nuclear recoils; it is conventional to refer to the corresponding recoil energy in units of keVee (electron-equivalent) and keVnr.

Comment 4. Experimental setup. New paragraph added, starting line 106: it is not at all obvious how the neutron detector will later on be used to determine the photon flux in the LAr chamber; a short sentence would be useful here.

Reply: agree. The paragraph is modified as follows.

In addition, a neutron scintillation counter made of stilbene ($C_{14}H_{12}$) was enabled [**?**]; it was placed close to the CRAD active volume, just underneath the neutron generator. The counter could effectively separate neutrons from gammas using a pulse-shape analysis and thus estimate the gamma-ray background due to (n, γ) reactions in the two-phase CRAD.

Comment 5. Experimental results. Line 121: using THE 59.5 keV line (missing word)

Reply: corrected

Comment 6. Line 127: suggest indicating that the resolution is expected to be reasonably spatially uniform, since this is relevant later on for the neutron data.

Reply: agree. Corrected as follows.

The resolution is expected to be reasonably spatially uniform and practically independent of the energy ...

Comment 7. Line 153: for THE given x-ray tube (missing word)

Reply: corrected.

Comment 8. Line 181: by THE Klein-Nishina formula (missing word)

Reply: see reply to the next comment.

Comment 9. Line 182: which can be approximated by a decreasing function of energy: this suggests that the KN formula can itself be approximated by such a function, but that statement refers I believe to the differential spectrum instead: so the word which on line 182 should probably be and. In addition, the shape of the spectrum depends on the energy deposit range and on the photon energy. This should be clarified.

Reply. The linear approximation is actually follows from Fig. 10.1 of ref. 37, which in turn can be derived from the KN formula. To clarify this, the sentence is simplified, removing reference to the KN formula, as follows.

At these photon energies the spectrum of electron recoils due to Compton scattering at the given recoil energies can be approximated by a linear decreasing function with energy (see Fig. 10.1 in ref. [**?**]).

Comment 10. Fig 3 caption: the contribution of the latter being set to: is set the right word here? Also, in the last line of the caption, the comma is unnecessary.

Reply: corrected.

Comment 11. Comparison with other experiments. Line 279: The models referred to in [42] are not GEANT4 models (i.e. they are not the responsibility of the GEANT4 Collaboration and are not released with GEANT4; the authors probably mean eGEANT4-based models or similar. In any case the Monte Carlo tool is not important here.

Reply: agree. The reference to Geant 4 has been removed:

...predicted in some computer simulation models [**?**]...

Comment 12. Line 325: but slightly disagrees in terms of absolute values is not a tenable statement as mentioned above. There is no way in which the disagreement shown in Fig. 6 can be described as slight!

Reply. See reply to comment 2.