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Referee report

The paper is clear and well written, the works seems carefully done and subject is interesting, I definitely recommend it for publications. I have only a number of minor improvements, to propose.

References. I think a few more references should be given. For example: the extensive work done on this subject since many years by the team of Coimbra (R. Ferreira Marques and co-workers) should be mentioned.
On time resolution achievable with LYSO, there are several excellent papers by the team of Stefan Gundacker and co-workers. (e.g. Phys Med Biol. 2015 Jun 21;60(12):4635-49. doi: 10.1088/0031-9155/60/12/4635. Epub 2015 May 28.
Sub-100 ps coincidence time resolution for positron emission tomography with LSO:Ce codoped with Ca. Nemallapudi MV1, Gundacker S, Lecoq P, Auffray E, Ferri A, Gola A, Piemonte C.)

The subject of the paper is the time resolution that can be achieved. Nevertheless, it would be good to have a brief explanation on how they thy plan to deal with the problem that in many events there will be a two or more interactions, (2 comptons, or a compton + photoelectric) in the volume.

Besides the above, I have the following two very minor suggestions
1) Title: PET scanner based ON liquid xenon.
2) Page 3: the photo fraction in xenon should be mentioned, I think it is 22%.

Editor report

The paper describes a Monte Carlo study on the possibility of achieving an extremely good time resolution in liquid xenon scintillation detectors which could be used in positron emission tomography. The photon time-of-flight information, although not sufficient for determining the positron source position with the required precision (of the order of mm), would substantially improve the signal-to-noise ratio in the reconstructed image by restricting the reconstruction procedure to a smaller region than the object size. This idea is being extensively explored in PET with scintillation crystals and, in fact, has been previously discussed in connection with liquid xenon due to its excellent scintillation properties. However, to my knowledge, it was never supported by such detailed simulations as in the present paper. From this point of view, I find the subject very interesting and the work worth to be done.

There is, however, a number of important issues that have to be clarified and corrected before the paper can be considered for publication in JINST. Some of the basic assumptions made in the simulation are not supported by solid arguments (and some others are not correct) that makes questionable the obtained results. The most important ones are listed here.

1. Practically all calculations are based on the idea of so called 'digital resolution' which includes a reduction factor of $1/\sqrt{12}$. I am personally not comfortable with this idea for the following reason. The response function of a pixelized detector with digital readout (i.e., 0 or 1 signal) is a box-like (uniform) distribution with a width equal to pixel pitch, p. Formally, the r.m.s of a box-like distribution is indeed $p/\sqrt{12}$. However, this does not mean that this value represents the capability of the detector to resolve two events (which is, at the end, the definition of spatial resolution) with the distance of $p/\sqrt{12}$ between them. It is still equal to the pitch. The *fwhm* value of the box-like distribution

2

is exactly equal to its width, i.e. p again. To make sure of that, it is sufficient just to look to the distribution. The passage from *rms* to *fwhm* by multiplying $p/\sqrt{12}$ by 2.35 is incorrect because $p/\sqrt{12}$ is for box-like distribution and the factor 2.35 is only valid for normal distribution. I think, the whole paper should be revised to correct this error.

2. It is assumed in the simulations that the spatial resolution of 2 mm is achievable with the proposed design both in XY plane and DOI. This assumption has significant impact on the results but is not supported by simulations. The value of 2 mm has been indeed recently published by the authors in Spectrochimica Acta, vol. 118, pp. 613, Apr. 2016, but one cannot find proves even there. I understand that some assumptions are inevitable. However in this case it must be absolutely clear to the reader that this or that **is your assumption**. Even better — make a table of all parameters used in the simulation stating the source or saying something like 'our assumption'.

3. Light yield of 60 photons/keV for liquid xenon is, again, an important assumption and must be regarded as such – see below (by the way, no reference to the source of this value is given).

4. Page 12, first paragraph. The concept of group velocity is not applicable for individual photons emitted in uncorrelated atomic transitions and propagating in the medium independently in different directions. Each photon, once emitted, is described with a well defined wavelength. The probability to have a certain wavelength is described by the distribution shown in Fig.1, though.

5. In general, I find that the paper lacks clarity in some parts (e.g. it is not clear what is presented in Fig.9 – see specific comments below), references to the data (e.g., xenon refraction index, PTFE reflectivity, light yield) and details on the simulation (e.g., the reflectivity profile for the walls).

Specific comments

1. Title – CRT is not a commonly used abbreviation. I think it is better to replace it.

2. Fig.1 – reference is needed.

3. P.2, last paragraph - the number of 60 ph/kev requires reference. As far as I am aware, such high light yield is not confirmed experimentally. Continue to be just a hypothesis. A reference to the source must be given.

4. Ibid. Do not understand why the well known and easily accessible data are presented as approximate, e.g. 'at a temperature of ~ 160K and atmospheric pressure LXe has a reasonable(y) high density (3 g/cm^3) and an acceptable attenuation length (364 **!!** mm)'. First, at 160K xenon is solid; second, for 1 bar vapour pressure the temperature should be 165K; third, at 165K the density is 2.94 g/cm^3; fourth, the attenuation length (for 511 keV gammas, I believe – should be said) is 3.78 cm.

5. P.3, number 2 in the list – check the first sentence '...then, than, when ...'

6. Ibid. Check the sentence 'in LXe it is possible to identify Compton events depositing all their energy (why all only?) in the detector as separate-site interaction, due to its (what?) relatively large interaction length.' Does not seem to be easy in a scintillation detector. However, this is indeed possible if charge signal is measured (give reference).

7. Number 3 – wrong temperature again. Suggest rephrasing the whole paragraph. I failed to catch the meaning, sorry.

8. Ref.4 should appear in the paragraph where decay processes are described, e.g. '... $\tau_2 = 27$ ns [4]'

9. P.4. Again, why $T_r \sim 15$ ns? *Kubota e.a.* give a well defined value of 15 ± 2 ns. Which value exactly has been used to build the plot in Fig.2?

10. Ibid. I think, the usage of the word *instead* is incorrect. Please, check throughout the whole text.

11. Fig.2 must have 0 on the vertical axis. It induces the wrong idea that almost all the light is emitted in 10 ns otherwise. Consider also extending the horizontal axis to large times.

4

12. Ibid. The figure caption is unclear. Suggest to indicate that the blue dots are obtained with Eqs.(2.1–2.6) using time constants and intensities from Ref.4, while the red line is the same data fitted with a function in Eq.(2.7).

13. Please, explain why do you need parametrization in the form of Eq.(2.7) of the data which are expressed in analytical form already.

14. Fig.3 – Indicate coordinate axes and the direction of incidence of γ-rays.

15. P.6, first paragraph. – MAJOR. The value of 3% contribution from photoelectron statistics and geometrical effect should be justified. Referring to Ref.3 is not helpful because that paper does not provide proves either.

16. P.6, second paragraph. – MAJOR. Factor of $1/\sqrt{12}$ – see the list of major issues above.

17. Ibid. – The following statement 'Weighting with the SiPM amplitude improves the resolution to about 2 mm FWHM. The DOI is obtained by computing the ratio between the total signal recorded in the entry and exit face, and its resolution is found to be also 2 mm FWHM.' is unclear. Does it mean that the digital resolution can be improved by using additionally analogue signal weighting, or you are speaking about an alternative (centre-of-gravity, I suppose). Anyhow, the figure of 2 mm needs to be proven.

18. P.6, last paragraph. – 'Conventional SiPMs can reach today a PDE of around 50%'. – Reference is needed. Do you really need Fig.4?

19. Fig.6, in my opinion, rises more questions than answers. The title of the horizontal axis is misleading. Can one conclude from it that depositing (somehow) 1 mm of TPB on top of 1 mm thick quartz substrate results in 2.2 ns decay time?? '...(thus its concentration)...' – concentration in what? '...asymptotic value of 2.2 ns...' – no asymptotic behaviour is observed in Fig.6. I would suggest removing the figure or dedicate a paragraph in the text to explain what it really shows, which does not seem to me easy to do, in a short way at least. Otherwise, it can only be understood after a deep reading of Ref.13.

20. P.8, first paragraph. 'For sufficiently large TPB concentrations, the decay constant may be of the order of 1–2 ns.' – proves are needed. I could not find them in Ref.13

21. P.8, last paragraph. Details on simulation of scintillation light propagation must be give: absorption, scattering, reflection model on the surfaces, reflection coefficients and profiles.

22. Ibid. References to the the values of refractive indices should be provided. '...we take $n_1 = 1.54$ as the refractive index of the SiPM window...' – Why is that? What is the window material? Is it the same for all types of SiPMs mentioned in the paper? How the wavelength dependence is taken into account?

23. Ibid. For liquid xenon, for example, I read 1.69 in NIMA516(2004)462 for xenon scintillation light, and about 1.4 in the visible region (J. Chem. Phys. 123(2005)234508) which is quite different from what is used in the simulation.

24. P.9, Eq.(4.1) – Expression of the difference between t_2 and t_1 on the left side of the equation in terms of $t_2 - t_1$ on the right side looks strange (although mathematically correct). Why not just the obvious $\Delta t = \frac{\Delta d_g}{c} + \frac{n \Delta d_p}{c}$? Please, explain the reason.

25. Fig.8 – A reference to the data must be provided. Is there a reason for not showing the visible wavelength region? Already mentioned J. Chem. Phys. 123(2005)234508 has this information.

26. P.9, footnote – What is 'number of FWHM'?

27. P.10, Fig.9 and the text. It is not clear what configuration is simulated – with SiPMs on one side or on both sides as in Fig.7? If Fig.9 shows the distribution of the arrival time of the first photoelectron (probably better say 'first scintillation photon'), how it happens that negative values appear? What does it mean '...after subtracting the time of flight of the **incident gamma** $(\Delta d_g/c)$...'? $\Delta d_g/c$ was defined as time-of-flight difference between the **two** gamma-photons if I am not wrong.

28. Ibid. What is the meaning of the vertical scale 'Fraction of events'? Fraction of what, precisely? Why the integral is about 0.25? What happened to the rest? What is the definition of 'event'?

29. Fig.12 – From Figs.1 and 8 one finds that for xenon emission spectrum the refraction index of the liquid varies by 20%, at most (taking limits at 1/10–th of the emission peak). Why is that the light speed changes by a factor of more than 2 in Fig.12?