Addendum to

“On an unverified nuclear decay and its role in the DAMA experiment”

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We reply to the critiques of our paper arXiv:1210.5501 by the DAMA collaboration which appeared in arXiv:1210.6199 and arXiv:1211.6346. Our original claim that the observed background levels are likely to require a large modulation fraction of any putative signal holds. In fact, in light of DAMA’s recent comment our claim is further corroborated. We identify the source of the discrepancy between our own analysis and DAMA’s claimed levels of unmodulated background. Our analysis indicates that the background in the signal region as reported by DAMA is indeed likely underestimated.

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In a recent publication [1] we pointed out that the empirical verification of a certain special decay branch of $^{40}$K remains outstanding and discussed the importance of such a measurement. In addition, we discussed the general role $^{40}$K plays as an important background in the DAMA experiment [2]. Shortly after our paper appeared, the DAMA collaboration has criticized some of its findings [3]. We replied in a previous version (v1) of this manuscript which prompted the collaboration to post another comment in [4]. To help everybody else to keep better track of the discussion, we address both comments by the DAMA collaboration, [3] and [4] jointly. All our previous statements remain intact. There are three points of substance in [3] and [4]:

**Critique 1:** In [3] the collaboration states that the contribution of this special decay of $^{40}$K to the total $^{40}$K contribution is only about 10%. They therefore criticize our discussion of this branch as "captious". In addition, in [4] it is claimed that our calculation for the theoretical prediction of the $^{40}$K decay directly to the ground state of $^{40}$Ar is not performed correctly because the branching from the K-shell had not been taken into account.

**Reply:** Our calculation of the ratio of K-shell electron capture rate to the $\beta^+$ emission rate is correct and does not miss the K-shell branching because we make direct use of the K-shell atomic wave-function. The mention of higher-shell captures by the collaboration is captious: these transitions are subdominant relative to the K-shell capture, $\sim 10\%$ (much smaller than the overall uncertainty on the ratio); they also contribute to energy depositions only below threshold. More importantly, the total decay of $^{40}$K is a serious background in the DAMA experiment and it was in fact one of our findings that this special branch is subdominant to the one where $^{40}$K decays into the $^{40}$Ar excited state, followed by a 1.46 MeV gamma-ray which escapes detection. In their comment [3] DAMA quotes (for the first time in print) that the former contribution is 10% of the total $^{40}$K contribution. This is indeed in good agreement with what we found using the theoretical branching ratio and using the independent Monte Carlo simulation of Ref. [5]. This agreement gives further credence to our treatment of potassium decays in DAMA.

**Critique 2:** In DAMA’s reply [3] to our paper the collaboration states that the content of $^{nat}$K has been measured by investigation of the double coincidence, that it is independent of the branching ratio into the ground state, and that its average value over all crystals is 13 ppb. Statements based on 20 ppb are therefore wrong. In [4] the collaboration reminds us that the number 13 ppb is published in [6] and refutes our critique that no discussion supporting this number is available would be unfounded and not justified.

**Reply:** No discussion or mentioning of the average $^{40}$K contamination of 13 ppb can be found in the published TAUP conference proceedings [6]. Neither could we locate this number in any other of DAMA’s published works. The average contamination of 13 ppb can indeed be found on slide number 8 in the TAUP talk by Nozzoli [7]. Importantly, however, no data is presented to support this number (such as e.g. the individual crystal-by-crystal contaminations,) nor does the collaboration provide the uncertainty associated with it. May that as it be, our results are presented for a whole range of $^{nat}$K contaminations between 1-100 ppb including the DAMA quoted maximum individual crystal contamination of 20 ppb, which appeared in the official publication describing the DAMA apparatus [8]. Using 13 ppb we find that the required signal modulation fraction is above 20%. In addition, in [1] we identified a way to measure the contamination level in a manner that is free from the Monte Carlo modeling required by the coincidence method. Considering the immense importance of the precise amount of potassium background to any interpretation in terms of a dark matter signal the lack of details so far provided by the collaboration is unsettling.

**Critique 3:** Finally, in their reply [3] to our paper [1] the DAMA collaboration claims an upper limit of $S_0 \leq 0.25 \text{ cpd/kg/keV}$ for the unmodulated part of the signal in the 2-4 keV energy bin. Given a residual rate of $0.0194 \pm 0.0022 \text{ cpd/kg/keV}$ [2], this would allow for a modulation fraction of 6-10% which can be accommodated with many models of Dark Matter [9], contrary to our conclusions. Our conclusions are based on the assumption of a flat background component at the conservative level of 0.85 cpd/kg/day. In the follow-up [4] the DAMA collaboration criticizes this assumption as "completely arbitrary" because 1) it is not based on the knowledge of background contributions and 2) an assessment of backgrounds without accounting for a signal contribution $S_0$ in a fit is “always methodologically incorrect.”
Reply: What regards DAMA’s critique of our “ad-hoc” assumption of a flat background: As argued in our original work [1], a flat background component is expected because it is a universal feature of $\beta^-$ decays for small electron velocities and it is also typical of low-energy Compton background. The question really is: how large is it? Admittedly, this may be very difficult to quantify precisely even with the full insight into the crystal-by-crystal spectrum as it presents itself to the DAMA collaboration. However, as we argued in our work, inspection of the “signal-sidebands” between 5 – 10 keV and 20 – 40 keV (there is no published data between 10 – 20 keV) strongly supports the notion of a flat background contribution at the 0.85 cpd/kg/day level. Given that such flat backgrounds typically span decades in keV recoil energy (depending on the Q-value of the parent decay) we do not think that a joint fit including a contribution from $S_0$ is mandatory for proposing our hypothesis, nor is it going to be rejected by it. In fact the value 0.85 cpd/kg/day in our original work is not the result of a fit but is the rate associated with the lowest point (at $E = 4.6$ keV, see dashed black line in Fig. 1). Our purpose in [1] was to raise awareness that such levels of background may very well be present in the experiment—and DAMA has not provided a detailed discussion to convince us otherwise—hence challenging the DM interpretations which come with weak modulation fractions $\lesssim 20\%$.

What regards the upper limit $S_0 \leq 0.25$ cpd/kg/keV on the unmodulated signal part as mentioned in DAMA’s comment [3]: The collaboration says that they account for this limit in [9] when assessing the viability of various DM scenarios. Similar to our reply of critique 2, we could not find this number or a discussion thereof in any of DAMA’s published works. Again we have to resort to slide 8 of the talk by Nozzoli [7] where the limit is quoted and a curve for the background model is reported. In Fig. 1 we show our reproduction of this background, which is a Gaussian centered at 3.2 keV together with a linear fit as explained in the Appendix. As is clear from the plot, the linearly rising function is not supported by the data above 4 keV (the statistical error bars are shown but are barely visible.) More importantly, the decrease towards lower energies strongly supports the notion that the background present in the signal region 2-4 keV is underestimated. In the Appendix we offer details and a further-going discussion of this.

What should be clear from this reply is that a more detailed and careful discussion of backgrounds by the DAMA collaboration is called for. That was among the main conclusions in [1] where we also identified measures which could help to clarify some of the uncertainties. We invite the interested readers to consult the appendix where more details are presented and to reproduce our results themselves.

1 However, the reader is referred to the text below and to Fig. 2 where we address directly the effects of including the signal in the fit.
FIG. 1. The dots (with error-bars) are the single-hit rate reported by DAMA in [8]. The thick red curve shows our reproduction of the background curve presented by DAMA in Ref. [7]. The dashed curve from 6.2 keV to 10 keV is the continuation of the linear trend. The linear trend is obtained by fitting the data between 5.3 keV and 10 keV and results in excellent agreement with the reported background [7], as explained in the Appendix. Using the same Gaussian, but with a better model for the background above 5 keV (flat + linear), we obtain the solid green curve. In blue, we show the resulting curve in the case when natK contamination level is increased to 14 ppb instead. We emphasize that we did not fit to the data below 5 keV, but used the Gaussian as reported by the collaboration in [7].

Appendix A: Details of the analysis

The unmodulated background presented on slide 8 of Nozzoli [7] is reproduced accurately by fitting a linear function to the data points between 5.3 keV and 10 keV and adding to it a single Gaussian whose parameters are taken directly from ref. [7],

\[
\text{BKG}_{\text{Nozzoli}}(E) = \frac{A}{\sqrt{2\pi}\sigma^2} \exp \left( -\frac{(E - E_{K40})^2}{2\sigma^2} \right) + \text{slope} \times E + \text{intercept}. \tag{A-1}
\]

The amplitude of the Gaussian is \( A = 0.64 \) which is in good agreement with the results of [5] with a natK contamination of 13 ppb. The center and spread of the Gaussian are in good agreement with the expected 3.2 keV energy deposition \( (E_{K40} = 3.15 \text{ keV}) \) and resolution \( (\sigma = 0.618 \text{ keV}) \). One would naively expect that the quoted upper limit \( S_0 \lesssim 0.25 \text{ cpd/kg/keV} \) was derived by subtracting this simple background model from the data. However, in contradiction to what the DAMA collaboration is quoting this yields only \( S_0 \approx 0.14 \text{ cpd/kg/keV} \) in the 2-4 keV energy bin as can be easily checked.

Given the negligible statistical error bars, it is seen in Fig. 1 that the linear fit in Eq. (A-1) provides an inadequate description of the data between 5-10 keV where little DM signal is expected. A flat background up to about 7 keV where it is broken and followed by a linearly rising one provides a much better agreement with the data.\(^2\)

\[
\text{BKG}_{\text{linear+flat}}(E) = \begin{cases} 
0.866 & E < 7.05 \text{ keV} \\
0.491 + 0.053E & E > 7.05 \text{ keV}
\end{cases} \tag{A-2}
\]

\(^2\) We have also tried a more general model involving two linear fits, but the results are qualitatively unchanged - the fit between 5-7 keV prefers to be flat.
Such a model is also physically well-motivated. The background from $\beta^-$-emitters is entirely flat at low energies. The rise above 7 keV is more difficult to interpret because the DAMA collaboration has not released the the immediate spectrum above 10 keV. For example, it could be a broad Iodine escape peak between 14-18 keV originating from external $^{210}$Pb contamination. Though such features have been measured previously with NaI(Tl) crystals [10], without further insight into the spectrum this remains a speculation.

We have also verified that a more general fit to the entire data (including the width and center of the Gaussian) does not result in any substantial change to the flat component which becomes 0.86 cpd/kg/keV, in agreement with what we used in our original work [1]. In fact, this model results in a disturbingly good fit to the entire data set as shown in Fig. 2. We emphasize that this fit is done with a fixed amplitude for the Gaussian determined by a contamination level of $^{nat}$K of 13 ppb as quoted by the collaboration. Only five parameters were allowed to float in the fit: the centre of the Gaussian; the resolution; the flat component; and the slope and intercept of linear trend. This surprising result shows how volatile the DM interpretation is to small changes in the background model and it again emphasizes the need for a more thorough discussion of backgrounds by the DAMA collaboration. In generating Fig. 1 above we attempted to remain as accommodating as possible and used the Gaussian curve quoted in [7] for modeling the bump and not the fit shown in Fig. 2.

One may also wonder how does the fit actually look like when signal is included. While the above discussion should make it clear that the inclusion of any signal with a small modulation fraction leads to a poor fit, we have included two simple examples to illustrate this point directly. We chose two benchmark points corresponding to two best-fit points of the DAMA-reported modulation amplitude: spin-independent elastic scattering[3] for low-mass DM ($m_{DM} = 12$ GeV and $\sigma_{SI} = 2 \times 10^{-40}$ cm$^2$) as well as high-mass DM ($m_{DM} = 60$ GeV and $\sigma_{SI} = 8 \times 10^{-42}$ cm$^2$).

The results obtained do not depend very strongly on the precise details of the model, but are mostly affected by the modulation fraction.
and $\sigma_{SI} = 8 \times 10^{-42}$) for halo parameters $\bar{v} = 220$ km/s and $v_{esc} = 550$ km/s. The unmodulated rate contributed by each of these signals is then included in the fit of the total rate. The results are shown in Fig. 2. The high-mass point is in complete disagreement with the data as can be expected since the modulation fraction in this case is small ($\sim 5\%$) and hence the contribution to the unmodulated rate is much too large. The low-mass benchmark is in slightly better agreement with the data since it enjoys a somewhat larger modulation fraction ($\sim 8\%$). However, it results in a very low flat component of $\sim 0.55$ cpd/kg/keV, which is in disagreement with the higher energy part of the unmodulated spectrum as discussed in the main text. This is a general feature of any signal for which the unmodulated component contributes significantly in the 2-4 keV region. On the other hand, a signal with a large modulation fraction ($\gtrsim 20\%$) would contribute very little to the total rate and can result in a satisfactory fit in accord with our original claim.

Finally, we stress that the purpose of the background analysis we present is not to claim a full understanding of the backgrounds. Our goal, as it was in our paper, is to call attention to this important background and encourage the collaboration to present a full account of its detailed understanding. That being said, the remarkable agreement of our simple background model with the data must give pause to anyone who wishes to interpret the DAMA results in terms of a signal of dark matter.

[1] J. Pradler, B. Singh, and I. Yavin. On an unverified nuclear decay and its role in the DAMA experiment. 2012.
[2] R. Bernabei et al. New results from DAMA/LIBRA. *Eur. Phys. J.*, C67:39–49, 2010.
[3] R. Bernabei, P. Belli, F. Cappella, V. Caracciolo, R. Cerulli, et al. Comment on ‘On an unverified nuclear decay and its role in the DAMA experiment’ (arXiv:1210.5501). 2012.
[4] R. Bernabei, P. Belli, F. Cappella, V. Caracciolo, R. Cerulli, et al. A few final comments to arXiv:1210.7548[hep-ph]. 2012.
[5] V.A. Kudryavtsev, M. Robinson, and N.J.C. Spooner. The expected background spectrum in NaI dark matter detectors and the DAMA result. *Astropart. Phys.*, 33:91–96, 2010.
[6] R Bernabei, P Belli, F Cappella, R Cerulli, C J Dai, et al. Technical aspects and dark matter searches. *J. Phys. Conf. Ser.*, 203:012040, 2010.
[7] Talk given by F. Nozzoli. 2009. [http://taup2009.lngs.infn.it/slides/jul3/nozzoli.pdf](http://taup2009.lngs.infn.it/slides/jul3/nozzoli.pdf).
[8] R. Bernabei et al. The DAMA/LIBRA apparatus. *Nucl. Instrum. Meth.*, A592:297–315, 2008.
[9] R. Bernabei et al. First results from DAMA/LIBRA and the combined results with DAMA/NaI. *Eur. Phys. J.*, C56:333–355, 2008.
[10] Susana Cebrian, J. Amare, J.M. Carmona, E. Garcia, I.G. Irastorza, et al. Status of the ANAIS experiment at Canfranc. *Nucl. Phys. Proc. Suppl.*, 114:111–115, 2003.