

Interactive comment on “Percent-level production of ^{40}Ar by an overlooked mode of ^{40}K decay” by Jack Carter et al.

Anonymous Referee #1

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Comments on the manuscript entitled “Percent-level production for ^{40}Ar by an overlooked mode of ^{40}K decay” by Carter et al.

The paper discusses a potential electron-capture (EC) branch from the ^{40}K ground state to the ^{40}Ar ground state. Although the paper contains some interesting aspects, I cannot recommend accepting this article for publication. In the following I’d like to give a reasoning for my opinion.

1. The title is a bit misleading. The paper is dealing with a potential EC branch that contributes with 0.2% to all decays of ^{40}K . Hence, it is not an additional “percent-level production of ^{40}Ar ”. I also do not agree with “overlooked” since many works consider this potential EC branch (as I will also explain in the following).

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2. The motivation of the article is – from my point of view – very weak. It is mainly based on the assertion that this branch is ignored or denied. This is not true. Considering nuclear decay data evaluations, nuclear physicists usually use only the most recent evaluation. Looking to two of the most important evaluation groups (ENSDF and DDEP), we find that an EC branch to the ground state was considered. Hence, the only remaining reference stated is Min et al. (2000). The authors do not mention several other publications which consider this branch. In line 53 they write “Many subsequent workers both in nuclear physics and geochronology have ignored this prediction.” but do not provide references.

3. “Egelkemeir” (line 70) or Engelkemier (line 73 and Fig. 2, ..) or “Engelkeimer” (Table 1)? Just one example that indicates that the manuscript was not prepared with great care.

4. Several parts in the paper correspond to textbook knowledge in nuclear physics and could be omitted (e.g. large parts in section 4). Moreover, I am wondering whether the theoretical approach presented corresponds still to the state-of-the art. The theory from Bambynek (1977) was a standard for long time, but in the past ~ 5 years, considerable progress was made by Prof. Mougeot in computation of beta (minus and plus) emission spectra and EC decay probabilities. I think his evaluation can be considered as state of the art. What is new or better in the paper under review? Note that the reference (Mougeot 2019) in Fig. 2 and in its caption should probably read “Mougeot (2018)”.

5. The notations used are sometimes confusing and/or false. For example various expressions are used for the ratio of EC and beta+ decay probabilities. Line 162, the results refers only to K EC.

6. Is section 3 (in particular its title) justified? The reasoning is based on the assumption that a beta plus decay exists. If it exists, I agree that we may also expect an EC to ground state. However, the existence of the beta + with its very low probability in the order of $1E-5$ is based on only few experiments. Did the authors consider that detected

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positrons could also arise from internal pair production as in the decay of ^{90}Y (i.e. from the beta minus side)? In this case, the whole reasoning would collapse.

7. Line 204: The authors question the uncertainty stated by Mougeot without providing any argument. At present, Mougeot is one person (perhaps the only) who can accurately calculate beta and EC decay with allowed and unique forbidden transitions.

8. Section 5: I cannot agree with the evaluation presented here. First, we must keep in mind that the results are correlated. Hence, a simple statistical consideration is not justified. Moreover, the choice of values that are taken into account appears to be very arbitrary. Most (actually all, except Mougeot) apply outdated models – as the authors do.

9. Section 6: Is it justified to use ^{22}Na as cross check? The nature of the decay (allowed for the dominant transitions and 2nd forbidden unique for the others) are different than for ^{40}K .

10. Section 6, Figure 4: There are more data. Why were they excluded? E.g. Applied Radiation and Isotopes, 66, 2008, 865-871 or Mougeot (2018) and Mougeot (2019) (Applied Radiation and Isotopes 154 (2019) 108884).

11. Line 241: “easy to measure” I do not fully agree. Also ^{22}Na is challenging, e.g. due to summing effects.

12. Line 245: x-ray with 511 keV? Perhaps “photon” or “annihilation photon”. Also “gamma-ray” would be acceptable.

13. Line 264: “The orbital electron with the highest probability of capture is from the K-shell; if this electron is captured, it results in the emission of a characteristic x-ray or Auger electron with an energy of 3.2029 keV, the binding energy of the K-shell of ^{40}Ar .” No! After K-capture we have the K binding energy available. To eject an X-ray requires the binding energy of another electron in an outer shell. Hence, the x-ray energy is lower than the K binding energy. For Auger processed even 2 additional shells are

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involved, which also means that x-rays and Auger electron do NOT have the same energy.

14. Line 269: “not tagged correspond to the the electron 270 capture to ground state decay”. Really? But then you need 100% detection efficiency for the gamma-rays. “the the”?

15. Reference “Di Stefano et al.”: List of authors is incomplete; Reference can be updated (Journal of Physics: Conference Series)

16. Line 60: “We describe experiments that could be made to measure this decay mode and also identify observations from nuclear physics experiments that offer evidence for its existence” I cannot find a sound description or proposal for such an experiment. Section 7 and the supplementary material are very weak. It is not clear how the two EC branches can be distinguished. What about the Auger contributions? If one could clearly identify x-rays as consequence of K EC, one would still need an x-ray emission probability. This is not even mentioned.

17. In general, the paper contains many formal errors and is not in compliance with ISO standard such as the GUM.

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