

Geochronology Discuss., referee comment RC1
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Comment on gchron-2022-16

Reto Trappitsch (Referee)

Referee comment on "Technical note: A software framework for calculating compositionally dependent in situ ¹⁴C production rates" by Alexandria J. Koester and Nathaniel A. Lifton, Geochronology Discuss., <https://doi.org/10.5194/gchron-2022-16-RC1>, 2022

In the manuscript: "A software framework for calculating compositionally dependent in situ ¹⁴C production rates", Koester and Lifton present new results for calculating cosmogenic ¹⁴C production rates for various minerals other than quartz. These calculations are based on previous models and represent an extension that allows experimentalists to calculate exposure histories for quartz-poor samples.

These new production rates significantly broaden the applicability of exposure age dating via the cosmogenic ¹⁴C to a variety of minerals other than quartz. The results present a major contribution to the field of geochronology and are therefore ideally suited for publication in this journal. I highly recommend publication of this manuscript, however, would like to propose several clarifications / edits.

Major Comments:

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You present production rates based on various minerals and give the composition of these minerals. Would it be easier / simpler to present elementary production rates and have the user ultimately calculate the overall production rate in a given mineral based on their specific composition? This could allow a user to easier work with the results from this manuscript. Please feel free to completely ignore this idea, I am not very familiar with the general approach in the field of terrestrial cosmogenic nuclides.

Section 3.1, second paragraph: In Reedy (2013), excitation functions for the production of ¹⁴C from elementary O and Si are presented and not from ¹⁶O and ²⁸Si. Generally, cross-sections are measured using materials with natural isotopic abundances. This makes more sense, since these elements also occur in geological samples of interest in their normal isotopic composition.

For excitation functions from JENDL/HE-2007: Did you take the values for pure isotopic

compositions as stated in line 128? I don't expect that your samples contain, e.g., isotopically pure ^{48}Ti . Therefore, a $\text{Ti}(n,X)^{14}\text{C}$ excitation function where all isotopes are included in their terrestrial composition should be used for the calculations.

Uncertainty determination for JENDL/HE-2007 excitation functions, last paragraph in Section 4.2. You estimate the overall uncertainties for the purely calculated excitation functions at 10-15%. The estimate is based on comparing the JENDL/HE-2007 (calculated, generally GNASH) with TENDL (calculated, TALYS). This comparison is not exactly fair. A better comparison would be to compare calculated excitation functions with measured ones, as, e.g., Broeders et al. (<https://doi.org/10.14494/JNRS2000.7.N1>), however, you obviously cannot do this for the reactions you are interested in. A better estimate to use for the uncertainty of calculated production rates is given in Reedy (2013), section 3.1, third-last paragraph: "[...] most formulae and codes give cross-sections for an individual nuclide that typically differ from measured ones by factors of ~ 2 (Ammon et al., 2009)". Your uncertainty estimate seems therefore far too optimistic.

Minor Comments:

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Section 3.1, first paragraph: This paragraph contains quite a lot of information that is not understandable without reading Lifton et al. (2014) first. You are already describing what goes into the model in a very good way in Section 2.1. For this paragraph, it would be good if you could explain all abbreviations (LSDn, PARMA, SHA.DIF.14k - if this is an abbreviation). Furthermore, could you provide some detail on what the gridded R_C and dipolar R_CD models of Lifton et al. (2016) are?

Figure 1: For the measured curves, it might be good to present the measurements as symbols, in order to easier distinguish between interpolated and measured values.