

Interactive comment on “Cambial-age related correlations of stable isotopes and tree-ring widths in wood samples of tree-line conifers” by Tito Arosio et al.

Anonymous Referee #2

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Review of “Cambial-age related correlations of stable isotopes and tree-ring widths in wood samples of tree-line conifers” by Tito Arosio et al.

This study presents the correlations between different tree ring parameters as tree-ring width (TRW) and stable isotopes (δD , $\delta^{18}O$ and $\delta^{13}C$) from samples collected from the Eastern Alpine Conifer Chronology for different cambial-age groups.

The scientific output of this paper is very low. Since the authors already presented the cambial-age analysis in another paper, published last year in the same journal (doi.org/10.5194/bg-17-4871-2020), I did not understand why the authors did not include analyses presented here, in that paper and decided to publish them separately.

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My feeling is just they want to artificially increase their number of publications, because the data and the scientific output presented here are not enough for an independent research paper, especially in such high impact journal as Biogeosciences. More than that, the presented paper cannot be read independently, as an individual research paper, in order to understand which kind of data were used. To understand the actual age-related trends of the presented data, it is necessary to read another paper, of the same authors, which was published in the same journal.

The authors present the correlation between TRW and i) $\delta^{18}\text{O}$, ii) δD and iii) $\delta^{13}\text{C}$. Such kind of correlations are rather useless, first of all, due to the fact that the authors already presented in another paper the cambial age trend of these four different tree ring parameters and secondly, because between these parameters does not exist any links. The TRW does not influence $\delta^{18}\text{O}$ and $\delta^{18}\text{O}$ does not influence TRW, the same with other parameters. The variation of the TRW is independent of the $\delta^{18}\text{O}$, δD , or $\delta^{13}\text{C}$ variations. The correlations are made between parameters that do not have a cause-and-effect relationship. When correlation analyses are performed, it is supposed to be a connection or a relationship between those two parameters, but in this case, the only connection can be the presence or absence of the trend in juvenile cambial age of the trees, and this aspect was showed in the previous paper.

The obtained correlations are due mainly to the trend of the data, and the trend is already a demonstrated fact of these series. When you correlate two data sets with similar or opposite trends, automatically you will get a correlation coefficient (positive or negative). And the explanation of such correlations, correlations between parameters that do not have a link between them, based on the physiological processes of trees are only speculations.

The paper even does not have a conclusion section. The last two sentences (three rows) of the discussion summarize the conclusions of the paper, but which does not bring anything new from the last published paper by the authors. (Line 158: In conclusion, our results confirm the existence of a juvenile phase in the δD and $\delta^{18}\text{O}$

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isotopes. . .).

The figures are of very poor quality. Moreover, the paper contains only 2 simple figures (bar figures), while the figures from supplementary contain too many very small figures, which are hard to follow.

Considering all the above-mentioned arguments, I conclude that the presented results of the paper present very low scientific significance and do not bring new/important scientific information, thus I recommend the paper to be rejected.

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2020-406>, 2020.

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