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Interactive comment on "A triple tree-ring constraint for tree growth and physiology in a global land surface model" *by* Jonathan Barichivich et al.

Anonymous Referee #2

I wonder if the authors have considered whether the decision to analyze cellulose might also influence the ability of ORCHIDEE to simulate isotope composition of tree rings? The mechanistic modeling used in ORCHIDEE is not necessarily specific to cellulose content of trees.
For carbon isotopes, we used the simple formulation of Farquhar. Admittedly, this is more comparable with leaf measurements than with cellulose in tree rings. However, this formulation has been commonly used in modeling studies because of its simplicity. This limitation is acknowledged in the Discussion (second paragraph of p16). Future studies will include a more complete parameterization for carbon isotopes that integrates better the post-photosynthetic and mixing processes that affect the carbon isotopic signatures of cellulose.

Below are some other minor changes to consider, some of which are necessary: p. 3 line 20, "in this type of model"
p. 6 line 21, "represented following a similar formulation as in other isotope-enabled" line 22, "isotopic composition"
p. 10 line 25, "conducted for the rest of the sites"
p. 11 line 23, "This linear decomposition method a quantifies the contribution of different"
All suggestions taken.

p. 12 line 26, for clarity, if the following is correct I suggest revising this sentence to "The source water and leaf-water enrichment series forcing 18Op in MAIDENiso and ORCHIDEE are not significantly correlated over..." If this is not correct, sentence should be rewritten for better clarity.
We meant that the dO18 of precipitation series were not correlated. This sentence was revised for clarity.

p. 15 line 16, "in Fontainebleau and the other five sites across" line 17, "better simulates"
Correction done.

p. 17
line 9, "enables application of known mechanistic relationships between isotopes"
p. 20
line 24, "our findings suggest"
p. 23
line 5, author names should be "Anderegg, W. R. L." and "Williams, A.P."

p. 24
line 34, italicize "Pinus radiata"
p. 25
lines 21 and 22, "Fritts, H.C."
p. 26
lines 18-19, too many words in title are upper-case
p. 27
line 2, superscript "18"
line 23, italicize "Pinus radiata" line 32, superscript "13"
p. 28
line 31, subscript "2" line 33, superscript "18"
p. 29
line 5, superscript "13"
All corrections done. We thank the reviewer for spotting these issues.

Anonymous Referee #3

In this revised manuscript, the authors present a compelling case for using triple tree ring-proxies and their interactions as means to evaluate land surface model. The manuscript was substantially re-organized and the story is clear and reads well. The authors addressed most of the concerns raised in my previous review. This will be a relevant contribution for many communities in Biogeosciences, proposes an assessment of model parameters improvement, and demonstrate a tangible potential fo using tree ring data to evaluate land surface models.
We thank this anonymous reviewer for her/his careful reviews of our manuscript and helpful suggestions, which improved the final version of the manuscript.

Below I have minor comments for the authors to consider.
- The manuscript uses heavily acronyms (e.g. ITRDB), some of which were not spelled out and defined when first cited. I urge the authors to carefully check the text for acronym.
We revised the acronyms and corrected the mistake of not spelling out ITRDB in its first occurrence in the introduction.

- In their response, the authors argue that using the McCarroll and Loader 2004 atmospheric data has been the standard in the community (likely tree ring community). However, regardless of recent suggestion outlined in Belmecheri & Lavergne and your choice for not using the most updated data, note that the data used in McCarroll and Loader 2004 were recalibrated and are now the forcing data in CMIP6 efforts. I understand that ultimately the results will not change (interannual correlation) and the authors do not wish to re-run the analysis but it ought to be consistent in future work to use the most recent, updated, calibrated and vetted forcing data (similarly to model runs).
This advice will be taken in future studies to keep up with the most updated standards.

Detailed suggestion broken down by manuscript sections:
Introduction
- The following need references.
"a quasi-global network of eddy-covariance observations, Earth observations and forest inventories covering the last few decades"
The following references were added in corresponding order: Baldcocchi (2019), Orth et al (2017), Bellassen et al. (2011)

Methods:

- In the following, the fractionation parameters as well as the definition if the model parameters while summarized in McCarroll and Loader 2004, the latter in not the accurate reference for the model parameters and their values. Please cite the appropriate references.

"where a (4.4‰) is the kinetic discrimination associated with diffusion between free air and the stomatal cavity, b (27‰) is the fractionation during CO2 fixation by the Rubisco enzyme, ci is the leaf internal CO2 concentration simulated by ORCHIDEE and ca is the atmospheric CO2 concentration prescribed from measurements (McCarroll and Loader, 2004)."

The references were revised and Farquhar et al. 1982 and its update Cernuzak et a., 2013 were used instead of McCarroll and Loader 2004.

In the following "Six previously published tree-ring sites in northern and western Europe with simultaneous measurements of ring-width and..." one suggestion is to reword as: Six previously published tree-ring chronologies from sites in...

It was revised to "Six previously published tree-ring datasets in northern and western Europe…"

Discussion:

- "Disentangling the effect of isotopic signatures from source water and leaf evaporative enrichment revealed that the better performance of LPX-Bern to simulate _18 O variability was due to its higher sensitivity to leaf evaporative enrichment compared with ORCHIDEE (Fig. 3a), which is predominantly sensitive to source water..."

wouldn't this be dependent on site locations? where leaf water enrichment in drier sites will be a more important driver compared to more mesic sites where leaf water enrichment will not dominate the cellulose 18O. How does LPX-Bern parametrize the leaf water enrichment sensitivity or contribution. Is the latter variable per PFT/climate zone or just overall more important than the other 2 models?

As shown in Fig .2, LPX-Bern systematically outperformed ORCHIDEE in our six study sites. Although the contributions of leaf water enrichment and source water will vary in nature according to moisture regimes, this result seems to indicate that at least at our boreal and temperate mesic sites leaf water enrichment seems to much more important than represented in ORCHIDEE. As explained in the paragraph following this sentence, both LPX-Bern and ORCHIDEE use the same representation for the Peclet effect but their parameterizations for L and mixing proportions differ. It is also indicated that for each model the parameterization for L is the same for all PFTS. We believe this is clearly addressed in the first paragraph of p17 but revised the sentence of line 23 to "while we used a fixed value of 8 mm for all PFTs obtained…" in order to be more explicit on that these parameters do not vary by PFT.

- P17. Lines 25. This is a relevant suggestion for testing the L and péclet effect sensitivity in the model world. The uncertainties and sensitivity of these parameters hold also true for the empirical world when direct comparison of simulated and observed cellulose 18 O showed variability depending on (1) parameter values; but also (2) a model with or without the péclet effect. The péclet effect and effective path length are note measured but inferred and can vary seasonally or depending on aridity gradients as well. In sum, this not an uncertainty in the model parameters.

We agree that the Peclet effect and L are highly uncertain and warrant careful estimation and sensitivity analysis. We encapsulate this issue in our closing remark in line 29 in p17 "Future studies should evaluate model sensitivity to these parameters and constrain them with tree-ring observations."

- P17. Lines 30. This assessment is rather puzzling, it implies that 18O of precipitation is not a significant driver while it also determines the soil water? This warrant more clarification. The lack of correlation between the two models suggest that interannual variability is not important but the average value is? What would the correlation be if one precipitation (constant) value was used instead? This can show that

interannual variability or even seasonal change are not important but rather the integration period and average source water value.

The dampening effect of source water mixing in the soil is something that has been observed in earlier modeling studies. We cite Danis et al., 2012. They evaluated the effect of different precipitation 18O forcing series (empirical vs model-based) and found that the interannual variability in source water signatures did not differ much because of soil water mixing. We believe we explain this clearly enough in p17 where we state "However, the dampening effect of soil water mixing reduces considerably the impact of the seasonal variability in δ18O of precipitation on the more integrated δ18Osw signal (Danis et al., 2012)." Doing the test using climatological δ18O of precipitation would help quantifying the effect of the integration period but is beyond the scope of this paper. This is certainly something that warrants further analysis, as we imply in our closing remark in that paragraph "A dedicated model intercomparison study for tree-ring δ18O using the same meteorological and isotopic forcing will help to evaluate uncertainties and attribute differences to forcing data, parameters and model structure."