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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

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In the present study, Barichivich and co-authors explore processes and historical changes of tree growth and tree physiology in a land surface model by simulating three tree ring proxies (namely ring width, carbon and oxygen isotopes) and by comparing them to observations in temperate and boreal sites (one specific site in Fontainebleau and a network of 5 other sites encompassing deciduous and conifer tree species). Further, the land surface model performance is compared to two other models at site and network level. Such approach and evaluation of Land surface model for long term tree growth and tree physiology variability is relevant and will certainly contribute to the understanding of carbon uptake and evapotranspiration dynamics in forested ecosystems; and will improve the predictive skills of tree/forest growth and carbon water cycles

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responses to projected environmental changes.

The author conduct thorough simulations and analyses and the study is well designed. There are a few major points that can be addressed or explained better to clarify the results and their implications and highlight the relevance of the study presented here.

1- The introduction can be refocused into the potential of existing tree ring data to evaluate LSM and why is such work relevant to specific global change questions. The authors mention that but never make the case for it. What knowledge will be gained in term of processes by simulating tree ring attributes and comparing them to observations and output of other models. How do the three models differ which will contextualize the results and the discussion of their performance, specifically ORCHIDEE which is the major one being evaluated. 2- The results can be structured to better follow the study design. Site level (Fontainebleau) comparison of ORCHIDEE, MAINDENISO and observations and then the other sites where LPX-Bern model outputs are also used to compare with observations and ORCHIDEE. LPX-Bern is briefly described and then appears again in the discussion. In this regard the methods can clarify the forcing of all three models. 3- The Discussion relies heavily on descriptive results and does not highlight the physiological processes (beyond the use of carbohydrates and even so, this point needs more careful consideration) that can potentially explain the model-data comparison (or mismatch). In this regard, uncertainties in of tree ring proxies and modeling assumptions (iWUE Farquhar model, leaf water enrichment model, source water δ18O forcing) are not addressed or discussed. 4- The references can be more updated in terms of recent efforts in using tree rings to benchmark process-based models but also to reflect the appropriate papers describing the mechanistic links between tree physiology and isotope variations in tree rings (specifically the O isotopes, beyond the review of McCaroll and Loader 20004).

Detailed Comments:

Pg1, Line 20: Their responses to what? Increasing atmospheric CO2, changing cli-

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mate, disturbances?

Pg2, line 12: A suggestion would be to change adapt and perish as follows: how trees perish or adapt to environmental change is still limited.

Pg2, lines 14-15: additional references are relevant here specifically when using tree rings to either parametrize or evaluate mechanistic physiological models: âĂć Lavergne, A. et al. Modelling tree ring cellulose δ18O variations in two temperature-sensitive tree species from North and South America. Clim. Past 13, 1515–1526 (2017). âĂć Belmecheri, S., Wright, W. E., Szejner, P., Morino, K. A. & Monson, R. K. Carbon and oxygen isotope fractionations in tree rings reveal interactions between cambial phenology and seasonal climate. Plant. Cell Environ. (2018). âĂć Lavergne, A. et al. Historical changes in the stomatal limitation of photosynthesis: empirical support for an optimality principle. New Phytol. 225, 2484–2497 (2020).

Pg2, lines 19-20: These references correspond mostly to mature trees exposed to elevated CO2. The present study investigate historical records and model simulations of tree response to gradual increase of atmospheric CO2. As such, this ought to be highlighted as well.

Pg2, lines 25-30. This statement is misleading. Using a concept such as “cursed” imply an inherent unsuitability of ring width proxy for growth reconstructions. This is not true if the sampling strategy is adequately designed for that purpose. Indeed, the ITRDB repository includes trees collected mainly for climate reconstructions and it is well known that when using the same data for inferences of growth and specifically productivity, the data will reflect the growth dynamics and sensitivities of old, mature, climate sensitive individuals. It is not clear what is the point being made by the authors here? Why not test then model assumption based on collection specifically made for growth/productivity reconstructions? There are a few existing records (ecological sampling methods applied in Flux tower sites for e.g.). There is a great potential to tap tree ring data to benchmark LSM.

The introduction can make a stronger case for the use of both ORCHIDEE and MAIDEN iso. Why compare both models and what information or improvements can be gained from using then ORCHIDEE.

P4, Line 25, it is not clear whether the soil hydrology was modeled using an older version compared to the multi-layer cited after. If so, what is the motivation for this choice. Otherwise, it unnecessary to cite/describe what is not used.

P7 line 18, Where does the assumption of the effective path length of 8 mm comes from? How is this universally applied to different tree species/locations? See Roden et al. 2015, âĂć Roden, J., Kahmen, A., Buchmann, N. & Siegwolf, R. The enigma of effective path length for 18O enrichment in leaf water of conifers. Plant. Cell Environ. 38, 2551–2565 (2015). P11, lines 9-13. Why was this approach used to evaluate the relative contribution of source water versus evaporative enrichment, this is a statistical inference and will not reflect the mechanistic relationship between cellulose and leaf/source water δ18O. For tree ring observations, a more adequate test would be using a proxy forward model (Evans et al., 2006) to evaluate how recorded δ18O in tree ring cellulose compares to the modeled one using input of source water from observations (when available) or from the LMDz; and how varying source water and relative humidity (or VPD) affect the results. In the model world, these parameters are also used to simulate tree ring δ18O and could similarly be compared first to the observations in order to evaluate the relative role of source versus leaf water evaporative enrichment. âĂć Evans, M. N. et al. A forward modeling approach to paleoclimatic interpretation of tree-ring data. J. Geophys. Res. Biogeosciences 111, (2006).

This also brings the point of potential uncertainty related to the assumptions of the péclet effect and the fraction of oxygen atom during cellulose synthesis. These were shown to vary along aridity gradients and intra-seasonally (Cheesman and Cernusak 2016) and with cell-size (lumen area, Szejner et al., 2020). âĂć Szejner, P., Clute, T., Anderson, E., Evans, M. N. & Hu, J. Reduction in lumen area is associated with the δ18O exchange between sugars and source water during cellulose synthesis. New

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Phytol. 226, 1583–1593 (2020). âĂć Cheesman, A. W. & Cernusak, L. A. Infidelity in the outback: climate signal recorded in Δ 18 O of leaf but not branch cellulose of eucalypts across an Australian aridity gradient. Tree Physiol. 37, 554–564 (2016).

Replace carrying over with carryover and specify when first mentioned that it is a carry-over of carbohydrates from previous year or season. This could be discussed in more detail: the dynamic of stored versus recently assimilated photosynthates throughout the growing season (conifer vs deciduous) and under climate extremes (droughts) or other disturbances.

In the results section, when comparing ORCHIDEE and Maiden iso models, it is not clear whether the input data for running both models were the same (e.g. Source water, meteorology, etc)?

P12, lines $\sim$ 25. The effect of stomatal conductance on isotopic discrimination should also be recorded in leaf water enrichment. It is obviously not the case since δ18O does not show a linear relationship with ring width. How would you explain this decoupling of isotopic responses to a reduction in stomatal conductance?

Atmospheric CO2 and δ13C data used for calculation and simulation of δ13C should be revised to the most updated datasets. This is discussed in details in Belmecheri and Lavergne 2020 with suggested recommendation for historical data.

âĂć Belmecheri, S. & Lavergne, A. Compiled records of atmospheric CO2 concentrations and stable carbon isotopes to reconstruct climate and derive plant ecophysiological indices from tree rings. Dendrochronologia 63, 125748 (2020).

P14, lines 12. What is the evaluation metric for “well simulated” The model simulate less than 40% of the observed variability with 70% of unexplained variance. How do the authors assess that the model performance is good? Figure 5 is a great visualization for model performance summary. The result section can rely on this Figure for a consistent description of model performance and model-data comparison. For

instance, “moderate” category is never cited in the result text.

P14, lines 14. How could the model parametrization be biased towards temperate and deciduous forests since PFTs are informed in the model and climate drivers should reflect the forest/tree growing conditions (knowing that Mainden iso was calibrated for Fontainebleau site, but for ORCHIDEE?). In this case, which parameters are thought to be biased towards temperate forests.

P14, lines 15-17. If autocorrelation is removed from observations, does it improve model/data comparison since ORCHIDEE simulates poorly carbohydrate carryover.

P14, lines 19-20. Could the amplitude discrepancy between observed and simulated δ13C be explained by post photosynthetic fractionations?

The Farquhar model referenced in the paper and used in the present study describes the isotopic discrimination at the leaf level (See Frank et al. 2015). It is not clear from the methods that ORCHIDEE takes it into account nor that the measurements of tree rings were scaled to the leaf level.

âĂć Frank, D. C. et al. Water-use efficiency and transpiration across European forests during the Anthropocene. Nat. Clim. Chang. 5, 579–583 (2015).

The sensitivity of the simulated tree ring δ18O depends on the selected months of model outputs. In the methods, the authors describe selecting May-August. Was this informed by knowledge of the growing season? The authors stated that using this window ensures a standard time window to compare all sites and isotopes (although different time windows for C and O isotopes did show different levels of agreement between observed and simulated isotopic variations). While the choice of a standard time-window can be justified for the reason outlined by the authors, it is not clear that such justification is beneficial when there is a loss of statistical agreement between data/model and if this choice is not informed (even roughly) by the tree’s growing season. An important consideration for cellulose δ18O is the timing and duration of

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cell-wall thickness during which most of the cellulose is deposited. This will determine the isotopic ratio recorded in tree rings and the time window not only vary by latitude/attitude but can be narrower than anticipated (See Cuny et al. 2015).

Cuny, H. E. et al. Woody biomass production lags stem-girth increase by over one month in coniferous forests. Nat. Plants 1, 15160 (2015). In addition, because a fraction of leaf water will exchange with xylem water, C isotopes will carry more signal of previous carbohydrates compared to O isotopes (dampened carryover signal).

Pg 15, lines 5-10. Intuitively, GPP is expected to correlate with D13C , yet it is not the case here and D18O correlating better with GPP is rather intriguing. What is the mechanistic link to explain evaporative enrichment correlation with GPP. If this is driven by stomatal conductance so should D13C.

Pg16, Lines 25. This can be easily tested by removing auto-correlation from observed TRW prior to comparison with modeled TRW. As a side note (not a criticism to the present study). RW simulation can be tested using the Vaganov–Shashkin (VS) model to simulate TRW and compare ORCHIDEE performance to the VS model (similar approach to comparing ORCHIDEE and MAINDEN iso). This may shed more light into the poor ORCHIDEE performance in reproducing high frequency TRW variability.

Pg16, Lines 35. This assumption of drought legacy recovery has been tested for triple-proxies such as this study and the outcome depends largely on the detrending methods used for TRW. It also depends on the frequency of droughts. Hence, the results obtained by ORCHIDEE simulation might not be due only to a poor performance of the model. âĂć Szejner, P., Belmecheri, S., Ehleringer, J. R. & Monson, R. K. Recent increases in drought frequency cause observed multi-year drought legacies in the tree rings of semi-arid forests. Oecologia 192, 241–259 (2020). The authors make a case of the 1976 drought to discuss limitation of the processes represented in the various models. The impact and recovery from the 1976 drought could be further elaborated using Superposed Epoch Analyses on simulated and observed tree ring proxies.

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Pg 17, lines 25-30. This still contrast with an increase of D13C documented in atmospheric CO2 (Keeling et al., 2017). There is no discussion about the discrepancy between continuous increase of simulated D13C and the “pause” of D13C tree ring measurements since the 1980.

âĂć Keeling, R. F. et al. Atmospheric evidence for a global secular increase in carbon isotopic discrimination of land photosynthesis. Proc. Natl. Acad. Sci. 114, 10361–10366 (2017). P18, lines ∼5. Are the simulated historical trends of δ18O consistent with other observations (paleoclimate studies) in other forests, climatic regions?

P18, lines ∼15. How about the mechanistic representation of source water vs leaf water enrichment contribution in LPX-Bern model? If the forcing for the three models are not the same (specifically for source water 18O), the comparison of model performances is then biased.

P18, lines 30. Could it be that the sensitivity of stomatal conductance to factors other than CO2 be misrepresented (soil moisture for example).

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