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

Response to anonymous Referee #1

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This paper describes the parameterization and testing of a quasi-mechanistic large-scale model of forest growth. It tests the model against two other models and against measured data. The novelty of the manuscript lies in these model tests, especially because they include stable isotopic data, which both illuminate the physiological processes that cause the growth differences and provide tests of the mechanistic basis of the model. The significance is that this attempts to link a global-scale land-surface model to three kinds of tree-ring data. If the model performs well, it may be justified to use it to describe the long tree-ring time series–potentially well beyond the range of remotely sensed, or even instrumental data.

The paper is mostly well-written, clearly significant, and appropriate for this journal. I particularly enjoyed reading the introduction and the methods and materials, which provide access to this subject for a broad audience. The analysis represents a tremendous breadth of work. I heartily applaud the authors for building these isotopic tests into their models and appreciate the comparisons to other isotopically enabled models and to measured data. However, apart from the Intro and Methods, I found the paper difficult to read. There is so much here that the emphasis gets lost.

We thank the reviewer for recognising the scope and magnitude of our work and for constructively pointing to areas for improvement. As we explain below, we have taken on board the suggestions to improve the focus and highlight the key elements of our triple tree-ring constraint.

The abstract for a paper this complex should provide a roadmap that leads the reader to the main conclusion. It should mention not only ORCHIDEE, but also the other models, with a bit of explanation of why they were included. Also, Figure 1d-f is presented as a visual test of the models. If so, my visual impression is that MAIDENiso fails as the response surface looks quite different from that of the observations. This result should appear in the abstract. I suggest a change in the emphasis of the manuscript below. If accepted, this change should be reflected in the abstract.

We followed the advice of the reviewer regarding the improvement of the emphasis of the paper. In the revised abstract we now mention the other models as a brief model context for the performance of ORCHIDEE but we do not develop in their evaluation to avoid losing focus on ORCHIDEE. The poor performance of MAIDENiso is discussed in the new Discussion subsection 4.2 at the end of the model evaluation with the tree-ring triplet.

MAIDENiso is referred to as "specialized" in at least two places in the manuscript. The model is described briefly on page 3 L4-8, but I was left wishing for a clearer description of what makes it different. Like many of your readers, I have never used it. This will be especially important if you choose to emphasize Fig. 1d-f.

The introduction is now more streamlined to highlight how the main growth processes are represented in current modelling approaches. It is hopefully much more clear where each model stands in the model landscape. We also added a compact description of the setup for the published simulations and processes included in MAIDENiso and LPX-Bern in the last two paragraphs of section 2.3 (Simulations)

The simulated results are not always distinguished clearly from the empirical data. This is especially important because you are comparing the models to empirical isotopic data. In particular: 1/10-13: I presume all the "physiological" data here are simulated? If so, say so, especially in the abstract. Have there been any direct measurements of, e.g., GPP at the Fontainebleau site? The same question arises about source water below. As these are all simulated, they should be labelled as such (e.g., 13/6-9).
We revised the text to make a clear distinction of when the description is based on simulated or observed data. The GPP referred to in the manuscript is simulated by ORCHIDEE. We did not use the short eddy-covariance measurements available for a forest near Fontainebleau in this study. Similarly, all source water is simulated and is now explicitly labelled as such.

The LPX-Bern results are barely mentioned in the text and the only conclusion they lead to is that the model has "better isotopic forcing." What does that mean? Does LPX-Bern use different algorithms to estimate source water and water vapour? If so, it would be interesting to see how the predictions compare. The fact that the LPX-Bern model works better than either of the others for ïA˛d'18O dilutes the impact of the presentation of ORCHIDEE. I suggest, to create a clearer emphasis in the paper, to either move LPX-Bern to a supplement or to discuss it in more detail. A particularly interesting detail would be a discussion of what might be changed in future versions of ORCHIDEE and MAIDENiso to make them work as well.
We followed the advice of the reviewer to keep the focus on the tree-ring triplet and deleted the former Figures 2-4 (the triplet with only leaf water enrichment, climate response and water use efficiency). In the revised manuscript, the former Figure 5 is now Figure 2 (Taylor diagram of model performance). This gave us space for the new Figure 3 to analyse more deeply the behaviour of LPX-Bern versus ORCHIDEE with respect to d18O. The new Figure 3 shows a detailed comparison of the contributions of source water and leaf water enrichment to the simulated d18O signals simulated by both models and also the correlation of each component of the forcing (d18O of precipitation, d18O of vapour and d18O of source water) and the resulting simulations of d18O in leaf water and cellulose. This Figure allows disentangling and comparing the nature of the simulated signals in both models and evaluating the consistency of the isotopic drivers used to force the models (i.e., d18O of precipitation and d18O of vapour from the LMDZ atmospheric for ORCHIDEE and d18O of source water (i.e. soil water) from the ECHAM5 atmospheric model for LPX-Bern).

The critical process and areas of improvement identified in the evaluation of ORCHIDEE against tree-ring data are now more clearly discussed in Section 4.1 and 4.2. The casue of the issue of MAIDENiso is addressed in Section 4.2, but we refrained to develop detailed recomendations for this model since it was used only for comparison purposes. However we make clear that the cause of the poorly simulated growth-isotope surface is predominantly

due to the excessive carryover in simulated ring width, which decouples growth from leaf-level responses.

Section 3.1.2: I'm not sure I understand the purpose of this long section although I've read it several times. I think it is being presented as a test of the relationships embedded in the model structures and parameterizations. If so, this seems important and the isotopic methods seem ideally suited to it. I would make this the main emphasis of the paper.

Following the advice of the reviewer, the narrative flow of section 3.1.2 was revised and the section was renamed as "Simulated tree-ring triplet" to emphasize the triple tree-ring constraint. Former sections 3.1.3 (climate response) and 3.1.4 (20th century water use efficiency) were deleted to improve the focus of the paper.

However, I noted that the MAIDENiso response surface looks really different from ORCHIDEE and from the data in Fig 1 d-f. I did not find this described clearly in the text. There was some description of the r-values of the partial correlations, but it is the slopes that catch the eye. The slope differences result in very different geometries across the response surfaces and this is what I would emphasize. Please note that the presentation of the response surfaces was interrupted by inferences about temperature and stomatal conductance, which I would move to the discussion. This section should end with a general model evaluation that addresses the visual impression that MAIDENiso has a problem.

We thank the reviewer for pointing our attention to the slopes of the surface. Indeed, the simple regression slopes better quantify the geometry of the triplet and the coupling of the processes that it represents. We now labeled the regression slopes in Fig 1 from b1 to b3 and interpreted their magnitude in terms of processes. Since these inter-relationships are the basis of the novel tree-ring triplet presented in the paper we discussed the meaning of the slopes and relationships in terms of processes in the new section 4.2, which integrates the former sections 4.1 (Integrating tree-ring and carbon...) and 4.3 (Constraining model processes..).

The manuscript also describes isotopic changes in response to climate change and CO2. Although this is an interesting application of the model, it seems to belong in an- other paper. This impression is strengthened by the fact that the analysis neglects recent discussion of the effect of height growth on isotope ratios (and presumably growth) (Brienen et al., 2017; Marchand et al., 2020; Marshall & Monserud, 1996, 2006; Voelker et al., 2016). If it is to remain, the height issue must be addressed and infor- mation about height growth in these trees should be added. Are these trees are still young enough to be growing in height? How tall were they? It would be great to see these height effects added to some future version of the model!

Following the advice of the reviewer, we decided to remove the analysis of 20th century iWUE and publish it as a follow up letter. The effect of tree height on carbon isotopes is not explicitly represented in ORCHIDEE. This has been shown to be particularly important in deep canopy tropical forests and certainly can account for initial trends during the juvenile period. The age of the stand in Fontainebleau is about 120 years (given in Table 1) and tree height is between 20-25 m. A newer version of ORCHIDEE introduced a better representation of forest structure and light penetration

(https://gmd.copernicus.org/preprints/gmd-2020-29/) and could be used to address this issue, particularly for dense tropical forests.

The interpretation of tree-ring d18O data is notoriously difficult and the Scheidegger et al. approach, although clever, is too simplistic. Because the authors cite Roden and Siegwolf (2012) (19/13-19), I presume that they appreciate the difficulty, but they do not express it in a way that a naïve reader is likely to detect. I suggest clearly and bluntly recognizing these difficulties for the people who will follow down this path. Related to this problem is the question of how the source water and water vapour d18O were simulated for this analysis. It should be described, at least briefly. The results are contingent on how this was done and how well it worked. This is necessary in part because the source water data are emphasized, e.g., in Figs. 2 and 6.

The uncertainties and critical processes identified for the simulation of the tree-ring variables are now explicitly addressed at the beginning of the Discussion in the new Section 4.1. We explicitly recognise the difficulty for simulting d18O in the 5th paragraph.

As mentioned earlier, the external d18O isotopic forcings used for each model (ORCHIDEE, MAIDENiso, LPX-Bern) are now better described in Section 2.3 (simulations). The new Figure 3 also helps to understand the origin of the differences and similarities between tree-ring dO18 simulated by ORCHIDEE and LPX-Bern.

The temporal autocorrelation and its likely causes are interesting and important, but inadequately described. I would like to see a more carefully approach to this. In particular, there are mechanisms besides photosynthate carryover that could cause it. These include, for example, root or leaf mortality or production that might influence hydraulic balance in subsequent years. Monserud and Marshall speculate on some of these (2001). Whatever the mechanism, it would be great to have these effects described by the model and I support the emphasis placed on it.

Since the carryover is one of the critical processes to simulate tree-ring width variability, we further developed the discussion of its causal factors in the first paragraph of section 4.1 of the Discussion. Factors other than carbohydrate remobilization are now mentioned and referenced.

It would be unfortunate if the main points of this manuscript were missed or misunderstood because of the complexity of presentation. I urge the authors to emphasize the response-surface tests of the models. If so, they might also expand the discussion of LPX-Bern and its better performance, including a comparison of the source and vapour d18O simulations. I suggest dropping the climate-change analysis for now. Especially if the height effect were included in the model, the results would be significant enough to stand alone in another manuscript. Removing them from the current one would allow the model performance results to emerge clearly.

We thank the reviewer for all the constructive advice, which we followed in order to improve the focus and presentation of the paper. The revised manuscript highlights the response surface of the tree-ring triplet and the comparison of ORCHIDEE with LPX-Bern as global model benchmark, including a concise analysis of the isotopic forcings.

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