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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 #1

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Review of: "A triple tree-ring constraint for tree growth and physiology in a global land surface model"

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 modelsâĂŤand 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.

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.

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 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).

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 ïĄď18O 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.

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. 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.

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 another 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 information 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!

The interpretation of tree-ring ïĄď18O 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 ïĄď18O 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 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.

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 ïĄď18O simulations. I suggest dropping the climate-change analysis for now. Especially if the 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.

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