**We thank Martin de Kauwe for his constructive comments. A point-by-point response follows below.**

Tschumi et al. explore the timely topic of the impact of combined drought/heat extremes on terrestrial carbon dynamics, using the LPX model. The authors construct a suite of potential forcings and use a factorial approach to examine the follow-through impact on carbon/tree dynamics. I found the paper well written, the methods are well constructed and the analysis broadly interesting. I did feel that more could have been done to explain key assumptions (see below) and in particular to leverage these assumptions in the discussion text when discussing impacts/broadening how transferable the results are across models. I think the equilibrium analysis needs more motivation in the paper, currently, 2 of the 9 figures relate to this and there is no mention in the abstract.

**Thank you for your helpful comments and overall positive feedback.**

The first major issue relates to what LPX assumes, particularly with respect to temperature. The manuscript lacks information on the assumed bioclimatic limits and that photosynthesis and respiration estimates are simply down-regulated if temperature bounds are exceeded (I'm going off what I know about LPJ-GUESS, so please correct me). I think this information should be in the methods (a table by PFT) as it is likely to impact on how transferable the results are across models. For example, how much of the impact of the hot (or hotdry) is due to the assumed upper bounds of the model? Are these sensible in all geographic locations (see Australia, for example, where I also note the model has a continent of grasses - https://bg.copernicus.org/articles/18/2181/2021/). I would anticipate some return to this in the discussion when the authors try to contextualise their results. I want to be clear though, I'm not suggesting this invalidates anything the authors have done, but I would nevertheless anticipate a degree (a lot) of sensitivity to the assumptions of the DGVM. Similarly, some text on how well the model simulates the impact of drought.

**We will include a table with LPX's specific bioclimatic limits in the appendix (similar to the table shown here). In addition, we will extend the Methods section on model processes and discuss some of the models limitations in the Discussion section.**

| | minimum coldest monthly mean temperature | maximum coldest monthly mean temperature | minimum growing degree days (at or above 5°C) | upper limit of temperature of the warmest month |
|---|---|---|---|---|
| TrBE | 15.5 | no limit | 0 | no limit |
| TrBR | 15.5 | no limit | 0 | no limit |
| TeNE | -2 | 22 | 900 | no limit |
| TeBE | 3 | 18.8 | 1200 | no limit |

| | | | | |
|---|---|---|---|---|
| TeBS | -17 | 15.5 | 1200 | no limit |
| BoNE | -32 | -2 | 550 | 30 |
| BoNS | no limit | -2 | 350 | 30 |
| BoS | no limit | -2 | 550 | 30 |
| TeH | no limit | no limit | 0 | no limit |
| TrH | no limit | no limit | 100 | no limit |

This point about assumptions can be extended when the authors discuss how competition changes fractional cover with scenarios. I think the C4 grass component of the model will have assumed bioclimatic limits, in some places (Australia I know for certain), these are likely to bias interpretations …

**The Discussion will be extended with some limitations and biases, which include a wet bias in Australia in the EC-Earth forcing data and the fact that our C4 grasses are very water-efficient in LPX, which both leads to Australia being a bit too green in our simulations compared to observations.**

The second major issue relates to missing insight into how realistic the output from EC-Earth is? Is it biased warm/cold, wet/dry. The model will clearly have biases and I'm not suggesting that these biases would warrant the paper not being accepted; however, they do affect the subsequent analysis through LPX. It is really important that some text is added about this I feel. Having personally been recently looking at the mean and variability bias in future precipitation data over Australia, I think this is a significant issue.

**Note that we discussed the biases in EC-Earth in detail in an earlier manuscript: https://rmets.onlinelibrary.wiley.com/doi/full/10.1002/gdj3.129. For completeness, we will extend the section on the EC-Earth bias in the Data section and cross reference to our earlier paper. It is important to keep in mind that the analysis was done by looking at changes relative to the control (which has the same bias as the rest of the scenarios) and not to observations, therefore the EC-Earth bias does not play a relevant role when analysing the differences between scenarios.**

Finally, I think it would be great to add some figure (timeseries) that shows a case study of one of these very extreme summers to see how the model is behaving, for some region. I might be alone here, but I'd appreciate seeing how the model dynamics in terms of uptake/respiration play out across an extreme scenario vs one without events. It is quite hard to get this message from the maps.

**Thank you for this suggestion. We have tried to come up with a time series illustration that would be useful for the overall story but did not succeed. Generally we look at how changes in the climatology of the occurrence of droughts and heatwaves affect vegetation distribution and carbon dynamics.**

**Hence focusing on single events would not be very informative but rather illustrate the generic response of LPX to certain types of climate extremes. To address the reviewer's comment we will expand the Introduction to make sure the scope of this study is clear and add some discussion on how LPX deals with droughts and heatwaves.**

Minor

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- Line 23: Allen et al. isn't an appropriate citation for CO2-induced changes in WUE.

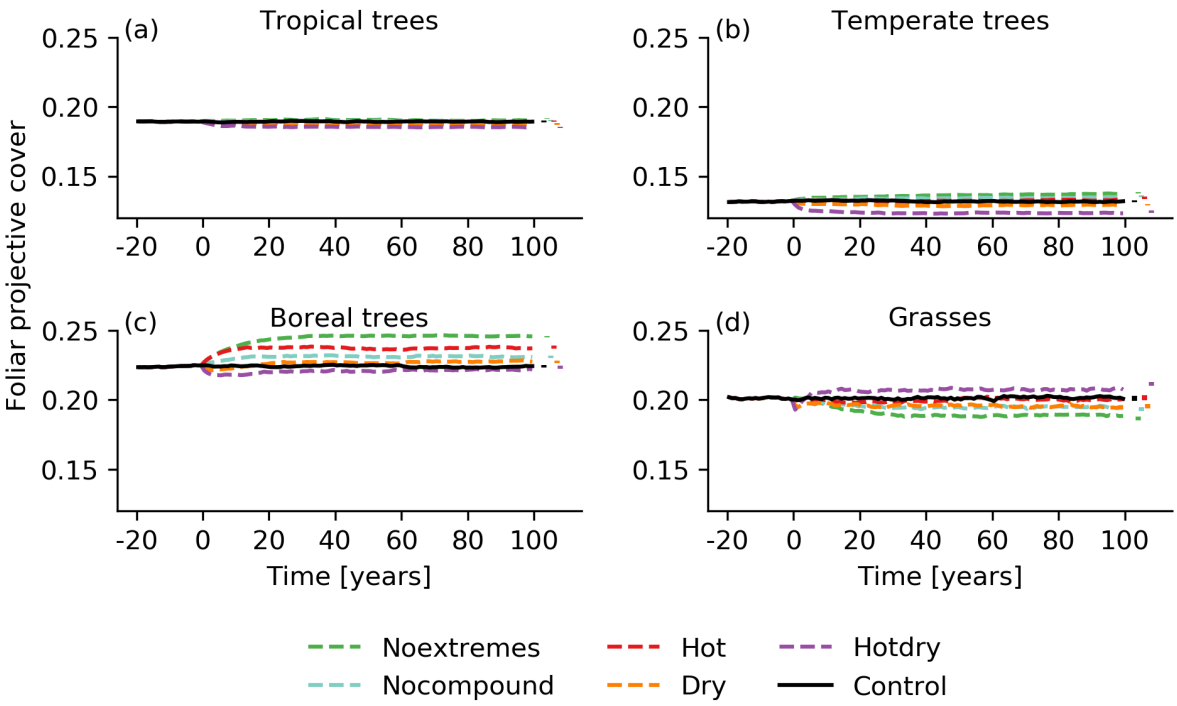
**We will change this reference to De Kauwe et al. (2013) (Global Change Biology, 19, 1759-1779, https://doi.org/10.1111/gcb.12164).**

- I think the scenarios sound well thought through and I note what I suspect is a deliberate naming choice to call one of the "dry" rather than drought. I think this is the right choice as the authors can't be sure it indeed does sample a drought (unless they check). However, the text surrounding Table 1 talks in terms of "drought". I think it would be better to be consistent with the language of the scenarios (?). I note further on they talk about checking for drought using SPI. Could they perhaps add a few sentences to explain a bit more about how they did this, it is currently unclear for me.

**We will make sure that we always talk about 'dry' conditions instead of 'drought'. We will expand on the SPI analysis and cross-reference our previous paper where this is explained in more detail.**

- I thought the equilibrium text was interesting but I'm not sure that the context was really given? If I missed it, I apologise but I would have liked to have seen more in the intro/methods to explain motivation. I also wonder whether each change in Fig 7 would be better on the same y-axis so the reader can compare relative impacts.

**The equilibrium analysis provides some insight into how quickly the model responds to a sudden change in forcing, here a change in frequency of hot and dry events, and how quickly the model converges to a new equilibrium. We will add some explanation to motivate this aspect better. Regarding Figure 7, we prefer to keep the different y-axes because the results are easier to read this way. See the following version of Figure 7 which has the same y-axes.**




- With the maps, consider chopping off Antarctica to show more of the land surface.

**We will consider cutting off Antarctica from the maps.**

Martin De Kauwe