



Comment on bg-2021-191

Christopher Reyer (Referee)

Referee comment on "The effects of varying drought-heat signatures on terrestrial carbon dynamics and vegetation composition" by Elisabeth Tschumi et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-191-RC2>, 2021

The paper is very well written, treats an important topic and is certainly an interesting study. I have some main and minor comments which I think can all be accommodated with wither few additional simulations and/or discussion of the results/model.

1) the main results seem to be a 4% change in ecosystem productivity. I think the paper needs a bit more work to carve out why these 4% actually matter, also in light of the many uncertainties. as written now, it seems the number is "just a number" and as such not very impressive since it is "only" 4%. So I think you should carefully check where you can give more depth to this result by either referring to regional patterns, eg. "the 4% may not seem much at global level but in region xxx, forests are strongly reducing productivity" or sth like that and by better placing the results into context, i.e. what do 4% really mean in terms of changing ecosystems/landscapes and other ecosystem functions and processes and in what direction are the uncertainties of the model etc. pushing this finding?

2) One other main point that deserves more attention: is LPX (or any of such vegetation models) actually up to the challenge of simulating the effects of compound events? I am sure the model has been well-tested for the control and noextremes scenarios but does it reflect the processes that really matter in a hotdry scenario? e.g. in many regions insect damage following a drought is far more substantial than the drought damage (direct through losses of leaves and branches etc... and indirect through changes in carbon cycling etc.) and hence if the model only incorporates the "physiological" responses (which it is probably still likely do underestimate because of a lack of processes such as xylem embolism in the model etc...) it might underestimate the effects of compound events. I think the manuscript should spell out more clearly which the crucial processes in the model are, if the model has been tested with regard to those and how realistic they are to actually cover the effects of compound events in their full breadth (or at least a large part).

3) the role of CO₂ needs clarification. in your experimental set-up you keep co₂-contant which is fine. but through the stomatal responses water-use efficiency might be higher under higher co₂ under which such compound events might occur. I know this introduces another dimension into your experimental set-up which you may not be able to accommodate but I think the issue is at least worthwhile further discussion.

minor comments:

L34: what is the observational period here?

L198ff: you should discuss later whether you expect this equilibrium to be reached soon or whether there is a need to lengthen the simulation time.