

Biogeosciences Discuss., referee comment RC1
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Comment on bg-2022-65

Anonymous Referee #1

Referee comment on "Exploring the impacts of unprecedented climate extremes on forest ecosystems: hypotheses to guide modeling and experimental studies" by Jennifer A. Holm et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2022-65-RC1>, 2022

Manuscript by Holm et al. titled "Exploring the impacts of unprecedented climate extremes on forest ecosystems: hypotheses to guide modeling and experimental studies" explores the responses of two dynamic vegetation demographic models (VDMs) to a series of unprecedented climate extremes (UCEs) scenarios that are designed to test two high-level hypotheses on the nature of ecosystem responses to UCEs (mainly in the form of droughts, with additional effects of increasing atmospheric CO₂ [eCO₂] and temperature). The study finds strong nonlinearities to extreme droughts in the responses of the two VDMs with different sensitivities to drought intensity and duration scenarios at the two test sites, and attempts to interpret the underlying mechanisms of such responses.

General comments:

The manuscript is in itself coherent, well-written, the structure is easy to follow with the aid of straightforward visualizations and tables. The experimental framework is well-thought-out in general although it is missing a couple of important aspects (more on that soon). It was interesting to see such different responses of models so clearly which I believe would lead to novel follow-up studies.

My main concern about this manuscript is how much novelty it brings in itself. If I may put it this way, one could compile Table 3 (the crux of the study) from the literature as authors themselves are partly doing here. Surely, the framework provides a systematic structure to highlight contrasting differences in model process formulations and bring these suggestions (Table 3) together, but after reading the manuscript I was left with a feeling that what genuinely new was learnt from this exercise was not clear. In other words, authors seem to be exposing mainly the *known* unknowns, and the link between some of the suggestions and simulations in this study seems rather weak. It was still a very informative read if it was meant to be a literature review paper, but as a research paper I wonder if it is missing the opportunity of putting its potential [L703: to drive progress in improving our understanding of terrestrial ecosystem responses to UCEs] into action.

To be more specific, while I acknowledge the fact that one paper can only tackle so much, I think this study would benefit from additional simulation experiments to explore one or more of the raised questions, and try to disentangle some of the potential driving mechanisms further. Below are examples of such "low-hanging fruits":

i) Additional no drought (truly) eCO₂-only simulations to inspect its role in mortality overshoot some more (L496-498)

ii) Running ED2 with less PFTs at Palo Verde (L550-551)

iii) Imposing additional mortality to emulate post-drought secondary stressors' effects (L613-615) - I believe at least ED2 has a stop-restart functionality and the disturbance interval in LPJ-GUESS can be played with.

iv) Testing different mortality thresholds from conductivity loss in ED2 (L653-657)

I am sure that the authors are capable of devising much more clever ones than these. Even if some of these configurations are unrealistic and outcome from such exercises are still inconclusive, the findings could be interesting and it would add a valuable dimension to this manuscript.

That said, I think there is no fundamental flaw in the study, and it is of interest for the Biogeosciences community. I would be happy to reevaluate after the revisions.

Specific comments:

As mentioned above, the presented framework is practical and logical but I was wondering whether the following points are obscuring the results:

1) While no model-data comparisons can be performed on UCE responses due to lack of data (L228), there is no (mention of) validation as to whether these models were able to capture the modern vegetation at the test sites. Please cite the studies if this has been done elsewhere.

2) Similarly, authors don't use any site-level observations to inform model parameterization (L290 no site-level tuning was conducted). At the least, this needs to be justified more.

Without some reliability that the model simulations are representative of the sites, it is not

clear how much of the responses could be due to misconfiguration, even if the scenarios are being evaluated with respect to a baseline.

3) The uncertainties in parameters and models' sensitivities to them are not accounted for. I am sure the authors are aware of such uncertainties and sensitivities (e.g. sensitivity to water- and mortality-related parameters in LPJ-GUESS, Oberpriller et al., 2022 doi: 10.5194/gmd-2021-287 and in ED2, Raczka et al. 2018 doi: 10.1029/2018JG004504). I believe the findings would have been more robust if the models were run in ensembles and results were reported using the ensemble means at least.

Please find more line-by-line comments below:

L62 - frequency: While this study has enough to investigate under the existing framework, one possible extension that comes to mind is to include the increase in frequency. Authors consider magnitude and duration but not the frequency of back-to-back UCEs. Please consider coming back to it in the discussion, potentially in an explicit "limitations of the current study and future steps" sub-section.

L77: Could the authors expand on the definition of 'moderate' quantitatively here? I.e. what were the treatments like in the mentioned studies, e.g. Beier et al X months, Kayler et al. Y amount

L86-87: Authors are not coming back to these studies that document ecosystem responses to extreme droughts later in the text. Please consider including discussion on how your findings compare to these studies (even if they are post-hoc and limited).

L152-154: While there is some observational support for linear response as cited by the authors, I wonder if expecting a linear response from these highly non-linear VDMs forms a plausible null-hypothesis as it sounds rather easy to refute (Sitch et al. 2008 GCB, McDowel et al., 2013 New Phyt, Rollinson et al. 2017 GCB, Bastos et al. 2020 Phil. Trans. R. Soc. B, Oberpriller et al. 2022 GMD) Could the authors elaborate with additional justifications in the text for not choosing a more specific null hypothesis?

L207: Again, it would be great if authors could consider giving numbers as to what makes these studies 'moderate' to motivate the readers.

L243: Can't one specify more PFTs or even species in LPJ-GUESS?

L288: Just curious, were there no droughts during these historical site-specific climate periods?

L291: Will the parameters sets be also made available in the Dryad repository? (the data availability statement says all model simulation data will be made available but I wanted to make sure all model configurations are also included)

L305: On table S1 - Are the "drought + temp + 200 ppm eCO₂" rows missing?

L352: Looking at Fig 2 LPJ-GUESS seems to show some differences to drought duration, maybe consider saying "very little" instead of "no sensitivity".

L404-406: This is an example of what I meant above by robustness of the results. Here authors identify it as a potentially anomalous signal because it was unexpected, but some of the other effects we are seeing could also be "anomalous", and *vice versa*, we may not be able to see certain effects because parameters were not varied in their potential ranges, limiting the overall impact of the study.

L457-459: While authors mention that these swings in LPJ-GUESS LAI could be contributing to mortality response they do not explain the reason for these swings in LPJ-GUESS. A historical validation/assessment of the model behaviour at the site could have been useful here.

L459-463: The reasoning behind how the model simulations lead to the suggestion of better representing morphological and physiological characteristics relevant to plant-water relations (e.g. leaf age) is not very obvious. Could the authors add some connecting and specific thoughts to the text?

L480-483: Again the transition from simulations to the suggestion feels rather abrupt and generic. This recommendation could easily have come from the cited literature without performing this study at all, what else was learnt from the simulations?

In sub-section 4.1.3 there is no referral to the process implementations of the models used in the study specifically. The only connection back to the results is the last sentence of the first paragraph, the rest of this subsection mostly reads like an introduction. Proposal to investigate allometric partitioning theory is not explicitly motivated by the simulations but rather by literature studies. Please consider re-wording this section and making the connections to this study more specific.

L532-547: Could the authors say anything about which model implementation is more realistic according to their simulations? Can there be some more concrete conclusions/suggestions than "the need to better understand NSC dynamics"?

L551-553: As mentioned in the beginning, it would have been nice to see some additional experiment as an attempt to disentangle whether ED2's resistance is due to functional trait diversity (or due to e.g. NSC implementation).

L596: Improved how? What are specifically lacking in ED2 and LPJ-GUESS at the moment?

L643: The thought process seems incomplete in this section. Was there going to be a suggestion?

L654: Again, it feels like a missed opportunity that there were no additional simulation experiments with different thresholds to investigate this further.

Table 3 - last row: Not sure if I'm following right but is the authors' recommendation here is to collect data on UCEs which they identified, by definition, to be unavailable? Please consider rewording.

L708: As far as I can tell, the LPJ-GUESS code is not publicly available to download via this link, but available upon request. Please check and revise the statement accordingly if needed.