

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2022-227

Anonymous Referee #1

Referee comment on "Implementation of trait-based ozone plant sensitivity in the Yale Interactive terrestrial Biosphere model v1.0 to assess global vegetation damage" by Yimian Ma et al., Geosci. Model Dev. Discuss.,
<https://doi.org/10.5194/gmd-2022-227-RC1>, 2022

Review for GMD-2022-227 by Yimian Ma et al.

The authors present a submodel for ozone damage to different plant types inserted into an existing biosphere model (YIBs). This new ozone model calculates plant damage, expressed as GPP penalties, based on a unified sensitivity interacting with leaf mass per area. They conclude that approx. 5% of global GPP is not materialized due to ozone damage.

I perceive the study as a major advance over previous approaches to model ozone damage to plants, taking into account latest findings on leaf mass rather than area as defining factors for ozone sensitivity across plant types. The manuscript is well written and the steps taken to develop and integrate the ozone model into YIBs are sound. All conclusions are grounded on the presented evidence. Nonetheless, I see several places where the study could be amended; they are detailed in the following. While I strongly suggest to consider these, none of them questions the relevance and overall validity of the approach, though.

In conclusion, I recommend a major revision of the article. The 'major' is a sum of many 'minor' elements. If the authors are able to address my concerns, I clearly support a publication of this article.

*** General comments ***

-> While the new mass-based approach may prevail over area-based calculations, a crucial factor of ozone sensitivity is also the abiotic environment of growth, e.g. water availability, temperature or CO₂ concentration. A change in these parameters, all others

being equal, may strongly modify the ozone response of plants. It remains unclear how much these confounding factors are already considered in the model by shaping the actual LMA, since the global LMA data are prescribed from data sets. The authors are encouraged to discuss this and, if necessary, amend the model to also consider climatic parameters in their ozone module.

-> There are several unclear points in the methods; these are detailed below in the specific comments. I mention them here as they sum up to a general comment.

-> The calibration partly remains unclear (see below). Most importantly, though, an out-of-sample calibration is missing where each PFT is removed from calibration - for both the unified and the supporting PFT-specific calibration - and the resulting estimate compared particularly for this omitted PFT. This is relevant especially for crops, as they are well apart from the other plant types (e.g. in Figure 2), suggesting that this difference could largely drive calibration and thus the resulting performance be overly optimistic. The perfect fit of S_S to S_O for crops in Figure 6b corroborates this hypothesis.

-> An additional, similar exercise could include another year of ozone data. The current study only uses 2010, for calibration and validation. Another year will have another ozone distribution and thus would be useful to validate the findings.

-> All of these new suggestions, once implemented, should then also be considered in the discussion section.

*** Specific comments ***

-> Methods, 2.1: if F is the UNdamaged fraction, why is there a ozone penalty in equation 1?

-> Methods, 2.1: explain f_O3 at first mention and add units to all variables

-> Methods, 2.1, eq 2: f_O3 depends, in turn, on F. Please explain this circular dependence in the text and also what it means for calculation - do you need an optimizing routine?

-> Methods, 2.1: how do water availability, temperature, CO2 et al. interact with the ozone uptake?

-> Methods, 2.2, eq 6: what happens with negative values of f_O3 - y in the integral?

-> Methods, 2.2: is every PFT dominant somewhere?

-> Methods, 2.3: the exact recipe for the calibration is missing. It remains partly elusive how you did the calibration - how many runs, which parameters were tuned, which step size, which algorithm, which target variables etc. Please augment, for all runs.

-> Methods, 2.3: a sensitivity towards environmental parameters would be useful to add

-> Results, 3.1, l199+: is the higher agreement between observations and mass-based simulations ($R^2 = 0.77$), when compared to area-based simulations ($R^2 = 0.54$), expectable already in the uncalibrated version given the design towards mass-based traits?

-> Results, 3.2: can you justify the use of S2007 as a reference, i.e. why is the new model good if it agrees with the old?

-> Results, 3.2, l232+: can you provide numbers on the difference components ([O3], LMA variation, land-use intensity etc.)?

-> Figure 3h (crops): a linear fit does not seem to be the best choice here, in contrast to all other PFTs. How to account for that or interpret this levelling off?

*** Technical corrections ***

-> Methods, 2.2: explain POD at first mention (abbreviation & what does it mean)

-> Methods, 2.2, l131: what do you mean with 'bio-indicators'?

-> Results, 3.3: this section requires language proof-reading

- > Results, 3.3, I250: the values (-0.2 and 1.7) are not %, but percentage points - the difference in % would be much larger
- > Figure 2: please add the 1:1 line and the out-of-sample line once it is calculated
- > Figure 3: add the grey simulated dots to the legend
- > Figure 6a: CRO is missing here?