

## ***Interactive comment on* “Evaluation of simulated biomass damage in forest ecosystems induced by ozone against observation-based estimates” by Martina Franz et al.**

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Review comments by Marcus Schaub, Maxime Cailleret and Marco Ferretti

The authors argue that so far applied damage functions result in impacts with large uncertainty in the magnitude of ozone effects predicted. They use the O-CN biosphere model to test four already existing damage functions in terms of their simulated whole-tree biomass responses against field data from 23 ozone filtration/fumigation experiments and found that biomass damage was overestimated (Lambardozzi et al. 2012) or underestimated (Wittig et al 2007; Lambardozzi et al. 2013). The authors tune/re-parameterize those damage functions towards a better fit with data from 15 fumigation

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experiments with young trees. In a second step, the authors tune DRRs again so that relative biomass (or NPP) simulated on adult trees fit the measured values on young trees.

The ms. reads very well and is certainly within the scope of BG. As a matter of fact, we appreciate this exercise as it addresses a crucial issue in ozone risk assessment and provides an excellent review on the state of the art.

While the first part, i.e. recalibration of existing damage functions makes sense to improve DRRs for young trees and better predict biomass loss due to ozone. We are, however, concerned about the second step, i.e. the reparameterization/tuning of those functions (for young trees) to better predict relative biomass for mature trees. The authors aim at improving the quantitative understanding of ozone effects on forest growth and carbon sequestration on a regional or even global scale. Using data from seedlings grown under (semi-)controlled experiments ranging over a few years may (still) not lead to reliable model functions for adult trees growing in complex forest ecosystems. The cited work by Franz et al. 2017 (GPP reduction, based on damage functions from Wittig et al. 2007) is an example how model exercises using modeled data may result in inaccurate predictions – if not validated with measured data from adult trees (see also Cailleret et al. 2018). Page 15, line 6-10 demonstrates the risk of applying models, based on former functions and stresses the need of validating model exercises with measured data (e.g. from ICP Forests). We suggest to either omit the second part or to extend section 3.3. and the discussion and to outline not only the advances but also the still existing lack of knowledge for estimating ozone induced biomass effects on adult trees, forest ecosystems respectively.

Novak et al. (2008) found that species competition may alter DRRs. We did not understand if and how competition is considered in the O-CN biosphere model. Please, elaborate on this in more detail and in relation to the anticipated forest ecosystem approach.

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The term “damage” is frequently used in the ms. in different contexts and scales: “Damage of photosynthetic apparatus”, “ozone damage”, “leaf-scale” to “global estimates”. In some parts it seems that damage functions refer to “the effects of ozone uptake on photosynthetic variables” and in other parts damage seems to refer to “the fractional loss of carbon uptake associated with ozone uptake”. We suggest that the authors define explicitly and very early in the ms. what they mean with “damage”, “ozone damage”, “damage function” and also specify the difference between “dose-response relationship” and “damage functions”.

Ozone and trees and forests: The actual extent to which reduction in tree growth due to ozone occurs in the real forest remains still unclear. There are several studies which found significant effects, others did not (they did not observe measured above-ground tree growth, which is not total biomass, but an often used proxy for it). We think these controversial results should be considered and discussed as they may help to better contextualize the paper.

Juvenile vs. mature trees: Despite the short explanation given on p. 8, line 21-28, it is not clear how DDRs for mature trees were simulated. Since this is a very important step (and output) for the non-modelers, a more detailed explanation will be very useful here.

Reduction of biomass: It is not clear what is intended here as reduction of biomass. While we understand the reduction of biomass increment, we can hardly see a living tree reducing its biomass due to ozone. The formulation (6) actually seems to refer to a difference of biomass of treated trees with respect to the controlled ones, and not to a reduction of biomass of the treated trees. This is somewhat acknowledged by the authors in the discussion, but perhaps it deserves more emphasis.

Finally, it will be important to have some statement how the authors - based on these results - see the value of the risk maps produced by EMEP for e.g. European forests.

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P2, L17: “simulated reductions in GPP due to ozone damage vary substantially between models and model versions”: please, provide some examples and values.

P2, L18: Here and elsewhere, you may consider Cailleret et al. (2018) as additional reference.

P3, L10: You may consider Schaub et al. (2005) as additional reference. M&M: Please, provide more details on O-CN structure and main assumptions, even though this model has been used and described in Zaehle and Friend (2010), and in Franz et al. (2017). It would help that the reader does not need to go back and forth between the current paper and these ones. e.g., What is the spatial resolution? Individual-based or cohort-based model?

P4, L31-33: No reserves?

P4, L31-33: Biomass growth seems to be dependent only on source but not on sink activity. Is that correct? If yes, this is a strong assumption and limit of the modelling approach (see Körner 2015) that has to be discussed later.

P5, L10: We suggest to add the equation(s) used to calculate  $An,l$

P5, L17: It seems that the authors assume that  $O_3$  concentration is constant within the canopy. Correct? Please, clarify and discuss.

P5, L25: “the Phytotoxic Ozone Dose (POD,  $mmolm^{-2}$ ) can be diagnosed by the accumulation of  $fst,l$  for the top canopy layer ( $l = 1$ ).” In most ozone-flux modeling approaches, POD is calculated based on a “big-leaf” approach (one layer of leaf area, but LAI can be  $> 1$ ; approach used in DO3SE) -> this is different from the accumulation of  $fst,l$  for the top canopy layer. See also P16, L18. Please clarify.

P6, L18: Correct "ration"

P6, L19: Correct "is is"

P7, L18: The initialization phase is not clear: we don't see how the model can run “from

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bare ground until the simulated stand-scale tree age was stable and representative of 1-2 year old seedlings”. And this is even less clear with the sentence P8, L5: “The duration of the initialization phase (...) averages 7.8 years”. Furthermore, did the authors run only one or multiple O-CN simulations per study case (per experiment)? We guess there are some stochastic processes in O-CN, these ones may induce some epistemic uncertainties that have to be considered in the modeling framework.

P9, L25: It is not clear how this “tuning” has been performed: was it a manual or an automatic optimization. Which algorithm was used? Bayesian framework? Which metric did the authors try to optimize (likelihood; rmse, r2)?

P10, L15: Please, show results in Supp Mat.

P11, L2: “The simulations L12PS and L12VC (...) strongly overestimate”. Yes, but this is less strong than the underestimation by W07 and L13.

Figure 2, panels a, b, c, d: what do the simulations without O3 fumigation (after the red line) look like?

Figure 2: Please, add simulated before cumulative in the legend; Idem P11, L11, add simulated before CUOY.

P11, L16: There is no control simulation shown in Fig. 2 (see our comment above)

P12, L5-7: In M&M

P12, L12 to P13, L4: This comparison between mature vs. young trees is not described in the M&M. How do the simulations differ in terms of initialization etc.?

P14, L11 and P15, L8: Please provide some values.

P15, L17 and throughout the paper: Note that Büker et al. (2015) used the Jarvis equation to simulate stomatal conductance while the Ball & Berry one is used here. Please be cautious when comparing both studies.

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P15, L20-22: We agree that this is a key aspect, which has to be more detailed. Discussion: The DRRs built in the present study are valid only for O-CN and may not work for other dynamic vegetation models (strongly depends on how biomass growth is simulated by the DVM -> sink vs. source activity etc.). This is implicitly written in P15, L32-33; but this has to be mentioned again in the conclusion. We suggest to rather highlight that the approach developed here is interesting and can be followed to calibrate “ozone submodels” in further DVMs.

P16, L15 and P17, L5: Idem show some results in Supp Mat.

P17, L30-33: Authors ask for monitoring programs “capable to measure the actual increment of biomass”. We assume that they know that these programs do exist, e.g. national forest inventories and international monitoring programs such as the ICP Forests. Please, quote these programs here.

P18, L1-10: The authors may also consider that trees usually occur in forests, and that forests are subjected to entire ecosystem dynamics that can offset / mitigate / adapt / compensate ozone effects. This should be discussed and considered in the conclusions.

Suggested references:

Cailleret et al. (2018) Ozone effects on European forest growth - towards an integrative approach. *Journal of Ecology*, doi: 10.1111/1365-2745.12941

Novak et al. (2008) Ozone effects on visible foliar injury and growth of *Fagus sylvatica* and *Viburnum lantana* seedlings grown in monoculture or in mixture. *Environmental and Experimental Botany*, doi: 10.1016/j.envexpbot.2007.08.008

Schaub et al. (2005) Physiological and foliar symptom response in the crowns of *Prunus serotina*, *Fraxinus americana*, and *Acer rubrum* canopy trees to ambient ozone under forest conditions. *Environmental Pollution*, doi: 10.1016/j.envpol.2004.06.012

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