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Discussions



# *Interactive comment on* "Development and evaluation of an ozone deposition scheme for coupling to a terrestrial biosphere model" *by* Martina Franz et al.

**Martina Franz et al.**

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Dear Referee,

we thank you for your detailed and constructive comments that helped considerably to improve the manuscript.

Yours Sincerely
Martina Franz



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## 1 General remarks

One of the key-equations (derived from Wittig et al., 2007) is equation 16. There are number of issues with the use of this regression equation.

Q: 1) As the authors remark in their discussion, a conceptual problem of using equation is that even at cumulative $O_3$ uptake of zero, the equation still predicts a -6 % impact on photosynthesis. Also the slope of the equation -0.22 % per mmol m-2 is low compared to some other studies. I would suggest that refitting of the data, and forcing the values to go through zero is one option for a sensitivity study. Possibly another option is to re-fit these data to cumulative uptake above the threshold. On page 3/l. 28 the parameterization of Lombardozzi (2015) is mentioned, however without discussion on why this relationship is not used.
A: A refitting of the Wittig damage function would be desirable. However, a data request to V. Wittig remained unanswered. A refitting can not be done without repeating the work done by the meta-analysis.
There are several reasons for not using the Lombardozzi damage function.
For tree species, Lombardozzi et al. (2015) assume a fixed reduction of net photosynthesis due to ozone independent of the actual ozone uptake. This fixed reduction is -12.5 % for broadleaved species and -16,1 % for needle-leaved species. Only for crops and grasses ozone damage to net photosynthesis depends on ozone uptake. In other words, the atmospheric ozone concentration and ozone uptake into the plants do not affect the damage estimate for tree species but only for grasses and crops. Due to the lack of impact of ozone uptake on ozone damage estimates, the offset implied by Lombardozzi et al. (2015) is actually higher. The effect of the step decrease in Lombardozzi et al. (2015) might be ameliorated by the fact that canopy conductance is affected in parallel. However, this results in a general decoupling of photosynthesis and canopy conductance. Our aim here was to investigate the effect of



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ozone damage to net photosynthesis under the assumption that photosynthesis and canopy conductance remain coupled. We have extended the discussion to make this point clearer.

Q: 2) I would appreciate some discussion with regard of the validity of the experimental relationship for leaf-level ozone, or whether it also suffers from some atmospheric diffusion effect? Ideally when using such parameterisations the experimental conditions should be reproduced. I propose the authors have a look at some of the data used in Wittig, to resolve this issue
A: The experiments used by Wittig et al. (2007) do not use the leaf-level ozone concentration to calculate ozone uptake but the atmospheric ozone concentrations. The ozone uptake calculation thus differs in this respect between our simulations and the experiments used to derive the damage relationship. However in the experiment, ozone uptake is not directly measured. Rather, it is calculated from mean ozone concentrations over the exposure period and the respective average stomatal conductances. Thus the estimated ozone uptake rates and hence the amount of accumulated ozone used to derive a damage relationship are a coarse approximation and underlie considerable uncertainty. Following this the error introduced by using leaf-level ozone concentrations instead of atmospheric concentrations seems small, especially since the use of the leaf-level ozone concentrations is the physiological more appropriate approach. We have extended the discussion to make this point clearer.

Q: 3) I have difficulties to understand equation (15) page 8. Several issues need clarificiation: a) Why is Fst, detox used? The cumulative ozone uptake is dependent on the overall flux, regardless whether it is detoxified or not.
A: The Wittig damage function bases on CUO which accumulates the ozone uptake without a threshold. We changed this equation and rerun all simulations. In the new version ozone damage is calculated on ozone uptake accumulated without a

threshold. We note that this does not affect any of our conclusions, but agree with the reviewer that this is a cleaner way to address the issue.

Q: b) what is the rationale of using the factor fshed? Why would young leaves be less or not sensitive to ozone damage? What is the reference for this?
A: $f_{shed}$ is the fraction of new developed leaves per time step and layer. In the revised version, this factor was renamed to ($f_{new}$) to facilitate the understanding. New grown leaves are assumed to be undamaged. For evergreen species the old damaged leaves still exist when new leaves are grown. In this condition, $f_{new}$ causes the canopy layer CUO to be reduced when new leaves are grown, because they are health do not suffer ozone damage yet, i.e. if 10 % new leaves are grown ($f_{new} = 0.1$), the CUO is reduced by 10 %. Without this equation, newly grown leaves would be assumed to be similarly damaged to already existing foliar, which is not correct, and would cause the CUO for evergreen species would continuously increase over the years.

Q: c) Rearranging this equation 15 gives CUO=cFst,detoxdelta_t/fshed - I guess in times that fshed is close to zero, the values of CUO can get very large. I suspect something is not correct with this equation.
A: The equation was rewritten as:

$$\frac{dCUO_l}{dt} = (1 - f_{new})CUO_l + cF_{st,l} \tag{1}$$

As already mentioned in b). $f_{new}$ (formerly $f_{shed}$) s the fraction of new developed leaves per time step and layer. $f_{new}$ can take values between zero and one. $f_{new} = 0$ when no leaves are grown in the present time step, and $f_{new} = 1$ when newly grown leaves make up all of the present canopy. The $CUO_l$ of the previous time step is reduced according to the fraction of new grown leaves $(1 - f_{shed})CUO_l$.

Q: d) I would expect that CUO is something integrated over the canopy, as mentioned in p. 8 l 18- but it would be good to have the equation already describing this.
A: An additional equation clarifying this was added (new eq. 15):

$$CUO = \sum_{l=1}^{n} CUO_l. \tag{2}$$

Q: e) see remark 2) but the equation 16 seems to be valid for cumulated ozone flux, not for fluxes corrected for detoxification, as suggested by equation 15.
A: Yes. Equation 15 was adapted to use $F_{st}$ (without a threshold), and all simulations are rerun. Plots containing $CUO^1.6_5$ are substituted with CUO of skipped. Equation 14 was skipped.

Q: f) Somewhat related to the point above: even if plants can detoxify ozone, some costs will be associated with this mechanism. Where is the impact of this process accounted for
A: Costs for detoxification are not accounted for in the current model version. To our knowledge no suitable data are available to parametrise e.g. the increased respiration costs according to ozone uptake, since it is very hard to disentangle costs for ozone detoxification from other factors influencing leaf respiration under elevated ozone exposure.

Q: 4) Missing processes: there are several publications suggesting that ozone damage advances senescence (e.g. Gielen, 2007). Further ozone can damage of stomata- leading to sluggishness (e.g. Paoletti) To what extent are these processes included and how would they affect results?
A: Reduction of photosynthetic capacity is one feature of early senescence, others are not included. Omitting effects like early litter fall will underestimate ozone damage.

Stomatal sluggishness is not included in the model version described here. Transpiration rates are thus underestimated compared to accounting for sluggishness. A model version of OCN exists where sluggishness can be accounted for however in this case it occurs permanently for all PFTs. This seems to overestimate the effect at least in regions where low ozone concentrations occur. Following this stomatal sluggishness is an important aspect of ozone damage however it seems not reasonable to generally include it in the base model version. The simulation of sluggishness might be very interesting in a sensitivity study where also other effects like detoxification (e.g. through various flux thresholds) are tested on their impact on ozone damage estimates. We have extended the discussion to clarify that the current model does not include all known ozone effects.

5) Coupled atmospheric dynamics-vegetation ozone models suggest rather strong atmospheric responses and feedbacks. E.g. Super, Vilàâ RGuerau de Arellano, Krol, JGR, 2015 as well as some papers cited here. I think the virtue of this publication is an increased understanding of the vegetation dynamic response (still with a lot of uncertainties), but in addition coupled atmosphere-vegetation simulations are still in its infancy. This should be clearly mentioned in abstract and conclusions.
A: We add this point to discussion and conclusion. However, we don't think that this issue is important enough to merit mentioning in the abstract.

## 2 Minor remarks

Q: p. 1 l.6 free troposphere is the region above the boundary layer. I guess the authors mean near-surface ozone in the planetary boundary layer
A: Is changed to 45 m height.

Q: p.1 l.9 Although it probably doesn't matter: are the authors comparing the model with or without ozone effects
A: The model 'including $O_3$ damage' is used.

Q: p. 1 l. 17- outside the leaves: suggest to call this near-leaves, or leaf-level ozone.
A: Is called 'leaf-level ozone'.

Q: p. 2 l. 3 As raised in general comments, are the effects of anti-oxidant mechanisms somehow included?
A: No, since the flux threshold is omitted in the final version no detoxification occurs.

Q: p. 2 l. 4 Better to include a range: a factor 2 to 5. I personally do not think a factor of 5 is realistic.
A: Changed to 'a factor 2 to 5'.

Q: p. 2 l. 11 delete 'less polluted' transport is taking place regardless of pollution levels.
A: Done.

Q: p. 3 l. 18 'no damage' is observed. Detoxification: explain what consequences for productivity this can have
A: Detoxification causes increased respiration costs and following this reduces NPP what may reduce growth and biomass.

Q: p. 3 l. 28. Explain why this parameterization was not used





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A: Atmospheric ozone concentrations and cumulated $O_3$ uptake only impact net photosynthesis of one plant functional type directly. For the two other plant types net photosynthesis is reduced in a step function independent of the accumulated ozone uptake.

Q: p. 4 l. 6 sensitivity analysis towards selected critical parameters?
A: The aim of the sensitivity study is to test the functionality of the deposition model, because it is calculates leaf-level $O_3$ concentrations and hence has a large impact on $O_3$ uptake estimates. The variable $R_b$ is also an important variable of the deposition model and was added to Fig. 3. The respective sentence on p 4 was changed from "provide a sensitivity analysis of the model to evaluate the reliability of simulated values of $O_3$ uptake' ' to 'provide a sensitivity analysis of critical variables and parameters of the deposition model to evaluate the reliability of simulated values of $O_3$ uptake'.

Q: p. 4. L. 9 accumulation of what?
A: Accumulation of ozone. Changed to '$O_3$ uptake and cumulated uptake'.

Q: p.4 l. 11-25 I would appreciate some more information on the models. How many canopy layers are in OCN?
A: There are maximum 20 layers. The number of actual simulated layers depends on the site and the PFT. Included in Methods section.

Q: Is there an interaction of N in leaves with ozone?
A: Yes. Photosynthetic capacity depends on leaf nitrogen concentration and leaf area, which are both affected by ecosystem available N. Increases in leaf nitrogen content enable higher net photosynthesis and higher stomatal conductance per unit leaf area. This in turn affects transpiration as well as ozone uptake and ozone damage

estimates. Included in Methods section.

Q: What version of the EMEP model (output) was used, regional, global, resolution? Explain vertical structure of EMEP- can a constant mid-of-the gridbox of 45 meter be safely used, or are the regions (e.g. in the mountains) where this value is different (i.e. is the coordinate system fixed altitude, pressure or hybrid)?
A: We used version 4.4 of the EMEP MSC-W model, in essentially the same setup described in the nitrogen deposition study of Simpson et al. (2014b). The model was run for the regional RCA3 domain, driven by RCA3 meteorology. The vertical structure is the standard EMEP one (see Simpson et al., 2012) with a lower layer of about 90m thickness. The coordinates are terrain following (sigma coordinates) though, so the mid-point of ca. 45m is relative to the assumed ground surface in such a system. The main assumption of all EMEP deposition modelling is that this 45m height lies within the surface layer, so that standard similarity theory can be applied. This assumption is not always correct of course, but in general the EMEP model's predictions of near-surface ozone (and even fluxes, e.g. Klingberg et al, 2008) suggest that the methodology is reasonable.

Q: I think the model can also output near-surface ozone (diagnostic). Why was this not used- it would avoid additional uncertainty in the recalculation of the atmospheric resistance.
A: The calibration of leaf-level ozone concentrations is impacted by e.g. LAI and canopy conductance. They both determine the amount of ozone taken up by the stomates and hence reduce the ozone concentration within the canopy air. EMEP and OCN differ in both such that EMEPs estimates for leaf-level ozone concentrations differ from the estimates by OCN. Furthermore leaf-level ozone concentrations are calculated separately for each PFT because the PFTs differ in their LAI values. EMEP and OCN simulate different groups of PFTs and a different number of PFTs. We added





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an explanation to the Methods section.

Q: p. 5 Ra is the resistance between the surface (near-canopy) and 45 meter (i.e. it is not at a level of 45m).
A: Changed to 'between 45 m height and the canopy'.

Q: p. 4 l. 18 Can something be said on how this conductance is distributed over the canopy layers- in general how vertical canopy structure is expected to influence ozone uptake
A: Leaf N is generally highest in the top canopy and monotonically decreases with increasing canopy depth. Following this stomatal conductance and $O_3$ uptake is highest in the upper canopy and lowest in the bottom of the canopy. Included in Methods section.

Q: p. 5 l. 19: was this calibration necessary for this study, or more general for OCN model results?
A: This calibration is generally necessary to yield reasonable conductance values in OCN.

Q: p. 6 mention which three PFTs were considered for this LAI+1 approach? Probably for the readers not wanting to go back to older papers, a table listing some characteristics of the PFTs (appendix) would be useful
A: The LAI+1 approach is applied for all tree PFTS (woody PFTs).

Q: p. 6/7 eq. 8,9,10 to what extent are these equations based on observations, or merely model assumptions (and what is the associated uncertainty)
A: These equations are largely the same as used in the EMEP model. As described in





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Simpson et al, 2012, Eqn (8), for $F_T$ is taken from Zhang et al. (2012), Eqn (9), $R_{inc}$ is from Erisman et al (1994), and Eqn (10), giving the effect of snow on $R_{gs}$, is also loosely based upon Zhang et al.

Although all such equations are uncertain (all depositions schemes are!), the EMEP model's deposition scheme (and associated D$O_3$SE module for $O_3$) has undergone extensive review and comparison measurements, see for example:

- Emberson, s. D.; Büker, P. & Ashmore, M. R. Assessing the risk caused by ground level ozone to European forest trees: A case study in pine, beech and oak across different climate regions Environ. Poll., 2007, 147, 454-466
- Emberson, L.; Ashmore, M.; Simpson, D.; Tuovinen, J.-P. & Cambridge, H. Modelling and mapping ozone deposition in Europe Water, Air and Soil Pollution, 2001, 130, 577-582
- Emberson, L.; Wieser, G. & Ashmore, M. Modelling of stomatal conductance and ozone flux of Norway spruce: comparison with field data Environ. Poll., 2000, 109, 393-402
- Klingberg, J.; Danielsson, H.; Simpson, D. & Pleijel, H Comparison of modelled and measured ozone concentrations and meteorology for a site in south-west Sweden: Implications for ozone uptake calculations Environ. Poll., 2008, 115, 99-111
- Simpson, D.; Tuovinen, J.-P.; Emberson, L. & Ashmore, M. Characteristics of an ozone deposition module II: sensitivity analysis Water, Air and Soil Pollution, 2003, 143, 123-137
- Simpson, D.; Tuovinen, J.-P.; Emberson, L. & Ashmore, M. Characteristics of an ozone deposition module Water, Air and Soil Pollution: Focus, 2001, 1, 253-262

- Tuovinen, J.-P.; Simpson, D.; Mikkelsen, T.; Emberson, L. D.; Ashmore, M. R.; Aurela, M.; Cambridge, H. M.; Hovmand, M. F.; Jensen, N. O.; Laurila, T.; Pilegaard, K. & Ro-Poulsen, H. Comparisons of measured and modelled ozone deposition to forests in Northern Europe Water, Air and Soil Pollution: Focus, 2001, 1, 263-274
- Tuovinen, J.-P.; Emberson, L. & Simpson, D. Modelling ozone fluxes to forests for risk assessment: status and prospects Annals of Forest Science, 2009, 66, 401
- Tuovinen, J.-P.; Ashmore, M.; Emberson, L. & Simpson, D. Testing and improving the EMEP ozone deposition module Atmos. Environ., 2004, 38, 2373-2385
- Tuovinen, J.-P.; Emberson, L. & Simpson, D. Modelling ozone fluxes to forests for risk assessment: status and prospects Annals of Forest Science, 2009, 66, 401

In any case, it can be noted that the low-temperature and snow terms given by Eqns (8) and (10) are only really important in conditions for which ozone uptake will be very small.

Q: p. 7 l.18 1 mmol/m2: is this referring to cumulative ozone uptake? Is this published (reference?). I am not sure if such sensitivity with an atmospheric model which would include chemistry feedbacks, can be translated into such small uncertainty for the vegetation $O_3$ uptake. Note that there is in general a quite large difference in PBL mixing in a variety of atmospheric models- which in itself already suggests a large uncertainty.
A: Yes, the 1 mmol/m2 is for CUO with threshold 1.6. This estimate comes from tests done for this paper, by running the EMEP model with different assumptions, but it only represented the uncertainty due to the OCN simplifications in resistance terms, not of course the overall uncertainty in the model system. In any case, since we now use a zero threshold, and have modified the OCN resistance terms, a new calculation was

needed.
The respective paragraph was changed to: 'However, a series of calculations with the full EMEP model have shown that the uncertainties associated with these simplifications are small, typically 0.5 - 5 $\mathrm{mmol\,m^{-2}}$. As base-case values of POD0 are typically ca. 30-50 in EU regions, these approximations do not seem to be a major cause of error, at least in regions with substantial ozone (and carbon) uptake. The coupling of OCN to a CTM would be desirable to eliminate this bias.'

Q: p. 8 l-1-20 See remarks above- need to get a better description if/how detoxification is included
A: The flux threshold simulating detoxification was skipped and all simulations were rerun.

Q: p. 8 l. 8: do I understand correct that in the rest of the text $\mathrm{CUO}_5^{1.6}$ would refer to equation 15; while CUO would refer to use Fst in equation 15. This needs to become clear- and the correct equations need to be given.
A: The flux threshold was skipped and following this also $\mathrm{CUO}_5^{1.6}$ was skipped. The cumulated $O_3$ uptake (CUO) derives from the accumulation of the ozone uptake without any flux threshold ($F_{stC}$).

Q: p. 9 l. 5. Would a sensible variation of dl (equation 16) also be a critical parameter?
A: The objective was to test functionality of the implemented deposition scheme. The validity of the implemented damage function is a very interesting topic however would have expanded the manuscript too much. We are currently working on evaluating different damage functions implemented in OCN in their ability to reproduce observe damage relationships.

Q: How was this subset of parameters selected.

A: Key parameters of the deposition scheme which determine leaf-level $O_3$ concentrations and hence $O_3$ uptake are investigated. The variable $R_b$ is also a key variable of the deposition scheme and was added to Fig. 3.

Q: p. 9 l. 22 What is the La Thuille dataset?
A: The La Thuile Dataset contains the data of all sites and years of the FLUXNET network. The respective web link is included as a reference: 'http://fluxnet.fluxdata.org/data/la-thuile-dataset/'.

Q: p. 9 l. 25 How many years were need to reach equilibrium?
A: 1200 simulation years for the vegetation and 12000 years for the soil secure equilibrium.

Q: What was the criterium for equilibrium?
A: Equilibrium is reached when the carbon and nitrogen pools in vegetation and soil show no trend anymore as mentioned on p 9 l 25 (of the manuscript in discussion).

Q: p. 9 l 28: Which EMEP simulations were providing this 100 years transient concentrations? Is there a reference?
A: p 9 l 25 indicates that more details regarding EMEP are given in section 2.5, including also a reference.

Q: p. 9 l. 29 Appendix tab 1? I think just table 1
A: The appendix section was unintentionally included into the main part of the paper.

Q: p. 10 I understand that the purpose of section 2.4 is to derive trust in the model, when testing to observable parameters. I would however need some more insight in why morning/evening fluxes need to removed, and data with the different soil moisture. What would be the effect of not removing such data?

A: The morning and evening hours are removed since in this time dew condensation on the leaves causes a wet canopy. This causes an alteration in latent heat exchange ($LE$) such that FLUXNET observed canopy conductance, which is inferred from $LE$, is prone to a high uncertainty in these times. Soil moisture constraints directly impact the simulated net photosynthesis (see $\Theta$ in Eq. 5). It is hard for a global model, not tuned for the specific site, to properly model the drying of the soil and onset of soil moisture stress (which depends e.g. on soil type and texture as well as the degree of root penetration). By excluding data under soil moisture stress this bias is removed.

Q: p. 10 l. 29- brought into equilibrium. How done?

A: The model is run with the 1961-1970 forcing until equilibrium of the carbon and nitrogen pools in vegetation and soil is reached. The forcing for each year of the Spinup phase is randomly chosen from the period 1961-1970. Changed in the text from 'with 1961-1970 forcing' to 'by randomly iterating the forcing from the period 1961-1970'.

Q: p. 11 figure 1: obviously the largest discrepancy is found for LAI and in p. 12/l. 18 the authors suggest that this is not important. How is it possible to have realistic GPP etc; and such a spread in LAI? Please explain

A: The LAI measurements presented here are point measurements of years outside the simulation period. The actual LAI values at the FLUXNET sites during the simulation period might be different. Furthermore in OCN GPP depends on LAI in a non-linear relationship where GPP saturates with increasing values of LAI (saturation point at a LAI of approximately 4). When LAI increases further the lower canopy does not get sufficient light to increase GPP. GPP however is not only determined by LAI, but also e.g by temperature, radiation and soil moisture stress what might ameliorate differences in LAI. Added: 'Modelled GPP does not only depend on LAI, but also on light availability, temperature and soil moisture.'

Q: p. 12 l. 24. While it is facilitating the discussion to focus on only 3 stations, some words on how representative these stations were for others would be welcome.
A: The three sites were chosen to be examples of the 3 major categories. The respective sentence was reformulated to: 'For further evaluation of the modelled ozone uptake, we analysed the diurnal cycles at three sites, one of the three categories broadleaved, needle-leaved and C3 grass sites respectively.'

Q: p. 13 l. 5-25 I would advise to also see Hardacre, Atmos. Chem. Phys., 15, 6419-6436, 2015, for further opportunities to evaluate the ozone deposition velocities and fluxes.
A: Hardacre et al. 2015 was included into the evaluation of the deposition velocities. The fluxes given in Hardacre et al. 2015 are total dry deposition values. In the manuscript here we evaluate the stomatal fraction of the dry deposition ($F_{stC}$). A comparison of both is not possible.

Q: p. 13 l. 35 Can you confirm that sapflow measurements are not reliable for this study?
A: We can not judge which measurements (eddy covariance or sap flow) are reliable. However we observe that between both techniques the estimates of canopy conductance differ by a factor of more than 10 and that our estimates reported here are more similar to estimates done by measurements conducted by the eddy covariance technique. Since canopy conductance drives $O_3$ uptake, a 10 fold higher canopy conductance results in an approximately 10 fold higher $O_3$ uptake rate (disregarding in this approximation the feed back of $O_3$ uptake into the leaf on decreasing leaf-level $O_3$

concentrations).

Q: p. 14 repeat here that Fr is the ratio of stomatal to overall flux. It would be interesting to give average values (per ecosystem/PFT) over the months. Perhaps for an appendix? I think this could be useful for comparison in future studies.
A: The explanation of Fr on p 14 was changed from 'The ratio between the vegetation ozone uptake and the total surface uptake (Fr)' to 'The ratio between the stomatal ozone uptake and the total surface uptake (Fr)'. A graph showing monthly mean values of key ozone metrics is added to the appendix (Appendix 11).

Q: p. 14 l. 34 I didn't quite understand the sentence not zero because accumulate over several years. Isn't it simply that there is already some photosynthesis activity?
A: Accumulated ozone is shed when leaves are shed. Deciduous PFT's shed all accumulated $O_3$ at the end of the growing season when the leaves are shed. Evergreen species only shed a fraction of their leaves and keep the leaves that have already taken up $O_3$ for several years. The CUO decreases in winter when the evergreens shed part of their leaves but since they do not shed all the CUO remains greater than zero.

Q: p. 15 l. 8-10. It is not clear to me whether OCN has croplands, and if so what crop? The authors mention C3 crops- I guess that would be mainly wheat?
A: OCN simulates 12 PFTs including 8 tree PFTs, 2 grass PFTs and 2 crop PFTs. The crop PFTs are a generic C3-crop and a generic C4-crop. As species are not explicitly simulated for the tree and grass PFTs this is also not done for the crops.

Q: p. 15 l. 17 Figure 6a is an EMEP model output?
A: Ozone concentration plotted in Fig. 6a is the forcing OCN uses for the simulations.

This forcing is provided by EMEP.

Q: p. 15 Appendix 12 ab missing. Do you mean Figure 12? See=>sea
A: The appendix section was unintentional included into the main part of the paper (Appendix 12 == Fig. 12).

Q: p. 15 l. 34 interesting dynamical/phonological feedback, but it also reminds that things like early senescence are probably not included.
A: Yes, early senescence is not included.

Q: p. 16 section 3.5 Please remind reader of what D-STO and ATM were? See section 2.6.
A: Changed to 'the D-STO model (non-stomatal depletion of ozone is zero) and 20-25% for the ATM model version (canopy $O_3$ concentration is equal to the atmospheric concentration) '.

Q: Appendix 11 and 13 are missing.
A: The appendix section was unintentional included into the main part of the paper.

Q:L. 13 uptake and accumulated: rephrase in:accumulated uptake.
A: Done.

Q: p. 16 spell out the meaning (remind the reader) of CUO1.5 and 5.
A: Due the omission of the flux threshold $CUO_5^{1.6}$ does no longer occur.

Q: p. 17 section 4.1, l..10 Interactions with VOCs (as well as soil NOx emissions,

see Ganzeveld's paper), are important. But I don't understand how they are implicitly included, especially in the OCN framework.
A: All ozone deposition models that we are aware of have terms for the stomatal uptake of $O_3$, and then for 'non-stomatal' terms in some form ($G_{ns}$ in Eqn. 4). The stomatal terms can be estimated quite well, e.g. from water fluxes. Unfortunately, the values assigned to $G_{ns}$ cannot be determined from first principles or even experiment because of the complexities of the surface characteristics (moisture films, chemical compounds on leaves, etc, Fowler 2009), and of interpreting flux measurements in the chemically-active conditions associated with vegetation canopies. Thus, the $G_{ns}$ terms encompass both deposition and chemical processes - they are essentially tuned to give reasonable values for deposition velocities across diurnal cycles for example.

Q: p. 17 l. 20: was $O_3$ needed to reach this good agreement. Probably not- explain.
A: Given the uncertainty of the observations and model results the inclusion of ozone damage does not improve the fit of the model results to the observations. The comparison to FLUXNET data was mainly meant to show that the model in general produces realistic values especially for the canopy conductance ($G_c$), since $G_c$ is a major factor determining ozone uptake and hence estimated damage.

Q: p. 19 l 3. As explained above, I think this warrants some additional analysis.
A: The validity of the implemented damage function is a very interesting topic however would have expanded the manuscript too much. We are currently working on evaluating different damage functions implemented in OCN in their ability to reproduce observe damage relation ships. This is a topic of its own.

Q: p. 19 l. 22 impacted=>determined?
A: Changed to determined.

## 3 Figures

Q: Figure 6: Why are the units of panels b and c different?
A: The units are different because different variables are plotted. In panel b the mean ozone uptake rate [$nmol\,m^{-2}\,s^{-1}$] is plotted. In panel c the mean ozone accumulation [$mmol\,m^{-2}$].

Q: The chosen range doesn't work well for panel b (all purple).
A: The color range is not the problem in the big purple area. The values of the mean uptake rate all lie between 1.9 and 2, which simply is a small range.

Q: Figure 7: it is hard to discriminate the colors in Figure 7.
A: The color palette is changed from rainbow to restricted color gradients (palettes from ColorBrewer 2.0).

Q: Figure 9: legenda describing a) can be improved.
A: Changed from 'no ozone deposition scheme (ATM),' to: 'canopy $O_3$ concentration is equal to the atmospheric concentration (ATM)'

Q: Figure 12: color scheme doesn't work (mostly red)- more resolution for low values is need (0-10%). For C4 crops- is irrigation considered?
A: Irrigation is not considered for crops. The graph is skipped due to it's minor value in explaining observed results.