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

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Review:

This is an interesting and well-written paper, describing assumptions and impacts of these assumptions regarding the inclusion of an ozone deposition scheme in terrestrial biosphere model on a number of ecosystem functions.

The authors make the point that the calculation of in-canopy (leaf-level) O3 concentrations is important for calculating stomatal uptake and impacts on photosynthesis- and this difference is one of the major causes of lower O3 impacts than suggested in other studies. A substantial part of the paper is about comparison of fluxnet data with

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a number of underlying parameters, which are important to calculate ozone fluxes. I think this is a very good approach.

With some exceptions, the description of methods and results is quite clear, although my feeling is that the uncertainly analysis is covering only part of the overall uncertainties, and perhaps not all relevant processes are included. I recommend publication of this paper, when taking into account the remarks and suggestions below.

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.

1) As the authors remark in their discussion, a conceptual problem of using equation is that even at cumulative O3 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.

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 parameterizations 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.

3) I have difficulties to understand equation (15) page 8. Several issues need clarification:

a) Why is Fst, detox used? The cumulative ozone uptake is dependent on the overall flux, regardless whether it is detoxified or not.

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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?

c) Rearranging this equation 15 gives CUO=c*Fst,detox*delta_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.

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.

e) see remark 2) but the equation 16 seems to be valid for cumulated ozone flux, not for fluxes corrected for detoxication, as suggested by equation 15.

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?

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 slugginesh (e.g. Paoletti) To what extent are these processes included and how would they affect results?

5) Coupled atmospheric dynamics-vegetation ozone models suggest rather strong atmospheric responses and feedbacks. E.g. Super, VilàâĂŘGuerau 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.

Minor remarks:

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.

p.1 l.9 Although it probably doesn't matter: are the authors comparing the model with or without ozone effects.

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

p. 2 l. 3 As raised in general comments, are the effects of anti-oxidant mechanisms somehow included?

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.

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

p. 3 l. 18 'no damage' is observed. Detoxification: explain what consequences for productivity this can have.

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

p. 4 l. 6 sensitivity analysis towards selected critical parameters?

p. 4. L. 9 accumulation of what?

p.4 l. 11-25 I would appreciate some more information on the models. How many canopy layers are in OCN? Is there an interaction of N in leaves with ozone? 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)? 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.

p. 5 Ra is the resistance between the surface (near-canopy) and 45 meter (i.e. it is not at a level of 45m).

p. 4 l. 18 Can something be said on how this conductance is distributed over the

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canopy layers- in general how vertical canopy structure is expected to influence ozone uptake.

p. l. 19: was this calibration necessary for this study, or more general for OCN model results?

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.

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).

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 o3 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.

p. 8 l-1-20 See remarks above- need to get a better description if/how detoxicifaction is included.

p. 8 l. 8: do I understand correct that in the rest of the text CUO1.6/5 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.

p. 9 l. 5. Would a sensible variation of dl (equation 16) also be a critical parameter? How was this subset of parameters selected.

p. 9 l. 22 What is the La Thuille dataset?

p. 9 l. 25 How many years were need to reach equilibrium? What was the criterium for equilibrium?

p. 9 l 28: Which EMEP simulations were providing this 100 years transient concentra-

tions? Is there a reference?

p. 9 l. 29 Appendix tab 1? I think just table 1.

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?

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

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.

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.

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.

p. 13 l. 35 Can you confirm that sapflow measurements are not reliable for this study?

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.

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?

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?

p. 15 l. 17 Figure 6a is an EMEP model output?

p. 15 Appendix 12 ab missing. Do you mean Figure 12? See=>sea.

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

p. 16 section 3.5 Please remind reader of what D-STO and ATM were? See section 2.6. Appendix 11 and 13 are missing. L. 13 uptake and accumulated: rephrase in: accumulated uptake

p. 16 spell out the meaning (remind the reader) of CUO1.5 and 5.

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.

p. 17 l. 20: was O3 needed to reach this good agreement. Probably not- explain.

p. 19 l 3. As explained above, I think this warrants some additional analysis.

p. 19 l. 22 impacted=>determined?

Figures:

Figure 6: Why are the units of panels b and c different? The chosen range doesn't work well for panel b (all purple).

Figure 7: it is hard to discriminate the colors in Figure 7.

Figure 9: legenda describing a) can be improved.

Figure 12: color scheme doesn't work (mostly red)- more resolution for low values is need (0-10 %). For C4 crops- is irrigation consired?

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