Editors review of paper acp-2019-538: by Delaria et al.

Dear authors, co-authors,

First of all, sorry for the delay in my decision on your paper "A model-based analysis Of foliar NOx deposition" submitted for publication in ACP. I was extremely busy last 4-5 weeks teaching full time and which didn't allow me to carefully evaluate once more again your response to the provided three reviews/editors comment and the revised version of the manuscript. Now that I have done so I see that you properly handled most of the comments and revision. However, I cannot accept it yet for publication "as is" given that there were still some minor issues that came up reading your ms once more again.
First of all, note that I am aware that some of these minor comments also might be "appreciated" as an editor promoting his/her own work but mainly want to secure that the different communities, the experimental-, air quality (AQ) and climate modelling communities are properly informed about the state-of the art approaches. In the past I noticed for example that there have been (AQ) studies being published that were ignorant about chemistry-climate studies (also due to being published in different journals read by the different communities?), such as the ones on model analysis of atmosphere-biosphere exchange, as a function of stomatal conductance including soil moisture limitation. Your study tackles an issue of relevance for all those communities and consequently support this publication that hopefully further increases the awareness that currently applied approaches to represent atmosphere-biosphere exchange in regional and global scale modelling studies should be revised. You explicitly mention one of those reasons, to properly consider some of the potentially relevant interactions and feedback mechanisms. But your study also shows some more of the subtle features that must be further explored in both experimental and modelling studies and hope that your study will reinforce those considerations.

Page 2, line 20., "CO_2"

Page 2: lines 23-25: Also to deal with my previous editors comment, you now mention the study by Ganzeveld et al. "However, Ganzeveld et al. (2002a) implemented a multi-layer column model in a global chemistry and general circulation model GCM-ECHAM (European Centre Hamburg Model)..."
To avoid any potential misunderstanding seeing the reference to the paper on the single column model study (Ganzeveld et al., 2002a), in the modified text on previous (large-scale) studies on canopy NOx deposition, it could be interpreted that we included this (atmospheric) column model in the global climate-chemistry system ECHAM. However, we actually only included the multi-layer canopy exchange model system in ECHAM to study the role of canopy interactions in global atmosphere-biosphere NOx exchange (Ganzeveld et al., 2002b). The 2002a reference focused on an extensive evaluation of this multi-layer canopy exchange model coupled to an atmospheric column model system, an approach similar to the one presented in your study.

Section 2: I realized that there is one important omission that was previously not captured by the reviewers nor by myself. In Figure 1 the vertical discretization of your column model is shown but think it is important to also mention explicitly in Section 2 how many layers actually represent the canopy and the overlaying atmosphere.

Page 11, line 34-35: Here you could also include the Ganzeveld et al. 2002_a and b references with these studies with the multi-layer canopy exchange modelling system both in a site-scale as well as a global-scale set-up confirming the numbers regarding the effective exchange of soil NOx reported by Jacob and Wofsy (tropical forest case) and Yienger and

Levy (global scale).

Some further minor comments that were triggered going through your reply and revision:

Reviewer #1

Comment: p3, L30: Δh is surely the height / depth of the box. I assume that the model has a horizontal scale of 1m2 or 1cm2, but please clarify this. 50

Response: "Each box layer is treated as well-mixed and homogenous"

Editor: This also addresses the issue I raised on providing some more information about the model set-up. Here I get the impression that you do not really address the comment: It is not so much about the vertical discretization but about for what of horizontal scale the model can be deemed being representative. Generally in such 1-D model approaches it is not so straightforward and being assumed that all the parameters are calculated for a $1m^2$ column but at the end the representative horizontal scale might be determined by the scale of the observations/data that are being included, e.g,, the scale of emission- or vegetation datasets used to constrain such model approaches.

Comment: p6, L5-7: If all processes are correctly included and parameterized there should be no need to use a compensation point; this is merely a formulation that is used when the production and loss terms are not fully represented in a model.

Response: We changed the sentence (P4, L29-30) to say: "We do not allow for emission of NO or NO2 from leaves, consistent with recent laboratory observations that have observed negligible compensation points for these molecules (Chaparro-Suarez et al., 2011; Breuninger et al., 2013; Delaria et al., 2018)."

Here it is worthwhile to mention that indeed more recent measurements indicate that there is no NO/NO2 compensation but older observations have suggested the presence of such a compensation point. This might also be due to measurement issues. But one other feature to bring in here, also given that you have evaluated your modelling system also for the UMBS site, the Seok et al. (2013) paper also reference in your study actually proposed the potential importance of an NO_2 compensation point for that site. This was based on a comparison of the observed and canopy exchange model simulated diurnal cycles in the NO_x concentrations above and inside this forest canopy.

Comment: P11, L2: I have a problem with the use of "wet" and "dry" in this context as deposition itself is referred to as wet or dry. Perhaps the authors can find an alternative way to describe wet and dry environments (I couldn't think of an obvious alternative I'm afraid)

Response: We have considered the reviewers point and understand how referring to conditions as "wet" and "dry" is less than ideal. However, we also were unable to come up with a more appropriate way of referring to these conditions.

Editor, often a source of confusion, wet deposition versus dry deposition to wet surfaces. One suggestion: wet surface and dry surface deposition?

Comment: Figures and Tables
p24, Fig 1: I am surprised that the authors have chosen only to vary PBL for the top two 20 layers in the active mixed layer. I would expect the lower 2 of these layers to similarly

evolve over the course of the day but with lower amplitude.

Response: We do not believe this additional complication would change the general themes presented here, although they would certainly change things in detail.

Editor: although I tend to agree with you that most likely it will not largely effect your results, it would have been good to have included some results on a sensitivity analysis changing the vertical discretization of your modelling system as well as the representation of the PBL depth. If you have indeed done so in the development stage of your model it might be good to explicitly mention this.