

# ***Interactive comment on “Representing sub-grid scale variations in nitrogen deposition associated with land use in a global Earth System Model: implications for present and future nitrogen deposition fluxes over North America” by Fabien Paulot et al.***

## **Anonymous Referee #2**

Received and published: 24 August 2018

The authors addressed well my previous comments during the initial assessment of the earlier version of this paper, and this ACPD version has a better focus in terms of the study goals. This work will complementary to ongoing efforts to quantify nitrogen deposition at various spatial scales and provide an approach for assessing future nitrogen deposition scenarios from changing climate, land use, and emissions. I only have a few additional minor comments below for the authors to consider.

[Printer-friendly version](#)

[Discussion paper](#)



Based on the discussions in Section 3.1, I feel that the modelled  $V_d$  used in this study may be biased higher, or at least among the upper-end range of existing models such as those shown in Wu et al. (2018). Very high  $V_d$  values for some of the N species measured in Nguyen et al., 2015 are close to or even higher than the maximum possible  $V_d$  controlled by aerodynamic and sublayer resistances, and cannot be reproduced by the existing dry deposition models even after adjusting model parameters to the upper range of reasonable values. To avoid too much overestimation of dry deposition, these values are not recommended to be generalized to other regions before more measurement evidences become available. As for the present study, I understand the typical magnitude of uncertainties in dry deposition estimation is about a factor of 2 (Wu et al., 2018), and such uncertainties should be included in the discussion of modeled results and associated impacts on ecosystem health assessment. A brief discussion and recommendation related to this issue may be helpful to readers.

A related point to the above comment: I noticed that the other two reviewers both recommended using the bi-directional approach for  $\text{NH}_3$  deposition. I agree this approach is better in theory, but may not be practical in global models with the current limited knowledge of emission potentials in various land uses. I feel that using unidirectional depositional approach for  $\text{NH}_3$  is still acceptable if the chosen dry deposition model provides conservative  $V_d$  for  $\text{NH}_3$  (which compensates some of the bidirectional fluxes). Under such a condition, the  $\text{NH}_3$  deposition estimates would be valid for non-fertilized land use types and would represent the upper-end estimates for agricultural areas. This point can be made in the revised paper.

---

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-572>, 2018.

[Printer-friendly version](#)[Discussion paper](#)