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Interactive comment on “Non-stomatal exchange in ammonia dry deposition models: Comparison of two state-of-the-art approaches” *by* Frederik Schrader et al.

Anonymous Referee #1

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The paper compares two alternative models to simulate ammonia fluxes and their comparison with the measured fluxes in five peatland and grassland sites, focusing on the non-stomatal fluxes. The motivation for such more empirical deposition models is the inclusion of bi-directional ammonia fluxes into chemical transport models, which requires few and easily available parameters for the surface resistance. The improvement of such models is needed and the respective analysis here is valuable, specifically as it includes a suffiecient number of sites. Focusing exclusively on nighttime fluxes with sufficiently turbulent condition is a good approach. It should, however, be discussed, if nocturnal transpiration could have confounded these observations. The parameters included in both models are temperature and, importantly, relative humid-

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Discussion paper

ity (RH), whereas different ways are chosen regarding the fate of depositing ammonia, either unidirectional (MNS model) or 'quasi-bidirectional' (WK model). Based on common patterns of the five test sites, systematic under- and overestimation of fluxes are then diagnosed and empirical improvements are suggested by the authors. Although ultimately I agree with the overall direction of the paper and the interpretation of the results (see few exceptions below), I find it difficult to follow. One of the reasons is the continuous introduction of a multitude of parameters which makes consequent reading sometimes time-consuming and frustrating. Even in case it does not agree with the usual policy of this journal, I therefore suggest a table detailing all used parameters with units and possibly also other abbreviations. A second and somewhat related reason is the initial explanation of the two models which in some important places is not sufficiently detailed – some examples are given below. I wonder why the effect of RH is so little discussed in the paper - it has an exponential influence on Rw and thus is the most important independent parameter. It could e.g. be included in a analysis similar to Fig. 5. I also wonder if the effect of backward-looking moving averages shouldn't be evaluated together with the RH history. Saturation effects (as mentioned in p.2, l. 20) could play a role at low RH.

P. 5, l. 24/25: This is difficult to follow. Can it be supported by a formula? What happens if RH decreases?

P. 8, l. 19-23: This is indeed intriguing, but on the other hand I cannot really believe that MNS works so well in the prediction of fluxes at VK, when looking at the cumulative fluxes in Fig. 3. Even during the flat part, there is an underestimation of 0.3 kg ha-1. The shape of the cumulative fluxes at BM is considerably different from VK, while the shapes of ïĄĎGw differences in Fig. 4 are very similar between BM and VK. Please check if the statement is really correct.

P. 11, l. 19: which parameterization?

Figure 2: Why is Rw lowest at T=0°C?

Figure 4: Upper row: There seems to be a mismatch between the number of binned values used for MNS and WK comparison, at least for VK. What is the reason?

Minor issues P.6, l.11: ‘approach’, better: ‘reach’? (also p. 9, l. 2) P. 6, l. 16: ‘compensation point Xw decreases’ P. 6, l. 23: why ‘moderately’? I would suggest to omit this word P. 7, l. 18: event P. 10, l. 1: omit ‘and’

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