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Biogeosciences Discussions

Interactive comment on "The annual ammonia budget of fertilised cut grassland – Part 2: Seasonal variations and compensation point modeling" *by* C. R. Flechard et al.

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We thank the reviewer for their constructive comments on our MS.

1- On the scatter in the relationship of Rw to T and RH: we acknowledge that scatter is large, and the end of Section 4.1.3 in the discussion already does provide a few possible clues as to why the variables T and RH do not explain the whole variance (for example l21-24, p9654: "Much additional scatter is expected to be caused by variations in the sink- and source strengths as disscussed above (SO2, rainfall and leaf surface chemistry, histeresis, leaf litter, soil wetness)."). The point is made through the manuscript that a static approach to describe the non-stomatal sink strength (a

deposition-only Rw) is necessarily a gross simplification of dynamic soil and canopy processes, but this actually isn't the focus of this paper and merits further examination in another publication using a dynamic model. We believe that despite the large scatter the proposed Rw parameterisation is robust enough for the purpose of gap-filling and calculation of the annual NH3 budget, and that the data do demonstrate that T and RH are two key variables controlling non-stomatal exchange, albeit not the only ones.

On the use of VPD instead of RH and T: yes, we also tested this originally, but the noise in the relationship was equally large. In fact, as we argued in the paper, we cannot expect to explain the whole variability with only physical variables such as RH, T or VPD, as leaf surface water film chemistry and soil processes are important dynamic drivers for which we have little or no information (eg NH4+ and pH in both water films and soil solution)

2- On a possible underestimation of Xs due to a higher Rw: we show in Fig. 8 that mean micrometeorological estimates of Xs, or rather of Gamma_s, depend on the relative humidity threshold used for the selection of "dry" conditions. We explain p 9638, l2-5 that "Under the hypotheses 1) that stomata are open, 2) that Rw is very large, and 3) that therefore stomatal exchange may reasonably be expected to represent by far the major pathway, then Xs may be approximated to X{z0' }, as estimated experimentally from Eq. (6) ". The assumption here is that the non-stomatal sink is switched off ("Rw is very large"), so we don't actually use any estimate of Rw for deriving Xs (the whole flux is assumed under these conditions to be of stomatal origin). We do not concur with the referee that "deriving a higher Rw parameterisation could lead to an underestimation of Xs", since the two parameters were derived entirely independently of each other.

3- On differences between 2006 and 2007: we agree that it would have been interesting to compare estimates of background Xs, but as we stated p 9644, l21-24: "...very few estimates of Gamma_s were obtained in 2006 as the exchange in the INT field was then dominated by deposition (Fig. 1) with therefore very few flux sign reversals...", so that this comparison was not possible.

4- We have replaced the phrase "...reached upwards of + 50 microg NH3 m-2 s-1" by the following: "...reached values above + 50 microg NH3 m-2 s-1"

Interactive comment on Biogeosciences Discuss., 6, 9627, 2009.