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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.

Anonymous Referee #1

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General comments

The paper (and the twin paper by Spirig et al. (1999)) represents results from a long-term measurement campaign of ammonia fluxes over a fertilised grassland, which were seldom carried out in Europe. The actual time series, conducted so far in Europe, only last for 6 weeks and are too short to make a reliable annual ammonia budget.

The paper is innovative given the application of a new gradient measurement system (involving AIRRMONIA equipment for gradient measurement) and the original treatment of fluxes (subdivision of fluxes into fertiliser-induced "disturbances" and background fluxes). I was also highly impressed by the high data coverage of the new

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gradient system. The paper is straightforward and well written. The figures are clear and well-organised.

Specific comments

1. Table with basic soil characteristics (page 9631, lines 0-10)

The authors should add a table with soil characteristics (C/N ratio, pH or CEC) of the intensive and extensive managed grassland to give the reader an impression of the emission potential of their soils. Results suggest similar background ammonia exchange for both sites. Could this be corroborated by soil analyses from the two sites? Are C/N ratio, pH, base saturation or texture similar despite the contrasting management? Higher C/N ratios or lower pH encountered at the EXT site could for example be conducive to higher N deposition in general.

2. Emission fluxes during background exchange (page 9641, lines 10-15)

Were there indications that cuticular desorptions were involved in the daytime emission episodes at the INT and EXT site? Could emission fluxes or sudden increases in χ(z0') be noticed when water films on the leaf surface evaporated, leading to volatilisation of previously deposited ammonia or decreased deposition in the morning hours? This could have been likely given the high Rw values encountered during the measurements

3. Parameterisation of the external leaf surface resistance (page 9643, lines 20-25)

Authors found an impact of the surface temperature on the leaf surface resistance. There is, however, substantial scatter in the parameterisation of Rw. Could the authors underpin how robust the empirical relationships (equation 14) are, by adding an overall R2 or standard errors from the derived coefficients?

The Rw values are derived from night-time Rc values. As the authors mentioned in the discussion, sound parameterisations with T and RH could have been hampered by other variables like SO2, rainfall, leaf surface chemistry, hysteresis, leaf litter and soil wetness mediating the sink strength for ammonia (page 9654, lines 13-24). Was there

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any indication that there was stomatal opening (and hence stomatal uptake) during certain night-time conditions, rendering the interpretation of the night-time Rc value even more intricate? Did the present dataset reveal any seasonal differences in the derived Rw values? Were there differences in Rw depending on the growth stage of the grassland?

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