

Interactive comment on “Aerodynamic gradient measurements of the NH₃-HNO₃-NH₄NO₃ triad using a wet chemical instrument: an analysis of precision requirements and flux errors” by V. Wolff et al.

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Despite the importance of NH₃, HNO₃, NO₃⁻ and NH₄⁺ for ecosystem eutrophication and acidification, relatively few measurements exist of dry deposition fluxes of these compounds. Because their fluxes can be non-conserved with height, due to chemical conversion, in many conditions it is necessary to measure fluxes of these compounds simultaneously. The GRAEGOR represents a state-of-the-art instrument to monitor gradients of all four compounds simultaneously, with online analysis, at relatively high (hourly) time-resolution. The paper of Wolff et al. presents a thorough

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analysis of the uncertainties involved in this measurement, which is important to consider when the results are interpreted. The paper is not only relevant, but also well written and laid out. I recommend the work for publication in ATD after the following minor points have been addressed:

1. The authors acknowledge that above rough vegetation like forest, gradient measurements are conducted within the surface roughness layer, where, in general, site-specific flux-gradient relationships need to be applied (end of Section 3.2). They then proceed by explaining that such analysis for the EGER site will be published elsewhere, and instead use the standard flux-gradient relationships to calculate approximate fluxes. This seems to be the wrong way round. If these site-specific relationships are being derived, they should be used in the revision of the manuscript and the other manuscript should be cited as 'in preparation'.
2. The problem with filtering fluxes for significance (e.g. page 2447, line 13) is that such tests tend to remove small fluxes. Extreme care has to be taken how the filtered dataset is used afterwards. For example, for the calculation of robust average fluxes, these periods smaller fluxes need to be intelligently gap filled in order not to bias the dataset. But even if the data are used to investigate processes, the removal of the smaller fluxes may in some instances bias the interpretation. In addition, if non-significant fluxes are averaged, the longer term average may become statistically significant. These aspects should be discussed in the revised manuscript.
3. The scope of Table 6 is unclear. It appears to contain both measurements of several compounds and estimates of single compounds. If the latter is to be included, the table is grossly incomplete. Many more continuous/semi-continuous measurements of NH₃ fluxes exist with and without error estimates.
4. In several previous studies, errors were derived in terms of statistical errors on the gradient derived from multi-point measurements. This applies in particular to gradient measurements with filter packs at 5 heights or more (e.g. Sutton et al., 1993a, b;

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Nemitz et al., 2004a). This is an alternative approach to considering the error on the flux, which clearly only works if measurements at more than two heights are available. This could be mentioned and discussed in the revised manuscript.

5. Why are the uncertainty ranges for NO₃- fluxes constant with time, while the others change (Fig. 12)?

6. How does Ra calculated by Eq. (6) compare with the alternative formulation of $Ra = u/u^*{}^2 - (\psi_H - \psi_M)/(ku^*)$? The difference is that Eq. (6) is based on an average relationship between u and u* (as expressed by an average z₀), while the other formulation uses the relationship between u and u* actually measured for each period.

7. The height/fetch rule of 1:100 (page 2437, line 18) is only a rule-of-thumb. The exact fetch requirement depends on atmospheric stability. I am sure the authors are aware of this. The 1:100 rule is only cited to develop the argument that above forest, it cannot easily be avoided that flux measurements are made in the surface roughness layer. However, the effect of stability should probably be mentioned in order not to mislead the reader.

8. How do the authors explain the relatively large deviations from the 1:1 line for the side-by-side intercomparison for NH₄⁺ and NH₃?

9. I agree that as u* becomes smaller, the relative error increases (e.g. Nemitz et al., 2009) as mentioned on page 2444, line 2. What are the implications for the error calculations presented here?

References in addition to those already contained in the manuscript:

Sutton, M. A., Fowler, D., Moncrieff, J.B.: The exchange of atmospheric ammonia with vegetated surfaces. I: Unfertilized vegetation, *Quart. J. Roy. Meteorol. Soc.* 119(513), 1023-1045, 1993a.

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Nemitz, E., K. J. Hargreaves, A. Neftel, B. Loubet, P. Cellier, J. R. Dorsey, M. Flynn, A. Hensen, T. Weidinger, R. Meszaros, L. Horvath, U. Dämmgen, C. Frühauf, F. J. Löpmeier, M. W. Gallagher, and M. A. Sutton: Intercomparison and assessment of turbulent and physiological exchange parameters of grassland, *Biogeosciences*, 6, 1445-1466, 2009

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