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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Gas-particle interactions above a Dutch heathland:III. Modelling the influence of the NH_3-HNO_3-NH_4NO_3 equilibrium on size-segregated particle fluxes" *by* E. Nemitz and M. A. Sutton

Anonymous Referee #1

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General comments

This paper deals with a very complex issue; the influence of gas to particle conversions and evaporation from aerosols on the exchange of gases and aerosols at the earthŠs surface. Unfortunately I haven't been able to go through all mathematics in a detailed way. But as far as I can tell it all looks sound. It seems to me that the authors have made an important contribution to the field. They have looked into many aspects of the subject and studied it from different viewpoints. In a number of papers published in the literature these issues have been addressed only partially. The authors have brought together many aspects in a comprehensive model and used a comprehensive set of

field data for testing. Even though still some, experimental information is lacking and as usual more noisy (for whatever reason) than one would hope for, they have come to reasonable comparisons with the model output. At the end they draw carefully some useful conclusions based upon the results of the model calculations as well as the experimental results. It is my opinion that the paper is very well written; everything is fine! Consequently I have only very little comments on this paper and would recommend it to be accepted.

Specific comments

Page 3 end of first paragraph. Flux reversal of NH_4 is more likely than that of NH_3? In the only example on this page (3rd paragraph) ammonia is emitted. Is it not so that the bulk NH4 deposition is hardly changed since the mass carrying larger particles are not affected by GTPC or evaporation?

Page 3 I am not a native speaker but is bi-directional the right word? I would think this means that the flux is in two directions at the same time. I know it has been used before of course.

Page 5 equation 3 appears strange in print.

Perhaps β in equation 4 needs to be clearer defined.

Page 6 relative humidity gradients may be small because of compensating effects. Still I could imagine that inside a canopy particles could grow because of a high humidity.

Page 9 title of paragraph R_{IH} Subscript should be non capital (also paragraph 2.3.2)

Page 21 Imagining the amount of (field and modelling) work that has gone in, I think it is fair to say that fig. 10 is a nice result!

Page 25 Would it be possible to estimate the equilibrium height from the time scales of mixing (some z/u*) and τ_c?

Page 25 If the results could improve if one assumes a higher equilibrium height (per-

ACPD

4, S547–S549, 2004

haps it is only a virtual height) is it still needed to conclude that R_c for HNO_3 is non-zero? Perhaps the evaporation of NH_4NO_3 from plants is a more logical explanation.

Page 26 I see some problems with the recommendations (even though it makes sense and I support the recommendation) If you go up higher with your equipment you meet all problems: Fetch, stationarity problems, constant flux layer assumptions etc. etc. Everything is worse there.

What if you would carry out an experiment in a condition where the direction of the surface flux of ammonia changes from emission to deposition? Many parameters may be unaffected but some may be and it would perhaps lead to a better test for the model.

Page 29 end of first paragraph: How large could the effect on the real deposition flux of N into the ecosystem be? Many people would be very interested in the answer.

General: many problems addressed in the paper are somehow artefacts because of the use of micro-meteorological methods. Could other methods provide a clue to independently testing the model?

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 1567, 2004.