

Interactive
Comment

Interactive comment on “Improved modelling of atmospheric ammonia over Denmark using the coupled modelling system DAMOS” by C. Geels et al.

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In General: The paper is clear and well written. The subject is not very new but still very relevant especially in the light of Natura2000. The main scope of the paper is to show that more detail is needed in assessing the N-deposition. Again that as such is not so new. I'm more curious in how well the system works. The validation is only again concentrations in air. Why not use other components as well, i.e. wet deposition, ammonium aerosol. Besides how well does the model calculate the other N-components? Main concern I have is about uncertainties. There is hardly any information on uncertainty in modelling and more specific on how well the depositions are modelled.

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Particularly in comparing the depositions to critical loads this plays a crucial role. So can the authors give an estimate on that? This should be indicated in Figure 9 as well. Since the parametrization of the dry deposition is quite uncertain I specifically would have liked to see a discussion on that and specifically how the authors think their results will change comparing the current parameterization to the one with bi-directional fluxes included? I have indicated the paper to be reconsidered after major revisions. In fact this is too strong but there is no rating in between. The main revision I want to see and review is on the uncertainties.

More Specific: p 1589, ln 23: footprint: Be more precise here: the deposition pattern?

P 1590, ln 25: Vd on what time scale?; as is indicated above the flux is bi-directional so episodes of negative Vd will occur. So the indicated range is not complete.

p 1591; ln 22-25: I suppose it is meant: the goal of this paper; the hypothesis as such is not so new and has been proven to be true.

P 1594; ln 3: area sources: how high are these crop emissions and how are these incorporated in the model calculations. That is: how do you parameterize deposition to the crops while meantime you have emission from the crops?

P 1596; ln 26; what is meant there? Diffuse sources on an resolution of 16.67x16.67 km grid?

P 1597; ln 14-18; what does this validation say? If the concentration is so well modelled, does this mean the background contribution (>20 km) is not important? This is not in line with the conclusions of the paper where it is stated that over 50% is from non-local sources.

P 1597; ln 26; this conversion is dependent on the SO₂-concentrations. Is the factor corrected for the difference between the SO₂ concentrations between 1989 and now.

P 1598; ln: just to get it right; OML is only used around monitoring sites or nature reserves and as a one way coupling? So from DEHM to OML and not back?

P 1600: In.23-26: what would be a possible explanation for the overestimation in spring and summer?

P1602: section 3.4: Runs are made with and without local emissions. How are non-linear processes treated in those runs (like wet deposition). (Or: how is the split made between local and non-local wet deposition?).

P1603: In 15-20: you conclude a bit contra intuitively that the large scale model DEHM overestimated ammonia concentration. Since long it is found that CTMs underestimate ammonia concentrations because of their large horizontal scale. I think you should add to your discussion that DEHM already overcomes this problem by calculating at a much higher resolution.

P1605: In 26: what are large computational costs? What will be the run time for evaluating 100 N2000 areas on a 400x400m grid on a domain of say 16x16 km?

P1606; In 1 and further: since dry deposition is a very important term in the mass balance of ammonia and so plays a very important role in the comparison to measurements, what do the authors think the uncertainty is in the dry deposition velocity and what effect it has on the modelled concentration and local deposition?

P1606: In 14; references are not in list.

P1606: In 25; why haven't the authors checked their calculations of ammonia aerosol and ammonia concentrations in precipitation to measurements? That also gives a very good insight whether the ammonia balance is well modelled.

P1607: In 3: not nicely formulated; I think models refer to DEHM and DAMOS and not the 4 models mentioned above?

P1607; second bullet: do the authors mean the same effect as is found for DEHM, i.e. emissions further away from a measurement site do have a too large effect at the measurement site because of the emissions spread over the grid? Be more clear.

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P1607: In 9, 10: which not witch; what is an up-concentration? What would be the reason for an overestimation of the emissions during nighttime?

P1607: fourth bullit: why should there be an underestimation? Where is this suggestion based on?

P1607: In 21: borders not boarders

P1608: In 1: do not does

P1608: In 1-3: this effect is in my opinion rather far-fetched. If this is not underpinned by some estimates it has no value.

P1608; In 15: formulation is a bit strange. In general: line 4-15: are elaborations of the bullits above and should be merged there. Is more clear.

P1608: In 22: how many sites?

P1609 and Figure 9; crucial in this assessment is how accurate are the modelled depositions? If firm conclusions are drawn about exceedences of Natura2000 areas it should be indicated what the uncertainties are? So it should be indicated in Figure 9 with bars what the uncertainty in deposition is as is done for critical loads. And than again draw conclusions.

Ln 15: I do not agree that the annual variation in deposition due to meteorology should be taken into account. It is doubtfull if this is ecologically relevant and the issue of the total uncertainty in the deposition calculation is far more important.

P1610: In 13: overestimates

Fifth bullit: is not very well elaborated in the paper. Where is this the case?

Sixth bullit: signal = deposition?

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