

Interactive comment on “Modeling atmospheric ammonia and ammonium using a backward-in-time stochastic Lagrangian air quality model (STILT-Chem v0.7)” by D. Wen et al.

Anonymous Referee #1

Received and published: 2 November 2012

GENERAL COMMENTS

It is useful to model the concentrations of NH_3 and NH_4^+ . The paper is in general well-documented, but does not pay enough attention to the description of some important aspects of the components it focuses on. This makes it difficult to judge whether the model is appropriate or not. At least some important information is missing.

SPECIFIC COMMENTS

The title of the paper is confusing. The trajectories are backward trajectories, but the chemistry and loss processes are forward. So leave “backward-in-time” out.

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p. 2755. Emission/dry deposition. Emission of NH_3 occurs mainly from (near) surface sources. Dry deposition of NH_3 occurs at the surface. In agricultural areas the NH_3 concentration near the surface is much larger than the average concentration of NH_3 over e.g. the lowest 50 m of the atmosphere. The authors should make clear what the limitations are of their model with regard to correctly modelling the NH_3 concentration and dry deposition in source areas. If their model is unable to take into account the vertical resolution in source areas, e.g. near receptor points, the model is not very well suited for $\text{NH}_3/\text{NH}_4^+$. It is in that case, however, maybe possible to come up with some correction factors.

p. 2761. It is referred to emission inventories. No information, however, is presented on the diurnal and seasonal variation in the emission rate, which is very important for NH_3 . I would suggest that this information is added. If no diurnal and especially seasonal variation is taken into account, then the article should be refused.

p. 2763. It is difficult to compare modelled and measured NH_3 concentrations when no information is available of the emissions/spatial variation in the emission density near the stations. So it would be very useful if the authors could provide information on that.

Figs. 6 and 7. It is difficult for me to understand what Figs. 6 and 7 actually represent and what we could learn from that. It would be nice if the authors could explain that more clearly or otherwise the figures could be left out. What could be nice to look at is the contribution of emission areas as a function of the distance to the measuring site, e.g. sources from 0–1 km contribute for x1 %, 1–10 km x2 % etc.

p. 2768. “However, contributions from dry deposition are all negative, with smaller absolute magnitudes”. This is not a relevant remark. It is always so that dry and wet deposition are sinks by definition. The only situation where this could be different is if the exchange with crops is modelled taking a compensation point into account and such a parameterisation is not chosen here. Chemical conversion is of course a loss for NH_3 and a gain for NH_4^+ . This is also very logical: this is put into the model and

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will therefore become visible in the results.

It is also stated that the production of NH_4^+ is almost equal to the loss in NH_3 . This is again an irrelevant remark. They should be exactly equal as otherwise the model does not have mass conservation.

I feel that the wording is also a bit strange: "negative contribution". I guess it is more normal to speak about loss.

So section 4.3.2 (and the figures belonging to that) should be rewritten

Interactive comment on Geosci. Model Dev. Discuss., 5, 2745, 2012.