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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spatial, temporal and vertical distribution of ammonia concentrations over Europe – comparing a static and dynamic approach with WRF-Chem" *by* M. Werner et al.

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Modelling the reactive nitrogen budget is a challenging endeavor. A good representation of the ammonia emission fluxes and their variability is key to any modelling effort. The current practice of parameterizing the ammonia emission variability in regional chemistry transport models is very basic and needs detailing. Any improvement to the knowledge of the spatial and temporal distribution of NH3 is relevant as there are large uncertainties in the emission and deposition budgets. The paper by Werner et al. describes an effort to apply the emission module developed by Skjoth et al. (2011) in WRF-CHEM. I agree with first reviewer and his or her motivation that this paper does

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little to advance our knowledge of the subject. Hence, I feel this paper is not publishable in its current form.

I have a main comment on the Base-case emission profile as this is unfortunately not the profile used within LOTOS-EUROS as stated in this paper. A separate comment was written on this issue.

The paper covers an improvement to the WRF-Chem by introducing a dynamical emission model to the WRF-chem model. The model results are then compared to a number of stations and a number of statistics are given. Figure 8 shows scatter plots of the modelled and observed NH3 concentrations at a number of sites. The scatterplots show overestimations by the model, whereas I source areas most model tend to underestimate. For most sites the Dynamic simulations show a decrease in the correlations and an increase in the RMSE for most seasons besides the winter. This contradicts the claim that an improvement is made, can you further clarify? The authors try to show that statistically the model improves, while small changes can be observed for the at Harwell. When we check Table 4. the combination of stations shows a reduction in the correlation for the autumn, at the same time a range of statistical parameters are given but not explained or commented upon in the manuscript. Overall the paper does not convince me that the DYNAMIC approach is an improvement to the WRF-chem model.

The ammonia budget is affected by many other parameters than the ammonia emissions. The loss of ammonia to particulate ammonium sulfate and nitrate can be large, typically 5-10% an hour, with higher values in areas with low ammonia levels. It would be helpful to see the evaluation of the aerosol components. In addition, the wet deposition fluxes can be evaluated to assess if this term is looking OK. The paper itself feels rushed with only the boundary layer height being discussed as a possibility for the modelled ammonia concentrations being out of phase with the measured observations.

The paper provides very little details on the implementation of the dynamic emission model. How was the allocation to agricultural subsectors done exactly? Were the

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emission totals per grid cell kept as a constant or allowed to be changed? The remark in the discussion that the emission distributions significantly changed suggest the latter. In other words, are the BASE and DYNAMIC emissions the same?

A few other general comments/questions:

1. The dynamic model in itself is not new, only the introduction of the model to WRF-Chem is. As it is presented now it seems no modifications or improvements were made. It seems that the lessons learned from the former 3 papers (Skjoth 2004,2011 and Werner et al 2015) are not addressed or applied. Is that correct?

2. The claim that there is only hourly concentration data available for Harwell is not true. A fast check at the EMEP/EBAS website shows 190 datasets including time series of hourly data. Furthermore hourly data from the Dutch LML network is freely available. The UK has a large ammonia network which could be used for investigating seasonality. In short, a much more thorough evaluation is possible.

3. A figure illustrating the difference in emissions between both models would be helpful.

4. The model has an increased number of layers with a thickness of only 20 meters, at the same it has a horizontal grid of 36x36km. As a reason behind this move the authors mention the importance of chemistry and the vertical distribution of ammonia in the boundary layer. Why was the vertical resolution increased and was a higher horizontal resolution not considered? I appreciate the large computational effort made in this study, but I feel that the use of a slightly simpler model targeting at least several years would have been a better choice to evaluate the dynamic emission model.

5. What are the major sources near the Harwell site? Is Harwell representative for the gridcel it is in? If the site is located in the left corner of an WRF-CHEM cell this would mean it is in the same cell as part of London, and thus all industry near it.

5. Most of the figures could use some titles indicating the seasons/data. Figures should

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be understandable without the text.

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