

# ***Interactive comment on “Understanding emissions of ammonia from buildings and application of fertilizers: an example from Poland” by M. Werner et al.***

## **Anonymous Referee #1**

Received and published: 15 March 2015

This paper provides an interesting case-study of the importance of time-variation for ammonia. The paper is generally well written, and has much valuable information, so that I think it will be a good contribution to Biogeosciences after attention to the points raised below and by the other referees.

I agree with the other referees that the biggest problem with the paper is the over-interpretation of rather few measurements. I will not repeat their points, but this aspect needs to be toned down. Other points follow.

Abstract (and related discussions)

My only real objection here concerns line 25, which claims that dynamical approaches

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are a 'viable objective' for all CTMs. I am not convinced that this is really true. What are data requirements and shortages? Do the authors expect data on fertilizer practices, irrigation, soil characteristics, and legislation and farming traditions to be available in the near (or foreseeable) future?

Other points on the abstract:

- could be shorter
- omit or define NWP

P2024,L8. The cited Riddick paper is for tropical seabird colonies, which is a bit exotic for a paper dealing with Poland. The paper by Simpson et al. (1999) suggested that in Europe the NH<sub>3</sub> emissions from 'natural' sources were almost negligible compared to agricultural.

P2024. First paragraph - explain which regions are being discussed by the cited studies.

P2025 and elsewhere. There is no such thing as the 'WRF-Chem model for Poland'. WRF-Chem was not built for Poland, and there is no unique model version; there may even be several groups running WRF-Chem for Poland. Please state whose implementation of WRF-Chem you are referring to, and give this a name.

P2026,L14 - 'default values were implemented...'. Who, where? (In this study, or in Skjoth?)

P2028,L3 refers to Sect. 2.1.1, but no such section exists.

P2077,L5 how and when is W as ventilation used and estimated?

P2028,L11. I found these scenarios and their explanation confusing. Usually one begins to explain the 1st scenario and then develop explanations for the following ones. Here the authors begin with the last. And as noted by referee #3, the names change at different points in the paper. I miss also an explanation of the motives and thinking

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behind NOFERT.

Please itemize better and explain each scenario, and then stick to the chosen naming convention throughout.

As a minor point, it seemed odd to put scenario 3 (FLAT) in the middle of the non-WRF scenarios.

P2031,L18. What are 'specific' geographical areas.

P2032,L12. Why 250m and 750m?

P2038,L18. I assume you mean dissociation, not evaporation? You should give a reference for that process also (eg Fowler et al, 2009 for a recent review).

P2056, Fig. 3. The legend gives function names, but the axis says emissions. These are different things. Also, the yellow Fct10 line is very hard to see in my copy. Different line styles, bolder, and maybe some markers would help.

P2057, Fig. 4. Why compare one day's 3 hour period of emission with a monthly mean from FRAME? Compare like with like.

P2060, Fig 7. Which scenario is this - be explicit in the captions.

P2061, Fig 8. It would be easier to see the trajectories with bolder lines. Also, are these 250m or 750m trajectories.

Refs:

Fowler et al, Atmospheric composition change: Ecosystems–Atmosphere interactions, Atmos. Env., 43, 5193-, 2009

Simpson, D. et al, Inventorying emissions from Nature in Europe, J.Geophys.Res., 104,D7,8113-, 1999

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