Biogeosciences Discuss., 12, C377–C380, 2015
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Biogeosciences Discussions
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Interactive comment on "Understanding emissions of ammonia from buildings and application of fertilizers: an example from Poland" *by* M. Werner et al.

Anonymous Referee #3

Received and published: 1 March 2015

Modelling the reactive nitrogen budget is a challenging endeavor. A good representation of the 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. The paper by Werner et al. describes an effort to adapt the emission module developed by Skjoth et al. (2011) to Polish conditions. I feel the paper is acceptable for publication in GMD after a number of concerns is addressed.

My main concern is the strong conclusions made in the paper which appear to be based on two measurement stations. The paper concludes that the performance is much better based on two locations, where the other 3 locations do no show improve-

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ment. Based on the time series I see only improvement in Rzecin. Hence, it seems that the evidence for an improved modelling of the ammonia budget over Poland are indicative.

In addition, I think the motivation and discussion on the use of the simplified chemistry transport model needs some more attention as the validation shows that the stations are not really located in source areas. Are the assumptions of the simplified chemistry warranted? Frame was ran on a monthly time resolution. What does this mean for the ammonia emissions? Is part of the connection between meteorological dependent ammonia emissions and meteorological dependent fate in the atmosphere lost due to this set-up?

The definition of the scenarios runs is not consistent throughout the paper. And sometimes 3 or 4 scenarios are mentioned. - Default (Skjoth et al. 2011) - No application emissions - Is the existing emission method in WRF-CHEM a constant emission over time as it is termed FLAT in section 2.2? If so, this is not common practice in European chemistry transport models. I would call it “constant emissions” - Polish regulation and practice: Often regulation is mentioned but practice could be a better word for this simulation. In our modelling system we found that the change in diurnal cycle of the emissions can induce large changes in modelled annual mean ammonia levels (using the same emission total). You have changed both the day to day variability as the diurnal cycle. Do you have an idea how much this effects your results?

Specific comments:

P2020,L8-9 It is stated the model is robust with respect to stable and storage emissions. What do you mean?

P2026, L13: Default values for the contribution of the total ammonia emission to each activity i.

P2026, L23: In equation 1 and 2 I miss the consequent use of the index for the

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hour/time of the year. The explanation of the equations in the lines below is not really understandable without the original publication. Please provide the calculation of Epot as well.

P2027, L3: refer to section 2.2

P2028, L6: I assume from the text that all fields in a province get the same amounts of fertilizer and manure. Or is the manure application performed per commune? The provinces are rather large. Do you think this affects the results?

P2029, L10 Are the Poland default settings in Table 2 consistent with the Polish emission inventory?

P2033 L17: Figure 3 shows large emissions for FKt(15) although this emission source accounts only 1% of the annual emission total. Please explain. Is Jarczew the best location to show this plot for?

P2034, L6: In the description of Figure 5 it is mentioned that there is a large variability between day and night. This variability is only 25 %. I would remove the word large and insert the quantification. 25% is rather small compared to traditional estimates in variability as commonly used in other modelling studies.

P2034, L20 is much higher than

P2035. The discussion in 3.3 and the figure highlights the need for hour-by-hour calculations.

P2037. L17-29. The conclusions here are based on two sites that compare favorably, whereas the other sites seem to say something different. The evaluation at more remote locations seem to show that in Poland the atmospheric transport and transformation are important processes. The conclusion that it is only emission driven seems not warranted. In my opinion it is not possible to conclude for Poland that this study obtained as good results as for Denmark with DEHM and DAMOS. The evaluation basis is completely different to support this statement. The study is a step forward in ammonia

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modelling over Poland, but maybe these statements are a bit too enthousiastic.

P.2038. The results for Diabla Gora show clearly that the our understanding or modelling approach is not sufficient to explain the measured concentrations. Assuming that in Poland temperatures are well below zero and snow cover and frozen open water are often present I wonder if the presented explanation is more than speculation. Are these conditions represented well in the model system? Why are so many references being made to WRF-CHEM? There are more models available to study this issues on higher temporal resolutions that are further concerning ammonia modelling and easier to handle.

A multi-year simulation which is easily performed with FRAME could have made a stronger case.

Interactive comment on Biogeosciences Discuss., 12, 2021, 2015.