Atmospheric Chemistry and Physics Discussions

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Comment on Li et al.

Anonymous Referee #1

Referee comment on "Improved gridded ammonia emission inventory in China" by Baojie Li et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-439-RC1, 2021

General comments:
This study presents an optimized ammonia emission inventory with a focus on fertilizer application relevant sources over mainland China in 2016. The approach adopted in this study considers the time variability of fertilizer application and thus enables this inventory accurate capture the ammonia emissions over time. To illustrate the capability of this emission inventory against existing emission inventory (MEIC developed by Tsinghua University), the authors evaluate the emission inventory by comparing modelled NH3 concentrations by WRF-Chem with ammonia inventory in this work and MEIC. Further, the model performance is validated with NH3 observations from AMoN-China and IASI NH3 column. This paper is well-written and presents results that would be interesting to the air quality modeling community. However, I have several concerns that the authors should consider when revising the manuscript, as listed below. I recommend this work to be published after the following comments are adequately addressed.

Major comments:

1) While the authors illustrated that large uncertainties remain in the NH3 emissions from agriculture sector thus motivates the improvement in NH3 emission inventory, I think vehicular NH3 emissions co-emitted with nitrogen oxides (NOx) are also important sources for NH3. This additional NH3 source has been always underestimated in emission inventory (such as MEIC). Although this work is focused on agriculture ammonia emissions, I would suggest the authors briefly discuss this in the Introduction because this work is entitled “ammonia emission inventory in China”.

2) In Sec. 2.4, the uncertainty assessment of NH3 emission inventory established in this work based on the Monte Carlo method is missing. Please provide the uncertainties for each sub-sector if possible.

3) The authors employed the WRF-Chem model for performing numerical simulations over

eastern China and compared with model outputs driven by MEIC and IASI satellites. However, I could not find the detailed configurations of WRF-Chem simulations (initial and boundary conditions for WRF-Chem, physical parameterizations, chemical mechanisms, etc). I suggest the authors add a section in the supplementary providing the configurations of WRF-Chem.
4) In the WRF-Chem simulations with NH3 emission inventory developed in this work and MEIC, I wonder whether other air pollutants emissions (such as SO2, NOx, VOCs) are both based upon MEIC. If it is, please clarify it.

5) For readers not familiar with the China geography and the location of each province, providing a map with province names marked would be valuable.
6) In Sec. 3.4, it seems the WRF-Chem model with optimized NH3 emission inventory yields better performance in July while large bias still existed in January as compared with the MEIC driven WRF-Chem model. Could you be more specific on the reasons for the discrepancy between the simulated NH3 concentrations and ambient measurements?

7) I think Figure 6(a) is misleading with the erroneous higher concentrations of NH3 in January than July due to the different scales in colormap. Please use the same color scale for both January and July. Otherwise, it is misleading to the readers.

8) Conclusion: This section would be stronger if a discussion on future work that the community should consider toward improving the representation of ammonia emissions in chemical transport models (For example, investigating the impacts of optimized ammonia emission inventory on simulating PM2.5 concentrations and aerosol pH changes).

Specific comments:
1) Line 17: “differences”-->“variability”. “observed” -->“identified”.
1) Line 84: “huge”-->“considerable”.
2) Line 215: “30 samples” should be “30 sampling stations”.
3) Line 223: “represented”-->“contributed”.
4) Line 223: The 13.1 Tg is derived from Kong et al., (2019). Thus, there is a lack of citation for Kong et al., (2019).
5) Table 2: I suggest the authors include the ammonia emissions quantified in this work in Table 2 as well.
6) Line 277: Please further clarify the reason why located in north China is relevant to elevated ammonia emissions.
7) Line 313: Please rephrase this sentence for read.
8) Line 324: Delete “it”.
9) Line 349: Delete “Thus”.
10) Line 359: The spatial resolution for MEIC is 0.25°×0.25°.
11) Figure S2: Small typo for the caption of subplot for Others.
12) Some figures are hard to read due to the small font size. Please consider improving them. 13) The manuscript is hard to follow in several places but that could be addressed with thorough language editing.