



EGUsphere, referee comment RC3
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Comment on egusphere-2022-873

Anonymous Referee #3

Referee comment on "Global nitrogen and sulfur deposition mapping using a measurement–model fusion approach" by Hannah J. Rubin et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-873-RC3>, 2022

General comment

The manuscript of egusphere-2022-873 presented the approach of MMF for global nitrogen and sulfur deposition. I will recognize this is important research to constrain depositions through fusion with measurements. However, my critical concern is how can we reinforce the usefulness of MMF. It will be a difficult and cumbersome task but independent evaluation by other measurements seems to be needed. Please address the following major concerns and specific comments.

Major concerns

- Independent evaluation: We find many statements of MMM status by HTAP2 within this manuscript; however, the independent evaluation supported by other measurements can increase the persuasion of this MMF result. I agree that MMF will be better in theory but we do not lead the proof that "MMF does give better deposition estimates by incorporating in-situ measurements" (P14, L262-263) without independent validations.
- The largest change over the ocean: I can follow that one of the reasons for coarse grid resolution will lead to the largest changes over oceans as listed in Table 1. However, because this was not helped by the observational fact (e.g., ship-borne measurements), how can we interpret this MMF result? Is it possible to only focus on the grid where the observation was available within the 1 by 1 grid in Table 1?

Specific comments

- P2, L18-19 (Abstract): Why sulfur trends were not stated? Moreover, according to my major concerns, please rewrite this abstract. It should be clarified the validation of this MMF result.
- P2, L45: It is ambiguous what "it" indicates. Is it ambient concentration or dry deposition?
- P3, L55: No need for the repetition of EANET.
- P4, L78-79: I noticed Tan et al.'s paper is updated recently (<http://dx.doi.org/10.1016/j.scitotenv.2022.158007>). What are the differences between this update and this study?
- P4, L86-88: This sentence is the result and is not appropriate to be stated in this introduction section.
- P4, L89: How about preparing table summarization for these available datasets? It will be kind to wide readers.
- P6, L137: It is one of an approach to use wet deposition itself, but their elements (concentration in precipitation and precipitation amount) could be the target of MMF. I can see some relevant discussion in Section 5, but for example, the project of MICS-Asia used the fusion for monthly-accumulated precipitation (<https://doi.org/10.5194/acp-21-8709-2021>). It will be better for readers why wet deposition is targeted as MMF in this study.
- P8, L183 (Table 1): It is kind to provide the region map for this analysis as a supplemental figure.
- P9, L189 (Figure 2): How about presenting the difference between MMF and MMM to clarify the difference driven by data fusion in this study? This result will clarify the impact of MMF compared to MMM, and can help to understand the result listed in Table 1.
- P10, L202 (Figure 3): But MMF used EMEP dataset itself, so this kind of comparison seems to be meaningless.
- P10, L202-203: I do not follow this sentence for East Asia. From this figure, MMF still underestimated the observational values.
- P12, L226-229: Within this context, TDep is regarded as truth?
- P12, L230: Why NH₄ is only presented? In addition, because MMF uses NADP dataset itself, what is the meaning of this kind of evaluation?

Technical corrections

- P7, L169: In this figure 1, "concentration in precipitation" multiplied by "precipitation" should be "wet deposition"? Please confirm this illustration.