

Geosci. Model Dev. Discuss., referee comment RC1  
<https://doi.org/10.5194/gmd-2021-374-RC1>, 2022  
© Author(s) 2022. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Comment on gmd-2021-374

Anonymous Referee #1

---

Referee comment on "An aerosol vertical data assimilation system (NAQPMS-PDAF v1.0): development and application" by Haibo Wang et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-374-RC1>, 2022

---

This paper describes a chemistry-transport model that is implemented with a data assimilation compartment using the Parallel Data Assimilation Framework. First of all, I would admit that I don't expertise in the atmosphere chemistry area, all my comments are from the data assimilation with PDAF. As far as I know, the online data assimilation approach with PDAF is first thoroughly described in Nerger et al., (2019) GMD paper, where the structure, the algorithm, the implementation are shown based on a climate model AWI-CM. Actually, such implementation has been widely used also for other ocean models such as FESOM, MITgcm etc. Another group like C-Coupler (Liu et al., 2020 GMD) provides similar data assimilation functions as well.

I found this research is comprehensive with both technical and experimental perspectives. The results are clearly present and well organized. I would recommend acceptance after minor revisions.

1. As for the paper structure, I would suggest the authors rephrase 2.3.1 and 2.3.2, i.e., the technical parts. Most of these implementation details are already well described in Nerger et al 2019. I didn't see too many differences compared to Nerger's. Currently, it's rather a repetition. Please cite this paper directly and show your differences to condense the context.

For 2.3.2, again, not necessarily a repetition of these algorithm details, which are well-known in amount of studies. The authors should concentrate on things that are distinguishable from others' work. For

example, localization radius,  
whether it is sensitive to your configuration (I see the authors cite two other researches, but actually these are different stories if the configuration changes based on my practices), are you using the same localization radius for different observations; forgetting factor, which value is set, why is that; things like that.

2. Please use a larger fontsize for Figures 10 and 11. The caption tells "the increments in the RMSEs of the surface PM2.5 forecast (g, h, i) and the increments in the RMSEs of the surface PM2.5 forecast (j, k, i)". Something goes wrong there?

3. Taking Figures 14 and 15, the authors found that the system seems not well constrained by DA in high level. Could the authors add some discussions about the physical reason or other aspects behind this problem?

In general, I think the paper is well written with adequate evidence for their results.