Geoscientific Model Development Discussions

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2021-10

Anonymous Referee #1

Referee comment on "Position correction in dust storm forecasting using LOTOS-EUROS v2.1: grid-distorted data assimilation v1.0" by Jianbing Jin et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-10-RC1, 2021

The authors present the development of a novel data assimilation method, grid distorted data assimilation, to correct the position of dust plumes. They apply this technique to a case study of a dust storm event in East Asia with or without the additional correction of dust load intensity. By validating their simulation against reference observations, they show the benefit of correcting both position and intensity. The paper is clearly written and well structured. There are however some aspects that should be clarified. I have some questions for the authors and some comments that could help to improve their manuscript.

1) In 5.1 when the authors describe the emission inversion, they state "The optimized emission fields could then be used to drive simulations that have a better forecast skill than simulations with the original emissions.". Hence, first they estimate the emission through data assimilation and then they use the corrected emissions to re-run the simulation? This is not what the Diagram of Fig 6 shows. Could you please clarify the use of the estimated emissions. Also, how are the emission estimated exactly? Are you taking into account that current dust concentrations are the results of emissions activated in previous times? Do you estimate emission over a sufficiently long assimilation window?

2) The authors are omitting a discussion on the assimilation of vertical profiles of extinction or backscatter coefficients. By adjusting the vertical structure, their assimilation has the potential to correct the plume position.

3) They should acknowledge that the ensemble they have used might not characterize well the uncertainty of their model. They state in the conclusions and, with similar wording also in other parts of the paper, that "For the dust storm studied here it was however shown that the spread in meteorological conditions is not sufficient to explain the position error in the simulations". They are excluding the fact that the uncertainty in the meteorology might be not well represented by that ensemble which, for example, is based on an original different horizontal (7 km) and finer vertical resolution compared their

simulation (0.25 degrees and only 8 vertical layers).

4) Related to the point above, they stated in the conclusions that "Traditional assimilation approached require definition of a background error covariance that should account for the observation/simulation positional discrepancy". They should emphasize also in the rest of the paper that they are offering an alternative solution to what can be solved with what they call a "traditional assimilation approach", since the message they convey throughout the paper is that the position of the dust plume cannot be corrected by the current used assimilation algorithms. I agree that little has been done in this respect, but it would be worth investigating whether more work on the characterization of model uncertainty (either through the definition of a covariance matrix or the design of an ensemble) could account for the position discrepancy between observations and simulations.

5) I miss more explanation on the modeling scheme used: emission scheme, transport, how the ECMWF forecast drives their simulation: is it a nudging? How do they cope with the different resolution and number of vertical levels? Some of these details could point to the cause of the mismatch with the observations.

6) In 3.3. can you explain why the maximum and not the standard deviation of the ensemble has been used to estimate the spread in simulated dust concentrations? Could this affect your results?

7) In 5.1 they should add an analysis of the additional computational burden when running the hybrid assimilation system. Is it feasible for an operational forecast? Or for which application?

8) Could you please add more information about the observation uncertainty used?

9) In 5.2 are you using assimilated (not independent) observations to calculate the RMSE metric? Could you please clarify this point?