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Comment on gmd-2022-207

P. Armand (Referee)

Referee comment on "Implementation and application of ensemble optimal interpolation on an operational chemistry weather model for improving PM_{2.5} and visibility predictions" by Siting Li et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-207-RC1>, 2022

The article by Siting et al. is about the implementation of an Ensemble Optimal Interpolation (EnOI) method in the numerical chemistry & weather prediction system GRAPES_Meso5.1 / CUACE and the application of the method to try to improve PM_{2.5} and visibility forecasts of pollution episodes in Eastern China. The authors strive to calibrate the parameters of the EnOI, namely the "localization length scale", which is the spatial range of the assimilation, the ensemble size, and the time at which assimilation should be carried out. They also investigate the impact of assimilating PM_{2.5} observations on the simulated PM_{2.5} concentration field. The mean error (ME) and the root mean square error (RMSE) on the initial PM_{2.5} concentration field is reduced when assimilating the data. According to the authors, the forecasts of the PM_{2.5} concentration and visibility fields are also improved throughout the lead time window of 24 hours, especially when the assimilation time is 1200 UTC because the discrepancy between simulation and observation is larger compared to 0000 UTC. Again according to the authors, the visibility forecasts by assimilating PM_{2.5} are further improved for light pollution episodes in comparison with heavy pollution episodes, which are more affected by humidity. Thus, for extreme low visibility during severe haze pollution, the authors recommend to assimilate both PM_{2.5} and humidity observations.

The work carried out by Siting et al. essentially consists of optimal interpolation where the model error covariance matrix is evaluated by an ensemble approach with the members of the ensemble being previous timeframes of the PM_{2.5} concentration field. Beyond the fact that the PM_{2.5} field which assimilates data at a given time and at the close following times actually presents better statistics compared to the observations, the results obtained do not seem very convincing to me. However, this work constitutes an approach which deserves to be studied and is therefore worthy of publication. The article could be improved by taking into account the following remarks.

L55 to 61 – Instead of listing a very large number of references, authors should briefly indicate what they contain.

L95 – Develop the acronym "EnOI" at least in the title of the section.

L102 – I don't understand the point of the double notation "x" and "psi". If there is a good reason, the authors should give it, otherwise the notations should be simplified.

L110 - In fact, the ensemble is used to estimate the error covariance matrix of the model. The authors could say it simply and directly.

L112 – The relationship between "psi_a", "psi_f" and "psi_i" should be given.

L113 - What the acronym "AF" corresponds to is not indicated (even if we understand well). The many acronyms used throughout the text should be made explicit and, in my opinion, fewer abbreviations should be used.

L114 – As the scalar "alpha" is used to weight the model and the observations, one would expect to see "1 – alpha" in formula (6). Are the authors sure about this formula?

L118 – Although the use of a length scale or spatial range in data assimilation is understood, this concept is poorly introduced. The expression "to avoid all observations..." is not clear at all and needs to be rephrased.

L118 – What does the "localization scheme" look like? The authors should give the formula of it.

L125 – We understand later in the reading of the article how the ensemble of PM2.5 concentration fields is constructed. It would be better if the authors explained it in this section of the article.

L129 – The sentence "Compared with the traditional EnOI..." can only be understood after reading the rest of the article. This sentence should be explained at this point in the article.

L134 – The horizontal and vertical dimensions and resolutions of the simulation domains used by the GRAPES_Meso5.1 and CUACE models should be indicated.

L166 – What do the authors call a "warm" restart? Is that really the right term?

L183 – Finally it is said that "the N hourly model forecasts before the assimilation moment were used as the ensemble samples to approximate" the model error covariance matrix. It could have been mentioned before.

L207 – If we look at figure 4, it seems very difficult to me to see what is the optimal "localization length scale" especially since the metrics on the correlation (CORR) and the errors (RMSE, MB and ME) are close.

L248 – This is the same problem as for my previous remark. The number of members in the ensemble seems to me chosen in a practical, if not "ad hoc", way. It should be studied whether other pollution episodes lead to the same choice of parameters "ensemble size" and "localization length scale".

L265 – Thanks to the authors for expanding the acronyms to facilitate reading.

L271 – What is a "sheet-like" concentration distribution? Is it really the correct term?

L287 – In Figure 8, there are very significant differences between the forecasts both with and without data assimilation and the observations, in particular on December 19 and December 23. Not only the amplitude, but mainly the dynamics of the concentrations are very different. Do the authors have an explanation on this point?

L391 – The final part of the sentence "... and relatively consistent in (20)" seems to be lacking. Please, correct.

L305 – Figure 9 does not seem to me to show convincingly that the assimilation at 1200 UTC (DA12) is better than the assimilation at 0000 UTC (DA00). Have the authors looked at, from a more fundamental point of view, why this should be the case?

L311 – Is it a general property that assimilation at 1200 UTC would be better than assimilation at 0000 UTC? The authors should be more careful about their assertion.

L323 – There is no linear relationship between visibility and PM2.5 concentration. Thus, it is not surprising that the result of assimilation improves the result on visibility for light

pollution episodes, whereas this improvement does not exist or is insignificant for heavy pollution episodes.

L334 – Also in Figure 11 (as reported for Figure 8), there are large discrepancies between the PM_{2.5} concentration predictions with or without data assimilation and the observations. Would the authors have an explanation on not only the amplitudes, but above all the dynamics which are very different?