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Atmospheric Chemistry and Physics Discussions

Interactive comment on "PM_{10} data assimilation over Europe with the optimal interpolation method" *by* M. Tombette et al.

M. Tombette et al.

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We thank the reviewer for her/his thoughtful remarks. We took into account most of her/his remarks, including on χ^2 diagnosis which required to run several experiments for calibration and to run again all experiment with the optimized error variances. Hereafter, the comments of the referee are quoted in italic.

General Remarks

1 *January 2001 is taken for the statistical part of validation. The authors should recognize that biogenic organic aerosols are missing for larger parts of Europe and France, rendering some discussion in the paper concerning aerosol splits questionable. More problematic is the fact that the temporal and horizontal impacts for different DA parameter configurations described in section 6 includes only a single validation forecast*

starting from January 6th , 2001, a Saturday, with the preceding five days for spin up with hourly data assimilation. This is virtually 1 winter case with a specific meteorological situation and emission conditions for a weekend.

Referee #2 is right. The discussion about the temporal impact of DA was reported for one case only (January, the 6th), that could be specific, and the conclusion did not appear as verified for the other days (although we actually made the verifications, but without reporting them). It is indeed needed to verify that the impact of DA actually decreases the first day after assimilation in any day of January. So we did it for the whole period of the operational forecast. The averaged hourly evolutions of the statistics are showed in Figures 3, 4 and 5 of the revised paper. The following explanations have been added in Sect. 5 : "Figures 3, 4 and 5 show the daily evolution (averaged over the period 2001-01-04 to 2001-01-30) of the RMSE, the correlation and the mean concentrations respectively for the model without assimilation and for the one-day forecast. These figures underline the tendency of the assimilation procedure to be almost ineffective after 24 hours of forecast. Actually, after 12:00 UTC, the differences in RMSE and in mean concentration are lower than $1 \mu g\ m^{-3}$, and the difference in correlation is about 2%."

The authors also agree with the fact that the same study should be carried out in summer. Anyway, the number of cases to explore is large and the subject of this paper is to start discussing about data assimilation for aerosols. A sensitivity to meteorological situations could be investigated separately and could constitute an independent study in the continuation of this work.

2. *Moreover, the authors do not include any a posteriori validation of the DA procedure in terms of the now state of the art Observation-minus-Forecast (O-F) and Observation-minus-Analysis (O-A) and chi-square validation as for example described in Talagrand (Proceedings of the Workshop on diagnosis of data assimilation systems, ECMWF, 1998), to assure at least a rough consistency between forecast and observation error covariance matrices.*

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We agree that this a posteriori evaluation may be quite useful. We therefore introduced the χ^2 diagnosis and applied it. We optimized the observation error variance with this diagnosis (applied to several assimilation experiments). We finally found a reasonable error variance, and we run again all experiments with this new variance. All details are provided in the revised manuscript.

Specific Remarks

1. *Introduction, line 106 : It is claimed that aerosol CTMs do not reproduce observed highest PM10 peaks due to missing processes of exceptional events (Saharan dust, . . .). Then, in section 3, line 231, this statement is generalized to usual conditions, with the exception of nitrate winter conditions. Literature references are indispensable for these claims. Please provide.*

This sentence has been modified and a reference has been added: "However, the numerical models sometimes miss some important events because of the lack of the description for some emission sources (for example wildfires, Hodzic et al., 2007)."

2. *Section 4, line 304: Table 1 does not contain information, which is suitable for presentation in a table. Does "model" mean background field for assimilation? It is suggested to remove Table 1.*

Table 1 has been removed.

3. *Subsection3.2: To what extend can EMEP and AirBase data be used for validation of BDQA data? Do EMEP and AirBase data repositories not include BDQA data? Or is care taken, that there is no coincidence for validation?*

As referee #2 points out, the databases are not exactly independent. The following sentence has been added : "Also, AirBase compiles several European databasis, including BDQA."

The Airbase and EMEP databases are not taken exactly for a validation of BDQA data. We do not claim to verify the measurements in this paper. But, we use different

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databases to evaluate the influence of DA on model performance, just like in operational conditions. The Airbase database allows us to evaluate the impact spatially (country by country). The EMEP database only includes background stations which should be especially relevant for our continental simulations. Then, as the evaluation is made in the same conditions as for a classical evaluation, no care has been taken to the coincidence between the databases.

4. *Section 4, line 307: On which information has the 2 grid cell selection of the scale parameter Lh been made, preferred to be used for the full month? Given previous remarks that some stations have been dropped from the assimilation procedure due to their poor representativity, a uniform 2 grid cell scale parameter (decorrelation lenghts) appears to be questionable.*

L_h is related to the background errors and it is not related to the station representativeness. Given stations were discarded because of their poor representativeness (by the way, much lower than one grid cell): their errors should show very high variance.

L_h is usually assumed to be about the width of one grid cell or of a few grid cells. This is at least common for a continental pollutant like ozone (e.g., Wu et al. 2008). This is also what gives good results in other practical experiments we did not report here.

However, we do not know which value should be taken. In the revised version, we chose L_h equals to 1 mesh cell. Note that the results are not highly sensitive to this parameter–which probably explain the lack of knowledge about it.

5. *Same paragraph: What does the statement mean: "The error variance for observations is lower than the instrumental uncertainty."? In terms of data assimilation relevance, the observational error variance and the error variance for representativity is required. Section 4, line 315: As mentioned above, care should be taken to keep error covariances consistent. Formally, there is no freedom to "assume" observations to be "highly accurate" to assess the potential benefits of data assimilation. On the contrary, OI (as variational and Kalman filter approaches) is based on statistical assumptions of*

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error characteristics to be followed as much as possible. In the specific case here, if observation errors are larger than assumed in the DA set-up, later comparisons with observations will likely to be outside the to tightly selected error margins and hence engender worse skill scores. Unexplained impacts like phenomena described around line 350 may be a consequence.

We addressed this issue much more carefully with a full optimization based on the χ^2 diagnosis.

6. *Section 4, line 323: Figure 2 lacks a scale. There appears to be no more information than in Table 2. It is suggested to remove Figure 2.*

The scale has been added to Figure 2. Figure 2 still appears in the revised manuscript, as it is more a visual support that could be useful for readers.

7. *Section 4, lines around 385: Does this mean that EMEP stations are not appropriate for validation? Is a 0.5 degree mesh size grid not more appropriate for the EMEP site deployment policy?*

Referee #2 is perfectly right: the EMEP database is appropriate to evaluate the models at that scale. In our paper, we do not evaluate the model but the assimilation procedure. The assimilated observations are not all of the same nature (there are some urban stations), which might lead the assimilation to drive the model state further from the background concentrations (given by EMEP). Formally, this would mean that the covariance errors are not large enough for some stations, or that the system is not enough observed.

8. *Section 4 last paragraph, around line 333: Which lessons can be taken from the given description? Are today's aerosol modules oversophisticated for data assimilation, if only "lump" information in terms of PM10 is given?*

The questions of referee #2 is totally relevant. We now discuss about that in the paper, in Sect. 4, by adding the following text : "Despite this remark, these results high-

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light the need for more chemical measurements in the DA method presented here. The partitioning in different species could then be corrected by assimilation, while it is constant here. Moreover, the aerosol chemical composition is the model variable. At the moment, without chemical data, a more deeper knowledge of the uncertainties on modeled concentrations for each aerosol component would certainly improve the system. Actually, the repartition after DA could be changed according to tendencies in uncertainties."

9. *Section 5, 1st and 2nd paragraph: The description is confusing. First sentence: Is Near Real Time data of PM10 not available to the authors? What does mean "assimilation during the first 3 days, then model forecasts to the next two days". Does it mean hour by hour data assimilation during the first 3 days, and the free forecasts after that? Please clarify.*

The principle is the one described by referee #2. We changed the description by : "the BDQA data is assimilated every hour during the first three days, after which the model runs freely and produces forecasts for the next two days."

10. *Tables 3, 4, and 6 should also include a column with biases, in addition to other statistical quantities.*

As Table 3, 4 and 6 include the mean concentrations for the observations and the simulation, the bias would be redundant.

11. *As a rule, figures should include SI units of presented quantities. Fig 6: For completeness SI units and time should be included in the caption.*

The SI units have been added in all figures and in the caption of Figure 6.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 9607, 2008.