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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evaluation of a multi-model, multi-constituent assimilation framework for tropospheric chemical reanalysis" *by* Kazuyuki Miyazaki et al.

Anonymous Referee #2

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Overview:

The authors evaluate and synthesize the results of four global chemical tropospheric reanalyses for the year 2007. All reanalyses were produced with the same EnKF DA system, that includes the optimisation of emissions and assimilates satellite retrieval for multiple-species. The main difference between the reanalyses is that a different Chemistry Transport Model, each having its own resolution and emissions input, was used in the EnKF DA system for the production the reanalysis data set. The authors demonstrate that all systems perform well by showing that the four reanalyses of atmospheric concentrations are considerably closer to independent and assimilated observations

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than corresponding control runs without assimilation. At the same time the authors find that posterior CO and NOx emissions (i.e. analysed) vary considerably between the four reanalyses because a different relation between CO and NOx emissions changes and CO, NO2 and O3 concentration changes is simulated by the different CTMs.

From the four analyses the authors calculate a "integrated" analysis as weighted average of the four analysis. The weights are the analysis uncertainties, expressed as standard deviation, derived from the EnKF DA system. This "integrated analysis" is used to calculate regression estimates of the response between concentration changes and emissions changes.

The evaluation of the re-analyses with observations is carried out for the 4 reanalysis and a "multi-model" analysis, which is an unweighted average of the four analysis but not the "integrated" analysis.

General comments:

The paper provides a consistent inter-comparison of state-of-the-art EnK chemical DA systems. Exploring the importance of the modelling approach, the authors show (i) that the analyses are an improved (compared to the models) representation of the atmospheric concentration for the assimilated variables and (ii) the limitation of current EnK approaches to come to consistent, i.e. model independent, posterior estimate of the emissions. The later point is of particular interest to the community and needs to be more emphasised.

Presenting the calculation of the weighted mean of the four analyses ("integrated reanalysis") as "Multi-mOdel Multi-cOnstituent Chemical data assimilation (MOMO-Chem) framework" sounds like an inflation of terms. But more importantly, it is misleading because the term framework implies to me that the four EnK DA system runs were connected in any way. After reading the paper carefully, I came to the conclusion that this seems not the case. (I hope it did not miss anything). I believe the averaging procedure could be carried for any set of KF DA analysis even without the need to

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use the same EnKF software. I strongly recommend changing the title and modify the abstract accordingly.

Combining four analysis using their standard deviation estimate as weights, is of course an advantage of the analyses compared to model runs for which this uncertainty information is not available. On the other hand, the analysis error standard deviation is a derived quantity (from P and R) and not estimated using independent observations. Therefore, it would be beneficial to show that the weighted mean approach leads to better results than an unweighted mean ("multi model"), or perhaps even other approaches to represent ensembles such as the median or the choice of best performing system.

The validation section (4) only discusses the errors against independent observations of the 4 individual analyses and their unweighted mean ("multi-model") but not the weighted mean ("integrated). This seems not consistent with the intension of the paper and all the discussion before, which focuses on the merits of the weighted-mean ("integrated") analysis. A comparison of unweighted ("multi-model") vs. weighted ("integrated") would be an important aspect of the paper, which is missing. Tables 3 to 6 should also include the respective figures for the weighted ("integrated") means. Further, moving the validation section 4 (Validation results) before section 3 (Data assimilation statistics) seems advisable because having established trust in the data set by the validation could make the discussion on emission response more convincing.

The authors use the standard deviation to represent the variation between the four analyses. Given that 4 is a relatively small number and Gaussianity can not be assumed, using a different measure such as the range should be considered.

The paper is quite long. Presenting the four reanalysis for concentrations and emission response would be one very valid paper. Comparing the different approaches to combine the reanalyses could be a second paper.

Specific comments

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Abstract:

p1 l14. Please consider not to introduce a complex acronym for an averaging procedure of independent data sets.

p1 l 21. The sentence starting " These improvements .." is difficult to understand. Improvement would imply a reduction in uncertainty of the emissions but this seems no the case. Please make a quantitative statement on the global or regional total of the prior and posterior emissions.

p1 l25. The last sentence is not a consequence of the statement before, but quite the opposite. It only shows how difficult the estimation of emissions is because of the complexities of atmospheric chemistry.

p1l 26. Please clarify "emerging constraints" in the abstract. Also why specifically the "integrated" reanalyses are better suited for this than the single re-analysis or the "multi-model" mean reanalysis.

p2 l20 These are climate runs over several years. In the data assimilation context, model errors are the errors in the forecast over the short assimilation window started from previous analysis. Is not clear that long-term errors are a predictor for the short-range forecast errors of the model. Inconsistencies between the analysis and the model equilibrium state and inadequate background error statistics (inflation factors etc.) may play a large role too, but do not effect the multi-year runs.

p3 l3 "a common data assimilation framework " , see my general comments. I think it is not a common data assimilation framework because the information from the error statistics (P) of each system is not used to come up with a better common analysis. Running different chemistry scheme as part of the ensemble would have been a common framework. The four systems are completely independent. You compare and average different analysis and emissions/concentration responses.

p3 l8 Please mention that the assimilated observations are not the same for all 4 re-

analyses.

p4 l3 Please provide more details on the quantitative value of the inflation factors and any differences between the four applications.

P4 l19 “a common data assimilation framework to four CTM frameworks” better “the same data assimilating system using 4 different CTM”.

p4 l20 Please include a general statement that the CTMs differ in emissions, chemistry scheme, resolution etc.

p4l 23 - p5 l24 The model descriptions vary to much in detail. In particular section 2.2.4 (MIROC-Chem) seems shorter than the others and does not have detailed information about the emissions. Please avoid too much duplication of information included in the text and provided in table 1. I recommend including the facts in the table and discuss specifics in the text.

p7 l3 S and A should be in a bold font as in the formula below

p7 l1o Please clarify this sentence. Biases in the observations, in particular if the instrument biases in NO2 and Ozone are not consistent, may severely degrade the performance. I would even speculate the a potential inconsistency between the NO2 and Ozone retrievals are a major reason for problems with the performance of chemical DA systems.

p8 l11 Please make a comment on the differences between the two MOPITT versions.

p9 l2 “We construct integrated data assimilation analysis using multiple models combined with multiple-species measurements.” I am not sure if the word “integrated” implies what you do. You calculate a mean of 4 analyses, which were produced with the same DA system using each a different modelling approach. This is not integration or data assimilation as such but simply a weighted average. Please consider replacing the word (“integrated”) throughout the text. Please make clear a distinction between the weighted and unweighted mean.

p9 l4 Please provide more details on the standard deviation field sigma_j. It would be helpful to include a formula in section 2.1 Also say if sigma_j is a field of the same time-space dimension as the analysis.

p9 l5 Please clarify what the spatial resolution of mean reanalysis is, given that the for re-analysis are carried out at different resolutions (table 5)

p9 l16 Please discuss here (or elsewhere) quantitatively the standard deviations to give an indication which model dominates the weighted-mean analysis.

p9 l13 Please clarify why this is not “meaningful”

p9 l26 The longer assimilation window for GEOS-Chem could have implications for the assimilation of NO2 and CO because their life time can be short and the diurnal cycle of the NOx emissions is very pronounced.

p11 l20 Please provide evidence for the fact that TES plays the largest role.

p12 l24 instead of “integration” use “mean”

p12 l31 The smaller standard deviation seems “trivial” given the formula of its calculation. Only the comparison with independent observation can demonstrate that the mean is a more reliable representation of the field.

p13 l16 Please provide more detail how the regression coefficients are calculated. Do they represent the mean over regions, month and all hours of the day? Please explain how the response is calculated for the weighted mean (“integrated”) analysis.

p13 l29 Please clarify that sentence. The study comes up with a different response functions for each model but gives no guidance which is the correct one. Given the non-linearities a mean over conflicting responses might not be the most suited approach.

p14 l17 I would think “conflicting” is a better description then “unique” for the difference in the response

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p15 l13 Please clarify" inter-model correlations"

p15 l29 Please clarify if the only difference between "mean" and "integrated" (formula 10) is the weighting with the analysis error standard deviations. If this is larger than the unweighted mean, this means a model with a larger "ozone response" had a smaller analysis error standard deviation. In this sense the standard deviation of the analysis error is also used as an indicator for the reliability of the "ozone response". Is this true an how can this be justified?

p11-p18 Please discuss also the errors of the weighted mean ("integrated") analysis and update the tables

p18 l10 Please clarify this sentence. What are the estimated mean errors (the biases?) and how do they relate to the analysis uncertainty, which is expressed as (bias corrected) standard deviation

p18 l13. Are you suggesting the AGCM-CHASER and MIROC-Chem should be given more weight in the weighted ("integrated") analysis.

p22 l22 The reduced multi-model spread for OH is an important finding and should presented more prominently. It shows that the assimilation of O3, NO2 and CO can improve the OH field because different models come to the similar results even without assimilation of OH observations.

p23 l11 Please give here also the respective numbers for the prior emissions.

p23 l20 The comparison seems not quite correct. The prior emissions are biased against each other, which will also strongly influence the posteriori spread.

p25 l5 Could these rather large and diverse correction of the CO emission indicate that the important impact of the VOC emissions on CO concentrations is not considered with the setup of EnKF DA system?

p28 l2 Please mention that these results were obtained by a regression approach.

p27l18 Please clarify uncertainty ranges in this context. Does it included that the biases or is it just the standard deviation. You should mention that the prior emissions had smaller differences between each other than a posteriori emissions.

p28 l5 “The obtained results also suggest that the multi-model integrated fields could provide fundamentally different chemical relationships than those in the individual models” I find this statement not convincing. A divergent response of the different model shows predominately that a robust conclusion can not be drawn from this multi-model approach. If only, it means we need more research to find more realistic chemical relationships.

Table 8 Please include multi-model mean and spread for prior emissions Caption: Please indicate what the number in brackets are

Figure 3: Indicate what the dashed lines represent

Figure 8: Correct spelling of “deveation”

Figure 15: The differences between the different a posteriori and prior estimates between the models could be shown better.

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