Geosci. Model Dev. Discuss., 8, C2393–C2398, 2015
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GMDD
8, C2393–C2398, 2015

Interactive Comment

Interactive comment on "A low-order coupled chemistry meteorology model for testing online and offline data assimilation schemes" *by* J.-M. Haussaire and M. Bocquet

Anonymous Referee #2

Received and published: 4 October 2015

Geoscientific Model Development - gmd-2015-129. Authors: J.-M. Haussaire and M. Bocquet

04 October, 2015

Title: *A low-order coupled chemistry meteorology model for testing online and offline data assimilation schemes*

Recommendation: **Minor Revision**

This paper address a very important topic in modern data assimilation: the joint meteorological and chemical species estimate. In particular this study presents a new fully coupled meteorological-chemical low order model and use it to experiment with state-of-art data assimilation schemes. The subject is extremely relevant and a clear contribution of this paper stands on its analysis of the effect of coupled data assimilation in different regimes of nonlinearity as well as observational constraint (*i.e.* different observational frequency and distribution). It has to be said that there is urgent demand in the data assimilation community of new manageable tools to study coupled data assimilation and this paper is also very timely along this line.

I recommend the paper to be accepted subject to a minor revision to address the points listed below.

Specific Minor Points

1. Page 7350 last line. You might want to say that the direction is given by the sign of the variable.

2. Page 7352 lines 5-9. This part may need improvements. There is a mix between the origin of the uncertainty (initial condition error in the meteorology and parametric error in the transport part) and the mechanism of increasing uncertainty (i.e. the exponential grow in the chaotic part). Uncertainty is inherent to the initialization procedure and is later increased via exponential growth. I understand that the uncertainty on x_m affects c_m via Φ_m (Eq. 3 and 4), but it is not clear what do the author mean by "...grow within the transport subsystem". Furthermore, have you perfect initial conditions in the concentration ? I understand that this may be a minor issue if that part of the model dynamics is stable, but this should be clarified here.

GMDD

8, C2393–C2398, 2015

3. Last paragraph of Section 1.1. You might want to specify here in which section L95-GRS will be described.

4. Page 7353 lines 1-4. This fact and its consequence may not be clear for a general reader without expertise in data assimilation. It is not just the reduced size of the ensemble that matters, but the mere fact that an ensemble exist and that the way how ensemble perturbations evolve is used to estimate the linear and adjoint dynamics.

5. Page 7353 lines 5-6. Do you expect this in the present study ? In the present form this is not clear in the text, and given that results already exist in the literature that shows the ability of IEnKS to estimate parameters in nonlinear model, the "expected" can be confusing.

6. Page 7353 lines 8-12. These two sentences can be condensed into one by saying that, under its conditions of use, the IEnKS solves the full Gaussian estimation smoothing problem, that is to say it provides an updated ensemble which is used to compute the covariances. Also, what is the difference between 4D-Var and "standard" 4D-Var for you?

7. Page 7354 line 16. The use of the verb "replace" here can be confusing, given that the resulting L95-GRS model still has a transport part which is structurally similar to L95-T, but it also has an additional GRS component.

8. Page 7355 line 1. Do you mean “ROC” instead of "VOC"?

9. Equation 11. More explanation and details on how Eq. (11) is obtained via the QSSA approximation are required. Also, I suggest to say at the very beginning of Section 2.1 that full details are provided in Appendix B. In the present form the reader may think that only specific points are highlighted therein.

GMDD

8, C2393–C2398, 2015

10. Page 7357 lines 21-23. Note that when defining the value of k_3, the reference at the equator has been taken (Appendix B). I guess this will not lead to significant quantitative difference, but it is worth mentioning why it was not used a mid-latitude reference in that case.

11. Page 7357 lines 26-28. Do you mean that you have tuned E^{ROC} and λ^{ROC} in the model to fit the real value of ROC ?

12. I think all figures need to be improved, in particular in their labels and titles size. In the printed version they are barely readable. I understand this may be fixed at a later stage, but it has to be done.

13. Page 7359 line 10. By looking at the O_3 panel, I would not say that O_3 remains at high levels, but just that it remains small but not zero.

14. Section 2.3, 2^{nd} paragraph. How do you define the maximal ozone concentration ? Is it the maxima in space and in time over a specified long simulation ? Then, if my understanding is correct, you are doing an analysis of the model behavior (the maximal ozone concentration) by changing two of its parameters (the emission of ROC and NO_x). This type of analysis, which is of course worth doing and much informative, does not automatically teach us about the nonlinear behavior of the model in terms of the relation between state-variables, but rather about the model phase diagram, that is to say how the structural properties of the model change (for instance from periodic to aperiodic) by changing parameters.

15. Section 2.3, last paragraph. It should be said that changing β the forcing and advection terms are changed, so that a modification of the stability properties of the L95 model is obtained in terms of, for instance, the number and amplitude of positive Lyapunov exponents or the Kolmogorov entropy. The conseguence for the performance of the data assimilation methods are significant. For some value of β L95 may not longer be chaotic. See Carrassi, Vannitsem and Nicolis

GMDD
8, C2393–C2398, 2015

(2008, Q.J.Roy.Meterol.Soc.) for a more extensive analysis of the L95 properties for different values of the forcing, dissipation and advection, and in relation with another data assimilation strategy.

16. Page 7362 lines 10-13. I would not just say with "state-of-the-art data assimilation methods", cause this may one think on methods that are operational in prediction centers. On the other hand those methods do not necessarily (and indeed almost never) make possible the propagation of information across model compartments. For this to happen one has to use global error covariance matrices defined over the full system, as you do here.

17. Page 7363 line 5. How do you select the dynamical regime ? Or am I getting wrong on what you mean by this.

18. Page 7363 line 12. What do you mean by "extrinsic model error".

19. Page 7363 line 13. Typo "offlne" $\Rightarrow$ "offline".

20. Page 7363 line 17. Do you mean "second" instead of "first" ?

21. Page 7364 lines 5-10. You might want to add the explicit formulas for $RMSE^{filter/smoother}$

22. Page 7368 lines 1-7. Can you comment on the choice of the values of observational standard deviations for the different variables ? Also, I suggest to state clearly that the normalization is done using the observational error standard deviation.

23. Page 7368 lines 9-16. It is not clear how the observations are distributed. Are they evenly distributed as in the results of Fig. 7 ?

24. Page 7369 line 20. Do you “40” instead of “240” ?

GMDD

8, C2393–C2398, 2015

25. Page 7369 lines 24-25. Why do you chose to have an unbiased initial ensemble for the emission rate (i.e. centred around the truth) and a biased one for the forcing ? You comment on this a bit later in the text, but it would be better to say something about this choice when you present it.

26. Page 7375. The numbering of equation seems to be incorrect. (B3) should move one line forward.

27. Caption of Fig. 8. I would change "several" into "three", and I would better say "observational error" instead of just "...observations." at the end of the caption.

Interactive comment on Geosci. Model Dev. Discuss., 8, 7347, 2015.