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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical Note: Propagating correlations in atmospheric inversions using different Kalman update smoothers" *by* J. Tang and Q. Zhuang

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General authors' comments: We sincerely thank the anonymous referee for the insightful comments to help us improve the technical note.

Our specific responses to each comment are below.

Anonymous Referee 2 (Comments)

General comments. This technical note presents extensions to Bruhwiler et al. (ACP, 2005) and Michalak (ACP, 2008), who presented Bayesian and Geostatistical versions of the Kalman smoother, respectively, together with terms that map the covariance be-

tween online and offline state variables. This technical note presents (1) a different version (eqn.8) of the covariance propagation equations in Bruhwiler et al. (their eqn. 25), (2) the equivalent equations for the ensemble square root Kalman smoother, and the sigmapoint square root central difference Kalman smoother, and (3) an implementation of an interval constraint (for the KS) and transform (for the ESRKS and SRCDKS) approach for constraining state variable values to within a desired range. I have several major concerns with this technical note, which make it not publishable in its current form.

Most importantly, the presented application is left to the last few pages of the manuscript, is not described in detail, and the benefits/advantages of the proposed methodological improvements are not clearly demonstrated.

1. The details of the inversion are not presented, and the sensitivity of the impact of the methodological modifications to these inversion set up choices are neither investigated nor discussed. Would the methods that perform "better" or "worse" remain the same if the inversion setup was changed slightly?

Our reply: In the revision, we extensively conducted the sensitivity of the inversions to different set ups with the presented methods, using different sets of observation data and different correlation lengths. We added the results and discussion to the revised text to justify the benefits and advantages of all three proposed inversion methods.

2. As the authors acknowledge, all of the examined methods (KS, ESRKS, SRCDKS) are approximations to the "batch" inversion. Given that the problem examined here is quite small (18 observations and 211 unknowns per month, p. 19233 line 2), the appropriate comparison is not between the three methods, or between these methods and the "true" fluxes, but between the methods and the best possible solution to the inverse problem (i.e. the batch solution).

Our reply: Following the reviewer's suggestion, we re-conducted the comparison experiments between using the three new methods and a linear batch inversion method.

The advantage and disadvantage of the methods presented in the Note are discussed in the revised text.

3. Incorrect metrics are used to evaluate the approach. The comparisons between the methods are presented in terms of their RMSE and R2 relative to the true fluxes, whereas Bruhwiler et al. (2005) showed that the main advantage of the tracking the covariance between online and offline elements of the state vector was in improving the estimates of the a posteriori uncertainties, in terms of making them more closely aligned relative to the batch inversion.

Our reply: In the revision, the comparison metrics were expressed as the difference between the posterior mean and covariance inverted by the three proposed methods in this Note and those from a linear batch inversion.

4. The technical note is quite long, and could be shortened substantially. For example, the detail in the presented derivations is likely unnecessary, and the detail used in describing the standard versions of the three approaches could be reduced.

Our reply: We significantly shortened the text by removing a number of equations.

5. The flow of the technical note should be improved to make its relevance to a broader audience clear. The authors jump from a relatively short introduction to a set of detailed derivation, without clearly presenting the problem that they are trying to solve. In other words, why are existing approaches for estimating CH_4 *fluxes unsatisfactory? How will the proposed work improve on this status quo? What practical difference to the flux estimates is sought / expected?*

Our reply: We expanded the Introduction to address the reviewer's comments.

6. The authors present an alternative version of the posterior covariance equation of Bruhwiler et al. (2005), which is indeed equivalent, as the authors acknowledge. This does not seem, in and of itself, to be a significant innovation. However, the authors also claim that their version is more general (p. 19221 line 22), but this is not discussed or

demonstrated (how can it be more general if it is equivalent?)

Our reply: In the revision, we rewrote some sentences and equations to highlight the need to incorporate the correlation. Our derivation provides more flexibility in implementing the ensemble based methods.

7. Overall, the main contribution of the technical note is arguably in the extension of the work of Bruhwiler et al. (2005) to the ESRKS and SRCDKS. If this is indeed the case, then the note should be rewritten with this purpose clearly at the forefront, and with the application demonstrating the advantages of the modified expressions for the a posteriori covariance, when evaluated against a batch inversion. In addition, the relevance of the presented work to the inverse modeling community as a whole would need to be more convincingly demonstrated.

Our reply: In this revision, we showed how this work can help the inverse modeling community with the new methods through presentation of our methods and comparison experiments with these methods and the batch method.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 19219, 2010.