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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Technical Note: A novel approach to estimation of time-variable surface sources and sinks of carbon dioxide using empirical orthogonal functions and the Kalman filter" *by* R. Zhuravlev et al.

Anonymous Referee #1

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The authors present a solution method for a Kalman filter estimation problem posed for carbon dioxide emissions. The method consists of decomposing the underlying flux field into a limited set of orthogonal functions for which source-receptor relations can be calculated. This is followed by a traditional matrix inversion of the Kalman filter equations. This method is not new as the authors themselves acknowledge through several references to geosciences papers. However, the authors do not cite a large number of papers where this method has been used in trace gas budget studies. In my opinion this would not be an insurmountable problem if the authors of this paper help the carbon-dioxide community to see the benefits of this method, and give a detailed description on how to implement it technically. That goal however is also not achieved as the technical description is only 1,5 page long and narrative rather than technical. Moreover, the claims made in the abstract on benefits of this method are not substantiated by the presentation of the results. In my opinion, this makes the technical note in its current form unsuitable for publication.

My specific comments are:

The method is not novel in trace gas budget studies and has been described in detail elsewhere. See for instance Verlaan and Heemink (1995,1996), Cohn and Todling (1995), Pham (1998), Zhang et al. (1999), Hanea et al., (2004), and references therein.

The authors claim that their method offers the advantages of (a) being more computational efficient, (b) giving smoother posterior flux fields, and (c) yields smaller errors. Although I am willing to believe (a) there are no results or numbers in the note that tell the reader how much can be gained and whether this is an important advantage. Please try to quantify your claim. With respect to (b): In what way are smooth flux fields an advantage? Do we know that 'real' flux fields are smooth and therefore smooth is better? Do smooth flux fields prevent known problems? It remains unclear to me what advantage is meant here. With respect to (c): I find this is a very dangerous statement to make. Smaller errors are not an advantage of a system and not better or worse than large errors. What matters is whether the posterior errors captures the true uncertainty well, and whether the balance between prescribed errors and the skill achieved is correct.

The derivation of the number of EOFs needed is partly based on carbotracker, which itself is constructed from a lower dimensional product (Olson ecosystem database). The carbotracker covariance matrix has about 75 degrees of freedom over land, and 30 over oceans. These numbers are very close to the number of EOFs needed to represent the variability of its fluxes. It is thus well possible that the number of EOFs

ACPD

11, C620–C622, 2011

Interactive Comment

chosen is not so much based on a property inherent in the flux fields, but one inherent in the discretization of CarbonTracker?

It remains unclear to me how the temporal domain was treated: on the one hand I read a description of a repeat of the T3 experiment, but I also see that there are time steps involved with 75 observations per step. Was this a fixed-lag filter? If so, how were the mean and covariance propagated? And most importantly: how was the error covariance inflated at each time step to allow new degrees of freedom to come into the EOF patterns? Or was the EOF pattern completely fixed throughout the inversion and was this a one shot matrix solution?

The assessment of the results from the different inversions is too brief in my opinion. First of all, the metrics shown are not explained (average, RMS, systematic errors in the figures) but also they pertain only to the match achieved against assimilated observations. This is purely a function of prescribed uncertainties and their degrees of freedom as the authors realize and acknowledge. The part of the analysis that focuses on fluxes just shows world maps but does not describe the true questions. To convince the reader that the EOF method is a good way forward the authors need to show (for instance):

- The annual mean fluxes per TransCom region for each estimate.
- Its seasonal cycle + error on for "old" vs "new"
- The error reduction achieved on each of the N expansion factors. Did the method really constrain all of them or just the largest ones?
- How well the errors comply with the skill of the system, i.e, χ^2 of innovations?

If the authors can address the issues above, I propose that they resubmit this work as a true research paper and not a technical note.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 1367, 2011.

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11, C620–C622, 2011

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