We thank referee#1 for insightful comments and useful references. Our responses are following:

Problem 1.

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.

Answer 1.

We appreciate the reviewer's suggestion. Citations to the proposed papers have been added to the manuscript. We respectfully would like to point out that these papers do describe specific implementations of Kalman filters but not in conjunction with the EOF approach for state vector.

Problem 2.

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 capture the true uncertainty well, and whether the balance between prescribed errors and the skill achieved is correct.

Answer 2.

(a): From general considerations, we can point out that while utilizing the same transport model, similar inversion method (Kalman filter), and the same set of observations, computational requirements for the solution of the inversion problem are defined by the size of the state vector. Due to a decrease in the dimension of the state vector achieved by using the EOF approach, it is reasonable to conclude that the computational requirements would decrease accordingly. A statement clarifying this has been added to the revised manuscript.

We did not attempt to quantify such computational savings due to constraints in time and resources.

(b): In our opinion the reviewer is right in questioning the realism and validity of "smooth" versus non smooth fluxes at boundaries of emitting regions. In fact the EOF approach can in principle detect very sharp boundaries related surface type boundaries which are obviously present in the analyzed flux fields, so we discuss smoothness in a sense of the reducing presence of the artificial boundaries introduced by regional division. For instance, ocean boundaries are of course rather non smooth. Yet one might ask whether changing depths, temperatures, and resulting changes in the biological population of the ocean could make the actual emitting regions not the same as the geographic boundaries of the oceans. We don't attempt to answer this question, or the question of whether smooth emission flux fields prevent known problems. Instead we attempt to provide an alternative way of looking at the problem, hopefully to the benefit of the scientific community.

(c): We are uncertain if we understand this comment correctly. While proper statistical properties (chi2 for instance) of the posteriors errors are of course crucial, in practice smaller posteriori errors in our view are indicative of a better inversion or assimilation system performance, given limited number of control variables. Also, from a practical standpoint, it would appear to be reasonable to argue that smaller posteriori errors given effectively a same number of degrees of freedom would indicate a possible advantage of using the proposed approach. By no means we wish to position our approach as clearly superior, but we do wish to position it as an alternative that seems to be worth exploring.

Problem 3.

The derivation of the number of EOFs needed is partly based on CarbonTracker, which itself is constructed from a lower dimensional product (Olson ecosystem database). The CarbonTracker 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 chosen is not so much based on a property inherent in the flux fields, but one inherent in the discretization of CarbonTracker?

Answer 3.

Yes we believe that this is a very insightful observation and we commented on it in the revised text.

Problem 4.

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?

Answer 4.

According to the TransCom3 Level 2 experiment protocol it was a solution of one iteration inversion. The seasonal inversion consists of a 3 year forward simulation for each field and for each month. Then all response functions were collected into one model matrix for inversion. We will add formula for covariance propagation and add the detailed description for experiment statement. The EOF patterns were fixed during the each month.

Problem 5.

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. 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, $\chi 2$ of innovations?

Answer 5.

We agree with the referee and address the first 2 bullet point in the revised text. But we don't understand the last 2 points. Our understanding the expansion factors referred by reviewer are corresponding to principal components of the analyzed flux fields ranked by their order, and it is rather interesting to see if the each components uncertainty reduction correlates with its order in contribution to the variability of the original flux fields. The latest comment may need more clarifications, all we can prepare is explanation of posterior $\chi 2$.