We thank referee#3 for insightful comments and useful references. Our responses are following:

Problem 1.

There are many technical details to this procedure which are not adequately discussed in the manuscript, and the authors should take care to explain their procedures completely.

Answer 1.

Other referee also raised this question, for example with regard to calculations of the EOFs. We thank the reviewer for noticing the lack of technical details. The revised manuscript has additional technical details in it.

Problem 2.

The fundamental concept of representing flux variability in one inversion with a basis set derived from a different flux inversion is not adequately justified.

Answer 2.

We agree, thank you for this comment. Yet in the absence of a desirable quantity of observations falling back on a variability of a set of well established inversion results seems to be a reasonable approach. We added text to the revised manuscript emphasizing the point made by the reviewer.

Problem 3.

Do CarbonTracker fluxes form a complete (enough) basis set to represent actual flux variability? Some of the most interesting results in this paper are the requirements of numbers of EOFs to represent a certain fraction of the estimated variance. Are those values actually set by the degrees of freedom in CarbonTracker?

Answer 3.

From general considerations it is likely that CarbonTracker fluxes, being a product of reanalysis based on models, do not form a complete basis. Whether it is a satisfactory enough basis could probably be partially answered by figures 2, 5 (with bars RMS and so on). We believe that EOF values are determined by the degrees of freedom in CarbonTracker, since CarbonTracker is the only input dataset. It would be a simplification to limit the number of degrees of freedom in Carbontracker to a combination of number of parameters and vegetation types, because the fluxes are also driven by weather patterns and seasonal variations of the climate variables, which can generate a number of spatial patterns given a very simple carbon cycle model.

Problem 4.

EOFs do not resolve the diurnal cycle (p 1370, lines 24-29). Argument on p 1371, lines 1-5 is inadequate to explain this decision. Why is there an optimal averaging period (of 3 days) as revealed by figure 1?

Answer 4.

We thank the reviewer for this comment. In our research we tried to define the optimal number of EOFs relying only on the criterion like minimal number of EOFs for decomposition. This assumption resulted us in the decision of using 3-day averaged EOF fields (old figure 1). Our transport model is designed to smooth and reduce diurnal cycle, so we should use it to treat diurnal cycle effects and corresponding seasonal cycles. 3-day averaging probably corresponds to synoptic scale patterns which appear to carry large portion of the flux variability. According to your suggestion below we recalculated EOFs without fluxes from 2000. We agree that for reconstruction monthly fields (like describes in TransCom3 protocol) it is reasonable to use EOFs with the same time scale. In current research only monthly averaged field are used for recalculation new set of EOFs. Figure 1 present relative error of decomposition for new monthly set of EOFs calculated from 2001-2008 data set.

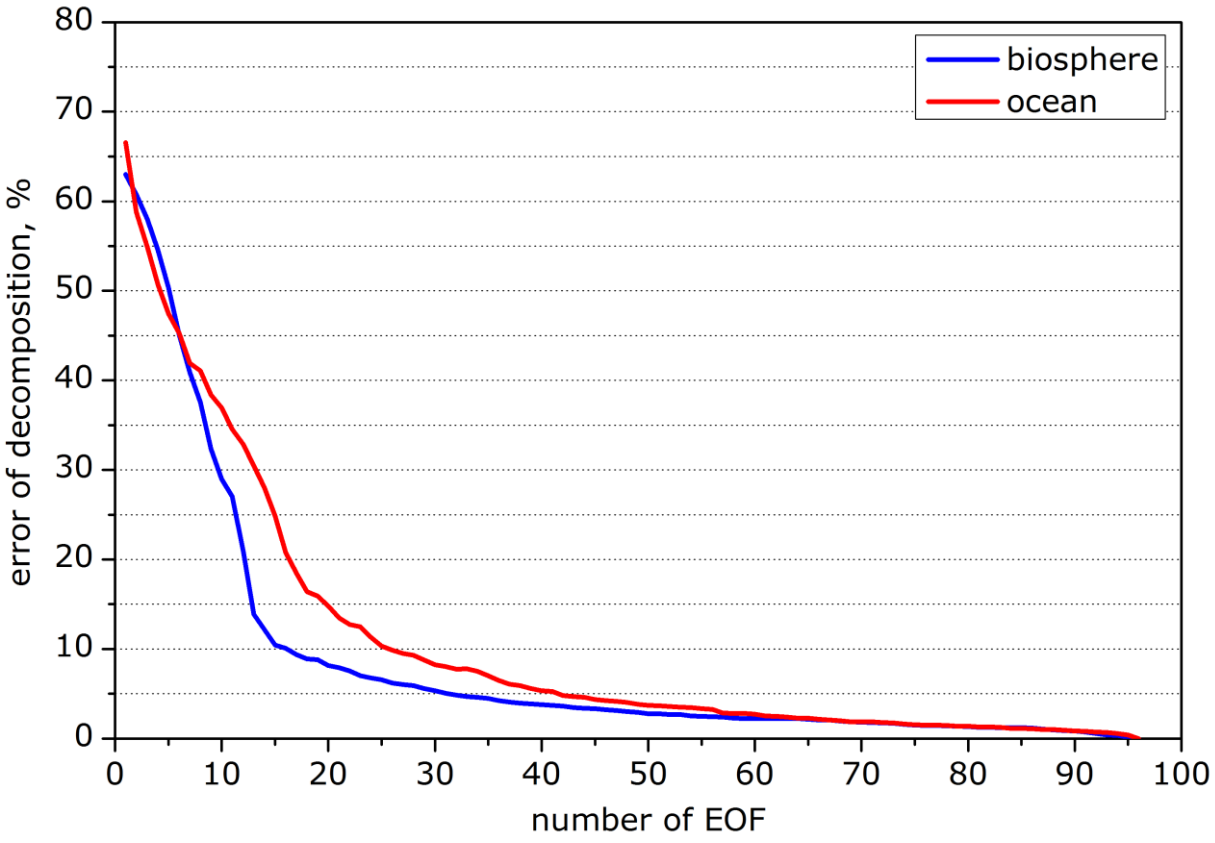

Figure 1. Averaged relative error of decomposition origin emission field depends on number of EOFs.

Problem 5.

Apparently, "anthropogenic sources" (cf p 1371 line 22) is used by the authors to indicate both fossil fuel emissions and fire emissions. This is a misnomer, since fire flux in CarbonTracker includes both wildfire and anthropogenic fires. Further, is there a need to compute EOFs for these sources? In CarbonTracker, these fluxes are imposed without optimization, so the variability is known a priori. The covariance of CarbonTracker FF emissions certainly does not span the uncertainty of the true FF emissions. Furthermore, there is at best monthly variability of FF fluxes in CarbonTracker, so 3-day averaging is pointless for that flux field.

Answer 5.

We thank the reviewer for this insight. We compared our results with TransCom3 results, where fire emissions are not used as separate source. So CarbonTracker fire fluxes were removed from our calculation at all. We left our calculations only using FF fluxes from TranScom3 in the revised manuscript.

Problem 6.

p 1369 lines 3-4: From context, it would appear that "shapes" means "within-region spatial distribution". The shapes of the regions themselves are invariant.

Answer 6.

In this part we wanted to note that in TransCom3 experiment land flux regions are identical for each month. This implies only 11 separate region fields and no seasonality in the maps. Ocean flux regions have different distributions for each month, that means seasonal changes in maps (due to changing ice cover in polar regions) and have been supplied as 12 monthly maps for each oceanic region.

Problem 7.

p 1369 line 24: "became" should perhaps be "has become".

Answer 7.

We agree. We made correction in manuscript.

Problem 8.

p 1370 line 16: from the time span of 2000-2008 the authors are probably using CarbonTracker release 2009. Please cite the exact release version.

Answer 8.

Thank you for your comment. We used CarbonTracker release 2009 for 2001-2008 period. We addressed these points in the revised manuscript.

Problem 9.

p 1370 line 17: CarbonTracker researchers exclude the year 2000 from flux analysis due to spin up effects. EOFs should probably not be computed using the year 2000.

Answer 9.

Thank you for this insight. We removed data from 2000 in our calculations of EOFs in the revised manuscript.

Problem 10.

p 1370 lines 19-20: Authors state that the spatial resolution of the EOFs is 1x1 degree, but the appendix suggests that emissions are applied at 2.5x2.5 degrees. Clarification as to what the spatial resolutions are used to compute the EOFs would be appreciated.

Answer 10.

We thank the referee for this reasonable comment. EOFs have the same spatial resolution as the CarbonTacker fields (1x1 degree). But the transport model grid has resolution 2.5x2.5 degree, so the input fluxes are remapped into a model grid. In fact it means that we use emission fields with spatial resolution 2.5x2.5 and could estimate fluxes only with the same resolution. But potentially we can use 1x1 degree fluxes with 1x1 degree model. We will describe this point in revised manuscript.

Problem 11.

p 1370 line 21: Why remove the T3 mean fluxes and not the actual CarbonTracker mean? The current flux anomalies will not have a mean of zero.

Answer 11.

The suggestion is useful. In our present setup, we likely have the bias as part of EOF set. But we cannot predict if we have a negative effect of it on in the following optimization.

Problem 12.

*p 1370 line 27: "ensembles *of* 3-hour emissions" (missing "of")*

Answer 12.

Thank you for correction. We changed this in manuscript.

Problem 13.

p 1371 lines 12-16: This technical explanation of EOF computation needs to be significantly expanded and clarified. What is meant by separating into seasonal groups? Are there different EOF basis sets used for each season? If so, the discussion following about retained EOFs needs to discuss the number required in each season. What are the season definitions?

Answer 13.

We thank the reviewer for the comment. As mentioned above we will expand technical description of EOF calculation and other details. The seasonal groups meant that there were several sets of EOF maps for each of twelve month. In case of monthly EOFs there is only one set of functions which describes stationary seasonal cycles (for each month) during the year. This case will be described in revised manuscript.

Problem 14.

p 1371 lines 17-26: If EOFs are computed using 3-day averages, why use monthly averages as the criterion to retrieve a certain fraction of the variance? Why not use 3-daily averages instead?

Answer 14.

We used monthly fields for decomposition because the task of TransCom3 Level 2 experiment is estimation the monthly fluxes in a stationary seasonal cycle. That is the reason why we are interested in monthly flux decomposition (for reasonable comparison). As mentioned above, in current research for correct comparison we use EOFs computed using monthly averages.

Problem 15.

p 1372 lines 6-9: Why should you expect a basis set computed over 2000-2008 to represent variability of the Transcom period (1992-1996)? There are significant differences between these periods, including instances of El Nino and the eruption of Mt Pinatubo. Please specify that the experiment being replicated is the cyclostationary Transcom inversion.

Answer 15.

This is certainly a valid point worth addressing in the revised manuscript. The assumption we made is that since TransCom3 Level 2 is a cyclostationary experiment, its results would adequately describe the main degrees of variability (EOFs) that would be approximately independent of temporal differences. Also there are 5-year (1992-1996) mean measurements for each month at 75 sites taken from the GLOBALVIEW-2000 dataset for inversion. During that period there were the same processes like El'nino or La'nino but may be with a weaker

strength. Clearly, such differences (e.g., volcanic eruptions) would affect the results as well, yet our assumption is that the main harmonics of the source variability would likely be unaffected. The main test, in our view, the size of the a posteriori errors seems to confirm this. We added a discussion of this point in the revised manuscript.

Problem 16.

p 1372 lines 8-9: Why set the prior emissions to zero? This is inconsistent with the observed atmospheric growth rate, and yields prior estimates which are known to be biased. Kalman filtering requires unbiased priors.

Answer 16.

We thank the referee for this comment. We have made recalculations with annual average values for coefficients as prior with covariance as standard deviations.

Problem 17.

p 1372 lines 11-14: I do not understand how this procedure produces a covariance matrix. Please explain more clearly.

Answer 17.

Since all basis functions are orthogonal, the covariance matrix should be a diagonal one. In order to determine values of the diagonal elements, monthly mean CarbonTracker emission fields for different years (2001-2008) were decomposed using the selected set of orthogonal basis functions. Each diagonal element of the covariance matrix has been set to be a standard deviation from the mean of the corresponding coefficient for each EOF. We added this description to the revised manuscript.

Problem 18.

p 1372 lines 20-27: Sequential Kalman filtering normally includes a temporal propagation step. Is that being done here? Is there any dependence of the February flux results on the January estimate? The equation giving the posterior covariance is missing. Does the matrix H comprise both transport and the (incomplete) EOF representation?

Answer 18.

All flux estimates are made in one iteration. So to answer the reviewer's question -- this is not a sequential framework. A paragraph clarifying this has been added to the manuscript. Thank you for noticing this. We added equation for posterior covariance matrix. Matrix H comprises responses from each EOF to each observation as a result of transport.

Problem 19.

p 1373 lines 1-2: What is meant by using only land and ocean EOFs? Does this mean that signals in observations that are associated by the inversion with "anthropogenic" basis functions effectively confound the land and ocean flux results? A cleaner comparison, if I understand the authors procedures, is to omit EOFs of fossil emissions to.

Answer 19.

According to the TransCom3 protocol fossil fuel fluxes are fixed during the year. Also estimating ff emissions was not the task of that experiment. “Using only land and ocean EOFs” means that we didn’t use FF EOFs, only fixed FF emissions from TransCom3 statement. As was written above we removed experiment with FF EOFs from our results.

Problem 20.

p 1374 lines 6-9: Why would systematic error increase with the number of EOFs used in the basis set? Can a statement be made regarding the significance of differences seen in Figure 5?

Answer 20.

We believe that this is caused by misalignment between number of used EOFs and number of available observations. In other words the problem appears to be underdetermined and larger number of EOFs results in over fitting. A statement clarifying this has been added to the revised manuscript.

Problem 21.

p 1375 lines 13-15: It would appear that this interpolation procedure does not conserve tracer mass, unless some fixer is applied. Please clarify. Non-conservation of tracer mass in a transport model can result directly in biases of estimated fluxes.

Answer 21.

Thank you for the reasonable comment. Of course there is exist mass fixer in NIES transport model. We omitted this point in our manuscript, because it explained in previous published paper. Detailed description of this fixer you can find in (Maksyutov, S., Patra, P. K., Onishi, R., Saeki, T., and Nakazawa, T.: NIES/FRCGC global atmospheric tracer transport model: description, validation, and surface sources and sinks 15 inversion, J. Earth Simul., 9, 3–18, 2008.), pages 7-8. The total tracer mass tendency by the semi-Lagrangian transport algorithm usually deviates from zero, which is often negligible in short term but can disturb the global trends and tracer budgets in long-term simulations. A variety of mass fixers are applied in

transport models in order to keep total tracer mass unchanged during transport (Hack et al., 1993; Rasch et al., 1995). We distribute the required correction proportionally to local advection tendencies as described in Taguchi (1996). The mass fixer is designed to conserve as total tracer mass.

Problem 22.

*Figure 2: Please make the caption more complete: Is this the RMS deviation of *monthly* fields?*

Answer 22.

Yes, this is RMS deviations for decompositions monthly fields.

Problem 23.

Figure 5: Why is averaged error not identically zero? Isn't that the expected result of optimization?

Answer 23.

The averaged error numerically doesn't equal zero, but it tends to zero. That could be caused by misalignment between number of used EOFs and number of available observations too. A statement describing this has been added to the revised manuscript.

Problem 24.

Figure 6: Why compare FF emissions (imposed for Transcom, and including fires for the current work)? This (and the great dynamic range of FF emissions) makes the comparison difficult to interpret. It would be better to show difference plots anyway, to highlight relative errors between the approaches.

Answer 24.

As was written above we removed experiment with FF EOFs, so in the revised manuscript now we don't estimate FF emissions (for correct comparison) because we used only fixed fluxes from TransCom3 experiment.

Problem 25.

Figures 7-8: Apparently the NIES, GISS, MATCH, and TM3 bars in these figures are identical to those in Figure 5. Find a way to remove this redundance (combine into one figure?)

Answer 25.

Thank you for your comment. We combined this figures into one.