

Interactive  
Comment

# ***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 #3**

Received and published: 17 March 2011

## SUMMARY

This paper shows an interesting approach to re-parameterizing the Transcom3 cyclo-stationary inversion using a basis set of EOFs generated from the CarbonTracker CO<sub>2</sub> analysis system. 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. The fundamental concept of representing flux variability in one inversion with a basis set derived from a different flux inversion is not adequately justified.



## MAJOR CONCERNS

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?

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?

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.

The dimensionality of the parameter space is apparently 993 (p 1371, line 24). How does this compare with the number of observations available (perhaps  $75*12=900$ )?

## DETAILED COMMENTS

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.

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

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.

p 1370 line 17: CarbonTracker researchers exclude the year 2000 from flux analysis

---

Interactive  
Comment

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



due to spin up effects. EOFs should probably not be computed using the year 2000.

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.

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.

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

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?

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?

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.

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.

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

---

Interactive  
Comment

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



---

Interactive  
Comment

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?

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

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?

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.

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

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

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.

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 redundancy (combine into one figure?)

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



---

Interactive  
Comment