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Interactive comment on "A Global Carbon Assimilation System using a modified EnKF assimilation method" *by* S. Zhang et al.

Anonymous Referee #1

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The authors present their data assimilation system GCAS-EK, which is based on the application of a Kalman Filter to the CO2 flux estimation problem. Some recent improvements to such systems were incorporated into this version, such as the inflation of covariances (on fluxes and observations) and the replacement of the forecast statistics with a better one, based on the analysis statevector mean. This system is described in a rather short description, that mostly states that all input data and settings were copied from NOAA/ESRL's carbon tracker website. One important difference with carbon tracker itself is the choice to also place CO2 in the statevector, which has been demonstrated to be beneficial in a joint meteorological-CO2 data assimilation method, which GCAS-EK is however not. The impact of the innovations in the extended statevector, inflation estimation, and forecast statistics are demonstrated in straightforward experiments, much similar to the original publication of these methods. Following these

OSSE's, a real global CO2 inversion is performed with as main result a better fit to the observed CO2 that was assimilated, and closer agreement to the published carbon tracker results at global, and at TransCom scales. Overall, I feel that this new system has a place in the ranks of current CO2 data assimilation methods, but the current paper does not highlight much novelty, does not convincingly show the added value of an extended statevector or shorter assimilation window, and does not demonstrate that this system is mature enough to estimate global carbon fluxes to a level of reliability comparable to existing methods. This is a consequence of the way the paper is structured: it does not fully document your system as I would expect for GMD, it also does not fully assess the details of extended state vectors or window lengths as could be suitable, and it also is not a sufficient paper to show you can estimate good carbon fluxes. The latter would be an interesting paper even for ACP or BG I believe. A clearer choice of the aim of this paper would in that sense help a lot.

The paper is very well written in appropriate English, and structured logically which makes it easy to read. Sufficient literature from the field is cited, although there are some blatant omissions in referencing data source as documented under (1). I think the design and application of this system is of interest to the GMD reader community, if the following four major points of concern are addressed in a next manuscript:

(1) This paper cannot be published without consent and acknowledgement of the CO2 data providers. You currently state that you got the data from the carbon tracker website but this is not an acceptable citation, nor the right source to get observational data. The data used by carbon tracker is owned by many individual PIs and the terms of use of this data state that these must all be informed when you use their data, and consulted to discuss acknowledgement. This has clearly not been done yet, and this must be rectified. Along a similar line, this study uses many products and details obtained from the carbon tracker website, but there is no acknowledgement for the carbon tracker effort as asked for on their website. Nor is there any reference to the original fossil, fire, and ocean flux data providers behind carbon tracker that also should receive fair credit

for their work. I find this scientifically unacceptable.

(2) Technically, the tests shown are not so interesting because they demonstrate improvements that were already described in more detail in previous publications. Their application in GCAS-EK is not much different from those papers and yields results which are quite predictable. Moreover, some of the questions that are important to the real-world application of GCAS-EK are not answered in this test. These questions are: (1) Why would the extended statevector be expected to outperform the regular flux statevector if they are fully related through a linear operator G? and (2) How much carbon mass is lost or gained per cycle/season/year due to the adjustments made directly to the mixing ratios rather than to the underlying fluxes? I recommend that the authors try to answer these questions as a prelude to the real-world application of estimating CO2 with GCAS-EK.

(3) You have chosen to apply your method globally, yet you use your Kalman Filter as a filter rather than a smoother. The only justification you give is that transport is uncertain and various choices are possible. This is not enough in my opinion. If you want to apply your system globally, you need to show that a filter captures the signals of CO2 sufficiently well in that period, and that going to a longer window or a lagged window has little advantage. My estimate is that your one week filter is too short for global flux estimates, and is partly responsible for the large flux differences with carbon tracker in your figures.

(4) The real-world application of the system is interesting, but I feel that the assessment of its realism needs to be expanded significantly. Now, we are just given a comparison to carbotracker fluxes that shows large differences but little evaluation. In the end, the question whether your system can produce good fluxes that match atmospheric concentrations well is not answered for me. The authors should look more closely at the evaluation of other systems that have recently been published such as from Liu et al., (2013) and Zhang et al., (2013). Important is to include an evaluation of mixing ratios, both those assimilated and non-assimilated such as from aircraft or other sites.

And to assess these at multiple time scales (diurnal, syncopic, seasonal, annual) and multiple location (tropics, SH, NH). Then, the sum of fluxes must be given for the globe and their sum must be compared to the global CO2 growth rate. Next, these must be split into ocean and land fluxes, and the land fluxes must be looked at to see where the land sink appears largest (tropics, NH boreal, or NH temperate, and Europe vs Asia vs North America). These must then also be split into forests and grasslands or cropland uptake. If all of these look good, a comparison can be made to the results of other systems, such as those in TransCom, or RECCAP, and perhaps carbon tracker. And again, this has to be done on seasonal, annual, and interannual scales. Finally, independent assessment against for instance GCP estimates, or eddy-covariance, or crop yields, or forest surveys could help. I realize this is not an easy task, but to publish a new inversion system one has to convince the existing community of its realism.

My recommendation is that the current MS is rejected and that the authors work on this manuscript some more before resubmitting it, since the changes I ask for are beyond a simple major revision. The first part of the paper should then focus on demonstrating that the extended statevector is an asset to this system and not just a liability for loss of CO2 mass. Also, it should demonstrate that the non-smoother version of the EnKF that they apply here is suitable for doing global inversions. Then, the global inversion should be presented, benchmarked in the method as described above under point (4). I hope my further comments on the manuscript in PDF help this effort.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/7/C2208/2014/gmdd-7-C2208-2014-supplement.pdf

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