Anonymous Referee #1

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

Acknowledgements

The authors gratefully acknowledge the anonymous referee #1 for his constructive and relevant comments, which lead to much improvement of this manuscript. We have checked our work carefully according to these comments and made the suggested changes.

General Comments

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 state vector 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 CO_2 in the state vector, 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 state vector, 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 state vector 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.*

Our reply:

Thank you for your valuable comments. Please see our reply to your following specific comments.

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.

Our reply:

Thank you for your comments.

We have added all the sources of the datasets we can find on the website in the following paragraphs and will send the manuscript to data owners to ask how to acknowledge them as soon as the manuscript is completed. We promise that all the mistakes you mentioned will be rectified. In the revised manuscript,

"The vegetation fire flux is taken from CarbonTracker 2011 dataset, which is modeled using the Carnegie-Ames Stanford Approach (CASA) biosphere model (Potter et al., 1993) based on the Global Fire Emission Database (GFED) (van der Werf et al., 2006) and resampled to an 8-day time step using MODIS fire hot spots (Giglio et al., 2006)."

"The oceanic CO_2 *flux is taken from CarbonTracker 2011 optimized results, whose a priori estimates are based on two different datasets: namely ocean inversions flux result (Jacobson et al., 2007) and* pCO_2*-Clim prior derived from the climatology of seawater* pCO_2 *(Takahashi et al., 2009)."*

"The fossil fuel combustion estimate is the dataset preprocessed by CarbonTracker 2011 from the global total fossil fuel emission of the Carbon Dioxide Information and Analysis Center (CDIAC) (Boden et al., 2011) and the "ODIAC" emission dataset (Oda and Maksyutov, 2011)."

"The atmospheric CO_2 *concentration measurements collected and preprocessed by Observation Package (ObsPack) Data Product (Masarie et al., 2014) are used in this study (Product Version: obspack_co2_1_CARBONTRACKER_CT2013_2014-05-08). The selected* CO_2 *measurements on 92 sites include observations of two main types: the measurements of air samples at surface sites and in situ*

quasi-continuous CO_2 time series from towers. Since some stations have multiple observations within a week, on average there are about 140 observations every week during 2002 and 2008. Five laboratories (NOAA Global Monitoring Division, Commonwealth Scientific and Industrial Research Organization, National Center For Atmospheric Research, Environment Canada and Instituto de Pesquisas Energeticas e Nucleares) provided these measurements and information of observation sites used in this study is listed in Table 1."

And in the Acknowledgement:

"We kindly acknowledge all atmospheric data providers to obspack_co2_1_CARBONTRACKER_CT2013_2014-05-08, and those contribute their data to WDCGG. We grateful acknowledge CarbonTracker CT2011 results provided by NOAA ESRL, Boulder, Colorado, USA from the website at http://carbontracker.noaa.gov."

(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 state vector be expected to outperform the

regular flux state vector 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.

Our reply:

Thank you for your comments.

Following your advice, we have deleted Section 4 of "simulation study".

We would like to answer Question (2) first. In this study, the background CO_2 concentration field at the beginning of a week is the analysis state at the end of the previous week. It is then updated using the observations within the week, so the estimated CO_2 concentration at the beginning of the week is different from that at the end of the previous week. This results in inexact carbon mass balance. To remove the imbalance, a corrected atmospheric CO_2 concentration can be generated using the sequential forecast of CO_2 concentration with the optimized carbon fluxes starting from the very beginning of the whole assimilation period. The corrected CO_2 concentration is denoted by $\mathbf{c}_t^{\mathrm{ca}}$. By this way the carbon mass can be balanced.

For question (1): Given an atmospheric transport model and its meteorological forcing data, the CO_2 concentration field is fully determined by the "initial condition" and "boundary conditions". In fact, if we have to find a state vector of "minimum length", it will consist of the initial CO_2 concentration field and the scaling factors. If the initial condition is inaccurate, there will be error in forecasted observations. There are two ways to reduce this error: one is using an assimilation window long enough to decrease the impact of the error of the initial CO_2 concentration field, which is done by CarbonTracker and many atmospheric inversions etc.; another is to optimize the initial CO_2 concentration field with observations, which is carried out by *Kang et al.* (2011,2012), *Liu et al.* (2012), *Miyazaki et al.* (2011) and this study. If a short assimilation window is used (for example, one week in this study), the error of the initial condition cannot be ignored. This is the main reason we include the CO_2 concentration field in the state vectors.

The benefit of this inclusion needs to be tested against the traditional approach without this inclusion. This issue is studied with the one-week assimilation window. A comparative experiment is designed as follows. At every time step, the CO_2 concentration is not updated. For maintaining the CO_2 mass balance, the analysis CO_2 concentration is derived by sequentially predicting atmospheric CO_2 concentration forced by the updated flux within the week. The results showed that the overall RMSE of analysis CO_2 concentration observations in this experiment is

8.5% larger than that of the corrected analysis CO_2 concentration $\mathbf{c}_t^{ca}$ by GCAS-EK. This suggests that inclusion of CO_2 concentration in state vectors can significantly alter the CO_2 mass balance and may have advantage in optimizing the surface CO_2 flux.

If the CO_2 concentration is not included in state vectors, the analysis CO_2 concentration at the beginning of each week is just the analysis CO_2 concentration at the end of the previous week, so the CO_2 concentration observations within the current week are not used to optimize the CO_2 concentration at the beginning of each week. However, when the CO_2 concentration is included in state vectors, all the observations within the current week and the previous weeks are used to estimate the CO_2 concentration at the beginning of the current week. So the CO_2 concentration at the beginning of each week estimated by inclusion of CO_2 concentration in state vectors could be more accurate than that estimated in the no inclusion case. Therefore, the estimated flux associated with the updated CO_2 concentration at the beginning of current week could have better quality. This is demonstrated by smaller RMSE with the inclusion than that without the inclusion.

Most of discussions above have been added in the revised manuscript. Please see the manuscript file for the revision.

References

Kang, J. S., Kalnay, E., Liu, J., Fung, I., Miyoshi, T., and Ide, K.: "Variable localization" in an ensemble Kalman filter: Application to the carbon cycle data assimilation, J. Geophys. Res., 116, D09110, 2011.

Kang, J. S., Kalnay, E., Miyoshi, T., Liu, J., and Fung, I.: Estimation of surface carbon fluxes with an advanced data assimilation methodology, Journal of Geophysical Research: Atmospheres (1984--2012), 117, 2012.

Liu, J., Fung, I., Kalnay, E., Kang, J.-S., Olsen, E. T., and Chen, L.: Simultaneous assimilation of AIRS Xco2 and meteorological observations in a carbon climate model with an ensemble Kalman filter, Journal of Geophysical Research: Atmospheres, 117, D05309, 10.1029/2011JD016642, 2012.

Miyazaki, K., Maki, T., Patra, P., and Nakazawa, T.: Assessing the impact of satellite, aircraft, and surface observations on CO2 flux estimation using an ensemble-based 4-D data assimilation system, J. Geophys. Res. [Atmos.], 116, D16306, 10.1029/2010JD015366, 2011.

(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.

Our reply:

Thank you for your comments.

Different lengths of the assimilation time window are used in various systems (5 weeks in CarbonTracker, 3 and 7 days in *Miyazaki et al.* (2011) and 6 hours in *Kang et al.* (2012)). We choose the one-week assimilation window in our methodology for the following three reasons. First, since most surface stations only have weekly observations, we need at least one week data to cover the globe. Second, beyond one week the errors of the atmospheric transport model may be significant, but they are very difficult to quantify. Third, the detailed information of observations may be attenuated with time by atmospheric diffusion and advection (*Enting*, 2002).

For comparison to longer assimilation windows, the following alternative experiments with moving assimilation windows were carried out. In the first alternative experiment, the length of the moving window is set to be two weeks while the forecast time step is still one week. The CO_2 concentration observation system is still the same as that described in Section 3, but is used to update the global carbon flux and the atmospheric CO_2 concentration within the current week and the previous week. This procedure is similar to GCAS-EK, which provides the ensemble forecast state of the first week in the assimilation window that is set as its ensemble analysis state at previous assimilation time step. Therefore carbon fluxes and CO_2 concentration every week is optimized twice with the observations in the current week and the next week. The corrected analysis of CO_2 concentration is also retrieved from rerunning the atmospheric transport model. The second alternative experiment is similar to the first one, but with the three-week moving window.

The linear trends of the observations, the corrected CO_2 concentrations averaged over all observation sites with one-week, two-week and three-week moving windows are 2.14ppm yr^{-1}, 2.17 ppm yr^{-1}, 1.59 ppm yr^{-1}, 1.13 ppm yr^{-1}, respectively. It seems that the longer the moving window is, the larger difference is the long term growth to the measurements. For further investigating the reason, the annual mean carbon budgets on 11 Transcom regions are shown in Fig. R1. It can be found that the longer the moving window is, the larger are the carbon budget adjustments. Long windows result in underestimation of the corresponding long term growth rate. These facts indicate that the one-week assimilation window may be most appropriate. Incidentally, the corresponding trend for CarbonTracker 2011 is 2.15 ppm yr^{-1}, also very close to the trend observed.

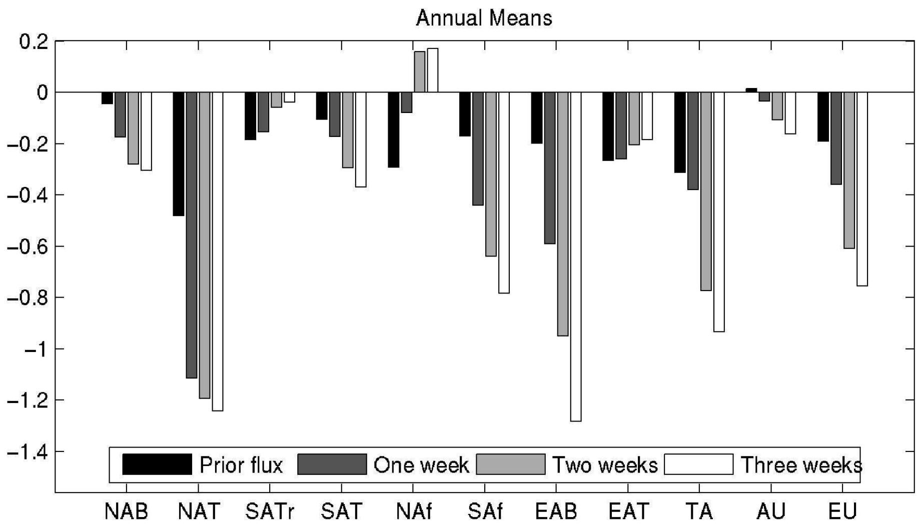

Figure R1. Annual means of carbon budgets (PgC yr^{-1}) on 11 Transcom regions in four different cases. Four cases are associated with prior values modeled with ecosystem model BEPS, assimilated results using GCAS-EK with one-week assimilation windows, two-week windows and three-week windows. 11 regions in

X-axis refer to 'North American Boreal' (NAB), 'North American Temperate' (NAT), 'South American Tropical' (SATr), 'South American Temperate' (SAT), 'Northern Africa' (NAf), 'Southern Africa' (SAf), 'Eurasia Boreal' (EAB), 'Eurasia Temperate' (EAT), 'Tropical Asia' (TA), 'Australia' (AU) and 'Europe' (EU), respectively.

To further investigate the long time and long distance impact of atmospheric transport on CO_2 observations, components of CO_2 concentration at observation sites associated with different Transcom regions in each day before their observation times are calculated in the following way. For a given region and some day before the observation time, prior fluxes on other regions and in other days are all masked. Then the atmospheric transport model can be run with a homogeneous initial atmospheric CO_2 concentration and forced by the masked fluxes to obtain the corresponding CO_2 concentration components.

These components at individual sites are then averaged in time to investigate general impacts of carbon fluxes from different sources. Results at 7 selected sites are shown in Fig. R2. For these sites, CO_2 concentrations resulting from carbon fluxes within 25 days are mainly from local carbon fluxes within 7 days (although mostly within 3 days). Carbon fluxes beyond 7 days or regions far from observation locations have very small impacts, indicating that they have little information in observations (i.e. the contribution is less than observation error), even if the atmospheric transport model is accurate. Actually the majority of continental observation sites used in this study (approximately 49) have similar properties to these 7 sites. If the errors of the transport and ecosystem models are considered, the information of fluxes one week before may be even more difficult to estimate.

The setting of length of the assimilation window is closely related to spatial and temporal localizations of forecast errors. For the observation network and the atmospheric transport model used in this study, the one-week assimilation window seems most suitable.

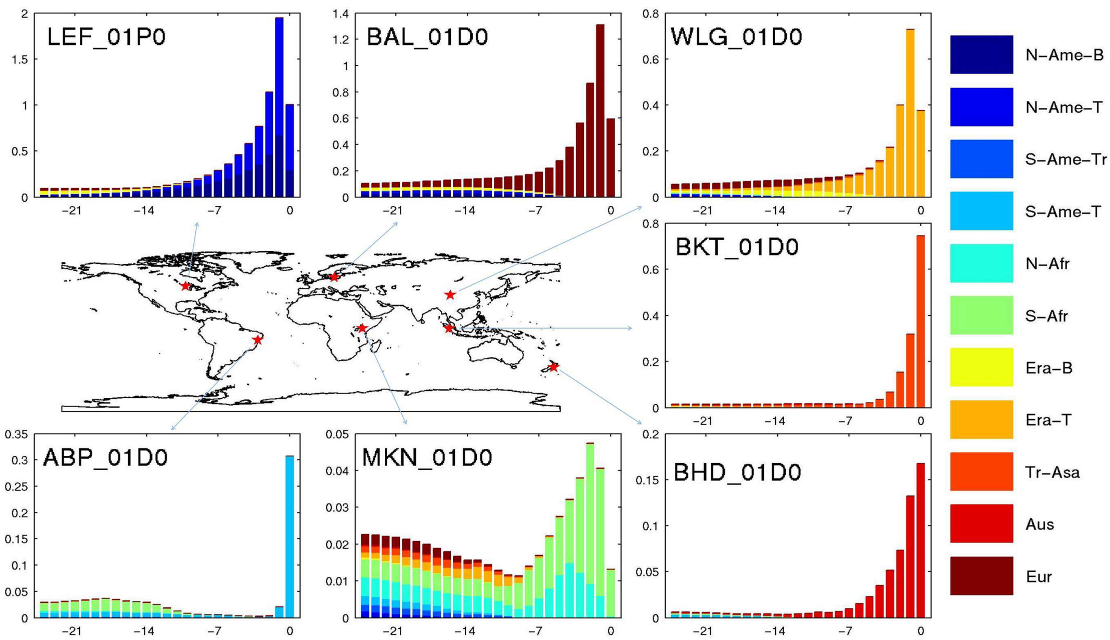

Figure R2. Mean components of CO_2 concentration at observation sites (Site IDs: LEF_01P0, BAL_01D0, WLG_01D0, BKT_01D0, BHD_01D0, MKN_01D0 and ABP_01D0) from 11 Transcom regions in each of 25 days before the observation time. X-axis refers to days before the observation time. Y-axis refers to the amount of CO_2 concentration in ppm. Different colors within a bar refer to CO_2 concentration from 11 different Transcom regions. 11 regions refer to 'North American Boreal' (N-Ame-B), 'North American Temperate' (N-Ame-T), 'South American Tropical' (S-Ame-Tr), 'South American Temperate' (S-Ame-T), 'Northern Africa' (N-Afr), 'Southern Africa' (S-Afr), 'Eurasia Boreal' (Era-B), 'Eurasia Temperate' (Era-T), 'Tropical Asia' (Tr-Asa), 'Australia' (Aus) and 'Europe' (Eur), respectively.

References

Enting, I. G.: Inverse Problems in Atmospheric Constituent Transport, Cambridge University Press, New York, 2002.

Kang, J. S., Kalnay, E., Miyoshi, T., Liu, J., and Fung, I.: Estimation of surface carbon fluxes with an advanced data assimilation methodology, Journal of Geophysical Research: Atmospheres (1984--2012), 117, 2012.

Miyazaki, K., Maki, T., Patra, P., and Nakazawa, T.: Assessing the impact of satellite, aircraft, and surface observations on CO2 flux estimation using an ensemble-based 4-D data assimilation system, J. Geophys. Res. [Atmos.], 116, D16306, 10.1029/2010JD015366, 2011.

(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.

Our reply:

Thank you for your comments.

The purpose of this study is to show some ideas that are potentially useful in assimilating atmospheric CO_2 concentration measurements into ecosystem models, including the inclusion of atmospheric CO_2 concentration in state vectors, the implementation of the Ensemble Kalman Filter (EnKF) with a short assimilation window, the use of analysis states to iteratively estimate ensemble forecast errors, and a maximum likelihood estimation of the inflation factors of the forecast and observation errors. We plan to put the assessment of the system into another manuscript, which is similar to the papers you mentioned (Liu et al., 2014;Liu et al., 2012;Zhang et al., 2014).

Following your advice, we have added more assessment in the revised manuscript. First, Chi-square tests were carried out to directly investigate the effectiveness of the techniques used to improve the estimation of forecast and observation error covariances. Second, long-term growth rates in several cases were tested and compared. Third, independent gridded net ecosystem productivity data such as that by *Xiao et al.* (2011) was compared to that by GCAS-EK. Xiao's data is based on eddy covariance and MODIS data.

Unfortunately, we carried out the assimilation from 2002 to 2008, when there is little aircraft or satellite data. Furthermore, direct carbon flux observations such as eddy covariance are sparse over the globe and their spatial representativeness is very limited, and thus they are not suitable for comparisons with our gridded results, although tower flux data at more than 100 stations are used to optimize the BEPS model that is used to produce the prior land surface carbon flux.

In the future, we are going to extend our assimilation to recent years using more observations, and comprehensive and systematic assessments of the methodologies developed in GCAS-EK will carried out.

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Zhang, H. F., Chen, B. Z., Machida, T., Matsueda, H., Sawa, Y., Fukuyama, Y., Langenfelds, R., van der Schoot, M., Xu, G., Yan, J. W., Cheng, M. L., Zhou, L. X., Tans, P. P., and Peters, W.: Estimating Asian terrestrial carbon fluxes from CONTRAIL aircraft and surface CO2 observations for the period 2006–2010, Atmos. Chem. Phys., 14, 5807-5824, 10.5194/acp-14-5807-2014, 2014.

Xiao, J., Zhuang, Q., Law, B. E., Baldocchi, D. D., Chen, J., Richardson, A. D., Melillo, J. M., Davis, K. J., Hollinger, D. Y., Wharton, S., Oren, R., Noormets, A., Fischer, M. L., Verma, S. B., Cook, D. R., Sun, G., McNulty, S., Wofsy, S. C., Bolstad, P. V., Burns, S. P., Curtis, P. S., Drake, B. G., Falk, M., Foster, D. R., Gu, L., Hadley, J. L., Katul, G. G., Litvak, M., Ma, S., Martin, T. A., Matamala, R., Meyers, T. P., Monson, R. K., Munger, J. W., Oechel, W. C., Paw, U. K. T., Schmid, H. P., Scott, R. L., Starr, G., Suyker, A. E., and Torn, M. S.: Assessing net ecosystem carbon exchange of U.S. terrestrial ecosystems by integrating eddy covariance flux measurements and satellite observations, Agricultural and Forest Meteorology, 151, 60-69, http://dx.doi.org/10.1016/j.agrformet.2010.09.002, 2011.

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.

In the supplement:

(1) Title: This title is a bit awkward as it contains the word assimilation twice, also the acronym EnKF is not known to all readers. Carbon Assimilation for many persons refers to the process of carbon fixation by photosynthesis. I suggest the authors make a better title.

Our reply:

Thank you for your comments. We have changed the title to "A Global Carbon Assimilation System using a modified Ensemble Kalman filter".

(2) P6520, L25: I am not sure I agree with this reasoning that including the CO_2 concentrations in the state vector should improve carbon flux estimations. How would that help? In principle, the CO_2 concentrations are fully determined by the surface fluxes, so putting them both in the state vector is not so intuitive to me. Of course, the reason they did go this direction is because the relationship between surface fluxes and atmospheric CO_2 is given by a transport model with uncertainties and putting CO_2 in the state vector allows you to correct for biases in transport, and also reduces the need to explicitly simulate the CO_2-flux relationship over long time periods.

Our reply:

Thank you for your comments. Please see our reply to the Question

(1) of your Major Comment 2.

(3) P6521, L25: In Kang et al (2011, 2012) and Liu et al (2012,2013) CO_2 concentrations are added to the state vector because they have strong correlations with weather variables that are simultaneously assimilated. This is much different from this study and the one by Miyazaki where only fluxes and CO_2 are added. This difference should be noted explicitly in the text.

Our reply:

Thank you for your comments.

We have added the following sentence in the Introduction section of revised manuscript,

"Kang et al. (2011) and Liu et al. (2012) also added CO_2 concentration to the state vectors due to their strong correlations with weather variables that are simultaneously assimilated."

(4) P6522, L10-15: Can you add a reference (URL or paper) to this source, as well as some form of acknowledgement for using this product? And note that NOAA/ESRL is often not the owner of these datasets themselves and true references should be made to the original data providers such as CDIAC, GFED, etc…

Our reply:

Thank you for your comments. Please see our reply to your Major Comment 1.

(5) P6522, L14: "rests" to "rest"

Our reply: Correted.

(6) P6522, L20: Please also mention the lack of knowledge on historical land-use change and land management, as this likely exceeds parameter uncertainties.

Our reply:

Thank you for your comments.

We have rewritten this sentence in the revised manuscript,

"*Errors in these parameters lead to biases of model results (Other uncertainties, such as lack of knowledge on historical land-use change and land management, also have influence on model results).* "

(7) P6523, L20: This is not acceptable as reference for the measurements used in this study. Carbontracker is not the source of this data and its is stated very explicitly that the original data owners must be contacted when these datasets are used in a publication. Then they must be asked how to be acknowledged. Simply downloading the data from a

website is not the way to go in our field. Please rectify this mistake.

Our reply:

Thank you for your comments. Please see our replay to your Major Comment 1.

(8) P6523, L25: What do you mean with "chosen to fit the observations?" Variances are not the same as mixing ratios…

Our reply:

Thank you for your comments.

We mean the "model-data mismatch" error in *Peters et al.* 2005. In the revised manuscript, the sentence is rephrased as:

"They were subjectively chosen and manually tuned to fit into specific atmospheric transport models and observations."

Reference

Peters, W., Miller, J. B., Whitaker, J., Denning, A. S., Hirsch, A., Krol, M. C., Zupanski, D., Bruhwiler, L., and Tans, P. P.: An ensemble data assimilation system to estimate CO2 surface fluxes from atmospheric trace gas observations, J. Geophys. Res. [Atmos.], 110, D24304-D24304, 10.1029/2005JD006157, 2005.

(9) P6524, L13: But in this setup, the spread in Ci simply reflects the spread in fluxes and the concentration variance is fully correlated with the flux errors. This is different from the methods in Kang et al (2012) and Liu et al (2012,2013) where CO_2 concentrations are added to the

state vector because they have strong correlations with weather variables. This difference should be noted explicitly in the text. The question becomes: why do you expect this method to work better than just having fluxes in the state vector? After all, the observations you have are not different, and the relation between fluxes and concentrations is fully explicit through G

Our reply:

Thank you for your comments.

Ci (4-Dimensional: 3D in space and 1D in time) reflects the spread both in fluxes (3D: 2D in space and 1D in time) and the initial CO_2 concentration field (3D in space) at the beginning of the week. The concentration covariance is also correlated to both the error of the initial CO_2 concentration field and flux errors. Since we are using a relatively short assimilation window, the error in the initial CO_2 concentration field is significant to the concentration covariance and we are trying to reduce this error by including CO_2 concentration in the state vectors.

Please see our reply to your Major Comment 2 for more details.

(10) P6528, L9

Our reply:

Thank you for your comments. We have deleted the whole section following your comment.

(11) P6530, L7

Our reply:

Thank you for your comments. We have deleted the whole section following your comment.

(12) P6530, L19: I am not surprised that the extended state vector does not really help as it contains no new information than the CO_2 observations you already had before, and it is fully correlated to the fluxes. The gain of time for not having to rerun the model forward must be weighed against the 'inexact mass balance': by adding or subtracting CO_2 from the atmosphere without a corresponding surface flux adjustment, you are creating CO_2 that is not accounted for by exchange between reservoirs. On longer time scales, this balance is very important. I suggest that you calculate this balance for your system by: (1) calculating per time step the change in mass of atmospheric CO_2 (2) calculating as well as the surface flux for that step (3) and compare these to each other to estimate the amount of 'ghost-CO_2' created in each step. If this number is small (say <1%) of surface flux then this issue might be minor. (4) Then, also compare this on an annual basis: does the ghost-CO_2 add up over time to a substantial flux, or does it average out over a year? And does it have a seasonal pattern?

Our reply:

Thank you for your comments. Please see our reply to Question (2) of your Major Comment 2.

(13) P6531, L4: The details should go to the method section.

Our reply:

Thank you for your comments. We have moved them to the methodology section.

(14) P6532, L10: You are describing here in words things that the reader can see in the figure. But what I expect is not a description, but an explanation of the differences: why are these fluxes not the same as carbontracker when you have copied almost the whole setup (observations, prior fluxes, variances, initial conditions, scaling factors)?

Our reply:

Thank you for your comments.

Although we have used the same observations and variances and initial conditions since the very beginning at 1st Jan, 2002 as well as a similar setting of scaling factors, the system is still very different from CarbonTracker in many aspects, such as prior ecosystem carbon fluxes (modeled with BEPS in this study vs CASA in CarbonTracker), data assimilation methodology (with several new developments of Ensemble

Kalman filter), length of the assimilation window (one week in this study vs 5 weeks in CarbonTracker) etc. Since the observation network is not dense enough to constrain the carbon fluxes that are inverted, small changes in system settings may lead to large differences in the results. Without a large set of modeling experiments and verification of independent estimates, it is difficult to give an exact explanation of the improvements in the optimized flux due to the introduction of new methodologies in GCAS-EK. We will put more effort on this issue and hope we can tell more on the reasons in the future.

We have deleted the unnecessary descriptions to the figure to make the manuscript more concise.

(15) P6545: Where are the error bars on these fluxes?

Our reply:

Thank you for your comment. We have added the error bars on the figure. With the ensemble methodology, we can get an ensemble of these fluxes and the corresponding errors are calculated as the spread of this ensemble.