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

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General Comments

This paper presents a new inversion method for CO_2 fluxes and applies it to the period 2002–2008. The novelty in the method is the inclusion of CO_2 concentration in the state vector allowing the relaxation of the perfect model assumption for transport. The ensemble method used makes this large augmentation of the state vector possible. The explicit treatment of transport error as part of the forecast error also allows better treatment of the observational error since this is now much closer to the observations (previously it was dominated by errors in the transport model). The paper also introduces to Ensemble Kalman Filter inversions the techniques of objective estimation

of covariance scaling parameters. These are called inflation parameters in this study but play the same role as the scaling parameters of **?**. Incidentally I think this paper should be cited. No doubt the authors came to their objective function via the KF literature but a citation would point out the familiarity of the approach to the conventional atmospheric inverse community.

The paper makes an important methodological contribution. It is well written and, most pleasingly, the algorithm is clearly enough described that it could be copied by someone with reasonable knowledge of the field. Analysis of the results is less developed but this is GMD and hopefully this can be taken up at a later date. I have no overall suggestions for the paper but do suggest a couple of small extra pieces of analysis in the specific comments below.

Specific Comments

overall It would be good to list the size of the state vector in various configurations (with and without concentration).

Eq. (1) Can you justify the 2/3 1/3 split? See later comment for why this might be important.

Sec 3.2 We need a little more discussion on the relationship between the iteration of the forecast and analyzed state and the tuning of the inflation parameters. This tuning is set up to ensure that the assumed and actual statistics of departures and innovations are consistent with those assumed in the relevant covariances. I'm not quite sure what consistency is enforced by the iteration in Sec 2.2 and am a little concerned that the observations might be implicitly used twice, once via the analyzed state now used to describe the forecast uncertainty then again in the update step. This probably reflects limited understanding on my part but I doubt I am alone.

P6530 it's a fascinating idea that by hugely increasing the size of the state vector (including concentration) you can actually reduce the computational cost. Shouldn't this be compensated by requiring different ensemble sizes to span the much larger space?

P6531 The bias in the simulation after analysis could be disturbing if it represents a miscalculation of the trend in concentration. Could you plot this bias as a function of time? If there is an error in the concentration trend this would suggest an error in the long-term fluxes. This is worth discussing since it's always seemed possible in these weak-constraint formulations that we might not match the long-term growth rate.

Sec 6.2] Some of the concern over low variability in λ may be explained by Eq. 1. The division by 3 should have the effect of strongly smoothing λ. What would happen if you replaced Eq. 1 with a pure random walk model?