

## ***Interactive comment on “A global carbon assimilation system based on a dual optimization method” by H. Zheng et al.***

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### **Referee Comment on: “A global carbon assimilation system based on a dual optimization method”**

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#### **General Comments**

This paper presents an optimisation of 8 years of CO<sub>2</sub> fluxes from the terrestrial biosphere and ocean using a method the authors describe as a dual optimisation. I am still a bit unclear on several methodological details of the paper so some of what I'm going to say in the following review is probably wrong. The authors should take notice of my misunderstandings though because they indicate places where the paper should be clearer. The most striking example of this is the elements in the control vector of the optimisation. I think this vector contains a series of multipliers ( $\lambda$ ) for patterns of terrestrial and ocean fluxes plus one global offset which is used to adjust the atmospheric concentration. The atmospheric concentration is adjusted once per assimilation window (six weeks) but I am unclear about the time resolution of  $\lambda$ . If it is also six weeks then the method seems to be an ensemble version of a classic synthesis inversion (e.g. Enting et al., 1995; Rayner et al., 1999) with the time windowing technique suggested by Law (2004). If this is correct then some of the claimed advantages of the method don't apply. For example, the authors claim (P14291) that the 1x1 degree resolution of

the model avoids the aggregation problem described by Kaminski et al. (2001). In fact the aggregation problem concerns flux patterns in the real world which are outside the subspace spanned by the control vector. The resolution of the flux patterns themselves (i.e the transport model) doesn't help this problem.

I also don't quite understand the computational burden of the problem. As I understand it, the authors solve for approximately 250 fluxes each six week window (weeks here defined like GlobalView with 48 weeks in a year). That's about 2000 unknowns per year or approximately 16000 for the whole period. That's not an immense problem even using the analytic matrix methods. There might be other reasons for the windowing technique, e.g. an effective weak constraint on transport but I don't accept the primary reason is computational.

Of course it's possible I'm completely misunderstanding the approach. The authors may solve for large-scale patterns plus deviations from these, in the spirit of the geostatistical methods pioneered by Michalak and colleagues. If so, please disregard the above but the authors should discuss the relationship with these techniques.

another concern is independent of the flux resolution and concerns the treatment of the initial condition for each window. Quite reasonably, the prior estimate for this is the result of the simulation of the previous window. The concentration is then corrected by a global offset to minimise the difference with surface values at the end of the window. This updated concentration distribution is used, without correction, as the initial condition for the next window. I see three problems with this:

1. The adjustment to match concentrations introduces a change of CO<sub>2</sub> mass in the atmosphere that is not associated with any fluxes. If this correction has a consistent sign it will lead to a flux series that is inconsistent with the change of CO<sub>2</sub> concentration over the whole timeseries, the aspect of atmospheric CO<sub>2</sub> of which we are most sure.

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2. Why correct only the mean concentration? Peylin et al. (2005) showed a method for improving those aspects of the 3-dimensional concentration distribution observable by the concentration measurement network.
3. No account seems to be taken of uncertainty in the initial concentration field when calculating fluxes. This is a pretty direct consequence of leaving the 3-d concentration out of the state vector. Peylin et al. (2005) also showed that errors in the initial field could affect the model-measurement mismatch for 20 days i.e about half the assimilation window so it would seem to be important to deal with this.

I also question the use of multipliers for flux patterns themselves rather than the more conventional use of separate multipliers for GPP and respiration. The problem arises from the diurnal cycle. I'm not clear whether the authors retain the diurnal cycle of fluxes from BEPS. If they do then a change of sign of the flux will also change the sign of the diurnal cycle. Since many of the observations in GLOBALVIEW represent particular times of the day this could affect the model-data mismatch at the heart of the inversion.

My other general concern is prior uncertainties. These are handled via uncertainties on the  $\lambda$  parameters. If I understand correctly these are set at 0.1% for regions outside China and 1% for China. These uncertainties are not arbitrary, they should represent the statistics of differences between simulations of the model used for the prior and the true fluxes. See Chevallier et al. (2006, 2012) for details on how they can be calculated and some indicative numbers from a different model. The uncertainties used in this study seem very low. For example they approach 0 in the transition season as the net flux approaches 0 although the uncertainty should not. This has consequences for the results. The relatively small changes in  $\lambda$  are a likely consequence of these very small uncertainties. I suggest this choice should be justified.

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## Specific Comments

P14271L10 note that we don't calculate the PDF by minimizing differences, that's for calculating the maximum likelihood estimate

P14276-7 I am confused about the time windows here. Is there perhaps an error? e.g. We hear that the system is run from time  $t-1$  over  $l$  steps but the observations listed are at  $t+1, t+2 \dots t+l-1$ , should this be  $t-1$ ?

P14281 You note that fire and fossil fluxes are not perfectly known and are excluded from the optimization. You need, then, to include their uncertainty in the observational error you use.

P14289 The comparison of posterior simulation and observations is a good idea but highlights some of the problems

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