

## ***Interactive comment on “Recent global CO<sub>2</sub> flux inferred from atmospheric CO<sub>2</sub> observations and its regional analyses” by F. Deng and J. M. Chen***

**Anonymous Referee #2**

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The authors lay out three goals on page 3500. The first goal was to improve upon previous inversion studies. This paper did not present why it provides a unique analysis of CO<sub>2</sub> fluxes relative to other studies. Are the choices of prior fluxes significantly different from or better than those used in other inversion studies (specifically the BEPS ecosystem exchange and the ocean flux model)? Are the mechanics of the inversion different than previous studies? Moreover, the authors never presented the inverted fluxes relative to the prior fluxes, so as a reader, I was left with small understanding of how the inversion impacted these priors. It seems that the biospheric prior fluxes already vary interannually based on LAI data and NCEP meteorology. How much of the interannual variability in the inverted fluxes owes to the GLOBALVIEW CO<sub>2</sub> and how much owes to the adaptive prior fluxes?

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Goal (2) was subjugated to goal (3), when it seemed that the authors could have used their results first to understand fluxes at larger scales and then addressed smaller scale IAV. Presumably the trends at large scales are more robust than those in the smaller regions. I am not convinced that the regional interpretation necessary to achieve goal (3) has the implications asserted by the authors. The authors have not indicated that there are not significant covariations in the inverted regional fluxes, particularly those in the small North American regions. Still, I would be curious as to how the posterior minus prior fluxes look in the smaller regions versus the larger ones, and how the interannual variability scales with region size and with posterior minus prior fluxes. If the North American regions are aggregated, does the IAV become comparable in size to that of Europe?

The authors could focus on making the language in the paper more concise and more precise. For instance, the last paragraph on page 3499 contains true statements, but I had trouble determining how they applied to this study. Likewise, on page 3500, the authors state that "consequently, this climate-carbon cycle interaction results in a positive feedback and an addition of atmospheric CO<sub>2</sub> that ranges between 20 and 220 ppm by 2100 from different models", but the authors never specify the forcing associated with these changes. The vague language becomes a problem when the authors discuss their methodology. More details on the methodology and data are necessary to properly interpret the results.

With regard to the inversion setup, the authors appear only to use GLOBALVIEW CO<sub>2</sub> where actual data has been collected. Still, these data have been smoothed and gap-filled. What would the results look like with actual observations? I did not understand how the  $\sigma_{const}$  values were determined in the model setup. I would also like more details regarding the weighting for data at several levels within a gridbox. It seems that the vertical profile information should provide more information, not less. Data from within the free troposphere should, for instance, provide constraints on the fluxes well upstream, while data at the boundary layer reflect more local fluxes. To underweight

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multiple observations within a gridbox, rather than to use them to constrain fluxes at different lengthscales, seems to underutilize available information.

The figures in the paper require major revision. Figures 6-9 have very small text, similar colors representing different years, and the timeseries are difficult to interpret owing to many lines on each graph. Many of the anomalies seem to oscillate on a monthly basis. Perhaps binning the data by seasons would eliminate some of the noise and clarify the points the authors are trying to make. In any case more attention needs to be given to visual representation of the data. Figure 4 also had a lot of data presented in a practically illegible manner yet still didn't address how different the flux estimates were from the prior.

#### Minor points

Page 3507, the paragraph beginning at 20: all of a sudden, the fossil and biomass burning fluxes are summed with the land biosphere exchange. This is done to support figure 3, but it does make it confusing when the term land fluxes sometimes include fossil and biomass, and sometimes only terrestrial exchange. It would be better to keep the terminology consistent throughout.

Page 3511, line 21: based on my knowledge of the scientific discussion regarding the Amazon green-up, it seems disingenuous to say that the issue has been reconciled by Samanta. Rather, this seems like an area of active research.

#### Typographical and grammatical issues

p3449 line 1: distributions should be distribution

p 3449 line 4 dataset should be datasets

p 3502 line 4 GLOBAVIEW should be GLOBALVIEW

p 3502 line 15 CIDAC should be CDIAC

p 3590 line 9, accents are wrong in Le Quéré

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p 3507 line 26 summaries should be summarizes

p 3509 line 26: Freasdale should presumably be Fraserdale

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