Biogeosciences
Discussions

*Interactive comment on* "An empirical spatiotemporal description of the global surface-atmosphere carbon fluxes: opportunities and data limitations" *by* Jakob Zscheischler et al.

**Anonymous Referee #2**

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This paper puts together a wide range of spatially explicit bottom-up surface-atmosphere CO2 flux data sets aiming to reconcile the carbon budget from bottom-up estimation and the atmospheric CO2 growth rate. While this type of research is needed for improving our understanding of carbon cycle, this study has serious flaws in generating the data and is lack of validation and deep analysis of the combined data set. The language is vague in many places. At this stage, I don't recommend publishing the paper.

Major comments: 1. The added value of the new combined dataset is very limited. The authors simply put different data streams together, and there is no effort trying to harmonize the data, even though some of the datasets do not cover the same time

period, e.g., the crops cover 2005 to 2010.

2. The paper compares the bottom-up estimations with the top-down inversion results (Figures 3 and 4, section 3.4), but it is lack of discussion about why these two approaches have different results, and which estimation is closer to reality.

3. In section 3.4, it says that "both estimates agree well in the extratropics", but the figure 3d shows that the NCE-FF and atmospheric inversion results also have large differences in the NH high latitudes (between ∼60N and ∼75N), with the NCE-FF indicating a source to the atmosphere, while the atmospheric inversion indicating a weak sink.

4. Even though the latitudinal pattern of the inversion results follows a pattern similar to that of the aquatic fluxes (Figure 3c), there is no direct evidence indicating the propagation of the marine signal into continents during atmospheric inversion. I suggest removing the discussion on the pattern comparison between aquatic fluxes and atmospheric flux inversion results in section 3.4.

5. Section 4.5 discusses the possible application of the combined dataset in model-data integration studies. It is an interesting idea. However, with such large uncertainties (with more than 10GtC disagreement with the atmospheric CO2 growth rate) in the combined dataset and a mixture of all different carbon flux components, it is not clear how such product can be used in carbon cycle data assimilation that focuses primarily on land carbon fluxes. What is the added value of using such data set compared to directly using flux tower observations? In addition, if such product were to be used as "observations" in a data assimilation system, a rigorous validation against independent observations is needed.

6. In section 3.2, it says that "13% of our runs we obtain a global C source that is consistent with the atmospheric growth rate", what are the spatial distributions of the fluxes from these 13% runs?

7. L26 (P14): what is the distribution of the different age classes of forests in FLUXNET? Is there solid evidence showing that the year and regrowing forests are overrepresented in FLUXNET?

8. Section 3.5 discusses seasonal cycle and monthly variability. It would be helpful to put this discussion in perspective, e.g., comparing to other independent estimations, so that the readers would know the credibility of this result. It is not clear what are the latitude ranges for the NH and SH in Figure 6.

9. Line 6 (p15), what is the basis for the 50% uncertainty?

Minor comments

1. In the abstract, "would require an offsetting surface C source of $4.27 \pm 0.10$ PgC/yr", should the offset be 4.27 + 6.07 PgC/yr in order to have 4.27 PgC atmospheric CO2 growth rate?

2. Line 13-16 (p3), it is not clear what the "background CO2 fluxes" means.

3. Line 23 (p4), "goal of this study the" should be "goal of this study to"

4. Line 16 (p6): what is "schused"?

5. Line 21 (p14): What does the "relevant drivers" refer to? Be more specific.

6. Line 29 (p14): what does the "global driver" refer to?

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