

Interactive comment on “Evaluation of atmosphere-biosphere exchange estimations with TCCON measurements” by J. Messerschmidt et al.

Anonymous Referee #2

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This paper compares the seasonal cycles in total column CO₂ from four observation sites to simulated cycles from three terrestrial models and one atmospheric transport model. Evaluating terrestrial models with seasonal CO₂ cycles is an important method for identifying biases in the spatial and temporal distribution of modeled CO₂ fluxes, and column-integrated CO₂ observations are well-suited to the evaluation because they reduce the sensitivity to local fluxes and errors in vertical transport in the models.

While the paper clearly demonstrates biases in the seasonal fluxes of CASA, it does not seem to make much progress beyond previous work by e.g. Yang et al. 2007; Nakatsuka and Maksyutov 2009; Randerson et al. 2009; Keppel-Aleks et al. 2011, 2012. It is interesting that SiB and GBIOME-BGC have a better match to the observations, but the authors unfortunately do not delve into the differences between the

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models that could explain the differences in their spatial and temporal CO₂ fluxes.

In order to make a substantial contribution with this manuscript, the authors need a major revision focusing on the development of a unique story and the broader implications of their results.

General Comments

The authors should clarify in the title and abstract that the evaluation focuses on seasonal fluxes.

The construction of the mean CO₂ across the four sites needs to be clarified. How is this calculation affected by the time period of observations at each site? For example, it looks like the CO₂ amplitude was smaller for 2009 and 2010, but is this only a result of the initiation and incorporation of the lower latitude sites? Even if this is accounted for in sampling the models, it could be misleading to show as a single time series plot of CO₂. Moreover, combining the data at all sites eliminates potential for insights from comparisons at specific sites, which are presently discussed only briefly in Sec. 8.1 and do not include the revised CASA or GBIOME-BGC.

Since the focus is on the mean seasonal cycle for most of the paper, it would improve the clarity in the figures to show detrended CO₂ and/or to focus on the mean cycle for one calendar year in several places. For example, part of the model-data discrepancy in Figures 3 appears to be due to a larger growth rate in the models (offset in 2009 and 2010). Figures 2, 3, and 6 could be removed in this case. In any case, Figures 5, 9 and 12 could be removed, since that information is already provided in a table (Figs. 5 and 9), or not relevant to the main topic (Fig. 12).

The authors do not address the effect of using one transport model and the potential for errors in transport to affect their results. While local vertical transport is less influential for total column observations, there may still be errors in lateral transport and interhemispheric mixing. Transport should also be addressed in the context of the

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Globalview comparison.

Specific Comments

12763, Line 13 – Is a one-year spinup sufficient? It's usually 2-3 years.

12763, Line 17 – Please clarify how GEOS-Chem was run. Were all the components in Table 1 included, and the “Balanced ecosystem exchange” swapped between the three models CASA, SiB and GBiome-BGC? So then the detrending needed to compensate the sink in GBiome-BGC was to avoid double-counting with the Transcom climatology?

12764, Line 1 – Why is an intermediate resolution of 5.5° used if the terrestrial model is 1° and the atmospheric model is 2° ?

12766, Line 11 – How are averages for 2006-2010 (used in Section 7) computed when data is not available at all sites for the whole period?

12766, Line 17 – Tables should be numbered consecutively according to their citation in the text

Figure 1 and 12767 – Top panel should be expanded vertically. Why not show a single calendar year in the lower panel (also for Figure 2)? Is IAV important in these plots? Why isn't CO₂ detrended here and in the other figures? Could use the term growing season net flux, used before by e.g. Randerson et al. 1997

12767, Line 4-5 – Boreal forests have a smaller latitude range

12767, Line 7 – Is the 0.7 Pg net uptake of anthropogenic CO₂ isolated to May-Sept months and 30-90°N in GBiome-BGC? This is not really a large portion of the growing season net flux (<10%).

12767, Line 9-10 – Should have a reference, e.g. Randerson et al. 1997

12767, Line 20-21 – Suggest using broad vs sharp rather than wide vs narrow

12767, Line 26 – Suggest using concentration or mole ratio rather than abundance

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Figure 2 and 12767-8 – Why not exploit the differences in sensitivity of the four sites to different regions by making model-data comparisons at each site, instead of the four-site mean? The mean latitude of these four sites is actually $\sim 45^\circ\text{N}$, and the amplitude is smaller than the mean amplitude over 30-90°N in Figure 2. Why show 700mb cycles as opposed to column averages?

12769, Line 1 – “turning points” should be “zero crossing times”

12769, Line 22-24 – NEE amplitude in GBiome-BGC was larger than CASA in Figure 1, but their CO₂ amplitudes are more similar. Please comment on this. Does this suggest the NEE spatial distribution and the amplitude both need to be considered?

12770, Line 4 – Please provide more detail on Keppel-Aleks 2012, particularly which sites/how the comparison was made.

12770, Line 8 – Please show the revised CASA NEE cycle and distribution in Figure 1. Is the addition of July NEE to May NEE, and thus introduction of a net sink, necessary? This is rather extending the growing season as opposed to only shifting it earlier. What happens if the CASA phasing is shifted earlier by 2-3 weeks, but the fluxes remain neutral?

12770, Line 21 – $+2\pm 1$ is not significantly better than -3 ± 1 days

12770, Line 25 – Should reference Table 7

12770, Line 28 to 12771, Line 2 – The revisions made to the CASA fluxes would not be characterized as “small changes”

12771, Line 2-3 – This sentence referring to local variability and synoptic scales does not follow, it seems out of place

12771, Line 17 – “the *modeled* CO₂ seasonal cycle is mainly driven by. . .” The observed seasonal cycle has more interannual variability which is not captured by the model

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Figure 11, 12 and Section 8.2 – Figure 11 is very hard to see. Why include Southern Hemisphere sites here when SH TCCON sites have not been shown? Why not show the mean seasonal cycle for one calendar year? Why is the mean bias given, when the paper has so far focused on seasonal amplitude and phase? Some discussion of the NOAA site comparisons should be given in the context of potential errors in the atmospheric transport model, which should be more important than for the TCCON comparisons.

Conclusions – The last two paragraphs don't reflect the main topic or the results of the paper

Table 2 – This table is unnecessary, since the text already states that SiB and CASA are neutral and GBIOME-BGC has a sink of 0.7 Pg.

Table 4 – The ranges provided here aren't particularly useful, in my opinion. NEE details could instead be given as amplitude and growing season start/end dates for each model. Differences in the CO₂ cycles are given in Table 6 so they can be omitted.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 12759, 2012.