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Atmospheric Chemistry and Physics Discussions

# *Interactive comment on* "Evaluation of atmosphere-biosphere exchange estimations with TCCON measurements" *by* J. Messerschmidt et al.

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Response to the anonymous referee #2 for "Evaluation of atmosphere-biosphere exchange estimations with TCCON measurements" by J. Messerschmidt et al.

We thank the referee #2 for his constructive feedback. We considered all his concerns and answered them in detail below.

General comment I:

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 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.

Answer: We have included an additional section addressing this comment directly. We also made changes to the abstract to include broader implications of the results: "We evaluate three estimates of the atmosphere-biosphere exchange against total column CO2 observations from the Total Carbon Column Observing Network (TCCON). Using the GEOS-Chem transport model, we produce forward simulations of atmospheric CO2 concentrations for the 2006-2010 time period using the Carnegie-Ames-Stanford Approach (CASA), the Simple Biosphere (SiB) and the GBiome-BGC models. Large differences in the CO2 simulations result from the choice of the atmosphere-biosphere model. We evaluate the seasonal cycle phase, amplitude and shape of the simulations. The version of CASA currently used as the a priori model by the GEOS-Chem carbon cycle community poorly represents the season cycle in total column CO2. Consistent with earlier studies, enhancing the CO2 uptake in the boreal forest and shifting the onset of the growing season earlier significantly improve the simulated seasonal CO2 cycle using CASA estimates. The SiB model gives a better representation of the seasonal cycle dynamics in the comparison with TCCON measurements. The improvement is derived primarily from the differential phasing of respiration with respect to gross primary production between these models."

General Comments II:

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

Agree. We have changed the title as follows: “Evaluation of seasonal atmosphere-biosphere exchange estimations with TCCON measurements”. The new abstract is described above.

2. 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.

It is correct that the time period of observations affects the mean CO2. Therefore Table 5 gives the days of measurements averaged for all sites. However, the comparison to GEOS-Chem is not affected by this method, because smoothed column-averaged CO2 amounts were calculated from the model sampled only at the times and locations of the measurements. This means that the averaging process affects the measurement and the modeled data the same way.

3. 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).



We are not interested in year specific differences, but in underlying patterns. Therefore we averaged over five years.

4. 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 Globalview comparison.

Aside from the biosphere-atmosphere exchange, the GEOS-Chem model was not changed, and so the transport should affect all model runs in the same way. The paper does not focus on transport, but we agree that this is an important issue that could use further study.

Specific Comments

1. 12763, Line 13 – Is a one-year spinup sufficient? It’s usually 2-3 years.

The model runs were started with the restart files provided by GEOS-Chem (starting at 01.01.2005). Runs using these restart files and different spin up times did not produce different results.

2. 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?

Yes, the reviewer is correct. We clarified our approach in the text and referred to Table 1.

3. 12764, Line 1 – Why is an intermediate resolution of 5.5° used if the terrestrial model is 1° and the atmospheric model is 2°?

Olsen and Randerson (2004) used the off-line model MATCH that uses meteorological fields, derived from the NCAR Community Climate Model version 3 with T21 horizontal resolution (5.5 x 5.5 grid). The fluxes were calculated with 1°x1° data ( Andres et. al (1996) and Takahashi et. al (1999) ). As inventory in GEOS-CHEM, the data are scaled to fit the grid size used in GEOS-Chem.

4. 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?

See answer to General Comments II, comment 2.

5. 12766, Line 17 – Tables should be numbered consecutively according to their citation in the text

Thank you for pointing out this error. We have corrected this in the revised document.

6. 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

As suggested, we have expanded Figure 1. The Figure shows the given input for all three biosphere models, which are used for the NEE estimation. This Figure is not part of the analysis, but shows the raw input. Therefore the CO2 is not detrended and we do not show a single calendar year. The Figure implies that IAV will not play a role. The term "NEE" is used as the data are used as NEE input in GEOS-Chem.

7. 12767, Line 4-5 – Boreal forests have a smaller latitude range

Thanks, we corrected the numbers to 50°-70°.

8. 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%).



The uptake is not isolated for May-Sept. months and 30°-90° N. Compared to the other models, the uptake is large (Table 2).

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

We included the reference.

10. 12767, Line 20-21 – Suggest using broad vs sharp rather than wide vs narrow

We corrected the wording.

11. 12767, Line 26 – Suggest using concentration or mole ratio rather than abundance

We followed the suggestion and used "concentration" instead.

12. 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 ∼45°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?

Concerning the averaging approach, see answer to question 3, Comments II. The importance of Figure 2 is to show that it does not matter if the results are averaged over the entire hemisphere or analyzed with TCCON sites. Therefore, we can analyze GEOS-Chem results with TCCON data.

13. 12769, Line 1 – "turning points" should be "zero crossing times"

We corrected the wording.

14. 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?

The NEE amplitude in GBiome-BGC was larger than CASA in Figure 1 because GBiome-BGC is not balanced. This brings up an interesting point. The zonal spatial

distribution within boreal latitudes does not make a large difference to the seasonality of XCO2 (Keppel-Aleks) because mixing is sufficiently fast to largely homogenize the airmass. Rather than the spatial pattern, the fact that the uptake in CASA takes a longer time produces the weaker amplitude in this simulation.

15. 12770, Line 4 – Please provide more detail on Keppel-Aleks 2012, particularly which sites/how the comparison was made.

We clarified this point as follows: "Keppel-Aleks et al. (2012) demonstrated that simulations of column CO2 at TCCON sites in North America could be significantly improved by [. . .]".

16. 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 ïňĆuxes remain neutral?

Figure 1 shows the input files of the three original biosphere models. The revision of CASA NEE was done according to the approach by Keppel-Aleks et al. For further discussion of this approach, we would like to refer to their publications.

17. 12770, Line 21 – +2 $\pm$ 1 is not significantly better than -3 $\pm$ 1 days

The wording was misleading. The improvement lies in the improved optimization as is now mentioned in the text.

18. 12770, Line 25 – Should reference Table 7

We inserted the reference.

19. 12770, Line 28 to 12771, Line 2 – The revisions made to the CASA fluxes would not be characterized as "small changes"

We changed it to "changes".



20. 12771, Line 2-3 – This sentence referring to local variability and synoptic scales does not follow, it seems out of place

We changed this sentence as follows: "Hence, the NEE distribution on large scales drives local variability in atmospheric total column CO2."

21. 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

The reviewer is right, we inserted the word "modeled".

22. 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.

In the revised manuscript, we have removed this section as it detracts from the main message.

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

We disagree on this point. In order to clarify on the main topics of the results, however, we edited the last two paragraphs as follows: "Large-scale errors in the estimates are identified through a comparison with TCCON data and the CO2 simulations are highly dependent on the choice of the atmosphere-biosphere model. It could be shown that the accurate estimation of carbon fluxes is crucial for the correct simulation of the seasonal carbon cycle. Generalized this means that errors on a large scale must be carefully evaluated before retrieving local fluxes. Variations in the total column are a

good validation resource for diagnosing errors in the hemispheric scale in the estimates of these fluxes, because they provide information on the largest scales. The inconsistency of some atmospheric inverse model results with vertical aircraft proïňĄles and total column measurements shown in recent studies point to a general problem in inverse estimates of carbon fluxes (e.g., Stephens et al., 2007). The inverse machinery must span hemispheric scales, otherwise errors in the inferred distribution at one spatial scale can alias into errors at other spatial scales. Variability in local CO2 concentrations is affected even by variations in the CO2 distribution on hemispheric scale. This means that atmospheric inverse modeling must extend globally to retrieve fluxes. We suggest that an inverse model designed to retrieve the north-south distribution of the fluxes from total column measurements and local fluxes from in situ surface sampling would be helpful."

24. 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.

We followed the suggestion and removed Table 2.

25. 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.

We agree and have removed Table 4.

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