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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.

Anonymous Referee #1

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General:

The work doesn't live up to the hype of the paper title. Using climatology is not as good as year-to-year NEE estimates – yep, trash in trash out! Unsupported statements abound in the paper. On balance, I don't think this is worth publishing in ACP without a substantial revision.

Specific:

1) Introduction. Reads like a review. I suggest this be more focused on the study.

2) Introduction. Posterior estimates do not always result in the optimal solution unless the inverse method used fully describes all the error covariance information.

3) Page 12761, paragraph starting at line 23. This is a very simplistic description of the situation, and not entirely accurate. If in situ boundary layer measurements were mostly driven by local sources the NOAA/ESRL network would not be as useful as it is.

4) Page 12762, line 4. The authors showed that the seasonal CO2 amplitude in the total column measurements is dominated by. . ..WHAT? At all TCCON sites? This must be just inaccurate prose. The authors have a model of CO2 for which they should have a capability to understand how much of the observed CO2 variations at different TCCON sites is due to different sources and sinks. They appear not to have used it (see also section 7).

5) Page 12762, line 8. In a sensitivity study, the NEE was enhanced by 40% in the boreal forest and the onset of the growing season was shifted earlier. This is an ad hoc approach for which, in my opinion, no robust conclusions can be drawn.

6) Section 2. The authors use the standard GEOS-Chem CO2 simulation that uses CASA NEE estimates for 2000. Obviously, these estimates are going to be inconsistent with data 2006-2010. I'm not sure why this is such a major result of the paper. The conclusion that SIB is better than CASA is unsupported unless the authors use year specific CASA fluxes for the study period.

7) Section 6. Please provide the averaging kernel equation for the reader.

8) Section 6. Just out of interest what is the error introduced by using NCEP data interpolated to the TCCON station? I suspect this will be reasonably large. Is NCEP data consistent with GEOS data used by GEOS-Chem?

9) Section 6. TCCON is stated to have a precision of better than 0.25% or 1ppm, with 0.1% being achieved sometimes(?). Presumably this is based on a wealth of coincident aircraft profiles (through the troposphere) at each TCCON site over a complete seasonal cycle? If yes, say so. A precision of 1 ppm is very useful.

10) Section 7, page 12768. This implies that studying these differences at the four TC-

CON gives information about the GEOS-Chem CO2 simulation for nearly the Northern Hemisphere. Wow, what a bold statement! Just because the differences are similar to those integrated over the whole NH doesn't support this statement.

11) Section 7.1, page 12768. "Comparing the XCO2, model values reveal the same yearly pattern as seen for the GEOS-Chem CO2 simulations at 700 hPa." I am not sure where the authors were going with this.

12) Section 7.1, page 12768, line 26. To analyze results in more detail the authors average over more years? Perhaps they mean to draw some more general conclusions that are not year specific? Otherwise, this reader is confused.

13) Section 7.2. The decision to enhance NEE over boreal forest by 40% is opaque. Are they scaling to the year 2000 CASA distribution? This is ad hoc science at its best. Just use year specific CASA fluxes! See point 6. If you're scaling the boreal fluxes then why should you expect a better correlation? The result show that the model can reproduce 1.7% more the observed variation– is this significant? Table 7 shows clearly that there is very little change in the correlation by using the range of NEE flux estimates.

14) Section 7.3. By using year-specific and constant year SIB fluxes there is no change in the correlation – doesn't this suggest that the spatial distribution of the fluxes does not change dramatically each year? What about bias? The conclusion of this section is that using year-specific biospheric fluxes is important.

15) Figures 3, 4, 6, 8, 9 and 10 should include correlations and bias values for model calculations.

16) Figure 11 is too small to read.

17) Figure 12 shows there is little difference between the models when they are compared with GLOBALVIEW. The authors show the mean different is small over the sites but they should also report the

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