**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 #1 for "Evaluation of atmosphere-biosphere exchange estimations with TCCON measurements" by J. Messerschmidt et al.

First of all we would like to thank the referee #1 for his feedback. We considered all his concerns and answered them in detail below.

Specific

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

We disagree and ask the editor to provide guidance. We don't understand what criteria the reviewer used in this off-the-cuff comment.

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

Agree. We changed the offending sentence: "The resulting a posteriori flux estimates are optimally determined by the assigned error covariance based on the assumed a priori distribution, the observations and the atmospheric transport model."

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.

The authors aimed to describe advantages and the additional information given by total column measurements. These measurements therewith complement the existing measurement sites. We did not intent to imply in anyway that in situ boundary layer measurements are useless. To make this clear, we changed the last sentence: "Hence, total column measurements act as an extension of the in-situ continental boundary layer network, which provides excellent constraints at local and regional scale."

4) Page 12762, line 4. The authors showed that the seasonal CO_2 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 CO_2 for which they should have a capability to understand how much of the observed CO_2 variations at different TCCON sites is due to different sources and sinks. They appear not to have used it (see also section 7).

Keppel-Aleks et al. (2012) analyzed the influence of regional and synoptic dynamics on total column measurements at TCCON sites. They could show that these measurements are mainly dominated by the NEE in the boreal forest. We are not sure if this reviewer is aware of the relevant and cited literature.

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.

This paragraph is a description of the analysis done by Keppel-Aleks et al. (2012).

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.

We disagree. First of all, GEOS-Chem is using CASA NEE estimates for 2000 for all years. In order to evaluate GEOS-Chem, it is useful to analyze the CASA NEE estimates for 2000. Additionally, one important result of our study is that year-specific NEE only slightly improves the agreement with the measured seasonal cycle, suggesting that the CO2 seasonal cycle is mainly driven by the spatial flux distribution and the atmospheric dynamics.

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

The averaging kernel equation is given in all technical papers written by the TCCON community, cited in Section 6. This manuscript is not a methodical paper about TCCON, we suggest that the interested reader be pointed to the fully described averaging kernel discussion included in the relevant technical publications. Nevertheless, if the editor feels that this is important, we can expand this section copying the relevant technical information.

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?

This is described in Wunch et. al, 2011 (Wunch, D., Toon, G. C., Blavier, J.-F. L., Washenfelder, R. A., Notholt, J., Connor, B. J., Griffith, D. W. T., Sherlock, V., and Wennberg, P. O.: The Total Carbon Column Observing Network, Philosophical Trans-

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actions of the Royal Society A: Mathematical,Physical and Engineering Sciences, 369, 2087–2112,doi:10.1098/rsta.2010.0240). The authors ran a sensitivity study where the NCEP a priori pressure, temperature and H2O profiles were perturbed by 1K, -0.1% and -5%, respectively. These changes each resulted in differences in retrieved Xco2 of less than 0.08% (or ∼0.3 ppm).

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.

"Sometimes" means under optimal solar viewing conditions (cloud-free viewing). The precision was estimated from high profile aircraft calibration campaigns, described in the cited publications.

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

As discussed, this follows directly from the peer-reviewed literature. Keppel-Aleks et al. (2011,2012) and Yang et al., 2007.

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.

This sentence just states that the comparison at one level (700 hPa) has the same annual pattern as the total column measurements. This is not obvious. It shows that 700 hPa is representative for total column measurements and that total column measurements are sensitive to 700 hPa.

12) Section 7.1, page 12768, line 26. To analyze results in more detail the authors

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average over more years? Perhaps they mean to draw some more general conclusions that are not year specific? Otherwise, this reader is confused.

The reviewer is correct. To clarify, we have changed the sentence to "In order to analyze the general patterns, the monthly means of the five years were averaged to give a mean seasonal cycle for each NEE input as well as for the TCCON data (Fig. 4)."

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.

See answer to point 6.

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.

To clarify this point, we changed the second paragraph as follows: "Figure 8 shows spatial anomalies of growing season net flux from the five-year climatological average. Interannual variability is clearly evident throughout North America, Europe, and Boreal Asia, with anomalies representing 10% or less of the climatological average. Figure 9 shows the monthly averages of the GEOS-Chem CO_2 simulation using year-specific NEE estimations, as already depicted in Fig. 3, and the GEOS-Chem CO_2 simulation using SIB 2009 NEE estimations for the entire time period. Despite interannual variability in the spatial distribution of NEE, the anomalies tend to be weak or cancel in the spatial average such that differences in seasonal cycles aggregated over high latitudes is small. The scatter plot [. . .]."

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

The bias is the model – observation differences in the annual (and 5 years) means and indicated.

16) Figure 11 is too small to read.

We agree; however, we have decided to remove this figure as it is tangential to the thrust of the study.

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 [. . .]?

As with Figure 11, we have decided to remove this figure as it is tangential to the thrust of the study.