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Biogeosciences Discussions

Interactive comment on "The imprint of surface fluxes and transport on variations in total column carbon dioxide" *by* G. Keppel-Aleks et al.

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We thank the reviewer for her/his thoughtful comments and address her/his suggestions below:

This manuscript uses northern hemisphere TCCON observations and model simulations to investigate the contributions from biospheric surface fluxes and transport processes to column-averaged CO2 mole fractions. The manuscript addresses a relevant scientific issue, since the carbon cycle science community has recently begun to apply new $\langle CO_2 \rangle$ *satellite observations to the estimation of* CO_2 *sources and sinks. The scientific quality and presentation quality of this paper are both good. Overall, this is a useful paper but I have some issues with their method, analysis and interpretation.*

The main issue that I have deals with the authors use and conclusions about what they refer to as "the CASA model", but is actually the output from one specific CASA model simulation. According to their description, this simulation is described in Olsen and Randerson (2004). The authors state that it has 3-h resolution and corresponds to the year 2001 (although I believe it is actually 2000). The Olsen and Randerson (2004) fluxes are available at http://ess.uci.edu/j̃randers/data/Diurnal_CASA/ . The available fluxes have a very coarse resolution of 32x64 gridboxes, or 5.625 deg x 5.625 deg. The fluxes give the seasonal and diurnal cycles, but are balanced to zero net annual uptake. Interpretation of simulated atmospheric CO_2 *using these CASA fluxes must acknowledge these limitations. To put it another way, any evaluation of the fluxes with atmospheric measurements (Figure 12) should not be interpreted (or referred to) as an evaluation of CASA fluxes, but rather of one specific CASA run being used in a simulation for a year to which it does not correspond.I strongly recommend that the authors clarify this by changing their terminology from statements like "CASA biospheric fluxes underestimate . . ." to something like "fluxes from the CASA model simulation used in this work underestimate . . .", beginning with the statement in their abstract, and also throughout the rest of the paper. On a related note, the fact that the growing season NEE in the CASA run is smaller in magnitude than CarbonTracker (Figure 13) is to be expected for a region where the biosphere is a net sink, since the CASA run is neutral (no net uptake) and assimilation of atmospheric observations would result in net biospheric uptake.*

We acknowledge that there are limitations to our approach, which uses climatological mean, balanced fluxes to simulate $\langle CO_2 \rangle$. However, we feel that the use of the Olsen and Randerson(2004) fluxes provides a representative estimate of net ecosystem exchange, and one that is used extensively by the carbon cycle community. The CASA fluxes we used in our model are climatological mean NEE, developed from the dataset compiled in Randerson et al., 1997. These are the same fluxes used in the Transcom inversion studies, and are closely related to the CASA-GFED prior that is used by CarbonTracker. To incorporate diurnal and synoptic-scale variations in the fluxes, Olsen and Randerson (2004) redistributed climatological NEE within each month based on year 2000 meteorology; the monthly net flux at any pixel was unchanged as was the annual mean flux.

By averaging five years of transport model output from climatological mean fluxes, we determine the climatological north-south gradient and seasonal cycle amplitude in $\langle CO_2 \rangle$, which can be compared to our observations. Based on our multi-year datasets from Park Falls and Lamont, it is clear that the seasonality and the north-south gradient are underestimated in these fluxes. We have reworded our discussion of the CASA fluxes to more clearly express some of the limitations, but also the validity, of using these fluxes.

Although CarbonTracker optimized fluxes infer a net sink in the boreal region while the prior is annually balanced, it remains true that the optimized CarbonTracker fluxes have larger seasonality and larger growing season flux than the CASA prior. A regional sink could also be inferred by reducing ecosystem respiration outside the growing season, which would reduce the seasonality of the net fluxes.

1) The citation to Rayner and O'Brien (2001) in the introduction (p7478) is an odd choice. While it is an important paper, it is not an example of inverse modeling with a variety of approaches, but rather a study to quantify an error threshold for satellite remote sensing observations of column CO_2 using simulated data. Since the statement associated with the citation is general enough, the citation should simply be removed here, although there might be somewhere else to cite the paper where it is relevant to the discussion of CO_2 columns.

We have removed the citation.

2) The description of the use of dynamical tracers and meridional displacement, located

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directly before equation 1 (p7482), could be clearer.

As per the first reviewer, we have modified the text to clarify the discussion of dynamical tracers; we also have added a recommendation that readers first review the companion paper in Atmospheric Chemistry and Physics for more complete discussion of this method.

3) The global fossil fuel emission value of 5.5 PgC for 1995 from CDIAC national totals (p7487) needs to be checked because it appears too low to me. I think the correct value is closer to 6.0 PgC for 1995.

The global emissions underlying our model were 5.5 PgC per year, and are based on a composite year. We have amended the text to reflect that these fluxes are not actually 1990 fluxes.

4) The ~2 ppm diurnal amplitude in $\langle CO_2 \rangle$ *mentioned in section 3.1 (p7489) and shown in Figure 4, although easily derived from the TCCON data, is significant since it is larger than the model-derived value of ~1 ppm in Olsen and Randerson (2004), which is often quoted. The authors might want to mention this somewhere, since I am inclined to think that their higher, measurement-based value is more reliable.*

We have added a statement to underscore the difference between measurement and model to the revised text.

5) The statement "Our findings show that by combining $\langle CO_2 \rangle$ *and boundary layer* CO_2 *observations, we can properly attribute variability to local or large scale influences based on the correlation of* $\langle CO_2 \rangle$ *with theta." (p7498, lines 23-24) is not accurate since this has not fully been shown. It would be more accurate to say "Our findings suggest*

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that perhaps by combining ⟨CO_2⟩ and boundary layer CO2 observations, we can properly attribute variability to local or large scale influences based on the correlation of ⟨CO_2⟩ with theta.'"

We have changed the text to reflect that our results only suggest, rather than conclusively show, that column and boundary layer observations can be used together to attribute variability to either local or large scale influences.

Figure 6 caption should state that these observations are from the INTEX-NA campaign, to clarify that they are aircraft profiles. Furthermore, Figure 6 demonstrates that the shape of the CO_2 profile is not easy to determine a priori, yet this work states that TCCON retrievals scale a model profile when calculating the column. I interpret this to mean that the shape of the profile is not changed in the TCCON retrieval. Obviously, this will be less of a problem at the TCCON vertical resolution than for the aircraft profiles, but does this highlight a weakness in the TCCON retrieval method?

We have changed the Figure 6 caption to indicate that the profiles were obtained during INTEX-NA. The reviewer is correct that the shape of the profile is not changed by the TCCON retrieval. We have analyzed the impact of the a priori profile shape in previous studies (e.g., Washenfelder et al., 2006 and Wunch et al., 2010), and find that the retrieved column abundance differs by less than 0.1% when a different shape is assumed.

Figure 7. The figure would be much clearer (especially to those with impaired color vision) if an open symbol was used for one of the two sites.

We have made this change in the revised paper.

Figure 8. It is not stated what the zero reference value is for ⟨CO_2⟩ or for theta. This

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needs to be clarified because the figure is not showing daily median $\langle CO_2 \rangle$ which would be around 385 ppm.

We subtracted the mean $\langle CO_2 \rangle$ and θ for the two month period shown to recenter the data. We have clarified this in the figure caption.

Figure 9. Same comment as Figure 8 applies, but also should state that positive values mean North.

We have changed the figure caption to reflect that the $\langle CO_2 \rangle$ values plotted are referenced to Lauder, New Zealand.

Figure 12. Same comment as Figure 8 applies.

Again, we have changed the figure caption to reflect that the $\langle CO_2 \rangle$ values plotted are referenced to Lauder, New Zealand.

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