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Discussions

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

Anonymous Referee #2

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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 <CO2> satellite observations to the estimation of CO2 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/∼jranders/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 CO2 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.

Specific Points

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 CO2 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 CO2 columns.

2) The description of the use of dynamical tracers and meridional displacement, located directly before equation 1 (p7482), could be clearer.

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.

4) The ∼2 ppm diurnal amplitude in <CO2> 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.

5) The statement "Our findings show that by combining <CO2> and boundary layer CO2 observations, we can properly attribute variability to local or large scale influences based on the correlation of <CO2> 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 that perhaps by combining <CO2> and boundary layer CO2 observations, we can properly attribute variability to local or large scale influences based on the correlation of <CO2> with theta."

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 CO2 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?

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.

Figure 8. It is not stated what the zero reference value is for <CO2> or for theta. This needs to be clarified because the figure is not showing "daily median <CO2>" which would be around 385 ppm.

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

Figure 12. Same comment as Figure 8 applies.

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