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Interactive comment on "Drivers of column-average CO_2 variability at Southern Hemispheric total carbon column observing network sites" *by* N. M. Deutscher et al.

Anonymous Referee #2

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In "Drivers of column-average CO2 variability at Southern Hemispheric total carbon column observing network sites", Deutscher et al analyze simulations of total column CO2 at three southern hemisphere TCCON sites for comparison with observations. The simulations used tagged tracers to separately track fossil, terrestrial biosphere, ocean, and biomass burning fluxes from different regions of the globe. As such, Deutscher et al. are able to attribute variability at seasonal timescales to various source processes and source regions. The authors also focus on a large mismatch in the phasing of the seasonal cycle in column CO2 at the tropical site, Darwin, between observations and models in 2006, which they suggest results from early onset of monsoon rains drawing down CO_2 earlier than usual.

The paper is clear and well-written, and is an important step in understanding how to utilize column CO_2 observations for carbon cycle science. I would suggest that more analysis on how the column observations provide different information from southern hemisphere surface CO_2 observations would make this more clear. The authors state that this will be part of a future analysis, but from the manuscript it is unclear what are the new findings that may allow total column CO_2 to be used differently than previous data streams. A major problem with this paper is that it is quite tied to CarbonTracker results, which result from assimilating surface CO_2 observations. Because surface observations are sparse in the southern hemisphere, the assimilation of atmospheric observations does not yield much added information about fluxes, as the authors acknowledge. Therefore, given the large inherent uncertainty in how CarbonTracker partitions fluxes, it would have been nice to see additional model runs using biospheric fluxes from other biogeochemical models, particularly those that do not use atmospheric CO_2 observations in the flux estimate. To a certain extent, the authors address the lack of generalizability of CarbonTracker fluxes by using the monthly pulse fluxes to represent biospheric fluxes in the tropical Australian region, but I don't think the analysis has gone far enough.

Analysis of how transport patterns, particularly tropical transport patterns, affect XCO_2 would have been particularly useful and would make the results from this paper more broadly applicable for interpretation of XCO_2 observations from satellites as well as other upcoming tropical measurements. For example, South Africa and South America are grouped together as "other" remote tropical fluxes, but understanding seasonal and interannual variations in tropical forest and savannah fluxes on these two continents is an important goal. If the authors could demonstrate whether tropical fluxes from different continents leave unique imprints on the XCO_2 at Darwin site, or in the other southern hemisphere TCCON sites, which are much "quieter" than the northern hemisphere sites, this would be a very compelling and unique use for column observa-

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tions. Such analysis would likely require using a transport model outside the TM3/TM5 family since convection patterns in an individual model would likely be a significant determinant of how remote tropical fluxes impact the XCO2 at TCCON sites.

The authors provide a justification for considering only variations greater than 0.4 umol/mol "detectable" based on a 0.1 umol/mol precision when averaging across a day and a 0.3 umol/mol error from the airmass correction. This threshold is not so much used in this paper as introduced conceptually, but I wonder what its actual implications are. For instance, the authors suggest that the biomass burning signals are generally undetectable, but in Fig. 6: the seasonal amplitude (peak - trough) for biomass burning at Darwin exceeds 0.4 ppm. Since most of the error (0.3/0.4 umol/mol) is systematic error from the airmass correction, the data could be analyzed and interpreted to minimize the influence of this correction. For instance, at Wollongong, taking the difference in XCO2_bb between months with similar solar zenith (say, March and October) angles/airmasses would yield a signal greater than 0.1 ppm (and by eye it looks like this is even the case at Lauder). Additionally, interannual variability would be detectable at XCO2 variations of 0.1 umol/mol because the airmass correction should be the same for a given month from year to year.

In Section 5.0, the authors state that the IAV during May-September is quite small at Darwin, corresponding to the dry season. Is this flux-driven or also transport-driven?

In Section 5.2, it is worth noting that earlier rainfall may also lead to enhanced and earlier increases in heterotrophic respiration, partially counteracting the effect of rainfall on stimulating photosynthesis.

In the discussion of biomass burning (Section 5.4), the authors use the term "remote" and "local", but don't define what they mean. It would seem that Indonesian fires are much more "local" to Darwin than to the SH midlatitude site and would potentially leave a larger and differently phased signal at Darwin than at Lauder or Wollongong, in contrast to what is stated on 14349.

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Interactive Comment

In the conclusions, the authors could be a bit more quantitative as to the influence of the local, remote, and NH TB on the mean annual cycle amplitude at each site.

Fig. 9: Why the residual BB enhancement in XCO2 in 2007 but not 2006?

Suggestions for figures:

Fig. 1: the green circles for the TCCON sites does not show up well against the gray background. Perhaps use a different color or larger symbols.

Fig. 2: Vertical gridlines, extending up from each 1 January tick mark on the x-axis would be helpful.

Fig. 3: Nice figure – maybe consider in the Darwin plot adding a thin trace for the 2006 anomalous year that becomes a large focus later on in the paper, so the reader can easily see how this year differs from the observed mean annual cycle and the simulated mean annual cycle?

Fig. 4: The dashes in the interannual variability subpanels make it harder on the reader – use solid lines or try shading around the lines in the main subplots?

Fig. 5: It would be nice if the local Australian fluxes stood out. Consider using warm colors for local fluxes and cool colors for remote fluxes, or making the local fluxes a slightly thicker line.

Fig. 7: Panels are reversed relative to the figure caption.

Fig. 8: It might be nice to subtract off (or add a dashed line indicating) the NH mean dXCO2/dt trend to more clearly see when the growth rate is above/below average.

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