

Interactive comment on “Drivers of column-average CO₂ variability at Southern Hemispheric total carbon column observing network sites” by N. M. Deutscher et al.

Anonymous Referee #1

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Review of “Drivers of column-average CO₂ variability at Southern Hemispheric total carbon column observing network sites”, by N.M. Deutscher et al.

Following earlier work on the seasonal cycle of column-averaged CO₂ in the Northern Hemisphere (e.g., by Yang et al, 2007), the authors have looked at the seasonal cycle of column-averaged CO₂ at three Southern Hemisphere TCCON sites, and whether a modeled CO₂ field (CT2011 posterior CO₂) can match their seasonal and inter-annual variations. In cases where there are significant model-observation differences, the authors have tried to explain where they come from.

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The goal in itself is quite interesting and novel, as is the approach of breaking down the total column signal by source category and geographical footprint. However, to determine whether a certain manuscript should be published in this journal, one question is the acid test: “Does this work say something new or puzzling about either the atmosphere or its interaction with the other reservoirs at its boundaries?”

The chief conclusion of this work seems to be that the seasonal cycle of column averaged CO₂ (XCO₂) in the Southern Hemisphere is mostly determined by the terrestrial biosphere, although biomass burning emissions can be important as well. Specifically, at Darwin, the mean seasonal cycle is mostly determined by the surface CO₂ flux from the Australian Tropical region. By itself, the first conclusion is neither new nor puzzling. We already know from existing flux estimates, including CT2011 (which is used by the authors), that the biosphere and biomass burning components together constitute the largest seasonal variation. Therefore it is unsurprising that those components should show up as the majority contributors to the mean seasonal cycle (MSC) of XCO₂. The decomposition of the twin-peaked MSC at Darwin as a single-peaked time series from the Northern Hemispheric biosphere added to a double-peaked signature from Tropical Australia is interesting, but to my understanding that is the only interesting result presented in this manuscript. Of the other novel ideas presented, none have been proved concretely, as I will explain below.

The authors use posterior CO₂ concentrations of CT2011 to distinguish between the MSCs due to the biosphere and biomass burning. However, as they themselves acknowledge (P14341, L1), it is impossible for a data assimilation system such as CT to disentangle terrestrial biosphere fluxes from biomass burning ones, and errors made in prescribing the biomass burning fluxes are aliased into estimates of biosphere fluxes. In areas with frequent fires such as South East Asia (which is relevant for the signal at Darwin), it is quite possible for GFED to underestimate biomass burning emissions. Therefore, the argument the authors make in section 4.1 (and throughout the manuscript) by separating biosphere and biomass burning fluxes stands on very shaky

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ground at best.

On pages 14346 and 14347, the authors point to the early onset of the 2005 monsoons as a factor behind enhanced photosynthesis in 2006 (the so-called relief of water-stress), resulting in a shallower XCO₂ maximum in the middle of 2006. However, it is not at all clear to me why an early onset of the monsoons in November 2005 should relieve water stress in July/August 2006. In fact, in figure 11 I see a shallower minimum in Feb-Apr 2006; does this mean the earlier onset of the monsoons causes less productivity in the following months?

The authors try to pinpoint the factors behind the model-observation mismatch of XCO₂ at Darwin, and ultimately pin it on surface fluxes from Tropical Australia. All this is done, however, with a single flux inversion (CT) using a single transport model (TM5). There is no attempt to see whether this problem at Darwin is systematic across different inversion frameworks and different transport models. The authors could have used the inversion products from <http://transcom.lsce.ipsl.fr/>, for example, to check whether the CT results are typical of other inversions or not. As it stands now, the manuscript reads more like an evaluation of the CT posterior fields and less like a significant statement about our ability to estimate surface CO₂ fluxes, or our ability to model column averaged CO₂. Moreover, if the Australian Tropical flux in CT2011 is the culprit, how come the surface time series at Cape Ferguson (http://www.esrl.noaa.gov/gmd/ccgg/carbontracker/co2timeseries.php?site=CFA_02D0&year) does not show big discrepancies as well?

A few other comments/questions:

1. P14334, L13. “Vertically integrated CO₂ . . . than in situ measurements.” This is a common-sense argument, but not necessarily true. While a wrongly modeled boundary layer will cause less of an error in the column averaged CO₂, the signature of surface fluxes in the total column is also smaller than at the surface. Thus, compared to the size of the signal one is trying to extract from the measurements, errors in the total column

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CO₂ can be just as important as errors in the boundary layer CO₂. This is compounded by the fact that the CO₂ signal travels quite fast laterally in the free troposphere, thus wrong boundary layer modeling can introduce errors in surface fluxes far away from the XCO₂ measurement location.

2. P14335, L6. Why is the underestimation only of the strength of the boreal seasonal cycle and not of the temperate as well?

3. P14337, L18. Why is the model sampled at 00:00 UT and not co-sampled with TCCON measurements?

4. Section 4.2 (and 5.2). I do not follow how the “fossil fuel” tracer can diagnose inter-hemispheric transport (IHT). The experiment performed by the authors only shows the impact of IHT on XCO₂ at the sites given the source patterns corresponding to fossil fuel emissions. Biosphere and biomass burning fluxes have very different source patterns, so the impact of IHT on XCO₂ could be different. If the goal was instead to evaluate whether the TM5 IHT was close to the “truth”, then why not use an SF₆ simulation?

5. P14348, L11. Why is a 40% underestimation in the strength of the uptake unrealistic? Isn't this within the ballpark of the underestimation found by Yang et al (2007)? CT does not assimilate any measurements from boreal Eurasia, so I can imagine such an underestimation in the uptake by that region.

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