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Interactive comment on "Global CO$_2$ fluxes estimated from GOSAT retrievals of total column CO$_2$" *by* S. Basu et al.

Anonymous Referee #3

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Global CO2 fluxes estimated from GOSAT retrievals of total column CO2 describes the authors' inversion of GOSAT total column CO2 data, in addition to in situ surface data, to constrain ocean and net ecosystem exchange fluxes. Overall, the authors seem to have constructed a very solid method for the inversion. The authors discuss nicely which features of the inversion they think are robust, and which may be artifacts, and the paper should be published as quality work with a novel data set. It is, however, unclear what the paper contributes to our understanding of the global carbon cycle. Figures 3-5 show the inversions with surface, GOSAT, and surface+GOSAT nudging the seasonal cycles in the right direction toward observations compared to the prior, but a regional break-down of the fluxes in Figures 7 and 8 show large disparities between where the different inversions are putting the fluxes, suggesting that the

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improved seasonal agreement is spurious or just reflects global-scale problems with seasonal carbon exchange in the prior.

The authors described quite nicely the details of their inversion, including their accounting for the initial condition problem. The authors have also added flexibility to their inversion by not having zero-flux gridboxes in the prior that could not be scaled up or down according to the data. One issue I took was with the validation approach for the authors' results. The authors average the TCCON observations and simulated total columns (from the optimized fluxes) over seven days to get ride of high frequency variations, but the DA system should capture these synoptic variations as they are quite sensitive to the large scale gradients (as well as the model's horizontal transport, which shouldn't be the dominant source of error if we are to believe the inverse results).

The authors also ran several sensitivity studies to quantify how model errors may impact the inversion. While this was a good start, I am not convinced that the tests fully explored the sensitivity of the results to potential errors. For instance, Test 1 on P 4553-4554, in which vertical resolution was changed, will result in different vertical transport patterns, but the authors do not provide a rationale as to why they expect that this sensitivity test fully explores the possible errors in vertical mixing.

Specific Comments

P 4538, L 6: "The poor performance of AIRS and TOVS..." If a perfect atmospheric transport model existed, would this statement still be true? If so, the authors should cite the relevant study that demonstrated this point with synthetic data, otherwise the authors should temper the assertion.

P 4538 L 20: "Tanso measures the intensity of reflected sunlight..." This sentence is largely repeating previously stated information

P4542 L 17: "GOSAT observations are screened for..." Are these additional criteria beyond the checks described in Butz et al., or are the authors reiterating the same

criteria? Please clarify. Also, how is RemoteC validating ocean pixels?

P 4546 L 7: "The uncertainty in Xfire is much smaller than the uncertainty in Xbio". This might be true in absolute fluxes, but not necessarily in relative fluxes.

P 4547 L 12: The authors should refer the reader to Table 1 here.

P 4547 L 16: Setting the sigma0 value to a small, non-zero number so that the inversion can adjust the fluxes in gridboxes with zero prior emissions seems like a good addition to the authors' framework that has not been done in other inversions. Were there any coherent patterns in space or time where the inversion scaled up/down the fluxes in these regions with close to zero prior fluxes?

P 4556, L 18: "Of those, soundings were deemed coincident ... within 0.5 ppm of the modeled XCO2 over the TCCON station". Coincidence criteria based on the variable that is under examination shouldn't be used. Wunch et al., 2011 present a more rigorous methodology for coincidence with the ground based network.

P 4569, L 24: "This suggests that at present, different XCO2 measurements consistent with the same set of TCCON XCO2 can yeidl dramatically different posterior flux distributions" How much of this difference is tied to the short period over which data were assimilated? The results of the authors' inversions show that the total land+ocean sink is unconstrained over the GOSAT measurement period – would 3 full years of data force the global sink estimates into convergence? 5 years? And if so, would this have an effect on the resulting northern vs tropical net sink distribution?

Figures

Figure 1 should have a legend for the marker size (or instead of using size, use color to denote the number of observations).

Figure 4: The authors focus on the seasonal cycle mismatch between the observations and the timeseries resulting from GOSAT-inverted fluxes, but the large synoptic variation, relative to the other timeseries, is quite striking. What drives this large difference?

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Is it indicative of larger horizontal gradients, or some sort of instability in local fluxes?

Stylistic Points

The authors should organize multiple citations within a single set of parentheses chronologically; at present, the order is seemingly random.

P 4541 L 27: "... that our coarse transport model cannot possibly resolve." perhaps more accurate to say "was not designed to resolve".

P 4543 L 3: "Going by the number of samples...". I don't like how the authors lead into this paragraph with a strawman argument. The authors should instead rely on making factual statements.

The authors should follow the journal's stylistic conventions for bold font for vectors and matricies.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 4535, 2013.

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