

## Response to anonymous referee 3

S. Basu, et al.

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We thank the referee for the constructive feedback. Please find below our responses.

One of the criticisms the referee has is that it's unclear what our manuscript contributes to the understanding of the global carbon cycle. We would argue that it takes several years before a new data set or instrument can contribute significantly to our knowledge of the carbon cycle. Right now the entire community is going through a learning process to figure out how to best use GOSAT data, and therefore it is important to report on progress made in that direction. For example, none of Chevallier et al. [2005], Chevallier et al. [2009] or Nassar et al. [2011] had anything dramatically new to say about the global carbon cycle, but each one of them was a valuable lesson on what we can and cannot learn from a new satellite instrument. We consider our manuscript to be similar in nature, in that it makes statements about the capability of GOSAT to constrain the global carbon cycle. Those capabilities should hopefully improve as we get longer term data from GOSAT, and more importantly with the launch of future CO<sub>2</sub>-sensing missions.

Furthermore, we do see a few results – such as the strengthening of the tropical source and northern extra-tropical sink – that are significant for the carbon cycle, and that have since been confirmed by other research groups as well. We are currently working on a manuscript further exploring those 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.

We only average over seven days for visual clarity. Initially we had plotted the full high-frequency time series for all the inversions, which made it difficult to see which inversion was doing what. Therefore we decided to plot multi-day averages. In fact referee #2 suggested we do the same for the surface time series at Park Falls (for the same reason), so in the revised manuscript we have had to present multi-day averaged surface CO<sub>2</sub> at Park Falls instead of the full time series.

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.

Test 1 does not fully explore the possible errors in vertical mixing. In fact, all the tests mentioned, i.e., changing the horizontal and vertical resolutions, and changing the driving meteorological data, change the vertical mixing. The point we tried to make was that *given our particular transport model*, these tests cover a broad spectrum of possible transport errors. It's true that if we were to use a different transport model, it might have a different vertical mixing, but testing our inversions with a different transport model is beyond the scope of the present study.

One point we would like to make in this regard is that given the almost-flat averaging kernel of GOSAT, inversions assimilating GOSAT data – which are the main focus of this manuscript – are less sensitive to localized errors in vertical transport than an inversion driven only by surface data.

“The poor performance of AIRS and TOVS...” If a perfect atmospheric transport model existed, would this statement still be true?

Neither AIRS nor TOVS has any near-surface sensitivity. Therefore, given a perfect transport model, we would still be constraining surface fluxes from their influence on upper tropospheric CO<sub>2</sub> gradients. These influences are small, i.e., the Greens functions connecting the fluxes to the measurements have small amplitudes. So even if an inversion of, say, TOVS data with a perfect transport model did not degrade the flux estimates [Chevallier et al., 2005], it would not significantly improve the estimates either [Houweling et al., 2004]. This is, of course, assuming perfect measurements. In practice, neither TOVS nor AIRS retrievals are perfect, which is a limitation that even a perfect transport model would not be able to overcome.

“Tanso measures the intensity of reflected sunlight...” This sentence is largely repeating previously stated information

Good point. That sentence has been deleted, and the non-repeated information, i.e., the footprint area, has been incorporated into the previous sentence.

“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?

RemoTeC screening criteria are very similar to the ones described in Butz et al. [2011], though updated as described in Guerlet et al. [2013]. Guerlet et al. use more TCCON stations, longer time series, and refined coincidence criteria to substantiate and improve performance of the post-processing quality filters. For ocean-glint soundings, validation is difficult since there is only little ground-based data available. So far validation of the ocean-glint soundings relies on a few coastal and island-based TCCON sites which, however, overall makes up only a sparse dataset. Quality assurance for the ocean-glint soundings rather relies on very strict data filtering based on the “upper edge” method described in Butz et al. [2013]. The method allows for screening data contaminated by particle scattering effects – one of the most important sources of error – with high confidence, though at the cost of a substantial reduction of the number of soundings.

“The uncertainty in  $x_{\text{fire}}$  is much smaller than the uncertainty in  $x_{\text{bio}}$ ” This might be true in absolute fluxes, but not necessarily in relative fluxes.

In is true that fire and land use change emissions can have relative uncertainties as large as biosphere fluxes. However, in our formulation,  $x_{\text{fire}}$  only includes direct emissions from fires and biomass burning, not the heterotrophic respiration of the burned/slashed product. Some of the uncertainty in fire emissions and land use change comes from this heterotrophic respiration, which in our case (i.e., in the CASA GFED model) is bundled into  $x_{\text{bio}}$ . Therefore we think that our  $x_{\text{fire}}$  is relatively more certain than our  $x_{\text{bio}}$ .

This is not to say that we know  $x_{\text{fire}}$  as well as, say, the fossil fuel emissions. In fact, it is impossible to separate  $x_{\text{fire}}$  from  $x_{\text{bio}}$  from a  $\text{CO}_2$  inversion. Any error made in our assumption of  $x_{\text{fire}}$  will be compensated mostly by adjusting  $x_{\text{bio}}$ . That is why, although in our inversion setup we have two different categories (fire and biosphere), in our results we always show the two of them bundled together, i.e., we subtract the fossil fuel emissions but never the fire emissions, even though it is “imposed”.

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

We refer to Table 1 in line 17, in the same paragraph. To be clear, we have replaced “category-specific parameters” with “category-specific L, T and  $\xi$ ”.

Setting the  $\sigma_0$  value to a small, non-zero number so that the inversion can adjust the fluxes in grid boxes 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?

Not really. There was no coherent pattern for areas of zero uncertainty in the prior flux, and hence there was no coherent pattern of flux correction over those grid cells. In fact, the average flux adjustment (posterior – prior) over cells with zero prior uncertainty was  $\sim 10^{-7}$  the average flux adjustment over the other cells.

“Of those, soundings were deemed coincident... within 0.5 ppm of the modeled  $\text{XCO}_2$  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.

As discussed in Guerlet et al. [2013], the coincidence criterion of Wunch et al. [2011] depends on the transport history of the air mass, while a criterion based on  $\text{XCO}_2$  uses a convolution of transport and sources/sinks, which is more appropriate for a tracer like  $\text{CO}_2$ , which has strong sources and sinks. For example, air masses that have the same transport history could (and often do) have very different  $\text{XCO}_2$  due to their trajectories over different source/sink regions. We refer the referee to Guerlet et al. [2013] for a detailed description of the coincidence criterion and typical maps for coincident soundings. Incidentally, a simpler  $\text{XCO}_2$ -based criterion was also used by Oshchepkov et al. [2012] for their comparison of GOSAT L2 retrieval algorithms.

“This suggests that at present, different  $\text{XCO}_2$  measurements consistent with the same set of TCCON  $\text{XCO}_2$  can yield 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?

There are two questions here. First, as the referee rightly notes, the global budget of  $\text{CO}_2$  is different from a surface-based and a GOSAT-based inversion. This is a spin-up effect, and most of the “missing” mass of  $\text{CO}_2$  between the two inversions is in the upper troposphere. An inversion of 3+ years would converge on the same global budget. The second question is whether our observation that different flux distributions yield the same  $\text{XCO}_2$  at TCCON stations is a spin-up effect as well; the answer is no. This has more to do with the coverage of TCCON stations and the small signal in  $\text{XCO}_2$  due to surface sources and sinks. On the coverage issue, we point to Oshchepkov et al. [2013] and Reuter et al. [2013], who show that at TCCON stations all present retrievals of GOSAT  $\text{XCO}_2$  agree reasonably well, but still diverge significantly from each other elsewhere. These retrievals would understandably yield significantly different flux estimates, all of which would yield similar  $\text{XCO}_2$  time series at TCCON sites. On the issue of TCCON  $\text{XCO}_2$  not having much signal from surface flux variation, we point to Chevallier et al. [2011], who showed that assimilating TCCON data resulted in an uncertainty reduction of zero over land.

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

Size legend added in the revised manuscript. Revised figure is figure 1 below.

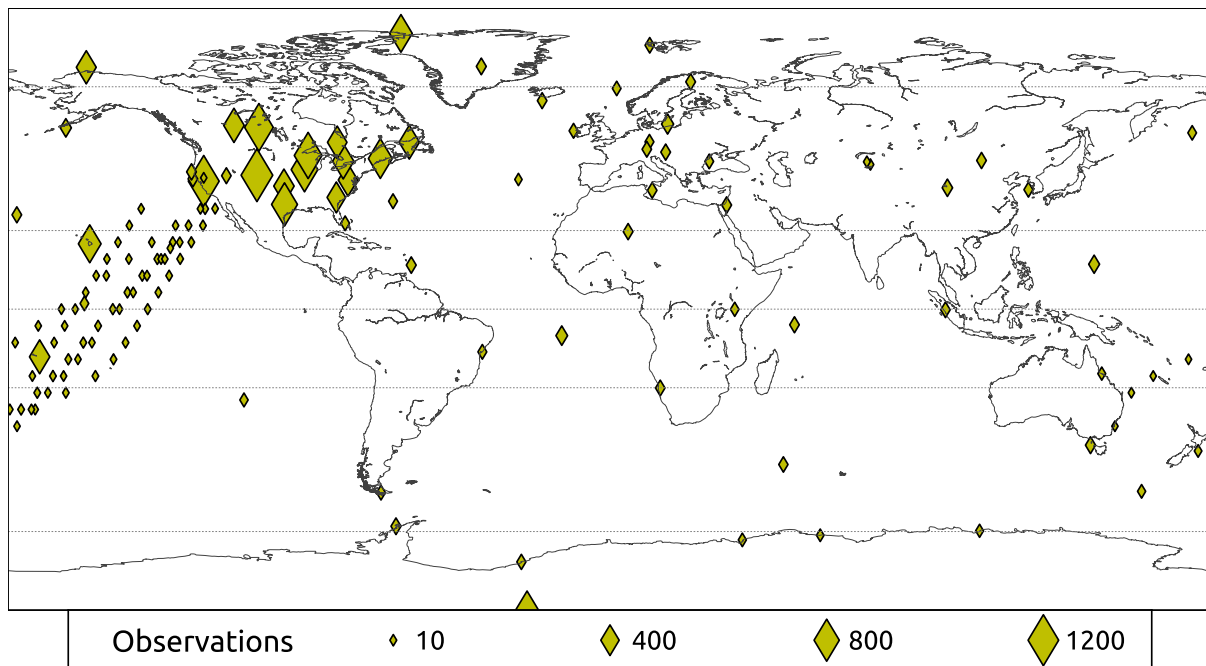


FIGURE 1: Revised figure 1, with legend indicating the number of observations assimilated from each point.

Figure 4: The authors focus on the seasonal cycle mismatch between the observations and the time series resulting from GOSAT-inverted fluxes, but the large synoptic variation, relative to the other time series, is quite striking. What drives this large difference? Is it indicative of larger horizontal gradients, or some sort of instability in local fluxes?

By “seasonal cycle mismatch” we had in fact meant both the phasing mismatch and the large synoptic variation. That large difference is driven by (a) the seasonal sampling bias between the two hemispheres, i.e., there are more XCO<sub>2</sub> soundings in the summer hemisphere, and (b) the surface type asymmetry between the two hemispheres, i.e., most measurements in the southern hemisphere were glint measurements over the oceans, whereas most measurements over the northern hemisphere were land measurements. These two factors, coupled with the sub-ppm land-sea bias in the XCO<sub>2</sub> retrievals (which we optimized later in the manuscript), contributed to the differences the referee mentions. As we show in figure 17, once we optimize an overall land-sea bias, most of that large-scale difference disappears. There is still some discrepancy between the joint inversion (with bias correction) and station data in figure 17, which we believe is due to retrieval artifacts. We are therefore working on improving our retrievals.

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

Changed in the manuscript.

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

The second referee had the same comment, and we have changed our text to omit the straw man argument.

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