

Dear James,

Thank-you for the work that you and your co-authors have undertaken to address the reviewers comments on your manuscript. Overall I think you have answered the majority of the concerns sufficiently. There are a small number of comments that I think could benefit from some further minor revisions, particularly where more than one reviewer raised an issue. In addition, my re-reading of the manuscript identified some technical corrections.

Description of uncertainties: Each reviewer asked for clarification around the observation uncertainties. Adding the model-data mismatch in Table S1 is helpful, but we still do not get a sense of how much that element contributes to the total uncertainty (which I suspect contributes to the confusion of reviewers 2 and 3 about the minimum of 0.01 ppm), or how the flask uncertainties compare to those from the continuous in-situ data or the satellites. I suggest adding a column to Table S1 to give the mean (total) uncertainty for each site (with the factor 2 inflation) so that the reader understands how much the between-flask variability adds to the model-data mismatch, and how the mean uncertainty on the 30s or 1 hour observations compares. With this extra information, I think the reference to the 0.01 ppm minimum could be removed since it presumably has no significant impact on the total uncertainty, which in that case will be dominated by the model-data mismatch. In this case, I suggest the following revision to the paragraph starting at line 228:

‘We estimate the uncertainties for the flask-air observations as the root sum square (RSS) of two components: (a) the standard deviation of the observations from multiple flasks within that hour or 0.3 ppm if there is only one sample and (b) a simple estimate of model transport/representation error. The transport/representation error ... from 0.4 to 4 ppm (Table S1). For the continuous measurements, we take the RSS of two components: (a) the afternoon root mean square (RMS) of the uncertainties [*replace with standard deviation?*] of the 30-second (NOAA) or hourly (JMA) observations divided by the square root of the number of observations, and (b) the standard deviation of all the 30-second/hourly In addition, we enlarged all in situ ... by a factor of 2 (mean values in Table S1)...’

It would also be useful to include the typical uncertainty applied to the GOSAT data. I suggest adding an extra sentence at line 271 perhaps giving the mean, minimum and maximum uncertainty used for the GOSAT data, possibly divided into land and ocean data if they are different (as implied by the different inflation factors).

Global total source difference between inversions: I remain curious about the large difference in global flux in the June-May period. Given your comment about the sensitivity to period being considered, I wonder if that points mostly to retrieval bias as the cause of the difference, particularly given the northern Africa issue that you raise in relation to Fig 6, and that most of the in-situ/GOSAT difference is found in the tropics.

Length of paper: At least two reviewers mention the length of the text or suggest where it could be compacted, and I also agree that it is a long paper. Although you have moved two figures to the supplementary material and removed some text, the additions in response to other comments have made the results section longer than originally. While ultimately the choice is yours, I think that a shorter paper is more likely to be read, and gives more focus to your main points. Here are some suggested deletions:

- Delete sentence ‘Our objectives ...’ (line 31-34) from abstract
- Figure 2 could be reduced (to one flask and one continuous site) and/or moved to supplementary material, given that it is barely discussed in the text. For those, like me, who

are much less familiar with the satellite data, the figure would be more interesting if it included satellite data from an ocean grid-cell and a well-sampled land grid-cell, especially if it these were chosen to be close to an in-situ site.

- How useful is Figure 5 and the associated discussion (line 471-499) for the main message of the paper? Apart from highlighting that you solve for fluxes every 8 days, it mostly just shows noisier versions of the results presented in Fig 6 – and as noted by at least one reviewer, since the uncertainties are not shown on Fig 5 (while they are on Fig 6), it is not easy to assess how much of the week to week fluctuations are outside the uncertainty range anyway. If this text is deleted, also delete ‘Again’ from line 683.
- Line 504-507: I don’t think the shift in sink is easily seen in Figure 6, so I suggest deleting this sentence and only discussing the shift in Sec 3.2. If this change is made, delete ‘As was noted earlier in Section 3.1’ at line 572.
- Line 520-554 and Figure 7: I think this section is less important than other parts of the paper since it is not relevant to the GOSAT inversion, only the in-situ one. The comparison to CT2013B could be restricted to Sec 3.2. Some of the information about the CT2013B inversion would need to be moved but it may be sufficient to give a reference rather than as much detail as is currently included.
- Delete all the flux numbers from line 575-578 and line 583-586, since readers can find these in the Table if they are interested in the detail.
- The prior could be dropped from Fig 9 to simplify the figure, and the sentence (line 614-616) deleted.
- Fig 13. You have said (line 839-840) that you are focussing this analysis on the GOSAT inversion, so perhaps delete the left and middle column from Fig 13 and only show the GOSAT posterior. In this case, lines 844-846 could be deleted.
- Line 917-918: delete the sentence in parentheses, since you’ve already said you are focussing on the GOSAT inversion.
- Line 966-967: delete ‘a shift in the ... in situ inversion’ and replace ‘;’ with ‘,’
- Fig 11. Since the figures are symmetric in the diagonal, could the two panels be combined with the in-situ inversion in the top-right and the GOSAT inversion in the bottom left?

Technical corrections:

- Line 112: I think you can claim higher temporal resolution than other batch inversions but not higher spatial resolution. Rayner et al, GBC, 2008 included batch inversions with 146 and 116 regions.
- Line 180-182. Suggest moving this sentence to line 192, and delete ‘discussed below’.
- Line 188-189. Suggest re-writing the sentence starting ‘The total amount ...’ as ‘Together these oxidation emissions are estimated to be $\sim 1\text{Pg C y}^{-1}$ (for year 2006; Nassar et al., 2010).’
- Line 208: add reference to Table S1 after Fig 1a.
- If Figure 7 retained, Replace first sentence of paragraph (line 520-521) with sentence starting ‘Results from our ...’ (line 535-537) and add ‘In Fig. 7,’ before ‘the two sets ...’ in line 537.
- Line 618, add ‘(Fig 10)’ after ‘Pacific’
- Line 620, replace ‘seen in the’ with ‘seen in Fig. 9 for’
- Line 650-653: I am not sure whether this explanation for the better fit of the high altitude data is correct. In either inversion, it is only ever surface fluxes that are adjusted, so any

signal at higher altitude must be driven by the lower level concentrations. In the GOSAT case it seems difficult to explain how the 800-300 hPa data can be too low but the 300-150 hPa data are OK while the opposite is seen for the in-situ inversion but both inversions use the same transport. Perhaps the age of the air at 300-150 hPa is important and maybe any fitting of the initial condition?

- Line 778: suggest add 'slightly' before 'better'.
- Figure 6 caption: perhaps note how monthly means are constructed from 8 day fluxes.
- Figure 10 caption: now that you have added labels (a-f) on the panels, these should be referenced in the caption.

Please feel free to contact me by email if any of these comments are not clear.

Regards,

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