Referee #2:

We would like to thank the anonymous referee #2 for his/her comprehensive review and valuable suggestions. These suggestions help us to present our results more clearly. In response, we have made changes according to the referee's suggestions and replied to all comments point by point. All the page and line number for corrections are referred to the revised manuscript, while the page and line number from original reviews are kept intact.

Referee: This manuscript presents the results from a numerical experiment in which two inverse estimates of land carbon uptake were made, driven by two different satellite retrievals. One of them results from OCO-2 spectra, while the other one comes from GOSAT. The inverse system is based on GEOS-CHEM and a 4D-VAR method, and spans the year 2015 completely. Posterior fluxes are evaluated by comparing against CT2016 fluxes, and against a set of flask observations, as well as TCCON XCO2 retrievals. The authors conclude that the inversion brings fluxes in closer agreement with all three of these, and differences between the two flux estimates are discussed in the text. Overall, the manuscript is easy to read and organized logically, and sufficient information is presented to allow the reader to appreciate the results.

What is missing from the current manuscript mostly is scientific depth. The experiment conducted is relatively straightforward, and the text at many points falls into long repetitions of numbers presented already in figures and tables. The differences are highlighted, but what drives these differences, what they imply for the use of these satellite data, and what to learn from the comparisons remains unclear. This does not invalidate the substantial effort, but it brings into question whether a publication like this should be considered scientific literature, or a technical report. I will leave this for the editor to judge.

But even for a technical report, I find the manuscript as presented currently incomplete. The demonstration of smaller biases relative to TCCON and flask observations, and the incidental agreement with CT2016, or GCP, or a set of Asian

inversions, brings me to hypothesize that the improvements are not due to the use of the spatially explicit satellite data, but simply a manifestation of a better global total land sink compared to the prior. This can be tested using the poor-man's-inversion first described by Chevallier et al., (2010), in which a global residual land sink (for example that from GCP) is projected onto the land biosphere following the pattern of Net Primary Production. This benchmark is more difficult to beat than a prior from CT2016, as it inherently is globally unbiased and follows patterns of vegetation activity. Improvements beyond those in a poor-man's-inversion due to the use of satellite data would imply that spatial patterns can indeed be estimated from such satellite data, and thus make this manuscript worth reading. Finally, the use of CT2016 as benchmark for a non-satellite inversion seems illogical to me, and should be replaced by a flask-only inversion using the same system as used for the other inversions.

Without these two additions, I feel that this manuscript is not ready for publication in ACP, either as a technical report or as a scientific paper. A long list of further remarks, and points that require further explanation and discussion comes inside the annotated PDF that accompanies this review.

Response: We accept the referee's comments on the lack of in depth analysis and discussions of the two satellite based inversions in the older manuscript. We are grateful to the referee for suggesting the addition of one inversion using surface observations and another benchmark inversion using global CO_2 trend as baseline, which are instrumental in improving our manuscript to its current level. During the revision, we add two inversions as suggested. An evaluation for the two satellite XCO_2 retrievals against TCCON XCO_2 retrievals and an examination of mismatches from both GOSAT and OCO-2 inversions are performed to better understand the uncertainties of the two satellite retrievals. We add more discussions on probable reasons for the differences of inverted carbon fluxes between the two satellite-based inversions. We have updated the manuscript with new inversion results, rewritten the conclusions and made the necessary modifications accordingly.

Main changes in the revised manuscripts: In Section 3, we add subsection 3.1

Inversions using satellite XCO_2 retrievals (see Lines 226-232) to describe two satellite-based inversions; subsection 3.2 Inversion using in situ measurements (see Lines 233-242) to describe in situ inversion setting; subsection 3.3 Benchmark inversion (see Lines 243-259) to describe poor man's inversion setting.

In section 4, we rewrite the whole subsection 4.1 (see Lines 262-273) to discuss the global carbon flux updated with results from two added inversions. We also update Table 1 (see Lines 274-276) with results from in situ and benchmark inversions.

In section 4.2, we rephrase the comparison between inverted regional carbon flux from OCO-2 and GOSAT inversions (see Lines 292-313); we added the comparisons of inverted regional flux between all four inversions (see Lines 320-327); we reorganize the discussion on the relationship between the changes of fluxes and satellite data amount for GOSAT and OCO-2 inversions (see Lines 328-353)； we redraw the Figure 4 (see Lines 313-315) to add results from in situ and benchmark inversions; we update Table 2 (see Lines 317-318) with results from two new inversions; we add analysis of model-data mismatches from two satellite data based inversions and the discussion on the difference of mismatches being the possible reason for the differences in the inverted flux (see Lines 363-377); we add an evaluation of satellite retrievals against TCCON retrievals and discussions for the impact of uncertainties of satellite retrievals on the inversions (see Lines 378-391); we add statistics of model-data mismatches from GOSAT and OCO-2 inversions into Table 3 (see Lines 355-358); and we add Table 4 (see Lines 392-393), which presents the comparison results of satellite retrievals against TCCON retrievals.

In section 4.3.1 Flask observations (see Lines 393-430), with the addition of the two new inversions and different flask observation dataset adopted, to present new evaluation results, we rewrite this whole subsection to show the uncertainties of posterior mixing ratios from 4 inversions and to discuss the improvement of posterior flux by different inversions; we replace Figure 6 and 8 in the old manuscript with Table 6 (see Lines 464-466) to present the evaluation results using both flask observations and TCCON retrievals; we rewrite the most part of subsection 4.3.2 TCCON

observations (see Lines 441-464) to update the evaluation results and give a more detailed discussion on the improvement of posterior XCO_2 by four inversions than the old version of manuscript; we redraw the Figure 9 in the old version of manuscript and rename it as Figure 7 (see Lines 466-467) to present the biases of posterior XCO_2 from four inversions at 13 TCCON sites.

In Section 5 Summary and Conclusions (see Lines 471-499): based on the results and analysis, we rewrite this whole section to summarize our findings and give our revised conclusions.

Line 1: This title is not very accessible to a wider public, who likely will not know what an "inverted terrestrial ecosystem carbon flux" is.

Response: Thank you for this comment. As suggested by referee #1, we have changed the title to "Terrestrial ecosystem carbon fluxes estimated using GOSAT and OCO-2 XCO_2 retrievals."

Line 11: Please mention right away the retrieval versions used, as this has a huge impact on inversion results.

Response: We have added the retrieval versions of "Version b7.3" (see Line 13 and 105)

Line 20-21: Given that the total is larger, this would be quite obvious

Response: A stronger total global land sink from GOSAT inversion doesn't necessarily means that carbon sink of most regions are stronger than carbon sink of corresponding regions estimated from OCO-2 inversion. For instance, several regions with very large carbon sink might dominate the global total, even with weak carbon sink or weak source in several regions, we may still have a very large global total sink. So this sentence "Regionally, in most regions, the land sinks inferred from GOSAT data are also stronger than those from OCO-2 data." is meaning and we keep it in the manuscript (see Lines 22-23).

Line 24: increased and decreased relative to the prior fluxes, which makes it important to know what these are to appreciate the sentence

Response: This sentence is not well organized and causes ambiguity. We have revised this sentence to **"**In temperate regions, the prior land sinks are significantly increased, while in tropical regions the prior land sinks are decreased." (see Lines 25-26)

Line 24: it is hard to see how a conclusion on carbon fluxes can be drawn based on atmospheric CO_2 columns and flasks. To believe the qualification "improved" I would need to see an independent comparison to fluxes. Also here, the term "improved" asks for a statement on what the baseline was that you compared to

Response: We agree with the referee that it is hard to draw conclusion on flux based CO_2 observations. It is an indirect way to evaluate the quality of inverted flux. However, with the lack of direct measurements of flux, there are not much options. Therefore, we follow referee's suggestions to run in situ and benchmark inversions and the comparison with these two inversions can help us to evaluate the quality of inverted flux in satellite-based inversions. We have revised the last sentence of the abstract to "Evaluations using flask and TCCON observations and the comparisons with in situ and benchmark inversions suggest that GOSAT data, can effectively improve the carbon flux estimates in the northern hemisphere." (see Lines 29-31)

Line 41: It is important to stress that these studies were all theoretical, and made many assumptions on the errors and their structure to show the potential of satellite observations. Studies done since then using actual retrieved XCO2 have not confirmed the idea that fluxes can be improved (yet)

Response: Thanks for referee for pointing out this. We have revised the sentence to "Studies have shown that, theoretically, satellite observations, though with lower precision than in-situ measurements, can improve the carbon flux estimates". (see Lines 42-43)

Line 45: This description is incorrect: these sensors can sense light at wavelengths affected by atmospheric mixing ratios of CO_2, but they never measure concentrations.

Response: Yes, you are right. We have replaced "measure" with "retrieve**"** (see Line 46).

Line 64: I would not say that the community is looking for consensus. Efforts are underway to determine the true fluxes through more direct measurements at the surface.

Response: We agree with the referee's comment. We have revised that sentence to "efforts to improve the accuracy of Europe carbon sink estimate are still ongoing**".** (see Lines 64-65)

Line 75: I am not sure that this is a question of interest to the community. The qualification of which one is better will not drive ongoing work on satellite-based inversions, it is the availability of different datasets over different time periods. In the end, we will use anything that helps us understand the carbon cycle better, and we will discard everything that does not.

Response: We agree with the referee that the availability of different datasets over different time periods is important. Nonetheless, before we put all those datasets into one inversion system, we need to know the merits and demerits of every dataset relative to each other. For instance, as added in the revised manuscript, compared with the GOSAT retrievals, OCO-2 retrievals exhibit a relative large positive bias and consequently, give rise to weak posterior land sink. Thus, when we perform the synergy use of two satellite retrievals to constraint carbon flux, the large bias of one satellite retrievals will degrade the improvement by another satellite retrievals with small biases.

Line 81: Abstract said July 2014, please check

Response: We have made the correction in the abstract. It is "Oct, 2014" (see Line 81).

Line 81: to = through

Response: We have changed “to” to “through” (see Line 81).

Line 83-84: Delete “We analyze the differences of inverted terrestrial ecosystem carbon flux between using two XCO_2 data.”

Response: We have deleted that sentence**.** (see Line 83)

Line 119: Please note here the near lack of any data >60N, which would cover the Boreal regions that are later discussed

Response: We have added sentence “Due to the cloud contamination, there are few retrievals in a large portion of tropical land. In northern high latitude area, especially in boreal regions, due to the low soar zenith angle, available satellite retrievals are very sparse.” into Section 2.1 to describe the lack of data in northern high latitude.” (see Lines 122-124)

Line 131: This suggests many more were available but you decided to limit it to 47? Why??

Response: Thank you for this question. Using “chosen” might cause confusion here. Actually, at the time of doing experiment, we downloaded all available dataset from World Data Center for Greenhouse Gases (WDCGG) website. We picked out all flask sites. There were only 47 flask sites with available observations over the year of 2015. Now that we added inversion using in-situ data from Obspack, we replaced flask observations from WDCGG with those from Obspack. We got 56 flask sites with valid observations for 2015. However, there are 4 sites, namely, HUN, HPB, SGP, and TAP, where the standard deviations of first guess mismatch are greater than 5 ppm. So we wouldn’t use those 4 sites for evaluation and end up with 52 flask sites. We have updated the relevant text on using flask observations in the manuscript. (see Lines 130-138, 395-405)

Line 143: Is this part of the products you downloaded? If so, then there is no need to mention it here. If not, how was the bias correction done by your team?

Response: Yes, it is part of the products we downloaded. In addition, this sentence doesn't provide much information and is repetition of statement made elsewhere. We have deleted that sentence. (see Lines 150-151)

Line 173: Is this term part of the summation?? Please add extra brackets if not

Response: It is not part of the summation. We have added extra brackets in the equation. $J(\mathrm{c}) = \frac{1}{2}\sum_{i=1}^{N}\left(XCO_{2,i}^{m} - XCO_{2,i}^{obs}\right)S_{obs,i}^{-1}\left(XCO_{2,i}^{m} - XCO_{2,i}^{obs}\right) + \left(\frac{1}{2}(c - c_a)S_c^{-1}(c - c_a)\right)$(see Line 179)

Line 186-187: These come from the ACOS data product I presume?

Response: Yes. We have added the sentence "These last four quantities are provided from ACOS Version 7.3 Level 2 Lite products." (see Lines 193-194) to give the origin of products.

Line 193, 200,202,203,205,206: I am confused about the fossil fuel fluxes you used. There is a reference to CDIAC here, but it also says that you used the CT2016 fluxes, which according to their website are not the same as CDIAC. Finally, you mention a number of other sources of FF-CO_2 from shipping, airplanes, and oxidation. Your tables however simply quote the CT2016 totals it seems. So what was done??

Response: Yes, the fossil fuel fluxes from CT2016 is not the same as CDIAC, but the average of CDIAC and ODIAC products. We have made the correction in the revised manuscript (Lines 202-205). GEOS-Chem CO_2 emission module added shipping emission since that part is not included in the CIDAC product. Aviation and oxidation are still part of total fossil fuel emission from CIDAC. GEOS-Chem just deducts from the CIDAC the part of aviation and oxidation and spread them into the level they should be released. Thus the total fossil fuel emission is still kept the same as prescribed.

Line 209: A citation or other proof is needed. I actually have not so much faith in

these fluxes compared to more pCO2 driven estimates such as from Rodenbeck.

Response: Yes, it is inappropriate to state that optimized ocean flux from CT2016 represents a more realistic state of ocean flux. However, in this study, first, we focus on the inversion of terrestrial carbon flux. Second, for all four inversion experiments, we use the same ocean flux. Therefore, the choice of ocean flux should not have much impact on the issues we are investigating in this study. We have rewritten the description of the prior flux in the manuscript. (see Lines 210-211)

Line 214: What are the number of degrees of freedom in the matrices? Likely to be less than a few hundred

Response: For one month, the number of degrees of freedom is around 470.

Line 217: Please justify this choice, and the difference between lat and lon scales. To me these scales seem very long given the large data density inherent to a satellite inversion. Why can this not be smaller? What would be the effect on the inversion?

Response: The difference between latitude and longitude are just for convenience of applying off-diagonal covariance matrix, which is difficult to provide in the GEOS-Chem adjoint model efficiently (Single et al., 2011). The scale length of 500 km is similar to what Chevallier et al. (2010) has used in their study. Though satellite retrievals have better spatial coverage than in-situ measurements, they are not dense enough and not evenly distributed. For instance, there are a few thousand retrievals per month in temperate regions but very sparse retrievals in northern high latitude and tropical regions for assimilation. The longer scale length should extend constraints over regions which are not well sampled by satellites.

Line 220-222: I do not understand this sentence. What are the five and three numbers quoted?

Response: The three and five are errors of scaling factors for land and ocean flux respectively. We have corrected that sentence as "the uncertainty of scaling factor for the prior land and ocean fluxes in each month at the grid cell level are assigned to 3

and 5," (see Lines 224-225).

Line 225: I do not see how this procedure accounts for correlated errors in the grid-based retrievals?

Response: Satellite XCO_2 retrievals are pixel values at instrument viewing resolution and are not grid-based retrievals. Within the model cell box with the size of 2.5x2 degree, there are many pixels which are correlated to some extent. The averaging of those pixels should account for correlated errors.

Line 231: Why did you not also perform an inversion against the surface network data only, to complement the satellite based effort? Now you need to compare your fluxes against CT2016 all the time, when you could have simply created your own. Then you also would not have had to deal with the difference in transport models that now confounds the comparison.

Response: We have performed an inversion using surface network data and need not to compare against CT2016. A subsection is added to describe in situ inversion setting in the revised manuscript. (see Lines 233-242)

Line 254: The CT2016 website suggests that this number should be -2.62 PgC/yr. What creates the difference?

Response: We have noticed the difference between what we got and the number from CT2016 website. We checked our calculation very carefully. Since we run GEOS-Chem model at a resolution of 2.5x2 degree, we regridded the CT2016 carbon flux at 1x1 degree resolution into our model resolution. The global totals are computed from carbon fluxes at 2.5x2 degree resolution. The regridding procedure we used might cause the difference of our number from CT2016 value. Since we have removed the CT2016 results from our comparison in the revised manuscript (see Lines 275-276), we don't need recheck the calculation again.

Line 254: The CT2016 website has this as 5.51 PgC/yr it seems...?

Response: The same reason as mentioned above. (see Lines 275-276)

Line 254: It is difficult to compare atmospheric growth rates directly with flux conversions, as the assumption of instantaneous mixing is that one makes is likely to be false at such short time scales. Please mention this in the text, because now the number of 6.23 looks "better" while it is simply a different metric (that happens to be close to the XCO2 based estimates).

Response: Now that we have conducted benchmark inversion, we can use benchmark result as a standard and have removed GCP estimate in the revised paper. (see Lines 275-276)

Line 275: Lack references

Response: We have added the missing reference "Gurney et al., 2002". (see Line 294)

Line 278-321: Starting from here, the text becomes a very long description of the numbers that are summarized already in the Table. The words written here do not add anything to add, and the numbers provided are simply copies of the information that is displayed. It can be removed fully from line 278 through line 321.

Response: We agree with referee that the description is rather long. However, detailed comparison of regional flux is important for us to understand the impact of satellite retrievals on inversions. Just checking the values in those tables might not be enough. So we will keep the description of comparison and have made this long description more concise. (see Lines 292-313)

Line 322: delete "basically"

Response: We have deleted it. (see Line 336)

Line 324: This sounds like an interesting observation: changes in fluxes are largest there where the coverage is least?!

Response: What we want to point out is that the changes in fluxes are largest there

where the coverage is the best. The region with least coverage is Northern Boreal Land where the inverted flux has the smallest change relative to the prior flux. (see Lines 335-338)

Line 333: changed from the prior you mean, or do you mean different between two inversions?

Response: We mean more carbon flux is changed relative to the prior. We have added "relative to the prior flux" to that sentence. (see Line 347)

Line 340: This sounds quite speculative, yet the statement is also quite obvious. The challenge is to quantify, through your experiments, what the sensitivity to sensor accuracy and spatial resolution is.

Response: We agree with the referee that this statement is speculative and obvious. However, based the results we have from our experiments, we are unable to draw any conclusions on the sensitivity to sensor accuracy and the spatial resolution of those two sensors. Thus we have deleted this statement in the revised manuscript. (see Lines 352-353)

Line 343: delete "basically"

Response: We have removed the paragraph containing "basically" as pointed out in the first response "**Main changes in the revised manuscripts**".

Line344: delete "CT2016"

Response: We have removed the paragraph containing "CT2016" in the section presenting comparison between inversions as pointed out in the paragraphs of "**Main changes in the revised manuscripts**" in the first response.

Line 350: In my opinion this debate is not so intense lately, and the community overall does not believe the large sink to be realistic. And instead of simply remarking on it, your framework could be used to shed some more light on the situation. This would require a flask-based inversion to compare the two satellite-based ones again though.

Response: We have performed an in-situ observation-based inversion to compare with our results using satellite retrievals. The carbon sink in Europe estimated from in-situ inversion is close to the estimates from the two satellite inversions. We are still working on the discrepancy of Europe carbon estimate between our in-situ inversion and other in-situ based inversions. In the manuscript, since we have removed CT2016 results related comparison. Thus, most part of this section is rewritten as listed in "**Main changes in the revised manuscripts**".

Line 364: probably better to use "suggest", as there is no conclusion possible based on this evidence discussed.

Response: The paragraph using "indicate" has been rewritten.

Line 389: What is quite interesting to discuss is the difference in global land uptake: the total would suggest quite different global growth rates in atmospheric CO_2, a metric that we know quite well.

Response: We have added more discussions on the difference of different global land uptake estimate. (see Lines 260-276)

Line 408: The fact that these do not improve suggests that it is mostly the new global balance that is improved in the inversions. Hence my request for a benchmark against a poor man's inversion

Response: After we conducted in situ inversion and benchmark inversion, we redid the evaluation using flask observations from ObsPack product and found that the little improvement in standard deviations are mainly due to the use of 4 sites, namely, HPA, HUN, SGP and TAP, since our transport model is unable to capture the CO_2 levels and variations of these 4 sites. At these 4 sites, the standard deviations of mismatch are larger than 5 ppm. So we exclude these 4 sites from our evaluation. The updated result show improvement in the reduction of both bias and standard deviation. In addition, the addition of benchmark inversion did allow us to better evaluate our satellite data based inversions. We have updated the relevant text in the revised manuscript (see Lines 395-410).

Line 419: For a bias, this is quite large.

Response: Yes, we agree with the referee that it is a quite large bias. In the updated manuscript, we have revised the statement and pointed out that the limited improvement is made by OCO-2 retrievals. (see Lines 436-466)

Line 422-423: I am not sure if the evidence agrees with the conclusion presented. First of all, the evidence concerns mixing ratio biases while the conclusion is on fluxes. Second, the reduction of the bias could just as easily have been obtained in the simple "null experiment" that I proposed earlier: take the global growth rate, subtract 2 PgC/yr of ocean fluxes, and spread the remaining flux across the vegetated land weighted by NPP. That land flux transported should be the baseline to beat for an inversion (not the prior flux). See Chevallier et al., (2010, JGR) for more details.

Response: We can evaluate the quality of inverted carbon flux indirectly by comparing the posterior CO_2 mixing ratio against observations. The reduction of bias of simulated mixing ratio with posterior flux should be a prerequisite for an inversion. We agree with the referee that it is not enough to simply use mixing ratio biases to draw conclusion on the improvement of posterior flux. Therefore, we have performed the poor-man's inversion as a benchmark to evaluate our satellite retrievals-based inversions.

Line 454: It seems that in OCO-2, also the pattern of the bias is conserved and thus little spatial information on the fluxes was derived.

Response: Yes, the biases of simulated XCO_2 from OCO-2 inversion remain relatively large. As analyzed and discussed in the revised manuscript, there are relatively large positive biases between OCO-2 and TCCON retrievals, and quite small mismatches between prior simulated and OCO-2 XCO_2, thus consequently leading to small adjustments to the prior flux. (see Lines 415-430, 436-466)

Line 458-459：I think you mean the other way around Change the sentence to ",

using the GEOS-Chem 4D-Var data assimilation 472-473)system."(472-473)

Response: We have changed the sentence to "using the GEOS-Chem 4D-Var data assimilation system." (see Lines 472-473)

Line 464： This is confusing. The land net flux is a positive number if one includes fossil fuels and biomass burning, and now it becomes larger in the inversions? I think you described the opposite in the text: the land sink increased from the prior and thus a smaller net land flux when all components are included.

Response: It should be "excluding fossil fuels and biomass burning". We have replaced including with excluding. (see Line 478)

Line 467-468: This is not a good representation of the data: the growth rate is a measured quantity in the atmosphere, and should be compared to the simulated mixing ratios at a set of background sites of one wants to comment on its realism. For the global total fluxes, one can compare them to GCP which is also a flux estimate. But please do not refer to a GCP global growth rate and mention it to be actually fluxes, this is incorrect.

Response: We now benchmark two satellite data based inversion with the poor man's inversion and no longer use GCP estimate as a standard. We have removed GCP related statement in the revised manuscript.

Line 470: delete "basically"

Response: We have deleted it. (see Line 485)

Line 470: Based on what metric do you qualify this as significant? Is the change outside the uncertainty specified? If the significance was not evaluated, then do not use this qualification.

Response: Use "significantly", we just tended to emphasize the large adjustment of priori flux made at northern and southern temperate regions in GOSAT inversion.

This wording may cause confusion. So we change it to “largely”. (see Line 485)

Line 480: "carbon fluxes of OCO-2 and GOSAT": this does not exist, these instruments measure radiances. Please rephrase more accurately.

Response: We have revised to “carbon fluxes estimated from OCO-2 and GOSAT XCO_2 retrievals”. (see Line 493)

Line 480: Again, based on what metric?

Response: With added inversion experiments, we can compare the simulated CO_2 field with posterior flux from four inversions against surface observations. The reduction of biases and standard deviations at global and regional scale could be taken as metric to do the evaluation. In the revised paper, we have added more detailed analysis and discussion of those statistics. (see Lines 406-430, 436-463)

Line 480: Again, I would bet that the poor man's inversion also causes such an improvement and it does not rely on these instruments at all...

Response: The poor man’s inversion does cause similar improvement in reducing bias. However, there is litter improvement in the standard deviation of mismatch. As shown in the revised manuscript, GOSAT inversion shows evident improvement over benchmark. (see Line 406-430, 436-463)

Line 484: This statement seems not true for GOSAT, for which biases seem to be equally large or even larger after the inversion.

Response: This statement is not clearly and may denotes improvement in Southern Hemisphere. Actually, we had hoped to point out the increase of biases by GOSAT inversion. In southern hemisphere, for GOSAT, the biases are indeed equally large or even larger after the inversion. We have corrected that sentence “biases are elevated to a certain extent to” to “biases are slightly increased”. (Line 493)

Reference:

Chevallier, F., Feng, L., Bösch, H., Palmer, P. I., and Rayner, P. J.: On the impact of

transport model errors for the estimation of CO_2 surface fluxes from GOSAT observations, Geophys. Res. Lett., 37, L21803, https://doi.org/10.1029/2010GL044652, 2010.

Singh, K., Jardak, M., Sandu, A., Bowman, K., Lee, M., and Jones, D.: Construction of non-diagonal background error covariance matrices for global chemical data assimilation, Geosci. Model Dev., 4, 299-316, https://doi.org/10.5194/gmd-4-299-2011, 2011.