Referee #2

Many thanks for the anonymous referee's comprehensive review and valuable suggestions, which 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 with tracked changes, while the page and line number from original reviews are kept intact.

The authors provide a detailed description of their new GCAS2021 dataset for monthly global NEE data over 10 years from 2010-2019, which were inferred by using their data assimilate system GCASv2 to assimilate the latest GOSAT XCO2 retrievals of version 9. The manuscript is well written, easy to understand while clearly covering important aspects of the dataset, from their inversion method to the comprehensive evaluation using independent data etc. The resulting dataset is meaningful, and easy to use. Together with other top-down and bottom-up NEE /NBE data, it can be used to improve our understanding of terrestrial biosphere-atmosphere carbon exchange, although (just like other NEE/NBE datasets) there are still some open questions on its reliablity, particularly over regions poorly covered by GOSAT observations. Hence, I recommend it for publication after minor correction.

Major comments:

1. As shown in Figure 6, discrepancies from other top-down inversions (such as CMS or CT2019b) are quite significant over tropical regions, and also over South America temperate and South Africa. As mentioned by the authors, it could be caused by different observation coverages. To help the reader understand the impacts of poor (GOSAT) observation coverage over regions (like the Tropical South America etc) on the top-down flux inversion, I suggest the authors include a simple comparison with their own and other groups' inversions based on the denser OCO-2 XCO_2 data for 2015-2019. Such comparisons may also help answer the question whether the high net flux in 2019 (Table 1) is realistic.

Response: Many thanks for this suggestion. We have conducted an additional comparison between this study and CMS-Flux for the periods of 2010 to 2014 and 2015 to 2018, respectively, since in the first stage, the XCO_2 used in these two studies are almost the same (both GOSAT), while in the second stage, they are different. We found that except for southern Africa, the differences between the two are significantly smaller in 2010-2014 than in 2015-2018 in the above-mentioned regions (Figure R1), confirming that the significant differences are mainly from the different XCO_2 products used in these two studies. For southern Africa, we further examine the prior and posterior NBE over southern Africa in these two studies, and find that the prior NBE used in these two systems are quite different (a strong sink in CMS-Flux, and a source in this study). During 2010-2014, the changes relative to the prior NBE constrained by GOSAT are rather small in both studies (Figure R2), resulting in the large difference in the posterior NBE between these two studies, while in the second stage, because of the better spatial coverage of OCO-2 XCO_2, the changes in CMS-Flux increase significantly, resulting in a shift of NBE from a priori strong sink to a posterior medium source, thus reducing the difference in the posterior NBE between these two studies. We have added following sentences in the revised manuscript (see page 11, lines 310-327), and added Figure R1 and R2 in the revised supporting information, which are named as Figure S6 and S7.

"The differences between this study and CMS-Flux NBE 2020 may be related to the different XCO_2 products used. As mentioned before, the NBE of CMS-Flux from 2010-2014 and 2015-2018 were inferred from GOSAT and OCO-2 products, respectively. In general, OCO-2 XCO_2 has much better spatial coverage than GOSAT XCO_2. Wang et al. (2019) pointed out that data amount is one of the most important factors affecting the inversion results, generally, in one region with more XCO_2 data, the carbon flux relative to the prior flux is changed more. Therefore, we conduct an additional comparison for the periods of 2010 to 2014 and 2015 to 2018, respectively, since in the first stage, the XCO_2 used in these two studies are almost the same (both GOSAT), while in the second stage, they are different. As shown in Figure S6, except for southern Africa, the difference between the two is significantly smaller in 2010-2014 than in 2015-2018, especially in temperate S. America, northern Africa, and Australia, confirming that the significant differences are mainly from the different XCO_2 products used in these two studies. In addition to XCO_2 data, the prior carbon flux can also have a significant impact on the inversion results (Philip et al., 2019). We further examine the prior and posterior NBE over southern Africa in these two studies, and find that the prior NBE used in these two systems are quite different (a strong sink in CMS-Flux, and a source in this study). In the first stage, the NBE changes (Δ_{NBE}, a posteriori minus a priori) due to the GOSAT constraints are quite small in both studies (Figure S7), resulting in the large difference in the posterior NBE between these two studies, while in the second stage, because of the better spatial coverage of OCO-2 XCO_2, the Δ_{NBE} in CMS-Flux increase significantly, resulting in a shift of NBE from a priori strong sink to a posteriori medium source, thus reducing the difference of the posterior NBE in these two studies. We also find that there is also an increase in the Δ_{NBE} in this study, which may be related to the increase of GOSAT XCO_2 data from 2010 to 2019 (Taylor et al., 2022)."

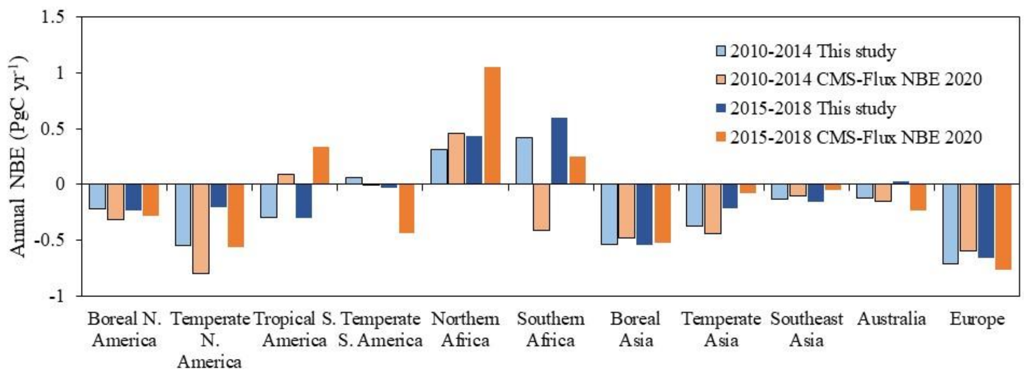

Figure R1. Comparison of NBE between this study and CMS-Flux NBE 2010 for the periods of 2010-2014 and 2015-2018

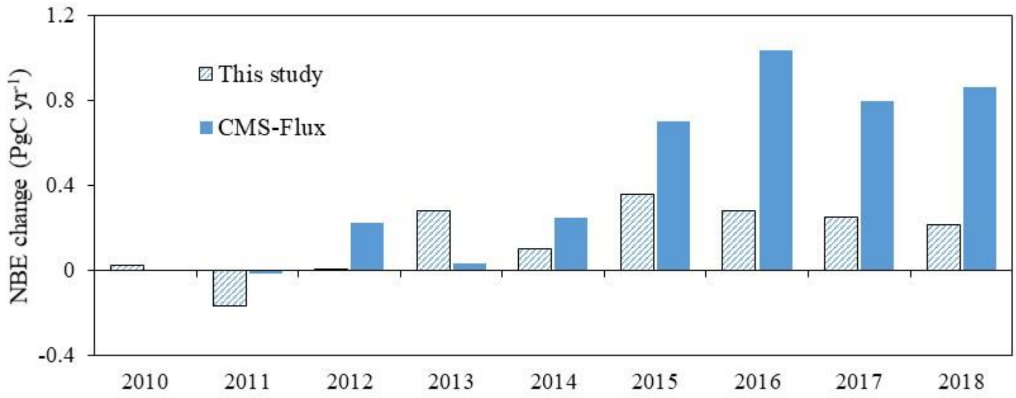

Figure R2. Changes in posterior NBE relative to prior fluxes in southern Africa (positive means source increase)

2. I'd like to see more details on the assumption of the a priori error covariance, and like to know how the authors aggregated posterior error across different assimilation windows to calculate the uncertainty for annual flux. Table 1 shows that assimilation of GOSAT XCO2 has reduced the uncertainty of the global annual NEE total by about 16% (i.e., from 0.6 PgC/yr to about 0.5 PgC/yr), which seems lower than other literatures. I am not sure whether the temoral error correlations between assimilation windows have been taken into account in the calculation of those annual uncertainties.

Response: Thank you for this suggestion. We have added more details about the method of calculating regional and global prior and posterior uncertainties in monthly and annual scales in the revised supplement (see Text S1 in the revised supporting information). We have analyzed the uncertainty reduction (UR) in our previous study (Jiang et al., 2021), and compared it with the other studies. The annual mean URs are in the range of 6 %–27 % across TRANSCOM regions, and the highest monthly regional UR is about 45 %, which are indeed lower than those given in previous studies. The temporal error correlations between assimilation windows were not considered in the calculation of those annual uncertainties, which may be the main reason for these lower URs. In addition, the shorter DA window (1 week) used in this study may be another reason. In addition to the Text S1 in the revised supplement, the following sentences are also added in the revised manuscript.

Page 6, line 158:

"… uncertainty for NEE and OCN flux about 0.6 and 0.2 PgC yr-1, respectively (for the method, see Text S1)"

Page 8, lines 222-223:

"… We also provide a Fortran program for the calculation of posterior uncertainties. The method for calculating posterior uncertainties is given in the Text S1 in the Supporting Information."

Minor comments:

1. Line 31, Page 1: 'We believe that this dataset will contribute to regional or national-scale carbon cycle and carbon neutrality assessment …'. I think this dataset can be very useful, particularly when combined with other top-down and bottom-up results. But for me, the above statement is a bit too strong, considering the significant discrepancies with other datasets over several regions critical for global carbon cycle. I also don't see direct comparisons/evaluation at national scale. I think further assessment are needed, and at this moment, it is better to say ''this dataset can contribute to …'

Response: Many thanks for this comment and suggestion. We agree with you that this statement is a bit too strong, because the comparisons and evaluations at national scale are not performed in this study. We have modified that sentence to "this dataset can contribute to …" in the revised manuscript, see page 1, line 31.

2. Line 73, Page 3: 'data are now available'. Change to 'data is now available…' (to be consistent with line 74 ', which spans…')

Response: Thank you! We have changed 'data are now available' to 'data is now available' in the revised manuscript, see page 3, line 73.

3. Line 135, Page 5: '… the product of CT2017…'. 'CT2017' has not been mentioned before. Should it be 'CT2019B' instead ?

Response: The initial field was indeed obtained from CT2017, not CT2019B. We have changed 'the product of CT2017' to 'the product of CarbonTracker, version 2017 (CT2017)' in the revised manuscript, see page 5, line 141.

4. Line 150, Page 5: Equation 1. Please define *i* and *N*. It is a bit confusing as the gridded product was at a horizontal resolution of 1x1, but the transport model was run at 1.9x2.5.

Response: Thank you! *i* is the identifier of the perturbed samples, *N* is the ensemble size. For the horizontal resolution, indeed, the spatial resolution of the optimized flux cannot be higher than that of the atmospheric transport model. In our EnSRF assimilation algorithm, the spatial resolution of the perturbation factor we adopted is 3°×3°, and the resolution of the prior fluxes is 1°×1°, that is, the prior fluxes within each 3° grid have the same perturbation factor. We have added these descriptions in the revised manuscript.

Pages 5-6, lines 152-153:

"*i* is the identifier of the perturbed samples, N is the ensemble size (here 50)."

Page 6, lines 155-156:

"… the prior fluxes of NEE, FIRE, FFC and OCN, respectively. The spatial resolution of the perturbation factor ($\delta_i \times \lambda$) we adopted is 3°×3°, and the resolution of the prior fluxes is 1°×1°, that is, the prior fluxes within each 3° grid have the same perturbation factor. In each 3° grid …"

5. Line 239, Page 9: '…which also shown'. Should be '…which also showed'

Response: Thank you! We have changed '…which also shown' to '…which also showed' in the revised manuscript, see page 10, line 270.

6. Line 251, Page 9: '…shown that'. Should be '...showed that '

Response: Thank you! We have changed '… shown than' to '… showed that' in the revised manuscript, see page 10, line 282.

7. Line 262, Page 9 & Figure 6 Caption. Please use ‘TRANSCOM’ or ‘TRANSCOM 3’ consistently

Response: We have changed the captions of Figure 3 and Figure 6 to use ‘TRANSCOM’ consistently in the revised manuscript, see page 34, line 979, and page 36, line 988.

8. Line 315, Page 8 & Table 1. It is better to include prior estimates for comparison. Also, why were the CMS and CT2019B results not included in this table?

Response: Many thanks for this suggestion. In this manuscript, we have placed all tables and figures on posterior estimates in the main text, and graphs comparing prior and posterior estimates in the supporting information. Since we have presented a comparison plot of the prior and posterior global NEE in Fig. S6l (Figure S9l in the revised supplement), we do not include the prior estimates in Table 1. However, we have added a description about the comparison between the prior and posterior NEE in that paragraph, and in addition, following this suggestion, we have also included the estimates of CMS-Flux NBE 2020 and CT2019B in Table 1, and added corresponding descriptions in that paragraph in the revised manuscript (see pages 8-9, lines 232-242; page 41, lines 1042-1045). The added descriptions are as follows:

“Compared with the prior NEE (Figure S9l), the posterior NEEs increase significantly from 2010 to 2012, and decrease to varying degrees (in range of 0.15 to 1.15 PgC yr^{-1}) from 2015 to 2019. Table 1 also lists the estimates from the CMS-Flux (CMS-Flux NBE 2020, Liu et al., 2021) and CarbonTracker (CT2019B, Jacobson et al., 2020) systems. CMS-Flux NBE 2020 is a product for the period of 2010-2018, in which the results of 2010-2014 was inverted from the GOSAT XCO_2 v7.3, and the rests were inverted from the OCO-2 XCO_2 v9 retrievals. Both GOSAT and OCO-2 retrievals were from the ACOS team, created using the same retrieval algorithm and validated using the same strategy (Liu et al., 2021). CT2019B is a product inverted from global surface, tower and aircraft CO_2 measurements. CMS-Flux NBE 2020 only presented the NBE results, and the FIRE emission used in this study and CT2019B are also different. Therefore, this comparison focuses on NBE. In 2010 and 2014, our estimates are close to CT2019B and significantly lower than the estimates of CMS-Flux NBE 2020; in contrast, in 2011, 2012, 2013, 2016 and 2017, they are comparable to CMS-Flux NBE 2020 and higher than those of CT2019B. In 2015, it is higher than both.”

9. Table 1: Table 1 shows a high net flux of 6.08 PgC/yr during 2019, which is higher than 2015 (5.95 PgC/yr). It seems inconsistent with NOAA atmospheric CO2 growth rates derived from the in-situ network (i.e., 2.57 ppm/yr (2019) vs 2.96 ppm/yr (2015)). Also, to my knowledge, some inversions based on OCO-2 XCO2 data or based on the surface insitu network showed significantly lower net global fluxes (up to 1 PgC/yr). Some discussions may be needed.

Response: Thank you for this suggestion. In our inversion, the inverted net flux in 2019 is indeed higher than that in 2015, and higher than the AGR in 2019 observed by NOAA, which is mainly due to the abnormally low carbon sink in the tropical latitudes (TL, 30° S ~ 30° N) in 2019 (Figure 7). The reason may be related to the XCO_2 retrievals of GOSAT. After detrending, the GOSAT XCO_2 in 2019 is higher than that in 2015, while OCO-2 is the opposite (Figure R3). The following sentences have been added in the revised manuscript (see page 9, lines 254-259), and the Figure R3 has been added in the revised supplement, and named as Figure S3.

"It also should be noted that in this study, the AGR in 2019 is higher than that in 2015, and significantly higher than the observed value, which is mainly due to the abnormally low carbon sink in the tropical latitudes (TL, 30° S ~ 30° N) in 2019 (Figure 7). The reason may be related to the biases in the GOSAT XCO_2 retrievals in TL. We analyze the monthly changes of GOSAT XCO_2 in 2015 and 2019, and compare them with the OCO-2 XCO_2 retrievals (OCO-2 v10). We find that after detrending, in TL, the GOSAT XCO_2 in 2019 is higher than that in 2015, while OCO-2 is the opposite (Figure S3)."

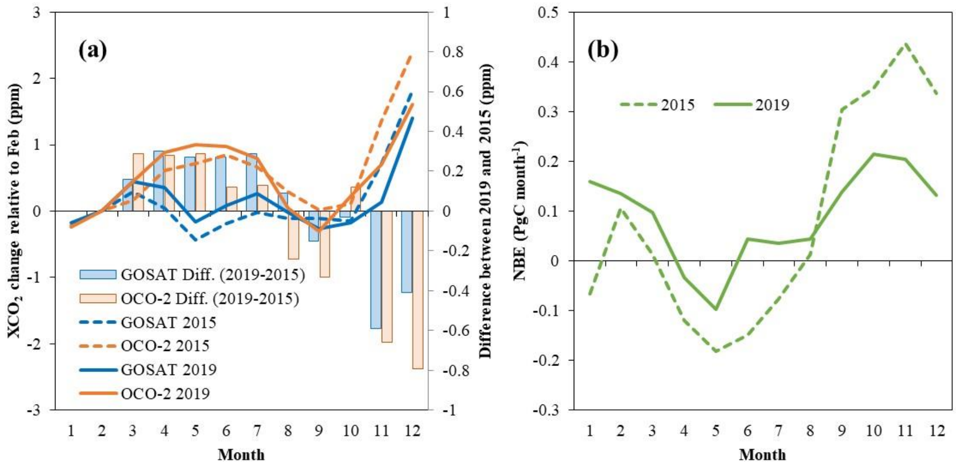

Figure R3. Monthly variations of (a) XCO_2 and (b) NBE in tropical latitudes (TL, 30° S ~ 30° N) in 2015 and 2019 (because GOSAT lacks data in January 2015, in each year, XCO_2 for each month is its change relative to February. It could be found that the carbon sinks in January-August and September-December 2019 were significantly smaller and stronger than those in the same period in 2015, respectively. Correspondingly, compared with 2015, GOSAT has higher XCO2 in March - August, and lower ones in September-December in 2019. Although OCO-2 has a similar pattern, compared with 2015, the XCO_2 increase in March-August is significantly smaller than that of GOSAT, while the decrease in September-December is significantly higher than that of GOSAT. On average, the GOSAT XCO2 in 2019 is higher than that in 2015, while OCO-2 is the opposite)

10. Line 425, Page 15: ‘ In the Amazon basin, the simulated, CO_2 profiles also agree well with the observations…’. I think the results (Figure 12) actually suggest there could be systematic bias in the posterior fluxes over Amazon basin.

Response: Thank you for this suggestion. Indeed, that conclusion is too arbitrary. We re-analyzed the comparison results of simulation and observation in the Amazon region, and found that there is negative BIAS (~ - 1.0 ppm) below 1 km, small BIAS (~ 0.2 ppm) at 1 ~ 1.5 km and positive BIAS (~ 0.9 ppm) above 1.5km. This indicates that there is indeed a systematic bias in the posterior fluxes over Amazon basin, which may be caused by the model vertical transport error. We have reorganized those sentences in the revised manuscript (see page 17, lines 494-501), which are shown as follows:

“In the Amazon basin, the MAE and RMAE of all 4 sites decrease with height, with MAE and RMSE decreasing from about 2 ppm near 1000 m height to about 1.5 ppm near 4000 m. For BIAS, below 2000 m, they increase significantly with height. There are negative (~ -1.0 ppm, data not shown), small (~ 0.2 ppm), and significant positive BIAS (~ 0.9 ppm) below 1000 km, at 1000 ~ 1500 m, and 1500 ~ 2000 m heights, respectively, indicating that there are considerable vertical transport errors, and the carbon sinks over tropical S. America may have systematic biases.”

Reference:

Jiang, F., Wang, H., Chen, J. M., et al.: Regional CO_2 fluxes from 2010 to 2015 inferred from GOSAT XCO_2 retrievals using a new version of the Global Carbon Assimilation System, Atmos. Chem. Phys., 21, 1963–1985, https://doi.org/10.5194/acp-21-1963-2021, 2021.