Interactive comment on "The role of OCO-3 XCO2 retrievals in estimating global terrestrial net ecosystem exchanges" by Wang et al. (2024)

The manuscript by *Wang et al.* (2024) analyzes global and regional terrestrial and oceanic carbon dioxide (CO_2) sources and sinks using OCO-2 and OCO-3 retrievals and the GCASv2 inverse modeling system. The purpose of this study was to determine the impact of using OCO-3 alone, and in conjunction with OCO-2, to estimate the global CO_2 budget. The results of this work show that there are large differences in CO_2 terrestrial fluxes in certain regions when comparing inverse model results when assimilating OCO-2 and OCO-3 separately. Also, when assimilating OCO-2+OCO-3 data it was determined that the results were similar to OCO-2 only model runs. When compared to estimated global atmospheric CO_2 growth rates and independent observations, it found that OCO-3 inversion results had larger biases/errors compared to OCO-2 only and OCO-2+OCO-3 simulations. The primary reason for this was attributed to the limited observational coverage of OCO-3 which does not observe CO_2 in the high latitudes. Overall, the paper is interesting and studies an important topic for using satellite-derived CO_2 for estimating global and regional CO_2 flux budget. The manuscript is generally well-written; however, lacks necessary information and context in many parts of the paper. After careful consideration of the minor and major comments provided below, I would expect this paper to be sufficient for publication.

Minor Comments

1. Line 56. Use "%" instead of "per cent".

2. Line 75. "The OCO satellites".

3. Line 77. Remove "a".

4. Line 91. "till" should be "until".

5. Line 114. Two-layer.

6. Prior emissions data. Do all 4 prior emissions data sets cover the entire time period of the model simulations (2019-2022)?

7. Line 195-196. Do the authors use GEOS-FP meteorological data? Please be clearer about the data used in the simulations.

8. Figure 1. Do the OCO-3 retrievals help with the lack of space-based XCO2 observations in the tropics compared to OCO-2? This would be very helpful to discuss.

9. Figure 3 c, f, i. It is very challenging to see much information from these subpanels.

10. Line 222. Did you mean to say "sources" here?

11. Line 334-336. This sentence is confusing. Why is the NEE from 2019 being discussed since the inversion was for 2020-2022? Also, where in Table 3 is it shown that the prior NEE is 3.5 PgC less than the posterior fluxes? I am guessing the authors might mean ppb instead of PgC?

Major Comments

1. Line 114-116. Can the authors please expand upon this two-layer localization scale used to filter observations to be used in the inversion? It's not clear from this sentence what is actually being done and why.

2. Section 2.1. The authors need to provide more information about the GCASv2 model. What is the horizontal spatial resolution of the system? What meteorological data is used to drive the simulations? How are prior emission and observation error covariance matrices developed? What are the main upgrades in GCASv2 compared to GCASv1? There is a lot of detail that could be added to this section in order for the reader to better understand the inversion system.

3. Use of OCO-2/3 Ocean Glint (OG) observations. The vast majority of research that assimilates OCO-2 and OCO-3 XCO2 data tend to avoid the usage of OG retrievals (e.g., Peiro et al., 2022; Byrne et al., 2023). This even includes studies which focus on constraining oceanic fluxes of CO_2 applying the newest version 11 OCO-2 retrievals (e.g., Jin et al., 2024). The reason for this is the OG retrievals from the OCO satellite sensors have been determined to potentially have unrealized errors/biases and spurious trends (Peiro et al., 2022; Byrne et al., 2023). I would suggest the authors run additional inversions only using land nadir and land glint (LN+LG) OCO-2 and OCO-3 retrievals to see how this impacts the results of this study. If there are noticeable differences, which is expected, the authors should update the results of this paper using LN+LG observations only or discuss the impacts of using OG retrievals on the results of the paper.

4. Model simulation spin-up and spin-down. The authors run their inverse model between August 2019 and December 2022 using a 5-month spin-up time. Did you allow for any spin-down time as well? Observations into early 2023 will still impact the inversion of regional/global CO_2 in 2022. It is common to apply multiple months of spin-up and spin-down when constraining global CO_2 fluxes with OCO-2/3 retrievals. The authors need to explain whether they provided spin-down months in their simulations and if not, consider running the simulation with at least 5 months (same as the spin-up time) of spin-down.

5. Sect. 3. Overall, more detail is needed about your inversion set up. For instance, what are the prior errors you apply for both terrestrial (NEE) and oceanic fluxes? This is extremely important for the results of this study. From Table 1 it appears ocean fluxes did not deviate too far from the prior estimates which leads me to assume the prior errors for these sources were low. What inversion method do you use? There is a glaring lack of information in this section.

6. Line 209. How was the average atmospheric CO_2 growth rate of 4.96 PgC yr-1 for 2020-2022 calculated from Friedlingstein et al. (2023)? Is the growth rate for each year provided in this report? I see values provided for 2022 by itself, but don't see the 2020-2022 average. There is interannual

variability in the global CO_2 growth rate so how you calculated this value can impact the value you compared to growth rates from OCO-2, OCO-3, and OCO-2+OCO-3 simulations.

7. Table 2. If you consider prior emission error/uncertainty, how many of these regions have inversion fluxes which are statistically different (at least considering 1 sigma uncertainty) from the a priori estimate? Also, do the authors calculate/consider posterior emission estimate uncertainty? Some of these values might not be different to a statistically significant level. The authors should expand upon this.

8. Line 352-354. Are the biases in OCO-3 only simulations really significantly smaller than OCO-2 only retrievals? The error bars in Fig. 6 don't suggest they are different to a statistically significant degree. Same thing when discussing the OCO-3 biases being higher in the Northern Hemisphere. It would be nice if the author provided more explanation in this paragraph about why there are large differences in biases. Is it all due to observational coverage differences between OCO-2 and OCO-3?

9. Line 89, 124-125. I am not sure it is correct to say that OCO-3 has the same temporal resolution as OCO-2. OCO-2 is in a near-polar orbit and observes points at the same time of day. OCO-3 is on the ISS orbit which differs from this and allows OCO-3 to observe points at different times of day. How might the fact that OCO-3 observes times other than the standard 13:30 LT observations from OCO-2 impact the results of the inversion?

10. The conclusion/discussion section of the paper could be improved with further information on the importance of these findings. How should studies in the future use both OCO-2 and OCO-3 for estimating global CO_2 fluxes? What are the pros and cons of OCO-3 alone? How does this project expand our knowledge of the global carbon cycle? Some additional text to identify the novelty of this study compared to the vast amount of literature using OCO-2 for inferring global and regional CO_2 fluxes would be very helpful.

References

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