Response to review 2 of “Seasonal and inter-annual variability of carbon fluxes in southern Africa seen by GOSAT”

We thank the reviewer of our manuscript for the constructive and detailed comments. The comments helped us improve our manuscript. We address them 1 by 1 below. Our response is given in red italic. Changes which will be applied in the manuscript are given in red and plain text. These updates have no impact on the conclusions of the paper.

Review of Metz et al.: Seasonal and inter-annual variability of carbon fluxes in southern Africa seen by GOSAT, submitted to Biogeosciences

14 Oct 2024

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Review Summary

The study by Metz et al. starts from satellite-retrieved XCO2 data analysis over a large southern African region dominated by semi-arid savannas and grasslands. They find a larger seasonal variability of XCO2 from GOSAT and OCO-2 compared to an ensemble of in-situ-optimized inverse models resampled at GOSAT soundings. They further point out that the discrepancy originates from the biospheric prior fluxes that control the optimized biospheric fluxes due to the lack of observational constraints. An understanding of the processes that affect the CO2 fluxes and their variability is gained through a comparison of the TRENDY v9 ensemble of Dynamic Global Vegetation Models (DGVMs).

The paper is well in the scope of Biogeosciences. The paper points out large discrepancies in DGVMs over semi-arid regions in southern Africa, and presents how satellite data can be used to evaluate these models. While the results of the paper do not include completely novel findings (like the authors already point out with appropriate references in the paper), the results are in agreement with previous studies carried out using other approaches, and should therefore be interesting to the readers of the journal. The paper is particularly well written, and the methods and results are clearly presented. I also like how the paper advances from one result to the next one in a way that is pleasant to follow. I recommend publishing the paper in Biogeosciences after the authors have considered the constructive comments detailed below. I have also gone through the comments from Reviewer 1 and try not to repeat their message, although some of my comments might be of similar nature.

Major comments

1)

I don't think the title of the paper is fully descriptive of its contents. Could the title be for instance along these lines: "The potential of CO2 satellite observations to inform and evaluate modelled carbon dioxide fluxes in Southern Africa" or something similar. At least, I would propose to change "carbon" to "carbon dioxide" or "CO2" because methane and CO are not discussed in the paper.

We tried to find a universal title as our manuscript has multiple facets including methodological findings (as described by your suggested title) but also findings about processes driving the African carbon flux variability. Therefore, we would like to keep our original title. We will change the 'carbon' to 'CO_2'.

2)

The study region is very large and the CO2 fluxes can be quite heterogenous within the study region (for example, on NOAA's website one can see that CarbonTracker concludes part of the study region to be a net source and part a net sink of CO2; https://gml.noaa.gov/ccgg/carbontracker/fluxmaps.php?region=afr&average=annual#imagetable and even though CT is not the best-performing model in the region as the authors point out, this underlines the heterogeneity of the region and makes one wonder how much you can average over without losing important information). I think that the averaged XCO2 time series (Fig. 2) is to some extent dependent on the sampling. The number of GOSAT observations likely varies from only few tens to some 1000-2000 per month over this region, depending on the season. It would be good to show the time series of the number of GOSAT observations (both products) for example added in Fig. 2 and also a map of the spatial sampling density for example around the XCO2 minimum and XCO2 maximum (this could be in the Appendix), and discuss how the sampling affects the results of the concentration-based analysis and the inverse modelling results.

First of all, it is important to note that the atmospheric inversion systems use single-sounding CO_2 concentration measurements. There is no averaging over the whole month or the whole region done, before feeding the satellite measurements into the inversion system. By modelling the atmospheric transport, the inversion system accounts for the sampling of the satellite data. So there is no information loss due to averaging in the inversion. In case of low measurement numbers, the inversion systems tend to rely more on the used prior assumptions while taking into account the prior error and measurement error assumptions.

We show the mean monthly CO_2 concentrations mainly for an illustrative purpose. Doing so, we want to give a first impression of the seasonal dynamics of the satellite measurements. Furthermore, we already want to point out that there are inconsistencies between satellite measurements and (co-sampled) in situ measurement informed inversions, so that satellite measurements are a promising additional information source. However, the rest of the manuscript and analyses do not rely on the concentration analysis. We will add the following sentence to the methods to clarify that:

"To this end, we use the model TM5-4DVar and assimilate GOSAT CO_2 concentration measurements over land and ocean together with the in situ measurements. We use the individual total CO_2 concentration measurements, i.e. we do not apply any detrending or spatiotemporal averaging. Detrending and spatiotemporal averaging is only applied for visualization purposes to show the variability in the monthly CO_2 concentrations (Section 3.1)."

Furthermore, we will discuss the satellite data abundance. Taking into account the suggestions of you and reviewer 1, we will add the following two figures in the appendix and sentences in the main text:

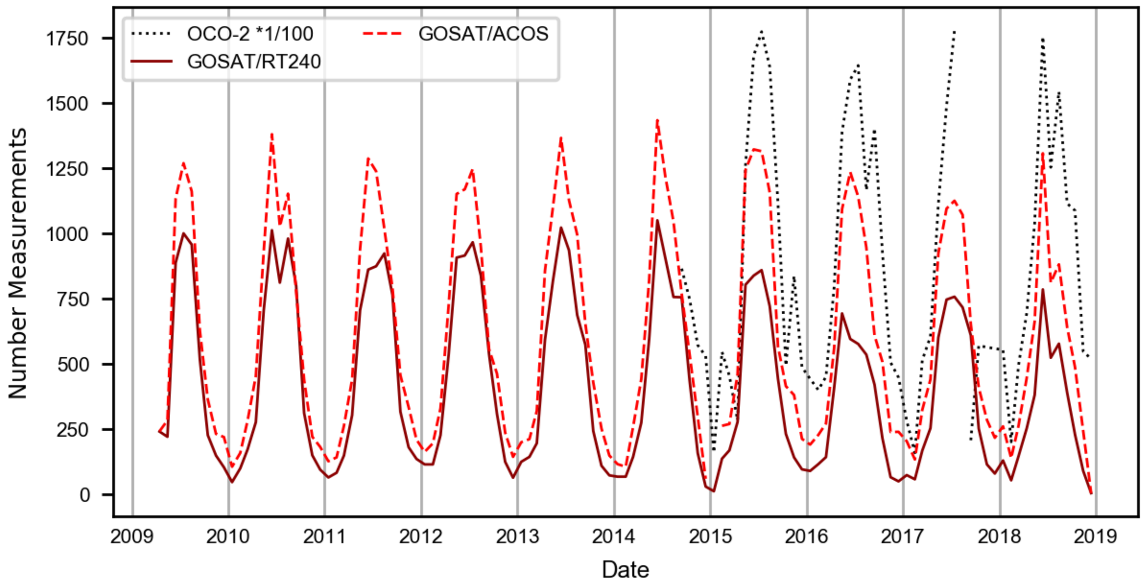

Figure A6: Number of satellite measurements per month. The amount of satellite measurements of the GOSAT/ACOS (red dashed), GOSAT/RemoTeC (dark red solid), and OCO-2 (grey dotted) dataset are given. Note that the number of OCO-2 measurements is shown divided by 100 to enable a comparison to the much less abundant GOSAT measurements.

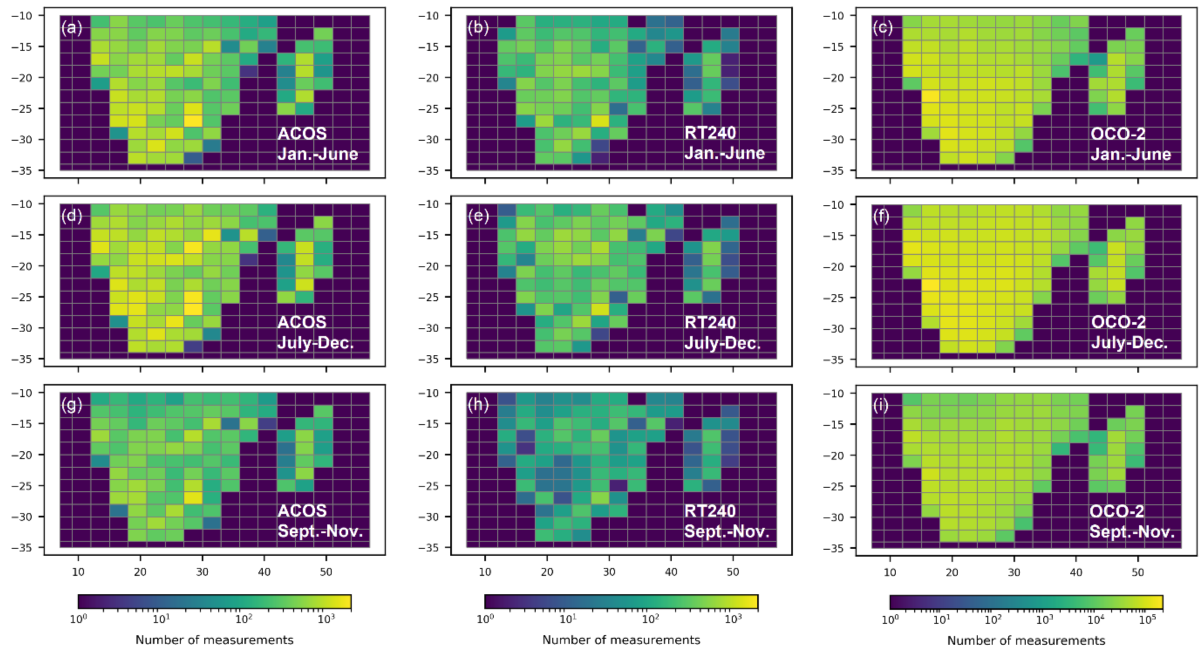

Figure A5: Number and distribution of satellite CO_2 concentration data above southern Africa. (a), (d), and (g) Total number of GOSAT/ACOS, (b), (e), and (h) GOSAT/RemoTeC, and (c), (f), and (i) OCO-2 data per 3°x2° grid cell for (a) - (c) the months of carbon uptake (January – June), (d) - (f) the emission season (July – December), and (g) – (i) the months with the strongest emissions. GOSAT/ACOS and GOSAT/RemoTeC measurements from 2009 to 2018 and OCO-2 measurements from 09/2014 to 2018 are included. The maximum of the color scale is the same for all time periods, but different for OCO-2 than for GOSAT/ACOS and GOSAT/RemoTeC. Compared to GOSAT/ACOS, GOSAT/RemoTeC has a reduced number of measurements, as RemoTeC applies stricter filtering of the GOSAT soundings.

We will add at the end of Section 3.1. (CO_2 concentration results):

"The number of GOSAT measurements (see Fig. A5 and Fig. A6) is variable throughout the year with the smallest numbers occurring during the rainy season around December and January. This leads to larger uncertainties in the monthly mean satellite CO_2 concentrations and satellite-based fluxes during the transition from maximum to minimum concentrations and fluxes."

The impact of sampling on the differences between GOSAT/ACOS and GOSAT/RemoTeC is further discussed below with one of your minor comments.

Minor comments

Abstract: I suggest highlighting the discovered role of grassland carbon uptake for IAV which I consider as a key result of the work. It would also be helpful to give key numbers of TgC so that the reader can contrast those more easily.

We will mention grasslands explicitly in the abstract. Also taking into account the comments of reviewer 1, we will adapt the sentence as follows:

"Doing so, our satellite-based process analyses pinpoint photosynthetic uptake in the southern grasslands to be the main driver of inter-annual variability of the southern African carbon fluxes, agreeing with former studies based on vegetation models alone."

We will furthermore add key numbers of IAV in the discussions of the main driver of NBP IAV:

"According to GFED (see Fig. A10), fire emissions play a minor role in impacting GPP and driving NBP anomalies. The variability of fire emissions is much lower than for NBP and GPP-RA. In the whole study region, IAV (calculated as standard deviation over the years) of GPP-RA and NBP fluxes are 97.7 TgC/year and 94.1 TgC/year, respectively. IAV of GFED fire emissions is 27.3 TgC/year similarly low as IAV of RH (27.1 TgC/year)."

Line 46: Spell Orbiting Carbon Observatory -2 with a hyphen

Thank you for the corrections. We will check and correct this and the following four mistakes.

Lines 56-57: CO2 exchange fluxes → CO2 exchange

Line 57: to flux estimates → to those (to avoid repetition)

Line 69: Valentinti → Valentini (check!)

Line 69: grass land → grassland

Figure 1: This is a massive study region. To make this clearer to the reader, I suggest adding a scale bar representing for example 200 or 500 km or similar. In addition, please add the locations of the Gobabeb COCCON and the Kruger National Park measurement with pins or equivalent symbols.

We will do that (please find the updated figure in the response to reviewer 1).

Line 76: I believe that the description of the timing of the fire season applies to the study region in question and not the whole Africa. Please specify in the text.

We will specify this in the text as follows:

"The fire season starts in May in the western part of southern hemispheric Africa and spreads …"

Section 2.2: Did you also include satellite data over the ocean in your analysis?

No, for the analyses in Section 2.2 we did not use measurements over the ocean. We will specify this in a revised manuscript:

"... measured by the Greenhouse Gases Observing Satellite (GOSAT**) over land in our study region**."

Line 82: column-average → column-averaged

We will adapt that, thank you.

Line 85: How different is RemoTeC v2.4.0 compared to RemoTeC v2.3.8? Please summarise the key developments after v2.3.8 in the text.

We will include: "The major updates between versions 2.3.8 and 2.4.0 are stricter quality filtering in the latter and updated ancillary input data, especially for the prior gas concentrations used."

Lines 87-90: I understand that both products have been used as two separate products (e.g., apply their own quality filtering and potential bias corrections). Did you compare ACOS and RemoTeC by considering only the exact same GOSAT soundings? If so, what did you learn about the product XCO2 differences? While this question may not be in the core of the paper, I think it is of high interest to learn as much as possible about the satellite observations' differences in particular over these regions that are poorly sampled by any other measurements. In addition, the differences in the satellite products can result in flux differences of several tens TgC per month for this region (Fig. 3), which is significant and interesting. Depending on the authors' view, this might even merit a sentence in the Conclusions to address the need of validation measurements in this region to be able to reconcile residual differences in the satellite products.

We also had a look at the exact same soundings included in GOSAT/ACOS and GOSAT/RemoTeC. There are 37563 soundings in this intersection (compared to 69708 ACOS and 47989 RemoTeC soundings). Please find below the time-series of monthly CO_2 concentrations given by the intersection. The monthly CO_2 concentrations of the intersection calculated by ACOS and RemoTeC are similar to the CO_2 concentrations of the whole ACOS and RemoTeC dataset, respectively. The deviations due to different sampling are in sub-ppm scale and do not explain the differences between ACOS and RemoTeC. We will add this figure to the appendix.

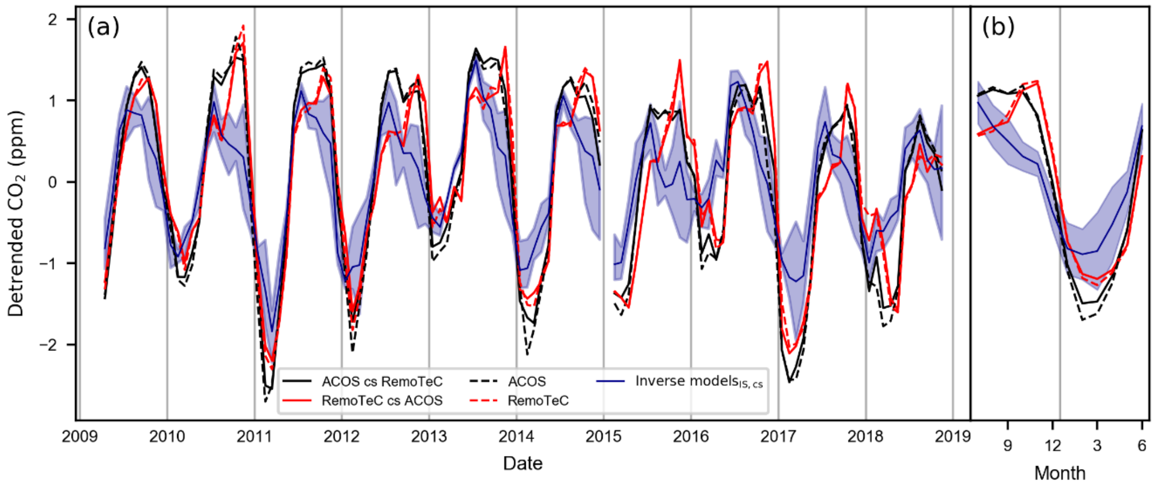

Figure A1: Monthly southern African detrended CO_2 concentrations measured by GOSAT. GOSAT/ACOS is given in black, GOSAT/RemoTeC is given in red. Dashed lines show the mean CO_2 concentrations over the whole dataset. The mean CO_2 concentrations of the soundings included in both datasets, ACOS and RemoTeC, are given as solid line. CS stands for co-sampled and indicates that only soundings, also included in the other dataset are considered. The deviations due to different sampling are in sub-ppm scale and do not explain the differences between ACOS and RemoTeC. Modelled posterior CO_2 concentrations of the in-situ-only inversions are co-sampled (cs) on GOSAT and depicted as mean in blue for comparison. The shading indicates the range among the individual in-situ-only inversions. Panel (b) shows the mean seasonal cycle 2009–2018 with the standard deviation over the years as shading.

Both, ACOS and RemoTeC, apply a bias-correction after initially retrieving the CO_2 concentrations from the measured spectra. Thereby, in RemoTeC only a global factor to correct the raw XCO_2 data over land is used. ACOS uses several correction terms including dependencies on aerosol optical depth and albedo structures. The differences in the raw CO_2 concentrations' seasonal cycle between ACOS and RemoTeC is even larger but reduces with the ACOS bias-correction. Therefore, fundamental methodological differences between the ACOS and RemoTeC retrieval cause the deviations. Besides the mentioned differences in the applied bias-correction, ACOS and RemoTeC differ largely in the handling of surface pressure. This might contribute to the observed differences. As we can only speculate about the reasons for the differences between the monthly ACOS and RemoTeC CO_2 concentrations, we will not include this analysis in the revised manuscript.

It is important to mention that the differences between ACOS and RemoTeC (see shading in Fig. 2) are smaller than their difference to and the deviations among the in situ based atmospheric inversions. Furthermore, as we assimilate ACOS and RemoTeC separately from each other in TM5-4DVar, we can estimate and do show (see shading Fig. 3) the uncertainties in the fluxes associated to the retrieval differences. Here again, the differences between TM5-4DVar/RT240 and TM5-4DVar/ACOS are much smaller than the differences to and the deviations among to other datasets (IS-based inversions, FLUXCOM and TRENDY) underlining that satellite CO_2 concentration measurements and satellite-based fluxes have a discriminating power and can be used as atmospheric constraints.

We prefer not to include a statement about the need for further validation measurements in the conclusion. Such a statement is already included in the manuscript at the end of section 3.1. Moreover, even though it is of scientific interest to have more validation measurements to further evaluate the differences between ACOS and RemoTeC, our analyses and conclusions do not rely on that nor would they change when resolving the differences between the retrievals.

Lines 87-90: In Fig. 2, we are seeing differences in the sub-ppm scale. It would be helpful to give guidance (with references) to the (global) accuracy and precision of these GOSAT products in this part of the text to help the reader understand to what extent we can trust the satellite observations.

We will add the following sentences in line 90, after introducing GOSAT:

"GOSAT/ACOS single measurements have a precision of 1.5 ppm and a mean bias of 0.2 ppm in validation against TCCON (Taylor et al., 2022). GOSAT/RemoTeC was found to have a slightly lower precision of 1.9 ppm (Buchwitz et al., 2017) and by construction a mean bias of 0 ppm in comparison to TCCON after bias correction. GOSAT/RemoTeC was found to have a regional and seasonal systematic error of 0.6 ppm and 0.5 ppm respectively (Buchwitz et al., 2017)."

Line 91: I think it would be more descriptive to say "evaluation" instead of "validation".

Agree.

Line 94: The COCCON data were not co-located with the satellite observations, right? Did you do any filtering to the data, for example in the early or late hours with large solar zenith angles? Please specify in the text.

No, we did not apply any further filtering. We will include that in the manuscript when we introduce the COCCON data and in the caption of Fig. A2:

"We use the full dataset of COCCON measurements i.e. we do not apply further filtering or co-sampling to GOSAT, as there are too few coinciding GOSAT measurements."

Sect. 2.3.1: The three inversion models also use different data assimilation schemes, right (4DVar vs. enKf)? This could be specified in the text.

Yes, we will include the following:

"TM5-4DVar and CAMS make use of a four-dimensional variational data assimilation, while CarbonTracker uses an ensemble Kalman filter."

Sect. 2.3.1.: Did you assimilate also GOSAT observations over oceans? Please specify.

Yes, in our inversion the GOSAT ocean observations are included. We will include: ''... assimilate GOSAT CO_2 concentration measurements over land and ocean."

Sect. 2.3.1.: Question, out of curiosity: did you also consider a GOSAT-only inversion with no in-situ data?

Yes, we also run a GOSAT only inversion. The GOSAT only inversions (ACOS and RT) show the same seasonal cycle as the GOSAT+IS inversions, but slightly larger amplitudes. We decided to use the GOSAT+IS inversion as it takes into account all measurement information available. Below you find a figure with the comparison.

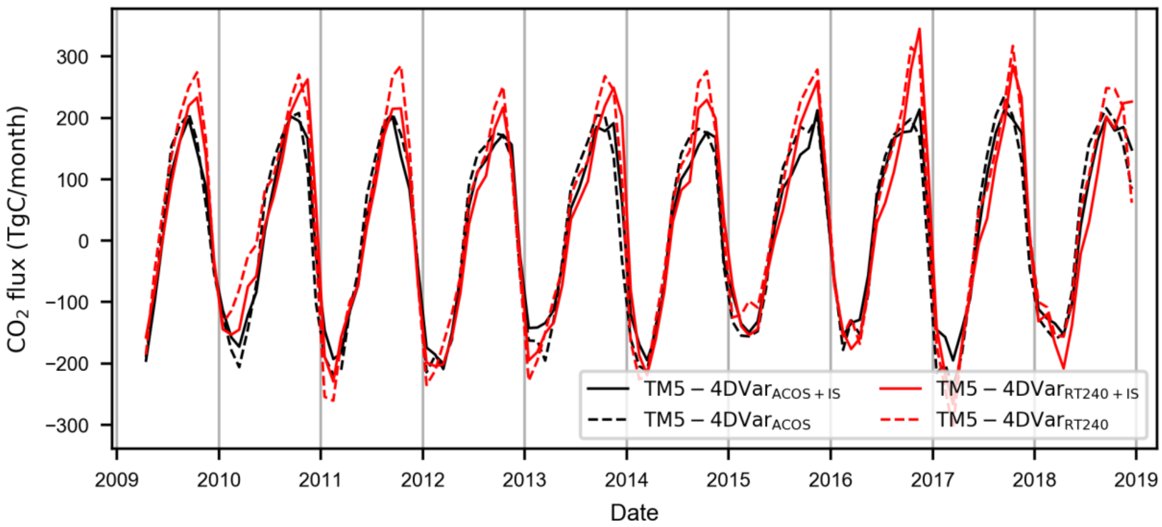

Figure RC2_1: Top-down southern African net CO_2 fluxes estimated with TM5-4DVar. Net CO_2 fluxes estimated by assimilating GOSAT/RemoTeC (red) or GOSAT/ACOS (black) only (dashed line) or in combination with in situ measurements (solid line) are shown.

Line 113 (and elsewhere): please use \citep[TM5-4DVar,][]{Basu et al reference} LaTeX command to get the rid of the extra parentheses within parentheses (if working with LaTeX).

We will adapt the parentheses in the revised manuscript.

Lines 141-144: Did you apply the averaging-kernel correction when comparing the cosampled model concentrations to satellite data? The effect is not likely to be large but it might be non-negligible since the differences in question are mostly in the sub-ppm scale.

In the figures in the manuscript, we did not apply the averaging-kernel (AK) correction. However, we checked its impact for CarbonTracker and CAMS. Below you can find a figure showing the effect of including the AK correction in the comparison to GOSAT. The effect is small and most importantly, does not change the seasonal variability. We calculated the AK effect using the following formula:

$$XCO2_{AK} = \sum_h \left[\left(CO2_{models,h} * AK_h \right) + \left(CO2_{GOSAT_{Prior},h} * (1 - AK_h) \right) \right] * p_{w\_h}$$

Thereby, h stands for the different height layers and p_w is the pressure weight of each layer. The formula is used internally in RemoTeC (CO2_model is then CO2_GOSAT) to *account for the AK of the measurements when calculation XCO_2. For CO2_GOSAT_prior an early version of CarbonTracker (v2019) is used. The effect of applying the AK is small and most importantly, does not change the seasonal variability. For this reason, we decided to neglect the AK.*

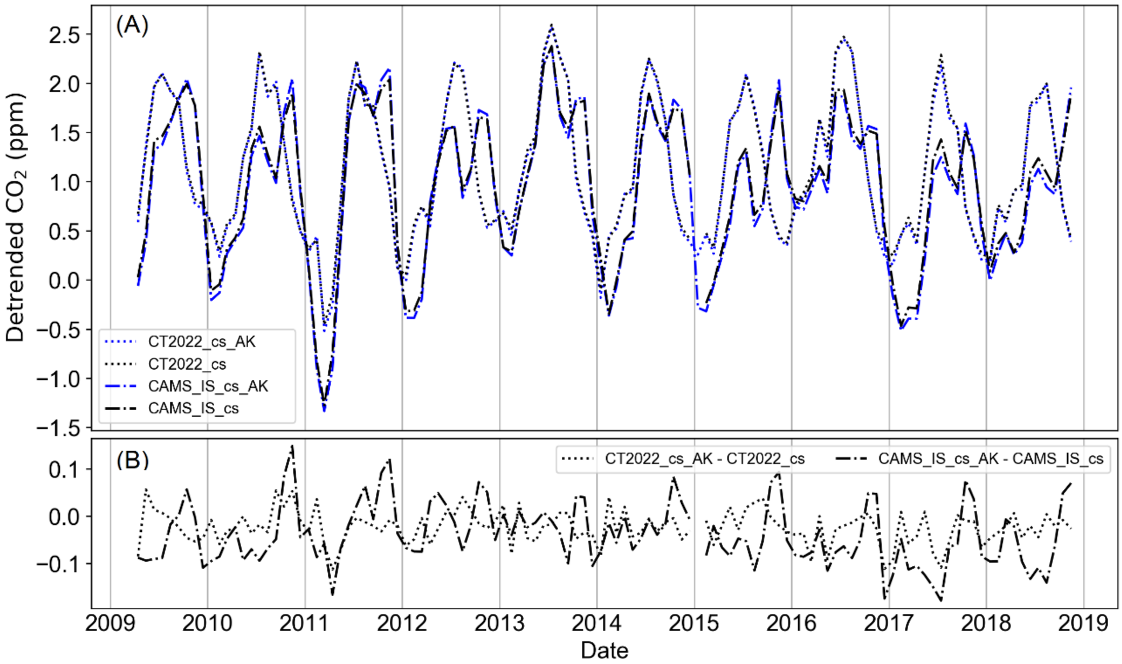

Figure RC2_2: Monthly southern African detrended CO_2 concentrations given by CT2022 and CAMS. The modeled CO_2 concentrations are co-sampled (cs) on the locations of the GOSAT measurements (CT2022 dotted, CAMS dash-dotted). In Panel (A) values with averaging-kernel correction (AK) are given in blue, values without the correction in black. Panel (B) gives the differences between co-sampled CO_2 concentrations with and without averaging-kernel correction.

Line 156: Does the FLUXCOM have a product version that could be referenced here? We have recently worked with a FLUXCOM product that had a strong positive bias in CO2 fluxes over both Northern and Southern Africa. This seems to not be the case with the product in the paper. In fact, if I interpret Fig. 3 correctly, the FLUXCOM NEE would give out either close to zero or negative values without GFED fire emissions, correct?

We use FLUXCOM version 1 with the RS_V006 setup. We will add this information in the main text and in the overview of the data in Table A1. The FLUXCOM version agrees with the data used in the overview paper Jung et al. 2020.

Yes, FLUXCOM NEE is mainly negative and only slightly positive around September. This agrees with the fluxes for whole southern Africa given in Figure 6 and 7 in the FLUXCOM publication mentioned above (Please note that in Figure 7 the mean annual flux depicted in Figure 6 is subtracted).

Line 173: Methods → Data and Methods

We will adapt that.

Lines 176-178: This reminds me of similar GOSAT-to-model discrepancies that were pointed out by Lindqvist et al. (ACP, 2015; https://acp.copernicus.org/articles/15/13023/2015/) and their reasoning, albeit with much earlier versions of the data and models.

Thank you for mentioning the paper. Lindqvist et al. (2015) compare the mean seasonal cycle (MSC) of GOSAT/ACOS CO_2 concentration measurements to the MSC of TCCON and to the MSC of in situ measurement based atmospheric inversion models. In the latter comparison they find differences between GOSAT/ACOS and the models, which are smaller than the deviations among the models themselves. They conclude that GOSAT/ACOS CO_2 concentration MSC may be sufficiently accurate to evaluate land surface models. That matches the methods we use. However, Lindqvist et al. 2015 base their conclusion only on an evaluation of the northern hemisphere. For this reason, we prefer not to cite the paper in our study.

Figure 2: Please consider adding the monthly data density or, if the result is too crowded, presenting the data density time series using other means.

We will include the two figures given in our response to your major comment in the appendix of the manuscript.

Figure 2: What does the CS in the subscript of the "Inverse models" stand for?

CS stands for 'Co-Sampled on GOSAT'. We will add the abbreviation in the caption of the Figure.

Line 190: e. g. → e.g.,

We will adapt that.

Sect. 3.2: I think it is interesting and worth pointing out how you can already "see" the impact of the fluxes in the detrended concentrations (Fig. 2). The well-aligned timings also suggest that the concentration maxima and minima are driven by the fluxes from this region as opposed to transport from elsewhere. This suggests that satellite data can already in its concentration form have potential and be useful for an evaluation of models in a region (even without inverse modelling!).

Yes, the timing of the minima and maxima in concentrations is the same for the maximum emissions and maximum uptake. However, disentangling local fluxes and transport effects is very difficult, even after subtracting a global background. The same timing in both does not necessarily imply transport effects to be irrelevant. For this reason, we prefer not to include such an additional statement.

Figure 3 (a): the legend is partly on top of the plotted data. Please revise such that the data are fully visible. This comment applies to some subsequent figures as well.

We will adapt the figures.

Line 204: remove "methods"

We will correct our manuscript according to this and your following two comments.

Line 244: a → an

Line 253: processed based → process-based

Sect. 3.3.: I understand that if the focus of the paper is to learn about the processes affecting the large-scale CO2 exchange in this region, it is meaningful to focus on the DGVMs that agree with the GOSAT inversions. However, as also Reviewer 1 points out, it seems like a missed opportunity to not discuss the models that agree the worst. I also wonder how generalizable these findings might be for other semi-arid regions in the world; one good example already published by the authors about the Australian fluxes (Metz et al., 2023). I get the feeling that the authors have already learned much more from individual model performance than what they are writing in the paper. I know it is not a preferred task for anyone to point out flaws in someone else's product but this is sometimes necessary to enhance openness that might further advance science.

In general, from the perspective of a non-vegetation modeler, it is difficult to identify the processes missing or inaccurately represented in the DGMVs as the models differ largely in the implemented processes. We found the dephasing of vegetation fluxes and heterotrophic respiration to be one of the key processes characterizing the southern African carbon exchange. Focusing on that and also taking into account the comment of reviewer 1, we will add the following in the discussion of the DGVMs gross fluxes:

"It is noteworthy that large parts of the not selected 'other' TRENDY models miss the dephasing between RH and GPP-RA. Their NBP estimates, therefore, do not agree with the emissions around October found by the satellite inversion. Implementing soil respiration due to rewetting more accurately in those models could improve their agreement with the satellite-based fluxes. Metz et al. (2023) found that the dephasing in the TRENDY models is most likely caused by a different response time of soil respiration and vegetation growth on precipitation e.g. water needs to percolate into the deeper soil layers with plant roots to initiate plant growth, whereas heterotrophic respiration is driven by upper soil layer soil moisture or precipitation. The implementation of such a time lag between heterotrophic respiration and GPP seems to be a necessary but not sufficient prerequisite to accurately capture the seasonal carbon flux variability in semi-arid regions. Our results call for studies on how to implement the response of ecosystems on soil rewetting more accurately to improve the consistency and accuracy of the TRENDY ensemble in semi-arid regions."

Figure 6: The shading mentioned in the caption is not visible (at least in my printed version). The legend covers part of the data, please revise the figure. Why is GOME-2 SIF used instead of GOSAT SIF or OCO-2 SIF? I wonder how different these would be.

Thank you for making us aware of that. The shading is visible in the digital version. We will make the shading darker, so that it is also visible when printing the paper and we will move the legend.

We do not use GOSAT-SIF because the data is much coarser than GOME-2 data as pointed out in Joiner et al. (2013) and Köhler et al. (2015). We also checked OCO-2 SIF. Please find Figure 6 additionally with OCO-2 SIF below (Fig. RC2_3). It agrees well with GOME-2 and would lead to the same results (i.e. TRENDY model selection). We decided to only use GOME-2 in the publication as it nearly covers the whole period 2009-2018 in contrast to OCO-2 which only provides data from late 2014 on.

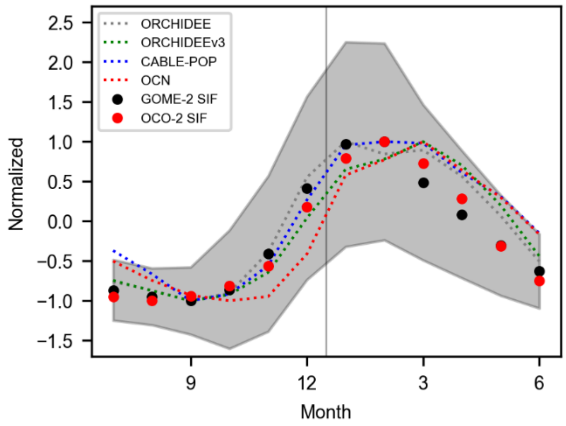

Fig. RC2_3: Like Figure 6, but with monthly mean OCO-2 740nm SIF.

Lines 278-279: CAMS cannot be identified from the spread of models in Figs. 2-3.

We will change the figure reference to Fig A2 (former A1) and Figure A7 (former A3), which show the GOSAT CO_2 concentration together with CAMS concentration (Fig. A2) and the difference of the individual MIP models including CAMS to TM5-4DVar/GOSAT fluxes (Fig. A7).

Lines 283-284: You could add a very recent reference in your discussion of fire emissions in the regions. This one also seems to suggest underestimated fire emissions. Van der Velde et al. (2024): https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2023GL106122

Thank you for pointing out this paper. We will include the citation in line 284.

Lines 300 and 304: I think the correct spelling is La Niña and El Niño

Thank you for this correction. We will adapt our revised manuscript accordingly.

Line 302: “enhanced surface near soil moisture” this sentence might be missing a word?

We would rephrase the sentence to: ‘enhanced soil moisture near the surface’

Sect. 3.4 title: I’m not sure what is meant by gross fluxes. Could you just say fluxes?

We use the term gross fluxes for the constituent vegetation fluxes (GPP, respiration). Combined, they result in the net exchange fluxes. This differentiation between net and gross fluxes is used in literature (e.g. Baldocchi et al. 2018, https://doi.org/10.1016/j.agrformet.2017.05.015). By including 'gross' in the title we want to define the section against the discussions of the net flux variability before.

Line 321: remove "fluxes"

We will remove the word.

Line 344: Suggest to rewrite the sentence starting "A by 1-2 months…" as it is not easy to read.

Going along with suggestions of reviewer 1, we will rephrase the sentence as following: 'A prolonged emission phase of an additional 1 to 2 months ...'

Lines 359-362: To me, carbon uptake by grasslands dominating the IAV of the entire southern African fluxes sounds like an important finding that should be brought to the Abstract and perhaps even contrasted in magnitude against fire emissions to the reader so that the magnitude is easier to grasp. At least to me this finding highlights the importance of an improved understanding (and modelling) of the sensitivity of grasslands.

We will adapt the sentences about the IAV in the abstract to also mention grasslands. Doing so, we also take into account the comments of reviewer 1 concerning the novelty of the findings.

"Doing so, our satellite-based process analyses pinpoint photosynthetic uptake in the southern grasslands to be the main driver of inter-annual variability of the southern African carbon fluxes, agreeing with former studies based on vegetation models alone."

Furthermore, we will add a discussion about the impact of fire emissions on the IAV of NBP. In the new paragraph, we also compare the IAV of GFED fire emissions to those of the vegetation fluxes:

"According to GFED (see Fig. A10), fire emissions play a minor role in impacting GPP and driving NBP anomalies. The variability of fire emissions is much lower than for NBP and GPP-RA. In the whole study region, IAV (calculated as standard deviation over the years) of GPP-RA and NBP fluxes are 97.7 TgC/year and 94.1 TgC/year, respectively. IAV of GFED fire emissions is 27.3 TgC/year, similarly low as IAV of RH (27.1 TgC/year). Furthermore, the annual fire emissions do not amplify the trend of the NBP anomalies. They have been on a normal level during the large positive NBP anomaly in 2016. Higher than average fire emissions counteract the sink anomalies in 2011 – 2012 and only the slightly reduced fires in 2017 amplify the sink anomaly."

Lines 359-362: This is an example where the sampling of the satellite data for GOSAT inversions might end up affecting the resulting reasoning (see my Major comment #2).

Line 359-362 describes that in 2013, 2015, and 2016 the emission phase is by 1-2 months longer than in the other years. The number of satellite measurements is smallest during the end of the emission phase and the beginning of the growing season. Therefore, the fluxes have a higher uncertainty than in the rest of the year. In 2013, 2015, and 2016 the number of measurements is not especially low compared to other years (see Figure A6 above). However, within a more detailed analysis of the prolonged emission phase, it would be necessary to analyze the measurement locations during these months further to make sure that the flux estimates are reliable. Such a detailed analysis is out of the scope of this paper.

Line 374: rain induces → should this read rain-induced?

Yes. We will correct that.

Figure A1 caption: und → and

We will correct the typo.

Figure A1 legend: what does "CS" stand for? Also, the legend is covering part of the data, please revise.

We will add that cs is the abbreviation for co-sampled. Furthermore, we will move the legend. See new figure below:

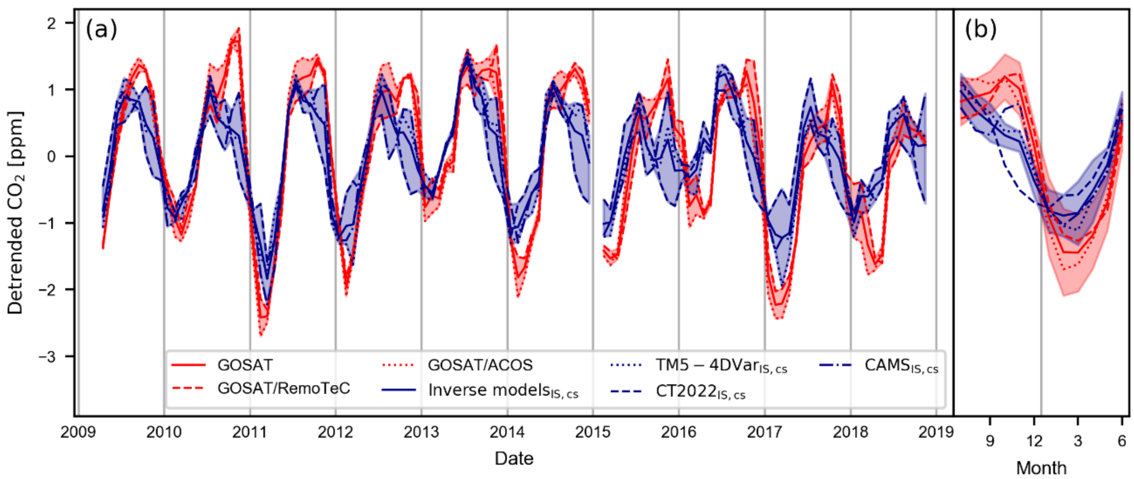

Figure A3: it seems that months 10 and 11 are particularly challenging. Could you please add a bit of discussion related to this in the text?

Taking also into account the questions and comments of reviewer 1 we will include an additional panel to Figure A3 (now A7), two new supplement figures and a new supplement text evaluating the MIP model fluxes in more detail. Doing so, we will also address the differences between the individual months.

"In general, the GOSAT flux mismatch and the OCO-2 XCO_2 mismatch is larger in October and November than in September. This is most likely caused by the prior fluxes in

September being closer to the GOSAT based fluxes than in the other two months (see panel b in Figure A9)."

Please find the new figures and complete discussion in text A1 in the response to reviewer 1.

Figure A8: Just a comment: this amount of precipitation is likely to affect the number of satellite observations of the region. Please see my Major comment #2.

Yes, we will include the connection between the rainy season and the amount of GOSAT data in the main text as mentioned in the response to your major comment:

"The number of GOSAT measurements (see Fig. A5 and Fig. A6) is variable throughout the year with the smallest numbers occurring during the rainy season around December and January."

Figure A9: Are these measurements assimilated in any of the in situ optimized inversion models discussed in the paper? The time span would match part of the time period discussed in the paper.

No, these CO_2 flux measurements are not included in the ObsPack dataset, which is used in the in situ optimized inversion models. In general, only CO_2 concentration (and not CO_2 flux) measurements are assimilated in the inversion models. Even though FLUXNET stations are also measuring CO_2 concentrations (not shown in Fig. A9 (new A15)) they are usually not included in atmospheric inversion systems.

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