

Point by point answers to reviewers comments

RC1

Comment 1: Throughout the discussion, the authors discuss the findings, but fail to provide some deeper context. In my opinion, the interesting findings of this manuscript should be placed in context better. This would improve the impact of this manuscript, as it allows for follow-up research. I would urge the authors to focus more on the (possible) mechanisms that drive the findings in this manuscript.

For example, the authors do not mention why the tropics account for such a large portion of the covariance. Is this because the IAV in the tropics outweighs IAV in temperate regions (not according to the inversions)? Or is this because droughts in the tropics (i.e. the ENSO cycle) covers the entire tropics (whereas droughts in temperate regions are more driven by synoptic variability and thus happen over smaller scales). Additionally, the authors should mention why TWS is a better explanatory factor than e.g. VPD or temperature. In L.616, the covariance between temperature and water availability is mentioned but not discussed sufficiently.

Response: We have added text describing reasoning to look at TWS rather than other variables L53-57. We have added text about the significant role of the tropics L235-239. We have also added more text about covariance between temperature and VPD L494-495.

Comment 2: Finally, the positive correlation between temperate TWS and carbon growth rate should be mentioned, regardless of the small contribution to the growth rate. Can this be explained physically?

Response: We mention this positive correlation and give possible physical reason such as opposite response to ENSO L219-222

Comment 3: Additionally, the analyses should be done with more than four inverse models. The GCB inverse fluxes are made publicly available (<https://meta.icos-cp.eu/objects/FHbD8OTgCb7Tlvs99IUDAp00> for GCB2023 and more recently <https://meta.icos-cp.eu/objects/GpFcABoKcZMVnRUILHRInhdM> for GCB2024). Some of these models indeed only cover the OCO2 period (2015 onwards), but for GCB2023 and 2024, 8 systems with sufficient data are provided. Therefore, I expect the analyses to be done with all available models.

Response: We have updated analysis to include all 8 inversions in GCB2023.

Comment 4: L.75: A reader might find it strange that the first sentence of the plain language summary says the CO2 increases every year, but reads here that there is a decrease (which I understand is in the growth rate, so there could still be an increase). I would recommend to maybe rephrase to larger and smaller growth rates with El Nino/La Nina

Response: Plain language summary is no longer included since it is not required for journal format.

Comment 5: L.96: "Most vegetation responds to soil moisture". Yes, but also to VPD (which is not included in TWS). These effects are difficult (if not impossible) to disentangle, but interesting to mention.

Response: We now mention it is difficult to disentangle L58-62.

Comment 6: L.106: I think your definition of NBE is the same as the definition of NEE used here. It could be easier for a general reader to use only one of the two.

Response: In my study I use NBE consistently when referring to my own work. I mention NEE only when referring to other studies who have spoken about 'NEE'. While the two quantities will show similar variability, NBP captures the total flux between land and atmosphere, including disturbances which NEE does not include. Therefore, I believe it is correct to use NBP for the work in my study but still need to use NEE when referencing the other studies.

Comment 7: L.237: Linear detrending removes the fossil fuel trend, but also any trend in biogenic and ocean fluxes. This should be mentioned

Response: This is now mentioned L165-166.

Comment 8: L.238: Why are the CGR and TWS smoothed using different windows? To me it makes intuitive sense to use the same window

Response: Why different smoothing windows are used is now mentioned in text L167-169.

Comment 9: Fig 2: Here, the growth rate can become negative (so the first sentence of the introduction is not true, or is this detrended growth rate?)

Response: This is detrended growth rate. 'de-trended' is stated L194.

Comment 10: Fig 3: The positive correlations in south Brazil and east China are quite interesting, and could be explained biophysically. This links to my first general comment as well.

Response: We mention this positive correlation and give possible physical reason such as opposite response to ENSO L219-222.

Comment 11: L.305: It would be nice to add a horizontal line showing the global correlation to make it more explicit that the tropics can indeed explain all the correlation

Response: Horizontal line has been added.

Comment 12: L.432: It's interesting that NISMON has a larger range, and I recommend the authors to (shortly) discuss any potential reason for this (e.g. prior model, observations used, transport model). The inclusion of other models might help in this.

Response: We discuss NISMON as outlier L335-343.

Comment 13: L.537: I'm not sure I understand the phrasing 'still a key factor'. Is this in contrast to other studies that pointed towards temperature? Otherwise just remove the 'still'

Response: removed.

RC2

Comment 14: The abstract reports a strong TWS–CGR correlation, but it lacks a statement explaining how this finding advances previous work (e.g., Humphrey et al., 2018). Please clarify further how this study uniquely extends or deepens our understanding.

Response: Abstract revised L17-18.

Comment 15: Lines 28-31: The sentence “tropical forests exhibit the strongest CGR correlations” is important but could briefly explain why—e.g., due to high productivity sensitivity to water stress—so help readers understand the physiological context.

Response: Abstract revised L10-12.

Comment 16: In Section 2.3, clarify whether all four inversion products use harmonized fossil fuel and biomass burning emissions (e.g., GFED versions). Differences in fire emissions datasets could bias regional flux attribution.

Response: Clarified in L155-156.

Comment 17: Table 1 summarizes inversion methods, but the main text should include 2–3 sentences interpreting key differences in transport models, meteorological fields, or prior flux assumptions and how they might affect tropical vs. extratropical estimates.

Response: New paragraph added discussing differences L141-149.

Comment 18: I don't think it's necessary to place Figure 1 in the main text. It is recommended to put it in the supplementary files. Overall, there are too many figures in the full text. It is suggested to combine them.

Response: Figure 1 has been moved to the supplementary.

Comment 19: Lines 293-294: Please explain why some regions with high local correlation contribute minimally to the global signal—e.g., due to small TWS variance or maybe small spatial extent—and explicitly state how these cases are handled.

Response: Explanation added and method to address this given L212-217.

Comment 20: In Figure 4, report the number of grid cells per land cover class and provide standard deviations or interquartile ranges to contextualize variability in contribution estimates.

Response: Regional areas are now given L227-231.

Comment 21: Annotate key ENSO or drought years (e.g., 2005, 2010, 2015–16) directly in Figure 5 to aid interpretation of CGR–TWS relationships and align with the narrative.

Response: ENSO years have been added to Figure 4 (previously figure 5).

Comment 22: lines 390–395: reference a figure or appendix that visualizes cross-regional TWS anomaly compensation (e.g., a correlation matrix or spatial covariance map), supporting the claim of cancellation effects in croplands.

Response: Figures added to supplementary supporting this claim.

Comment 23: The sensitivity analysis in Figure 10 is valuable, but the ecological interpretation of why tropical forests show both high correlation and high sensitivity should be more deeply discussed—e.g., in terms of water-use efficiency or rooting depth.

Response: Deeper discussion of this added L413-421

Comment 24: lines 530–534: Clarify whether TWS–CGR correlations were adjusted for or confounded by co-varying climate factors (e.g., temperature, VPD). If not adjusted, include a cautionary note on potential indirect effects.

Response: Cautionary note added L494-500.

Comment 25: lines 543–548: In discussing cases where TWS is weakly correlated with CGR (e.g., tropical Africa in 2016), could consider fire activity, radiation, or phenological anomalies as alternative drivers.

Response: Added discussion of this L435-440.

Comment 26: In the conclusion, clearly articulate how your findings can inform terrestrial biosphere model development. For example, suggest that ecosystem models should incorporate regional water constraints with higher fidelity, particularly in tropical forests.

Response: Added to conclusion L564-566.

RC3

Comment 27: A major concern with this analysis is lack of discussion of northern extratropics. Previous analysis has shown that these areas are important contributors to CO₂ IAV (Guerlet et al., 2013 – who showed that inversions using in situ CO₂ only did not see the flux variability that was visible from space-based data; Keppel-Aleks, et al., 2014 – who showed that taking annual averages of CO₂ data, which is what is done to calculate CGR in this paper – masked variations that were attributable to northern extratropics). I point out these two papers in particular because the authors use inversions driven by surface CO₂ rather than satellite CO₂, and because their global correlations are derived from global averaged CO₂. Given the four inverse models show a huge spread in the northern extratropical contribution to CO₂ IAV, with NISMOM showing these areas contribute only

5% to global CO₂ IAV and CT2022 showing contributions closer to 35% of global CO₂ IAV, more analysis of the impact of this discrepancy is required.

Response: We have added more text talking about disagreements in NH et and included the reference Guerlet et. al. (2013) L347-353. We also mention the seasonal sensitivity in extratropics and reference Keppel-Aleks et. al (2014) L467-469.

Comment 28: A second concern is that tropical Africa contributes almost as much CO₂ IAV as tropical America, but this variability is not as well correlated with variations in TWS. What, then, does this mean for the utility of the high correlation between CGR and TWS? The authors diagnose the regions that drive the apparent global correlation with TWS, but they do not provide much analysis or any conclusions about in what way the emergent global correlation can or should be used in carbon cycle research.

Response: Text added to discuss this in L444-451.

Comment 29: I am also somewhat skeptical how robust are the results from these four inverse models. The models do show substantially different regional fluxes, by at least a factor of two for each of the geographic regions considered. More inverse models are available for analysis, and I am curious about what a larger ensemble would reveal. Section 2.5 stated that correlations were calculated from an ensemble mean flux product. I am curious as to what the correlations with TWS variations would look like from each model separately. Would the conclusions be robust to this change in methodology?

Response: More inversion products are now included in the analysis

Comment 20: Analysis from the OCO fluxMIP shows significant distinctions between inversions that are configured to adhere tightly to the ocean prior and those where the ocean can move. Are the differences in terrestrial fluxes within this small 4 member ensemble associated with tight ocean priors? And related to a point above, what would the analysis look like based on inversions that use space-based CO₂ observations to constrain fluxes? In theory, the space-based data provides better spatial constraints than the sparse surface network.

Response: See response to reviewer 3 in original response posted.

Comment 31: In summary, I recommend more discussion and stronger conclusions about what it means that many regions that contribute a fair amount of CO₂ variability do not have a strong correlation with TWS, and other regions that have strong TWS variability do not show much variation in CO₂ flux. The paper would have more impact if it provided recommendations about what key carbon cycle inferences are being obscured by using the global relationship between CGR and TWS as a shortcut.

Response: Overall in updated manuscript we have added more in depth discussion in several areas, included a larger range of inversions in analysis, and tried to emphasise more clearly the use of results e.g. L564 -566.