Response to RC1:

The authors of "Strong relation between atmospheric CO2 growth rate and terrestrial water storage in tropical forests on interannual timescales" have provided a very thorough and comprehensive analysis of interannual variability of terrestrial water storage and its link to the atmospheric growth rate of CO2. This research adds to our knowledge of interannual variability of land carbon fluxes and opens new pathways for discovery in this direction. The results are interesting and the research should be published with some modifications. However, I would really implore the authors to deepen their discussion, which I will elaborate below. Additionally, I urge the authors to use a broader spectrum of inverse models. Some other, smaller comments are provided after.

We would like to thank the reviewer for their thorough and thoughtful feedback. We greatly appreciate the time and effort taken to review our manuscript. Below, we provide a response to each comment outlining our proposed revisions and clarifications.

General comments

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.

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?

We appreciate this comment and agree that providing deeper context and mechanistic insight will significantly strengthen the discussion. We believe the tropics account for a large portion of variability due to a combination of spatial coherence in the tropics and large amounts of IAV. These two things can be difficult to disentangle. In the revised manuscript we will include deeper discussion of this and refer to additional references which also highlight the importance of the tropics. We will include text such as:

- The tropics play a disproportionately large role in driving the covariance between CGR and regional TWS. This is due to a combination of factors: (i) the tropics exhibit some of the largest magnitudes of IAV (ii) Climate anomalies such as ENSO events often synchronise drought and temperature anomalies across vast tropical regions, creating spatially coherent signals that are amplified at the global scale. In contrast, temperate regions typically experience more localized and less synchronised climate variability, resulting in smaller contributions to global CGR variance.

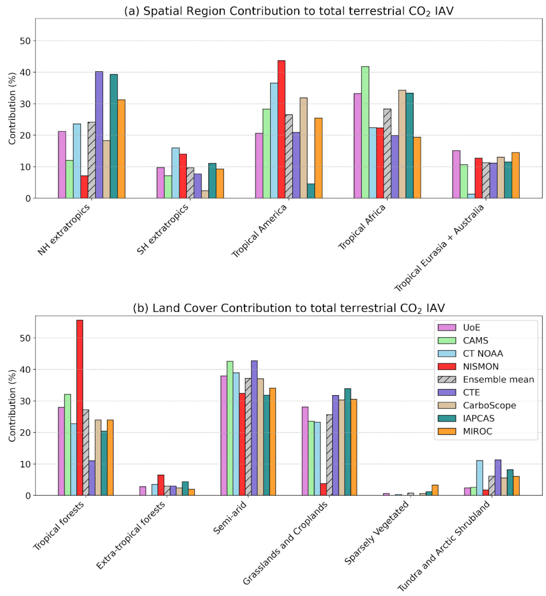
I don't think we can say for certain that TWS shows more explanatory power than VPD, but we will include discussion of this and highlight potential benefits of using TWS, and our rationale for our focus on TWS and the potential advantages it offers. One key motivation is the availability of large-scale observational data from the GRACE satellites. In contrast, at large scales VPD often relies on model outputs; for example, He et al (2022), demonstrated the significant negative correlation between VPD and NEP IAV using FluxCOM and TRENDY data, whereas our study emphasises an observational approach. Additionally, TWS reflects integrated water availability across all components of the hydrological cycle, while VPD primarily represents atmospheric demand. TWS can capture the cumulative effects of prolonged droughts on ecosystem productivity, which is particularly relevant for understanding carbon cycle dynamics. Although we do not claim that TWS is a better explanatory variable to temperature or VPD, it offers a complementary perspective and allows us to leverage independent observational datasets.

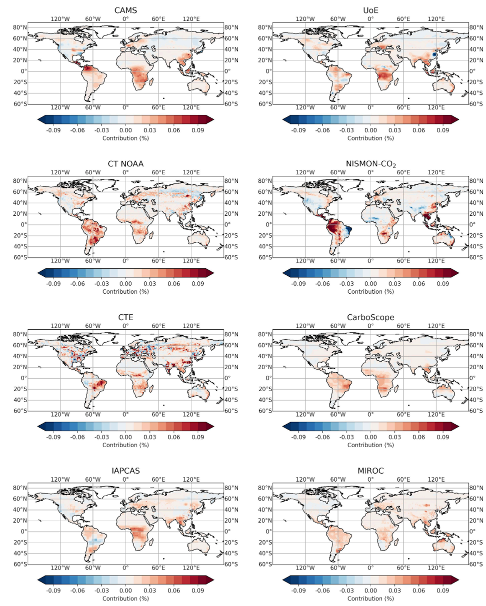
We will include more in depth discussion about covariance between temperature and water e.g highlighting methods Humphrey et al (2018) used to demonstrate the effects of water on CO2 variability held independent of temperature effects.

Additionally, we will discuss the covariance between temperature and water availability more thoroughly, including how multicollinearity may complicate interpretations. We will comment on the observed positive correlation between temperate TWS and carbon growth rate, particularly in regions like South Brazil and East China, and provide possible biophysical explanations such as these regions having an opposite response to ENSO.

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/FHbD8OTgCb7Tlvs99lUDApO0 for GCB2023 and more recently https://meta.icos-cp.eu/objects/GpFcABoKcZMVnRUlLHRInhdM 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.

We agree with the reviewer's suggestion that additional inversion products will strengthen our analysis. We will incorporate all inversion models made available through GCB2023 that have suitable temporal availability. The updated analysis will include CarboScope, CTE, IAPCAS and MIROC models along with the existing products used (CAMS, UOE, NISMON_CO2 and NOAA CT). This will involve updating Figures 6,7,8,9 and 10 and necessary text to be consistent with new results. Below shows updated Figures 7 and 8 with additional inversion products, Figures 6,9 and 10 look very similar to before and main conclusions have not changed.





<u>**Technical comments**</u>

1. 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

Thank you for highlighting this. We will revise the wording to clarify that while atmospheric CO2 levels are increasing every year the increase in some years is smaller than other years i.e larger during El Niño years and smaller during La Niña years.

2. 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.

Thank you for pointing this out, we very much agree with this point. We will mention the challenges in disentangling these effects due to the fundamental issue that VPD and soil moisture are coupled, where high VPD usually corresponds to dry soils and low VPD to wet soil. We will include references such as He et al (2022) which explores the impact of VPD on CO2 variability.

3. 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.

Will change text to be consistent throughout and use NEE.

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

We will clarify that while linear detrending is used to remove the long-term fossil fuel-driven trend in CO_2, it also removes any long-term trends in biogenic and ocean fluxes.

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

We chose these smoothing windows to be consistent with previous literature i.e Humphrey et al. (2018) who also chose different windows to give best readability of figures. The different smoothing windows were used because the CGR data is inherently noisier than TWS and required slightly stronger smoothing to reveal meaningful interannual variability. Note that the mean seasonal cycle is in any case already removed before smoothing. We will add a sentence to clarify this in text.

6. **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?)**

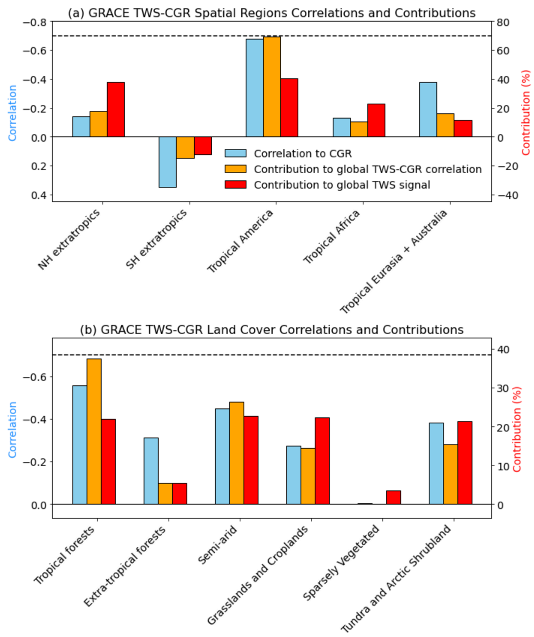
The growth rate has been detrended. We will clarify in text any confusion about what the negative growth rate means. By Figure 2 we have added "It is important to note that, although atmospheric CO_2 concentrations continue to increase each year, the CGR has been detrended; therefore, negative CGR anomalies reflect periods of below-average growth relative to the long-term mean growth rate."

7. **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.**

The area in South America is dominated by grassland. There could be a number of reasons for this result. These regions are thought to have an opposite response to ENSO.. However, it could also be the case that these regions are not having a large impact on the global CGR. We will include discussion of this in text.

8. 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

We will revise the figure to include a horizontal line indicating the global correlation value at -0.70. See updated figure below:



9. 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.

The inclusion of additional inversion models in our analysis does indeed highlight that NISMON stands out for the particularly large contribution from tropical forests. While prior fluxes typically contribute relatively little to interannual variability, a potential factor influencing NISMON's larger range could be the transport model (NICAM). However, a more targeted analysis—such as that done in intercomparison projects like TRANSCOM (e.g., Baker et al., 2006)—would be needed to draw firmer conclusions. A lack of observational constraints in the tropics may also contribute to this variability; however, this limitation is common to many inversion systems and does not fully explain the observed differences.

Peylin et al. (2013) also found that NICAM differed from other inversion systems, particularly in the tropics, where it produced a larger carbon release and broader IAV response during major El Niño events such as 1997/1998.

10. 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'

Our intent was not to contrast with studies emphasizing temperature, but rather to highlight that—even though the global water storage signal does not always align with the global CGR—regional water storage anomalies (as shown in Figure 5) do exhibit interannual variations that coincide with CGR changes during certain periods. Therefore, water may still play an important role, even if this is not immediately apparent from the global averages alone. We will remove the word 'still' to avoid confusion.

11. L.624: I disagree with the statement that atmospheric inverse models are "specifically designed to regionalise carbon fluxes". Atmospheric inversions cannot distinguish CO2 from different regions, and mainly constrain the atmospheric carbon budget. Especially observation-sparse regions like the tropics are hard to constrain by atmospheric inversions (even satellite-based inversions). This does not mean that inversions cannot be used to analyse regional impacts -- I think your use of multiple models is suitable as it covers a range of flux realisations. And although I appreciate the downtuning later in this paragraph, I think this statement needs refinement.

We will revise this sentence to say "Atmospheric inversion models designed to constrain the atmospheric carbon budget, but are often employed to regionalise carbon fluxes and investigate temporal variations, based on available atmospheric observations."

References

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