Response to reviewers for BG-2022-92: "Drivers of intermodel uncertainty in land carbon sink projections"

List of most relevant changes made in the manuscript:

- We follow the reviewer's suggestion and now use the sensitivity of NBP to CO_2 (sCO_2) instead of the sensitivity of GPP to CO_2 as one of the drivers of intermodel uncertainty in land carbon sink projections. We therefore updated Figures 5 to 8, as well as parts of the text.

Referee #1:

Padrón et al argue that in a multiple regression of the drivers of cumulative NBP, the explanatory variable related to CO2 should be based on the sensitivity of GPP to CO2 and not NBP as follows (Ln 162 – 168): "These simulations limit confounding effects from changes in temperature and soil moisture as they only account for the biogeochemical effects of rising CO2. However, when computing the change in NBP in these simulations, it is important to note that model differences can also arise from differences in RA, RH and DIS that are highly dependent on how these fluxes are influenced by temperature and soil moisture in each model. Therefore, we decide to use the sensitivity of GPP (instead of NBP) to CO2 as a driver of intermodel uncertainty in land carbon sink projections to better disentangle the influence of CO2 from that of temperature and soil moisture, even though the indirect effects of CO2 on RA, RH and DIS are ignored in this case."

I don't agree with this line of reasoning. In the 1pctCO2-bgc simulations there is no radiative coupling to increasing CO2 so there is no radiatively-driven trend in climate in these simulations. Thus the trend in NBP with CO2 should not be affected by trends in T and SM as there are no trends in T and SM unless affected through the physiological action of CO2 on stomatal conductance. I assume the authors are arguing that differences in model baseline T and SM may influence NBP in the 1pctCO2-bgc, which I guess they do, but also assume their influence on the response of NBP to CO2 is small. And baseline differences in model T and SM are accounted for in the cumulative NBP multiple regression already.

We appreciate the comment. Our main point is rather that there are important model differences in the sensitivity (i.e. the slope of the regression) of RA, RH and DIS to interannual temperature and soil moisture variability, which can explain differences in projected cumulative NBP from the 1pctCO2-bgc simulations due to the asymmetric nature of the response of NBP to cold/wet and dry/hot years. Therefore, we expect some additional collinearity between sT and sSM with sCO_2 when computing sCO_2 as the sensitivity of NBP to CO_2. This was our primary reason to use sCO_2 as the sensitivity of GPP to CO_2 in the main text of the manuscript. Nevertheless, we agree with the comments below and now use throughout the main text the sensitivity of CO_2 to NBP as a driver of the intermodel uncertainty in land carbon sink projections.

My original point stands that the various drivers of inter-model spread in cumulative NBP are not compared on an equal footing. While sT and sSM are NBP sensitivities, sCO2 is a GPP sensitivity. This is illustrated by the analysis in the supplement of sCO2 is calculated using NBP instead of GPP (compare Figure S17 a and b respectively). In almost all cases the multiple regression prediction of cumulative NBP is closer to the ESM cumulative NBP when sCO2 is calculated using NBP rather than GPP (Figure S17). In some cases the change is small (but never worse), while in some cases the improvement is substantial – e.g. the white and blue points in S17a are closer than in S17b for ACCESS, IPSL, CanESM, CNRM.

This is an important point because for almost all models, using NBP to calculate sCO2 also increases the proportion of cumulative NBP that is attributable to sCO2, i.e. their CO2 sensitivity (in addition to improving the multiple-regression model fit). It's not clear by how much from the presentation of *the results but it seems like differences in model CO_2 sensitivities are of similar magnitude as T and SM sensitivities at explaining inter-model spread in cumulative NBP.*

The CO_2 sensitivity needs to be calculated with NBP as the response variable, not GPP. This will require a major revision of some of the text and figures.

We agree with this insight. We now replace Fig. 8 with Fig. S17 and modify the text accordingly.

Ln 16-23: "Results indicate a primary role of the response of NBP to interannual temperature and soil moisture variability, followed by the sensitivity of photosynthesis to CO_2, and lastly by the average climate conditions, which also show sizeable contributions. We find that the sensitivities of NBP to temperature and soil moisture, particularly in the tropics, dominantly explain the deviations from the ensemble mean of the two models with the lowest carbon sink (ACCESS-ESM1-5 and UKESM1-0-LL) and of the two models with the highest sink (CESM2 and NorESM2-LM). Overall, this study provides insights on why each Earth system model projects either a low or high land carbon sink globally and across regions relative to the ensemble mean, which can focalize efforts to identify the representation of processes leading to intermodel uncertainty."

Three of these highest and lowest models have a significant shortfall in prediction, possibly due to interactions or drivers missing from the regression.

We agree with this point. Another possibility is that this underestimation of the magnitude of the anomalies occurs due to a non-linear response to the sensitivities which are not captured by the multiple linear regression. This is discussed in the main text. We consider it too detailed to mention it in the abstract. We also note that this shortfall in prediction does not contradict our conclusions.

These results and conclusions presented in the abstract need to be a lot more quantitative. E.g. Why not quantify the contribution of each driver to inter-model spread rather than use language like "which also show sizeable contributions."

We modified the abstract accordingly.

Before I can recommend this for publication, sCO_2 should be calculated with NBP not GPP as the response variable and used in the multiple regression and other areas of the manuscript.

We now follow this suggestion.

Editor:

The authors are arguing to use the sensitivity of GPP to CO_2 and not NBP. My feeling is that modelled GPP itself would be subjected to marked differences in the temperature sensitivity (Rogers et al. 2017 New Phytologist) and soil moisture sensitivities (De Kauwe et al. 2017 Global Change Biology) that act on GPP. As a result, I'm not as convinced that this limits confounding effects. I think the authors need to address R2's comment as it is important point.

We appreciate the comment and now also mention it in the text.