Authors response to comments:

Parameter uncertainty dominates C cycle forecast errors over most of Brazil for the 21st Century

We thank you for your positive and constructive comments which we believe improve the manuscript. Below we deal with reviewer 1's comments in turn. Reviewer comments will be shown in red. Our responses to reviewer comments will be shown in normal text while new text intended for addition to the manuscript will be shown in *blue italics*.

Reviewer 1:

The study of Smallman et al. presents a model-data integration study where a suite of terrestrial ecosystem models of increasing complexity is inverted and evaluated using spatially resolved data across Brazil. The finding that already with quite simple models, parameter uncertainty is more important than model structural uncertainty and uncertainty in forcing data has a large impact of the earth system science and is worth to be published.

Thank you for your supportive comments.

I enjoyed reading the beginning of the paper and appreciated the well designed study using multi-model, multi-biome, site-specific spatially resolved setup, and varying input data for future climate scenarios, within a fully Bayesian inversion setting.

However, I got disappointed when I more closely inspected Table 2. Even with the simplest model, the confidence intervals of the predictions are so large, that only vague and general statements or conclusions can be drawn from the results. All the elaborations and conclusions on model complexity and structural error would be a really good presentations, if the results were more constrained. However, with the large uncertainties I would recommend to only summarize them and omit the detailed presentation, because they are base on vague ground.

Thank you for this comment. Uncertainties estimated at pixel level are directly derived from the pixel specific ensemble of accepted parameters. These uncertainties are presented in maps indicating where our analyses are consistent with independent evaluation datasets (e.g. Fig. 4) and spatial differences in structural vs parametric uncertainty (Fig. 7, S15).

As you note our uncertainties at both national and pixel level appear surprisingly large on first viewing. However, we argue that in reality it is the uncertainty estimates of other individual estimates (e.g. FLUXCOM and CTE) which are likely underestimating. For example, as we noted in the introduction, the range of global GPP estimates by independent methods varies between 80-170 PgC yr^{-1} (Shao et al., 2013; Joiner et al., 2018; Jung et al., 2020). If we assume a median estimate of 130 PgC that suggests an uncertainty of ~69% in just one component of the carbon cycle, likely the best known. Similarly, the range of values in the global land sink from independent atmospheric inversion frameworks vary on the order of 1 PgC yr-1 similarly representing an uncertainty of 50-60 % relative to our best estimates of the mean terrestrial C exchange (Friedlingstein et al., 2020, https://doi.org/10.5194/essd-12-3269-2020). CARDAMOM is simulating the whole terrestrial biogenic C-cycle, not just a component. Despite this greater challenge CARDAMOMs national scale estimate of parametric uncertainty for GPP is 51-77 % (Table 2) - similar to that found between independent estimates. In this context we do not think it is surprising that our estimate of uncertainty for net exchange is large or even larger than the mean magnitude. Moreover, we argue that as other studies provide their ecological interpretation - in the face of unknown uncertainties - it is appropriate that we should also provide interpretation. However, we understand that this interpretation must be clearly caveated.

Uncertainties associated with Brazil-wide totals (Table 2, Fig. 5, 6) were estimated assuming errors are fully correlated. We consider it to be the most conservative approach given our lack of robust information on the spatial correlations of uncertainties. We should have noted this in the text. The corresponding alternate extreme assumption, to assume all pixel errors are fully uncorrelated, yields unrealistically small uncertainties. The truth will lie somewhere between these two approaches. For example, M1 GPP shown in Table 2 (fully correlated uncertainty propagation) is 17.7 (9.8 / 23.4) with the uncorrelated assumption being 17.3 (17.0 / 17.6) TgC yr-1. Note the small difference in the mean is due to pixel-level uncertainties not being Gaussian and undersampling the possible between-pixel ensemble combinations. Were we to present the uncorrelated uncertainties this would imply that parametric uncertainty is smaller than model structure which is inconsistent with the pixel level information (Fig. 7, S13), supporting our choice of presenting the fully correlated assumption.

To address you comments we propose to:

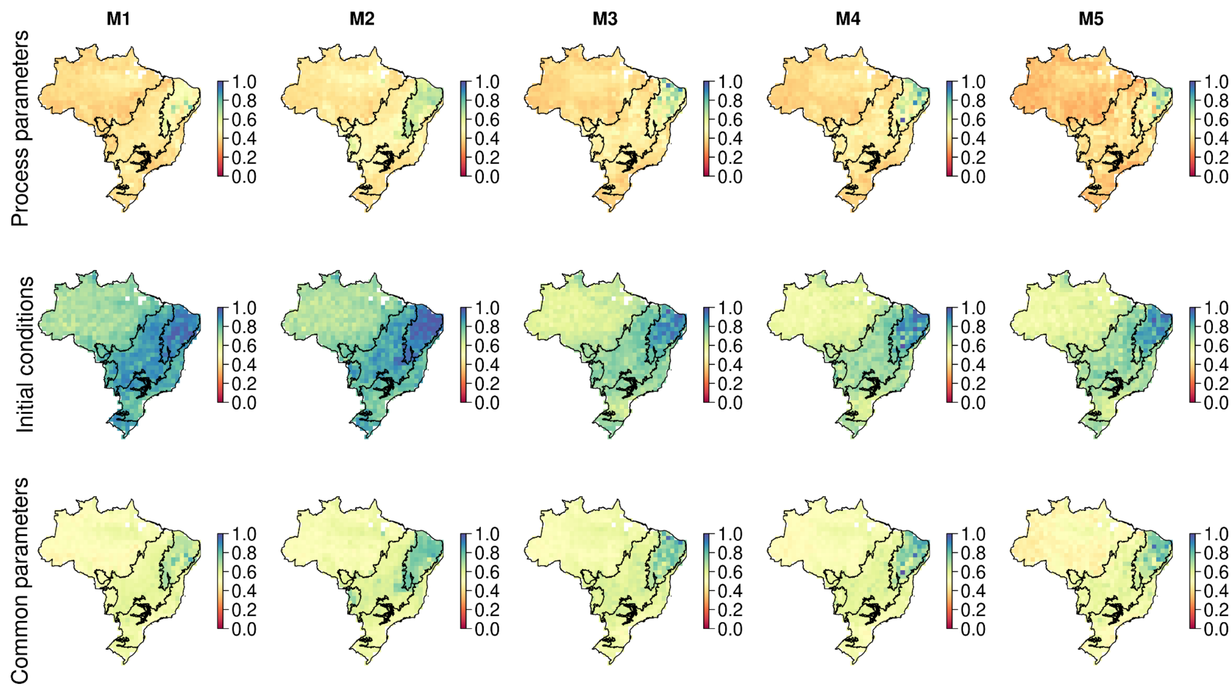
1) Provide improved context on the relative uncertainties represented by the range of independent estimates found in the literature (e.g. GPP and Net C exchange). Minor changes throughout the introduction and discussion will be made to clarify this.

2) To add the following paragraph to the CARDAMOM description (Section 2.1) to clarify our uncertainty propagation assumptions. "*Pixel-level uncertainties are estimated directly from the CARDAMOM retrieved ensembles of parameters, and their model generated C stocks and fluxes. However, we lack a robust understanding of how uncertainties are correlated in space, making the propagation of uncertainties from pixel-level to Brazil-wide challenging. Assuming an intermediate value would lead to an arbitrary estimate of uncertainty while assuming either fully-correlated or -uncorrelated uncertainties leads to either an over- or under-estimate in Brazil-wide uncertainties respectively. To be conservative, here, we assume uncertainties are fully-correlated when propagating from pixel to Brazil-wide estimates. To allow for non-Gaussian distributions in the pixel-level ensembles we assume that the fully correlated assumption is approximated by aggregating the pixel-level 5 % and 95 % quantiles across Brazil as previously done (e.g., Exbrayat et al., 2018b). Again to be conservative we will only discuss in detail between-model differences which are also supported in the pixel-level estimates.*"
3) Figure legends showing time series aggregates will include the following text. "*Note that uncertainties were propagated from pixel level to Brazil-wide totals assuming fully-correlated uncertainties.*"
4) Reduce the detail given when discussing the model structural impact on Brazil wide totals unless these are also substantial by the spatial patterning.
5) Add additional text in the manuscript that highlights the consistency / robustness of our conclusion from both the national scale estimates and at pixel-level which are not impacted by the poor knowledge of how to propagate uncertainty between resolutions.

Instead of studying increasing model complexity, the results show that the data is already not enough to constrain the simplest model variant. Statements like LL259 “simulated NEE was consistent with CTE ensemble at the 90% CI” does not tell me much about the goodness of the model, if the CI range is 400% of the median estimate. In order to defend the insights despite the large uncertainty, the posterior density of the parameters in comparison to the priors should be provided as a supplement or appendix.

Thank you for this comment. The issue of signal to noise is an important one which we have not directly dealt with in this paper as we consider it to be out of scope. However, it is important to consider the relative uncertainties of our model analysis versus the observational information available. For example, proportionally the CTE ensemble has a mean uncertainty of 3000 % due to much of their analysis being near zero while the DALEC models vary between 1200-3100 %, which we accept is currently biased towards a net uptake of C. However, the mean absolute uncertainty value for NEE from the CTE ensemble (0.5 gC m-2 day-1) is less than the DALEC models’ (3.1-3.5 gC/m2/day).

We now provide in the appendix maps (figure below) for each model showing 1-posterior:prior ratio (i.e. uncertainty reduction in the posterior relative to the prior) for the process parameters, initial conditions (C and where appropriate water) and parameters which are common to all models. A new table will be added to the appendix containing the Brazil wide mean posterior reductions per model for all parameters.



New Figure - shows the mean proportional reduction in the posterior parameter ranges relative to the uniform prior ranges sampled from by CARDAMOM. Columns show the results for each model M1-5. The top row shows the posterior reductions for the process parameters, the middle row shows the initial conditions (i.e. C and H2O pools at t=1) and the bottom row shows the parameters which are common across all models. A complete list of common parameters is provided in a new supplementary table.

In the main manuscript Sect. 3.1 Calibration Constraints, we will add the following text.

"*The reduction of parameter uncertainty between the 90 % confidence interval and the prior range is highly variable across Brazil, between parameters and to a lesser extent models (Table A2,3, Figure A7). The reduction in the parameter posteriors relative to the prior bounds (1-posteriorCI90:prior range) varies between model (M2 = 0.55, M5 = 0.46; Table A2) but with much larger variability between parameters (Rhet coefficient = 0.12, initial soil = 0.96) and* *across Brazil (Caatinga = 0.62-0.7, Amazon = 0.42-0.5; Figure A7). The spatial pattern across Brazil broadly follows the spatial distribution of precipitation (Figure A2). The greatest reduction in posterior parameter uncertainty is typically achieved in M2 with the lowest in M5 and broadly similar values in M1, 3, 4. Parameters related to initial C conditions and canopy phenology are best constrained, as expected given the majority of observations directly relate to these parameter groups, while NPP allocation and turnover / decomposition related parameters are least constrained in the posterior (Table A3).*"

The main conclusion about the dominance of parameter uncertainty is strengthened by this large uncertainty and should be published, with a much shortened presentation of the (to my opinion vague) comparison across model structures.

We appreciate the concerns of over-interpreting our results. But we do think it is appropriate to provide some interpretation around model structures to identify how model complexity affects analyses. Such interpretation would be expected in a paper using a classic land surface model approach with predefined parameters. However, in those circumstances model parametric uncertainty would be unknown. However, we will revise the results section to focus on cases with robust differences. To provide greater context, we highlight the differences between our analysis and information gained from a traditional land surface model analysis. We will add the following sentence to paragraph 3 of the introduction.

"*However, as TEMs typically lack information on their parametric uncertainty it remains unclear whether model differences are driven by different parameter estimates or model structure.*"

An alternative route, which requires a larger reanalysis effort, is based on the claim of the authors that the model can be constrained by repeated EO observations of biomass. In addition to the current model inversion, I suggest generating an artificial observation of this biomass data stream using the most complex model variant add noise and some slowly-changing bias and repeat the inversion including this artificial data. If the uncertainties decrease as much, the presentation about model structure could be kept, but based on this new (artificially) more constrained inversion results.

This is a very good idea and one we have discussed previously. The impact of assimilating repeat biomass observations has been quantified at site scale in a previous study (Smallman et al., 2017). However, we consider the synthetic study of repeat biomass estimates to be out of scope for the current study. Our primary focus here has been to quantify the relative contributions of uncertainty. This focus required a novel approach to assessing land surface models using ensemble based approaches which are typically not used for large scale land surface models due to their computational complexity. We will be focusing on testing the information content of repeat biomass maps explicitly in a subsequent study.

Specific comments

To gain an conception about the computational effort: At how many pixels was the model Inverted?

Very good question - this should have been included in the manuscript. There are 702 pixels. This information will be added to the opening sentence of the methods section (Sect. 3).

Line 220: It did understand how “future climate is imposed by determining the anomaly from the end of the analysis until 2100”. Please, extend this explanation.

We apologise that we have not provided sufficient detail on how we created the future climate drivers for our analysis. L220 will be expanded to the following.
“*The contemporary meteorology from observations differs from that generated in the climate models used to project future climate. As a result there are step changes in drivers between historical and future climate, impacting the simulation of the carbon cycle in an unrealistic manner. To avoid these step-change impacts future meteorology is imposed as an anomaly relative to 2018. Specifically, each month of the future meteorology extracted from the UKESM has the corresponding month from 2018 subtracted creating the anomaly time series, i.e. each month of 2018 anomaly would be equal to 0. The anomalies are then added to the absolute values of the monthly values from 2018 from the calibration meteorology time series but with sanity checks to prevent negative values in positive definite variables.*”

Line 223: I assume the model structural uncertainty was estimated for each climate scenario separately (and the climate uncertainty for each model variant separately), right? Or does the “between model range” span across all climate scenarios?

Sorry we didn’t clarify this point. We estimated the model structural and parameter uncertainty separately for each climate scenario and averaged across climate change scenarios to account for potential differences in model structural or parametric response under different climates. The text will be modified to clarify this situation.

"*Both parametric and structural uncertainties were estimated for each climate change scenario and then averaged across scenarios to provide an overall estimate*"

Fig 4: In my opinion, the stippling (indicating consistency within confidence range) does not tell much when considering the large uncertainties.

We believe that it is important and informative to indicate where across Brazil our model ensemble is consistent with observations. The stippling shows us that even with the large uncertainties some parts of the ensemble include a consistent assessment of carbon dynamics when considering the projections into the future. To provide appropriate context we highlight that our uncertainties are large and show where and by how much our analyses are biased (Figure A8) with respect to the independent data rather than considering a single metric of evaluation.

We will rebalance our results to provide a more comprehensive evaluation to avoid an over reliance on the stippling metric. We will strengthen our definition of consistency. In the existing framework we assumed consistency based on the mean flux over the relevant time period for each independent dataset. We will use a stricter measure which requires > 90 % of time steps in the overlapping period between the model simulations and independent estimates. This will provide a more granular interpretation of consistency. We realise also that our definition of consistency is not clearly defined in the text. We will add the following text in Section 2.4 to correct this omission.

"*A key evaluation metric is the degree of consistency at pixel level between the DALEC models and the independent historical evaluation data. We define consistency as the pixel-level* *ensemble of DALEC C-cycle estimates overlapping independent observations at >90 % of observed time steps*"

Fig 5: putting all the labels the center panel confused me first, I suggest putting the observation legends to the panels. Almost all the streams are encoded by color, which for me were difficult to read.

It is very important to us that our figures are as easy to read as possible. We would like to avoid adding additional complexity to the plots by adding figure specific legends or boxing the legend. We have moved the legend to the top panel to make it more noticeable and changed the line type for each of the models to help add greater distinction rather than colour alone.

L286, 294: Why is the NEE not improved with model complexity, if fire is improved and makes up 3 to 30% of NEE?

This is a very good question, the answer to which we have not made clear in the manuscript. NEE is not improved because of compensating changes in autotrophic and heterotrophic respiration (Table 2). Between M5 and M4 fire emissions increase, thus reducing the bias with independent estimates. However, at the same time respiration decreases providing a compensating impact on net exchange. This behaviour reinforces the need for greater constraint in our analysis using a range of new data such as those we summarise in the discussion section. To highlight the compensation challenge the following text will be added to the manuscript after L294.

"*Despite the improvement in estimation of C emissions due to fire there is no corresponding improvement in NEE or NBE due to compensating changes in both autotrophic and plant respiration (Table 2). This result highlights the need for greater overall constraint on the C-cycle, for instance independent estimates of respiratory fluxes.*"

L315: How did you assign priors to the MRT parameter? The sentence suggests a Normal distribution that includes also negative values. A lognormal prior would be more appropriate and you could report the multiplicative moments of the posterior and avoid negative residence times.

We do not provide prior estimates (with Gaussian uncertainty) for any C pool MRT parameters. Instead, all MRTs are provided only with a uniform prior range of ecologically plausible values from which the parameter proposals are drawn. Therefore, there are no negative values being proposed for MRT. In section 2.1 the following sentence has been revised to clarify "*Each chain assesses 100 million parameter proposals, drawn from uniform prior ranges, from which a sub-sample of 1000 accepted parameter vectors are stored.*"

Sec 3.3. reads lengthy. Are all the details necessary in the main text. I have, though, no specific suggestion how to shorten.

We will work to simplify this text to in a manner consistent with our other responses, e.g. ensuring that we highlight relevant results that are robust and support the interpretation.

L331: Hints to model error. Thanks for the discussion at L462ff, that could be referenced at this point. For me it did not become clear, how biomass removal was accounted for in the DALEC simulations, and the future scenarios.

We will clarify how biomass removal is imposed in DALEC in the model description. We will also add into the SI a figure (see response to reviewer 2 for figure) which shows how each of: biomass removal, fire and “natural” / unexplained turnover contributes to the overall estimated MRT. Critically we will see how these differing components vary across Brazil..

L 347: Can you quantify “most likely”? Can you infer p(deltaBiomass(t) > 0) from the posterior?

Yes we can. Our model ensemble estimates that in SSP 2-4.5W/m2 Brazil’s total biomass has a likelihood of 73-85 % of increasing while the likelihood of total DOM increasing is 64-84 %. We will add a complete table of this information for all scenarios to the appendix and the following line in paragraph 2 of Section 3.4.1.

“*Using our ensemble based approach we estimate that the likelihood of a net increase of C in biomass by 2100 is 73-85 % while the likelihood of a net accumulation in DOM is 64-84 % (Table A6).*”

L 450: may replace “a function of three factors” by “There are three possible interacting Explanations”

Done