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 2'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 2:

The manuscript " Parameter uncertainty dominates C cycle forecast errors over most of Brazil for the 21st Century" by Smallman et al. presents projections of Brazilian carbon cycling using a model-data hybrid approach. A terrestrial ecosystem model with varying degrees of complexity is constrained by remote sensing data before being used to quantify the evolution of carbon stocks into the future. Overall, I think the methodology is robust and I commend the approach, in particular separating and quantifying parameter, structural, and climate uncertainty on future stocks. The paper is well written, flows well, and addresses an important topic with substantial and robust conclusions.
Therefore, I would recommend the manuscript to be published after the following comments have been addressed:

Thank you for your positive comments.

In general, the paper has detailed reporting of results, however the explanations of

model behaviour are sometimes incomplete. This lets down the previous good work that comes beforehand. For example:

L406 - Can you explain why RT increased?

Thank you for your question. Our discussion tries to balance giving as full an investigation of the model outputs as we can without over-interpreting given the large uncertainties associated with our analyses. However, in this instance we didn't fully explain the likely pathway of the response as it may be specific to our modelling framework. The reduction in GPP (M3-5) coincides with the inclusion of the water cycle, resulting in water availability limits. At the same time CARDAMOM still aims to match the available observational information within the ecologically consistent parameter space defined by the priors and ecological and dynamical constraints. Thus, with reduced C inputs CARDAMOM responded by selecting parameters with longer residence times (i.e. reducing outputs) to maintain the C-balance consistent with the available observations.

L407 - I find it interesting that here the net flux correlates with GPP, but the long term net changes have low correlation with GPP. Can you explain?

Good question. GPP is a major component of C exchange particularly on seasonal timescales. However, on longer time scales the trajectory is determined by the residence times of slow turnover pools, i.e. wood and soil. This effect has been found in previous modelling studies (see our introduction). What we have done here demonstrates both responses within an observationally constrained framework. This result also shows our framework should also be

able to address this knowledge gap if we can assimilate high-confidence repeated AGB estimates.

We don't explicitly highlight the different processes dominating on differing time scales but we agree that it would be a benefit to do so. We will add the following text to make that connection when discussing the correlation between biomass change and residence time in the discussion (Section 4.3, paragraph 1)

"*The analysis of long term C trajectories contrasts with the correlation between GPP and net C exchange during the calibration period. This contrast highlights the importance of considering the timescale of change that is of interest, with wood and soil residence times driving long term net C exchanges.*"

L414 - Can you explain why Amazon flux is overestimated?

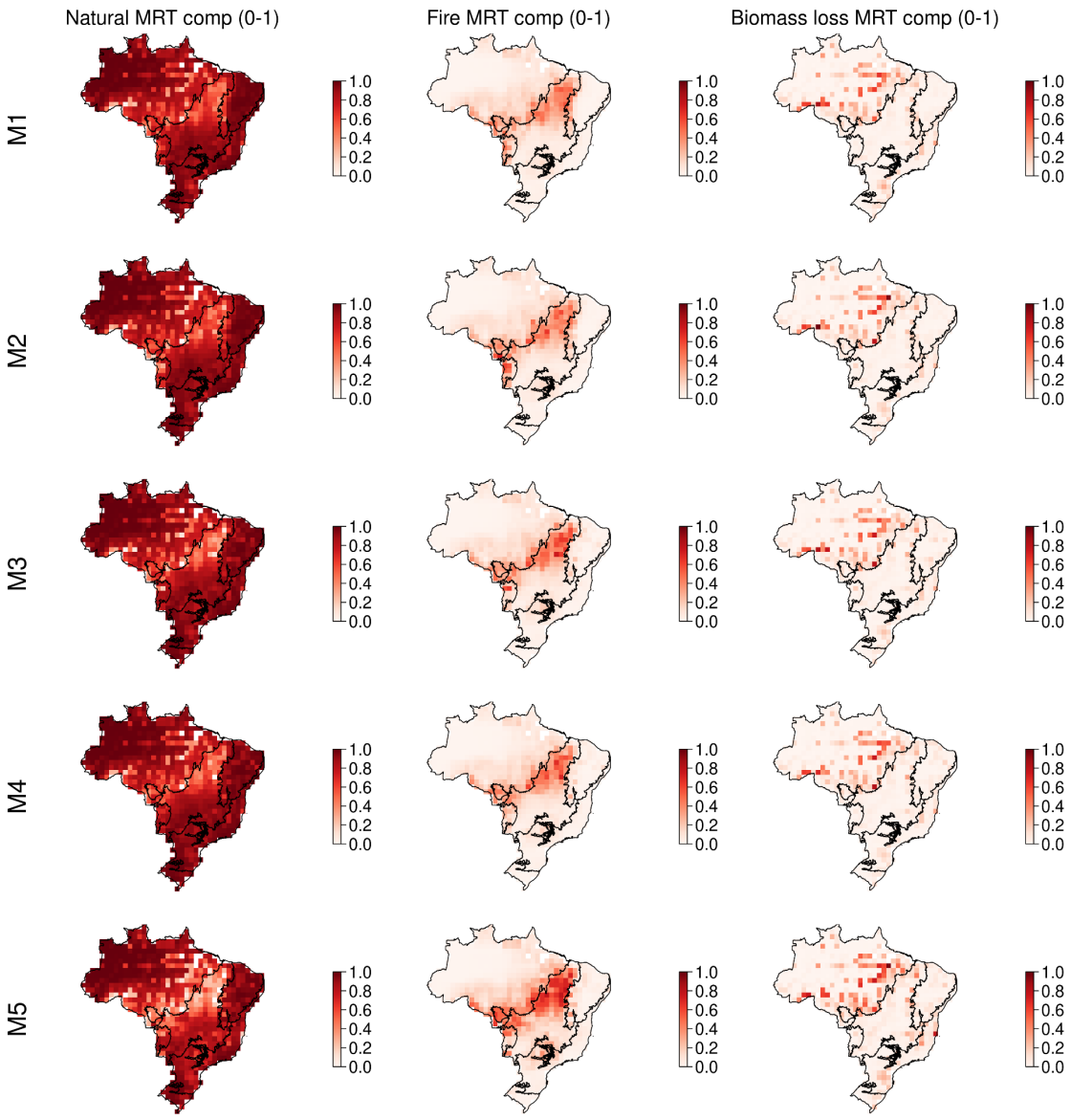
Despite the quasi-steady state assumption within the Ecological and dynamical constraints (EDCs) we find that CARDAMOM analyses tend to follow the trend in LAI, particularly in cases where there isn't a further indication of the C-balance e.g. high levels of disturbance. Across much of Brazil LAI observations are increasing over the calibration period. Finally, forest losses used to force the model (Global Forest Watch) are likely underestimated due to lack of degradation observations, which are significant (Milodowski et al., (2017) doi: 10.1088/1748-9326/aa7e1e). In the discussion we argue that the key missing information is repeated AGB estimates or other information which imposes a bulk C-balance constraint.

It would help the reader if more insight was offered into what drives model responses.

The introduction stresses the importance of land-use change and fire for the Brazilian carbon cycle, but there is little discussion on how these disturbances will influence future carbon stocks. Are they important? If not, why not? If they are, then how important?

You are quite right, we do stress the importance of disturbance as a driver of the C-cycle and impacting the community compositions of ecosystem traits. Much of the modelled response to future climate change is divided between the Amazon / Atlantic forests or dry regions, while disturbance (as observed in our current drivers) is focused on the arc of deforestation. We will add a new figure to the SI which explicitly partitions across Brazil the contribution of fire and biomass removals on the woody mean residence time. The following text will also be added to the results section to clarify the contribution of disturbance across the contemporary C-cycle.

Moreover, our analysis allows us to partition MRT into its constituent drivers, i.e. natural, fire and biomass removal, which indicates that given currently available drivers disturbance is only a major determinant of MRT across the Amazon Cerrado boundary (Figure A14).



New Figure - shows the fractional contribution of fire (driven by MODIS burned area), biomass removal (driven by global forest watch) and the remainder (i.e. natural / unexplained).

Finally, we will clarify how we extracted fire and biomass removal drivers from the future climate change scenarios with the following text in Section 2.5.

“Disturbance due to forest harvest is driven by the management scenarios associated with each SSP. However as DALEC does not represent land cover types we neglect land use change in the drivers. Thus any forest which undergoes biomass removal subsequently remains a forest and is allowed to regrow. Finally, as we currently lack a predictive model of fire in DALEC (i.e. we drive fire with EO burned area), we extended observed fire for the contemporary period into the future simulations.”

Also, it is not clear if/how land-use change is implemented in your future simulations. Is this implicit in the mortality parameter? It would be good to clarify this.

As indicated above we explicitly partition between natural, fire and biomass removal drivers of residence time. As described above we will add text clarifying both in the model and climate change source description how we use these drivers.

The writing style can be improved in places. There are a few dense paragraphs (that can sometimes read like a list), e.g. in Sections 3.3 and 3.4.3, there is often a detailed comparison between the five models that doesn't add much value. Can the values be moved to a table and then the text just discusses the most important parts? E.g. "Carbon allocation to wood as well as wood turnover rates determine future biomass stocks... Relative contribution of allocation and turnover varies across biomes…".

We will work to refine our presentation, divide paragraphs and remove overly descriptive text to improve the flow of the manuscript and focus on aspects picked up on in the discussion.

Also, at times the paper sounds quite negative. E.g. in Section 3.4, I feel some value is lost when all your results are caveated with "uncertainty is larger than predicted changes" or (L299-305) - "...insufficient observational constraint to confidently determine the sign of NBE or soil C dynamics. The same was largely true for wood stock dynamics..". I appreciate the uncertainty is important (and the focus of the paper), and certainly needs to be addressed, but this large uncertainty seems to undermine any projections you make about future stocks.

Thank you for this comment. We aim to be clear and open about the size of our uncertainties and their implications. However, we also recognise that we have not adequately set the context against other studies which discuss model structural differences but ignore their parametric uncertainty. We will seek to rebalance the tone of the manuscript as well as adding additional context on the size of uncertainties in the associated datasets. See previous comments for the first reviewer for examples.

Can M1-M5 capture the full extent of model structural uncertainty? You start to discuss this (L467) but in my opinion, I don't think this is a strong enough justification. Further, you state "...limited sensitivity of C cycling to future soil moisture stress.", which is at odds with drought experiments that suggest increased mortality and reduced productivity (e.g. ACL da Costa et al., 2010 - New Phytologist).

Thank you for your comment. We do not intend to fully capture the range of model structural uncertainty. Rather we wish to contrast parametric uncertainty relative to the impact of structural changes which are of a scale typically carried out in the TEM community and address specific

hypotheses. We have added text to clarify this in the introduction and in the text surrounding L467. Thank you also for highlighting that our analysis contrasts the results of the rainfall exclusion experiment. We have included this reference in our analysis. The paragraph starting L467 now read following:

Variations in C storage linked to model structure were smaller than those linked to model parameterisation, except in specific areas of Brazil (Caatinga; Figure 7, S15). The selection of five model structures was limited by our choice, so it is perhaps not surprising that the parameter calibration, which allows for multidimensional variation over broad priors, contributes more variation to projections than does model structural variability. However, the variation in model structure was designed to test whether hypothesised key processes were important in projections and similar to the kinds of developments which are tested and interpreted in ESMs and / or TEMs. For instance, we used models with and without a water cycle simulation to test the importance of carbon-water feedbacks in projections of C storage to 2100. Models M3-5 included dynamic simulation of soil moisture changes and its interactions with canopy processes. Projections with these models thus included the potential development of soil moisture stress, with an impact on GPP. Models M1 and M2 had no direct effect of soil moisture on C cycling. This soil moisture feedback on GPP only manifested itself in projections for north east Brazil, the driest part of the country, in the Caatinga biome, and some nearby parts of Cerrado (Figures 4, A8). This feedback does have an impact on projected C storage (Figure 7; Table A5), but these effects are of similar or less magnitude to parameter uncertainty. We conclude that for much of Brazil, outside of Caatinga, our model-data fusion shows a limited sensitivity of C cycling to future soil moisture stress. However, our modelled analysis contrasts the finding of the Caxiuana rainfall exclusion experiment which found drought enhanced tree mortality and reduced productivity (da Costa et al., 2010). Our result is likely a result of CO2

fertilisation leading to reductions in plant water demand that are explicit in both ACM GPP models. However, it is possible that land surface models like DALEC are overestimating CO2 fertilisation effect Wang et al., 2020) and/or by using time invariant parameters (i.e. traits) we are neglecting the impact of species change (i.e. biodiversity shifts) on ecosystem response. Collectively, these results highlight the need for further evaluation and refinement.

Minor comments:

- Looking at Figure A7, M5 looks like it still has a large fire bias similar to the other models, so can Can M5 say anything about the role of fire on future C stocks in the future?

You are correct that the spatial biases remain substantial in M5. What our analysis can tell us is that wood litter is an important combustible C pool which allows us at national scale to improve our C emissions. However, we still need more information to help constrain wood litter and other combustible pools. We argue that high confidence repeated AGB estimates will play a vital role in constraining both live biomass and dead organic matter dynamics (see Smallman et al., 2017 for site level experiment). More information on litter pools would be valuable too.

- FLUXCOM-RS+METEO is known to underestimate IAV. One option would be to use FLUXCOM-RS which uses interannual satellite LAI as an input and generally shows large IAV and trends. This could improve the comparison in Figure 5.

You are quite right to point out that our independent datasets will have limitations and bias' of their own. To improve the GPP contrast we have added two further independent GPP products

which use distinct approaches to that used in FLUXCOM and thus giving an improved representation of observational uncertainty associated with GPP. The newly included estimates are Copernicus GPP product (i.e. the same data provider as LAI assimilated here) and FluxSatv2 (Joiner and Yoshida. 2021, doi: https://doi.org/10.3334/ORNLDAAC/1835). Figure 5 and the GPP consistency metrics have been updated to account for this change.

L46-47 - Missing words?

We recognise that this sentence can be improved. We will revise this to:

"*However, estimated responses to environmental change are sometimes contradictory between studies indicating model ensemble (i.e. model) specific conclusions*"

L299 - "Carbon dynamics of wood, not soil, is the primary driver of net exchange." - Does this depend on timescales? Over the course of a century, won't slower (but potentially larger) soil C dynamics play a bigger role?

Yes time scales do make a difference. While wood as a relatively slow pool makes a major contribution to C balance. The reported values are assessing the contemporary period (2001-2017). Soil stock changes are highlighted in the future scenario element but we do not generate the corresponding correlation. Furthermore, as highlighted in the future simulations we show that both live and dead organic matter change substantially over longer time scales and we quantify the dominant controlling traits.

L394 - Missing words?

We apologise for that sentence. We have now clarified this and related sentences.

Increasing model complexity by including a wood litter pool (M5) reduces bias (by ~65 \%) between total fire emissions estimated by DALEC and independent estimates (Table 2, Figure 5). However, the spatial consistency remains largely unchanged with a persistent spatial disagreement across the southern Cerrado / Amazon boundary (Figure 4) and the DALEC estimated Brazil-wide fire emissions uncertainty increased on the addition of a wood litter pool (Table 2).

L395 - Increased from what? Due to increasing model complexity? Over time?

We apologise for this sentence we have revised it to:

"*However, the DALEC estimated Brazil-wide fire emissions uncertainty increased on the addition of a wood litter pool (Table 2) and there remains a persistent spatial disagreement across the southern Cerrado / Amazon boundary (Figure 4).*"

L394-402 - It would be good to place wood litter in the context of other drivers of fire. Is wood litter the most important variable if we want to correctly simulate fire? How about the local climate? Ignition sources?

You are correct that local climate and ignition source are important components of fire instance and emissions rather than just fuel load, as considered here. These topics are out of scope for the current study as we do not simulate the explicit impact of local climate (i.e. how dry is the

fuel load) or ignition. Our analyses are driven by satellite observations of burned area that integrate local climate and ignition source factors.

We will flag these issues in the discussion as you point out as components which are not addressed by this study.

L407 - Do you mean "GPP" instead of "net carbon uptake"?

No. A point we make on L410 is that NEE is becoming more negative (i.e. the net uptake is increasing) and that this is driven primarily by the positive trend simulated for GPP.

L454 - Why is it important that NPPWood is the second most important determinant?

Our results show that it's not all one or another. There is substantial co-dominance in many locations highlighting the need to target constraints on multiple components. However, we realise that this sentence is poorly written and will be revised in the following way

Similarly, it is important to note that NPPwood is the second most important determinant of future dynamics of both biomass and DOM, and in many areas is co-dominant with MRTwood (Figure 8). Efforts to constrain estimates of both MRTwood and NPP allocation are thus critical for more robust predictions of C storage

L456 - Don't some transient biomass maps exist already? e.g. ESA, or VOD based (Liu et al. 2015). L533 - But don't we have transient AGB and atmospheric NEE available now?

This is a good question and one that we considered out of scope to specifically pick up on limitations of current maps as approaches which address these shortcoming are already being developed. Briefly, the (ESA) CCI Biomass maps do exist for three years (2010, 2017 and 2018). However, the creators of these maps currently advise against using them for change detection due to their creation using different underlying field data and satellite information potentially introducing differing biases between years. We will note this in the discussion. The Liu et al., (2015) dataset derived using VOD remains an area to watch for us as the correct interpretation between plant water content and its biomass is yet to be fully refined. Finally, on the NEE / NBE side we choose to keep these data as independent evaluation to help provide a benchmark for future improvement. We will modify the beginning of the paragraph at L494 to highlight this choice but that assimilation is also a valuable opportunity.