Reply to Referees comments - Choice of Forecast Scenario Impacts the Carbon Allocation at the Same Global Warming Levels
Lee de Mora et al.

The authors would like to thank the editor, Somnath Baidya Roy, Anonymous Referee #1 and John Dunne for their efforts. Thank you all for taking the time to read the manuscript and share your comments. Your comments have been taken on board and the manuscript is in a much better place now after these revisions.

Both referees highlighted the need for a clearer articulation of the main findings. In order to clarify our findings, we have followed Anonymous Referee #1 suggestion and re-written significant parts of the paper, including the abstract, introduction, results, the discussion, conclusions. We have revised several figures, included a new figure that shows this result explicitly and added a new table of results numerically.

Below this introductory section is the reply to each of the comments point by point that we initially gave to the review. However, we have made significant changes to manuscript since the initial review and it is likely that many of these points are no longer relevant.

Our previous responses are marked in *blue italics*. For the technical and minor revisions, we will have implemented the changes below, but not all of the original text survived into the revised manuscript.

RC1: 'Comment on egusphere-2022-1483', Anonymous Referee #1, 03 Feb 2023

General Comments:

This article explores the carbon allocation with different choice of scenarios, SSP1-1.9, SSP1-2.6, SSP2-4.5, SSP3-7.0 and SSP5-8.5, at three different global warming levels (2, 3 and 4 degree Celsius). Authors comprehensively include a wide range of ESM outputs and design a quantitative analysis framework to calculate carbon fractions in different reservoirs. The current version of the manuscript matches the scope of ESD and the presentation of methodology is enough.

LdM: Thank you for a clear summary of the work. We're glad that it is within scope for ESD.

However, the main finding from this manuscript is not clear to me. Authors also need to heavily revise their results and discussion sections to provide logical and robust analysis and cross validation and comparison to previous studies.

LdM: We have revised the abstract, introduction, discussions and conclusions sections. Added a new figure to clear up our conclusions and removed the focus on UKESM as a standalone model. We hope that these changes are sufficient to address this comment.

Specific comments:

I have the following major comments:

1. You have some discussions about the implication of your study on the future carbon management and relevant studies in the discussion section, which is good. But the same information in the introduction part is missing. It would be nice to see more introduction about how carbon allocation is important for relevant research. For example, 1) how the calculated parameter can be important to the next stage of model intercomparison, benchmarking and 2) if this parameter can be helpful to indicate the strategy of carbon management for the next stage.

LdM: We have revised the introduction with a wider description of carbon allocation and recent research in this area.

2. Contents in Results and Discussion sections are stacked in a whole block and require more revisions to streamline your manuscript structure. Please summarize 2-3 subtitles and split your context and fill in these sub-sections.

LdM: We have revised both the result and discussion sections and added sub-headings.

3. Line 231: "In summary, fig. 3 shows that a model's sensitivity to CO2 concentration significantly affects the total carbon allocation between the

atmosphere, ocean and land at global warming levels, but is less impactful on the percentage allocation......the scenario has a much larger impact on the percentage carbon allocation at a given warming level than the ECS." But as I found in fig. 3, the carbon allocation fraction after normalization (left pane) are quite similar to each other under different scenarios at least for GWLs at 2 and 3 degree Celsius. To the opposite, certain models show very large discrepancy, e.g. EC-Earth3-CC compared to other models. Please explain how you get this conclusion?

LdM: We have added a new figure to the results section to help clarify this conclusion. We have also re-written this section to be more clear.

4. My understanding is that the authors plan to use UKESM as one of the examples to help understand how different processes in ESMs can influence the calculated carbon fraction. But I only find qualitative speculation instead of quantitative analysis. For example, in Line 340, "The UKESM1's higher AF at the year 2100 is likely due to the model limiting carbon uptake more than the other models. This could be Nitrogen limitation in the land surface or could be due to the model's higher ECS and thus warmer temperatures at 2100 than the multi-model mean." I expect to see more analysis, figures or tables to list evidence and prove these statements. Otherwise, there's no need to specifically highlight the result from one model and these conclusions from this manuscript are not robust.

LdM: We have removed the focus on the UKESM model now.

5. In the discussion section, the manuscript lacks enough cross-validation or comparison against other similar published studies. There are published studies discussing carbon storage, residence time and feedbacks in land and ocean components under different future scenarios. Just to name a few here:

Friend, A. D., Lucht, W., Rademacher, T. T., Keribin, R., Betts, R., Cadule, P., et al. (2014). Carbon residence time dominates uncertainty in terrestrial vegetation responses to future climate and atmospheric CO2. Proceedings of the National Academy of Sciences, 111(9), 3280–3285. https://doi.org/10.1073/pnas.1222477110

Jiang, L., Yan, Y., Hararuk, O., Mikle, N., Xia, J., Shi, Z., et al. (2015). Scale-Dependent Performance of CMIP5 Earth System Models in Simulating Terrestrial Vegetation Carbon. Journal of Climate, 28(13), 5217–5232. https://doi.org/10.1175/JCLI-D-14-00270.1

Katavouta, A., & Williams, R. G. (2021). Ocean carbon cycle feedback in CMIP6 models: contributions from different basins. Biogeosciences, 18(10), 3189–3218. https://doi.org/10.5194/bg-18-3189-2021

LdM: Thanks for these references, I particularly liked the Katavouta one, what a great paper! We have added used them to clarify our results and add some cross-sectional validation. We've also added a paragraph comparing our results to Friends 2013 work.

6. Your key findings are not properly highlighted. To improve this draft, authors need to conclude a more solid and informative key finding, for example, "choice of forecast scenario impacts the carbon allocation at the same global warming levels more than model's ECS/TCRE". At the same time, provide more qualitative analysis to prove your key findings.

LdM: We have added a new figure to the manuscript to clarify our conclusions with regards to the carbon allocation.

Technical corrections and minor comments:

Line 25: "and the land surface via primary production". Here "primary production" can be replaced by "terrestrial carbon fixation".

LdM: Done

Line 27: "known as carbon allocation". To avoid confusion with the "carbon allocation" widely used in terrestrial ecosystem modeling, I would suggest clarifying this point here, such as "known as carbon allocation in the Earth Systems (we simply use carbon allocation in the rest of the text)".

LdM: Done

Line 92: "land use emissions" contains how many different components? This LUE calculation may not contain the feedback from the settings of different ensembles.

LdM: Added more details on how LUE was calculated in Liddicot 2021

Line 125: "can gives" shall be "can give"

LdM: Done

Line 131: "Individual component models can be used by" can be clarified as "Same Individual component model can be used by".

LdM: Done

Line 137: Please clarify "All quoted values". What are these values?

LdM: Clarification added to text

Line 148: "In addition, several models may share contributing component models" seems to be a repetition of the content in Line 131. Shall think about how to merge them.

LdM: Fixed this.

Line 165: "These tools include quick ways to standardise, slice, re-grid, and apply statistical operators to datasets." Can you provide a table or figure to summarize and explain the mathematical algorithms of the operators you applied in this paper through using ESMValTool for data pre-processing? I think this is necessary information to understand your methodology.

LdM: We share the ESMValTool recipe and the source code, but the actual method is relatively simple. I've added the sentence to be explicit about our calculation:

In our case, we used the \textsc{annual_statistics} preprocessor to calculate the annual mean, the \textsc{mask_landsea} preprocessor to mask the land or sea areas, and the \textsc{area_statistics} preprocessor to calculate the area weighted global mean.

Line 193: "Figure 2 only shows the multi-model means, not single models." It will be helpful to add the spread of carbon allocation fraction using the results from single models in figure 2.

LdM: The spread of individual models is shown in figure 3.

Line 302: "Therefore, SSP3-7.0 can reaches" shall be "reach".

LdM: fixed

Line 302: "Therefore, SSP3-7.0 can reaches the GWLs earlier than other scenarios at the same CO2 concentration". I'm not quite sure about this conclusion. If we take a look at figure 4, SSP3-7.0 is later than SSP5-8.5 to reach all 3 GWLs.

LdM: I think we may have mis-communicated this result. This entire paragraph has been re-written for clarity.

Line 315: "Higher CO2 is causes" shall be "Higher CO2 causes"

LdM: fixed

Line 323: "the rate at which surface waters and dissolved CO2 is mixed downward will slow. This reduction is downward mixing reduces the overall absorption rate of CO2 into the ocean" This statement is confusing. Please rephrase.

LdM: This has been rephrased to:
This is likely because the surface layers of the ocean will be in equilibrium with the atmosphere, while deeper layers are not. However, much of the ocean is forecast to become increasingly stratified in the coming century, which would reduce downwards mixing of CO_2.

Figure 1: It's better to clarify that your prescribed DCO2 has accounted for the anthropogenic fossil fuel exploitation and the subsequent C emission from application.

LdM: We added this to the figure caption.

Figure 4: "the historical observations from Raupach et al. (2014) & Watson et al. (2020)," It will be better if you can clarify in which year(s) these observations represent.

LdM: The length of the lines represent the time over which the data was collected for these two observational datasets.

There are plenty of other **typos and confusing state**ments in this draft and the authors shall be responsible to double check the whole document before resubmission.

LdM: We can only apologise for these unfortunate problems. We have been more cautious with the revised manuscript.

[Reply](#)
Citation: https://doi.org/10.5194/egusphere-2022-1483-RC1

The manuscript "Choice of Forecast Scenario Impacts the Carbon Allocation at the Same Global Warming Levels" by de Mora et al provides an analysis of the carbon allocation across land, atmosphere, and ocean across a subset of CMIP6 models. While I was somewhat surprised at the degree of model agreement, The analysis and conclusions are fairly straightforward and of value to the broad audience of carbon cycle researchers.

LdM: Thanks for the summary and kind words!

I have detailed many specific examples of technical questions and points of clarification that I thought should be addressed before publication. It would also be helpful to add more information on caveats that might lead to an underestimation of the overall uncertainty. For example, while the CMIP6 historical simulations start in 1850, it is understood that changes to the carbon cycle began well beforehand which has implications for ongoing partitioning (Bronselaer et al., 2017 https://agupubs.onlinelibrary.wiley.com/doi/10.1002/2017GL074435 ; Le Quere et al. 2018 https://essd.copernicus.org/articles/10/2141/2018/).

Similarly, representation of dynamic vegetation, soil carbon and fire response is most likely undersampled in this ensemble (Arora et al., 2020 https://bg.copernicus.org/articles/17/4173/2020/bg-17-4173-2020.pdf ; Koch et al., 2021 https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2020EF001874).

LdM: This is a great point, thanks for pointing us towards these interesting results! We have added this discussion to the limitations section of the manuscript.

Specific comments:
Title – "forecast", which implies an initial value problem is inappropriate and should be "projection" which implies a boundary value problem.

LdM: Changed forecast to projection in the title

Abstract, line 13 – Albeit not having read the rest of the manuscript at this point, after hearing that the range of carbon allocation between scenarios towards 2C varies by only 3%, I find the conclusion, "However, the choice of scenario has a much larger impact on the percentage carbon allocation at a given warming level than the individual model's ECS". Difficult to understand/believe**...**are the authors only referring the ECS as an indicator of the differing model approach to 2C, or to the

overall ECS over CO2 doubling, which might vary from 2-5C or more? I believe the authors are only referring to the pace of attaining 2C which is far more specific than the current statement conveys. For example, approaching the equilibrium temperature at CO2 doubling or even 3C could have very different implications for carbon allocation than the scenario approach to 2C. (Note, upon finishing the manuscript, I felt like this issue was not resolved).

LdM: We have re-written the abstract, introduction and other parts of the manuscript in order to be explicitly clear in our results.

36 – "(" belongs before "Ukkola"

LdM: Fixed

37 – "that we have" is unnecessary

LdM: Fixed

37 – add comma after "fuels"

LdM: done

38 – "tool that we have to make forecasts of the future climate" should be "tools capable of projecting the future coupled carbon-climate system"

LdM: fixed

42 – "This means that the model outputs must use a common format and meet the minimum quality requirements." Adds nothing beyond the previous sentence

LdM: removed

44 – "...drift in the global volume mean ocean temperature of less than 0.1 degrees per year." Are you sure about this? A mean ocean temperature change of 0.1 C per year corresponds to a global radiative imbalance at the ocean surface of about 60 W per m2... about 100 times greater than the present day imbalance... are you sure that isn't supposed to be "0.1 degrees per century"?

LdM: Yes, indeed it should be per century.

54 – "forecast" should be "scenario"

LdM: fixed

74 – "breaks" should be "break"

LdM: fixed

75 – comma after "year"

LdM: fixed

85 – While the statement "and several members of the authorship team contributed to the development of the UKESM1 model" may be relevant to the execution of the manuscript and important to establish author contributions, it is not appropriate to provide in the manuscript content.

LdM: Removed this.

101 – The sentence "This is typically expressed as an annual total, so the total cumulative flux is calculated as the cumulative sum of the global annual total fluxes along the time dimension" is redundant in invoking "total" 3 times, and "annual" and cumulative" twice.

LdM: Simplified this sentence for clarity.

112 – The statement "Here, we take land-use emissions from the scenario, so they are not in balance with run-time model behaviour: this means that SLAND is only an approximation." Is unclear as to the need for an approximation. More information on how land use fluxes are treated is warranted. Why is a precise budget not possible? How much uncertainty is there in this "approximation"?

LdM: Sorry for the confusion, it is not that land use is not included in the runs, but that the impact of land-use on carbon stores is not able to be diagnosed. This is because changes in land carbon include natural and human-caused. Therefore we can't estimate total emissions from each model, only the fossil fuel component. This is standard – e.g. see figure in Jones et al 2013 (https://journals.ametsoc.org/view/journals/clim/26/13/jcli-d-12-00554.1.xml, fig A1), and this approach also taken in IPCC AR6 (as explained in caption for figure SPM.7).

132 – "may appear in several of the earth system models"… The word "may" here is inappropriate.

LdM: we have changed our language to be more precise here.

132 - In which of the models used in the present study is the same version of the NEMO circulation model used? This should be specific. How does the model diversity sampled here, in weighting the NEMO model. impact the overall diversity captured in the larger ensemble in CMIP5 and CMIP6, for example, including the GFDL results in the idealized experiments as was done in Arora et al., 2020 (https://bg.copernicus.org/articles/17/4173/2020/bg-17-4173-2020.pdf)

LdM: We do not want to include a table like Arora, as this information is widely available elsewhere. However, we have changed the text:

In addition, the same individual component models are used by several modelling centres. For instance, the NEMO ocean circulation model forms the marine circulation component model of six of the earth system models used here \citep{Heuze2021}.

139 – The word "weighted" is inappropriately vague here, since the "one-model one-vote" approach was used. The word should be "mean", or "median" as appropriate.

LdM: changed to "This table also shows the mean ECS of the contributing models for each scenario."

Table 1 – Why wasn't the GFDL-ESM4 model included? It has among the most sophisticated treatments of vegetation/land use and ocean biogeochemistry and is the highest performer in reproducing historical warming (Brunner et al., 2020; https://esd.copernicus.org/articles/11/995/2020/).

LdM: GFDL-ESM4 models was absent because our code excluded it. The reason is that it uses a non-standard grid label (gr1) in CMIP6 Amon and Lmon, so our tools didn't find it. We've added it into the ensemble in the revised draft, but it's presence doesn't change the overall conclusions. (GFDL-CM4 data remains excluded because it does not provide the nbp field required for the land component of the analysis.)

147 - What is the support for "These model pairs are likely only to have slight differences."? Similar to the assertion that multiply models use the same ocean, these characteristics should be justified. There are many previous intermodal comparisons on "uniqueness" and "independence" including the Brunner paper mentioned above that could be referenced on this.

LdM: This statement was unjustified and has been removed.

178, 183 – Should "SSP1-2.5" be "SSP1-2.6"?

LdM: Fixed

195 – I don't know what is being referred to as "This is known as survivor bias". What is "This" The lack of some models to meet a metric?

LdM: Change this to: "If we were to draw conclusions uniquely using models that reach this threshold, then those conclusions would be influenced by survivor bias."

226 – What do the authors mean by "strange behavior"?

LdM: Changed this to: "This model also exhibited outlier behaviour in CMIP5 (Dunning 2018)"

230 – The phrase "and if the atmospheric carbon concentration were allowed to rise sufficiently high" is not a necessary condition for warming based on TCRE – as long as emissions are positive, temperatures are expected to rise even if concentrations are declining. The statement should rather be "and if net CO2 emissions are positive"

LdM: Good spot! Changed it to: "if the model were allowed to run for long enough with positive net CO2 emissions."

264 – The assertion that ocean variability is larger than land variability in "The variability in the ocean is likely due to the wider range of circulation behavior in the scenarios." Seems very difficult to believe given the dominant role of land variability in historical interannual variability in carbon uptake as documented by the Global Carbon Project and IPCC… is this an indication of a lack of realism in the UKESM1 representation of interannual carbon variability on land, either through lack of ENSO variability or the land response? Perhaps I don't understand well enough how this is being calculated to average out land carbon internal variability, or if the models chosen do not have reasonable amount of historical variability. More explanation is warranted.

LdM: We have removed the focus on the UKESM section of the manuscript.

268 – comma after "land"

LdM: Done

292 – The end of the sentence is confusing to me as I do not understand how some models achieve "similar atmospheric CO2 concentrations" with "faster atmospheric CO2 growth" than others… "This means that even though two scenarios may reach the same warming level with similar atmospheric CO2 concentrations, the ocean and the land surface absorb less carbon in the scenario with faster atmospheric CO2 growth." Are the authors saying that the same GWL can be achieved at the same atmospheric CO2 concentration by both a high ECS model early in SSP585 as well as a low ECS model in SSP245? Some explanation and examples are necessary.

LdM: We have removed this explanation and added an ECS correlation testing exercise to help unpick some of these behaviours.

303 – Given that representation of methane and aerosol precursor emissions have been studied for decades and played a major role in both CMIP5 and CMIP6 (much of the focus of AR6 WGI Ch6), I do not think the word "infancy is accurate in the sentence "The impact of different methane and aerosol precursor emissions on the

climate response is still in its infancy in terms of realism in CMIP6." Rather I think it would be more accurate to stay that these topics remain highly uncertain.

LdM: Fixed.

314 – move "(" to before "Wang"

LdM: fixed

315 – remove "is"

LdM: fixed

324 – "reduction is" should be "reduction in"

LdM: fixed

325 – The logic here is reversed – "more saline surface layers" decreases stratification rather than increasing it.

LdM: We removed this.

329 – move "(" to before "Zeebe", also, remove "together"

LdM: done

331 – remove "which"

LdM: done

340 – remove second "could be"

LdM: done

[Reply](#)
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