

# Response to minor Revisions:

## Response to Malte Meinshausen:

Thanks and congrats for a very nice manuscript again. I have nothing more to add given that the authors addressed all the earlier comments.

Just some minor point I noticed that you want to address (can be during proof-reading). In your revised Figure 5, the middle row of panels has the wrong y-axis labels, as the normalisation period is 1850-1900, not 1970-1990. And the figure panels letters should be 'b' and 'c'.

*Many thanks for your help with the manuscript - the issues with the Figure have been corrected.*

## Response to Reviewer #2

*Many thanks again for the reviewer's time in improving our manuscript. The remaining minor issues are addressed as outlined below:*

L 54: what about the nitrogen cycle? I suggest adding it here too.  
*Done*

L60: maybe say km-scale climate models  
*Done*

L72-74: This sentence is difficult to read. Please rewrite.  
*Done as follows:*

*" In ScenarioMIP/CMIP6, scenarios were defined in terms of SSPs representing broad socioeconomic background states combined with global mean end-of-century radiative forcing targets (O'Neill et al., 2016; Riahi et al., 2017). "*

L 188ff: This section discusses RCPs, whereas the previous section focused mostly on SSPs. Then, on line 191, it switches back to SSPs. I found this confusing.

*Agreed. We've cut the offending line.*

L193: 'excludes carbon cycle uncertainties'. This is not entirely accurate. The changes in land and ocean carbon stocks still show a large spread in responses, even under concentration-driven simulations. However, these uncertainties do not feedback on atmospheric CO<sub>2</sub> and climate. I believe this needs to be clarified.

Revised as follows:

*“This has pragmatic advantages in terms of coordinating research across climate disciplines, but excludes uncertainties arising from feedbacks from the carbon cycle back onto atmospheric CO<sub>2</sub>. “*

L207: Clarify that this sentence is about CO<sub>2</sub> emissions and not any other emissions.  
*Specified 'carbon' emissions*

L210-212: Not sure if I follow the argument here. If they are idealized simulations, why should they apply to real-world carbon-cycle dynamics?

*Agreed - removing this sentence (which referred more to the ESM DECK proposal, less relevant to the scenario focus of the final paper)*

L214-216: Can you quantify this?

*Changed figure A1 to illustrate this better with post-2014 cumulative compatible emissions.*

L222-224: I am confused here. Why would the model output on the biome level be necessary to resolve the complete carbon budget? I guess output on the biome level (e.g. ocean or land biomes) would only be useful if you would like to calculate a regional ocean/land carbon budget. But for this, you would probably also need regional ocean interior carbon changes. Maybe I miss here something.

*Rewritten as follows: "However, CMIP6 models remain inconsistent in their outputting, unit conventions and definitions of component-level carbon fluxes, which complicate analysis. Such issues must be better addressed in emissions-driven simulations where reconstruction of the carbon budget is of first order importance to understanding the model response"*

L. 257: I guess the carbon-concentration and carbon-climate feedback parameter uncertainty was obtained from Arora et al. 2020, rather than Jones et al. or Friedlingstein 2006. Is this assumption correct?

*Agreed, corrected*

L278: Maybe add here references to Allen et al. 2009 and Matthews et al. 2009, which introduced TCRE.

*Done*

L761: Maybe add here reference to Terhaar et al. (2023, ERL; <https://iopscience.iop.org/article/10.1088/1748-9326/acaf91>)

*Done, thanks.*

L1015-1016: Please mention the technical challenges explicitly or delete this point (1).

*Deleted the reference to technical challenge*

L1015-1020: These are all very technical points to justify the esm-flat10 experiments, but they are easily solvable. I think point (4) is the most convincing argument and should be listed first.  
*Fair enough - changed as suggested.*

L1023-1027: Maybe I am missing something here. But using the 1%CO2 run to calculate TCRE and ZEC as well as AF was 'a consistent set of experiments'. So I am struggling to understand why the esm-flat10 esm-flat10-zec experiments would be more consistent.

*Deleting this paragraph*

Figure 7: I think the y-axis labels for the middle row panels are wrong. Should be vs 1850-1900

*Fixed*

Figure 7: I am a bit surprised about the behavior of the CNRM-ESM2-1 model, which seems to be quite an outlier and simulates an atmospheric CO2 concentration of only about 340 ppm in 2010. What are the reasons for this? Why does the model still simulate a temperature change over the historical period, which is about consistent with observations?

*Beyond scope to diagnose the reasons here - but we've double checked the numbers.*

L1098. Yes, 10 out of 13 models. However, the 3 models that are not included here show very strong biases. Would you argue, that such models should be excluded in any subsequent analysis?

*Yes - added the following:*

*"we suggest that the remaining outlier models may require greater attention to calibration of historical CO2 concentrations if emissions-driven simulations are the only runs provided and we suggest that the remaining outlier models may require greater attention to calibration of historical CO2 concentrations if emissions-driven simulations are the only runs provided."*

L1135: N2O is mentioned here, but not in the abstract.

*fixed*

L1693: Add Goodwin et al. 2018

(<https://agupubs.onlinelibrary.wiley.com/doi/full/10.1002/2017EF000732>) and Terhaar et al. (2022; <https://www.nature.com/articles/s41558-022-01537-9>)

*Done, thanks.*

L 1697: rather write 'consistent with any prescribed global warming target'

*Agreed, changed as suggested.*

L 1697-1700: It also allows for emission-driven simulations that can stabilize temperature at various global warming levels, enabling the assessment of impacts at different degrees of warming.

*Added - thanks.*