Answers to reviewers' comments regarding "Does the pace of carbon emissions matter in an atmospheric general circulation model?"

We thank the reviewers for their time to evaluate our manuscript and for sharing their perspective. Their comments have substantially improved the manuscript. Major changes include

- clarification of concentration-driven not emission-driven simulations and adaptation of the title and manuscript text accordingly

- strengthening motivation by studies that have become publicly available in the meantime, namely Katzenberger & Levermann (2025) and Hankel (2025)

- adding clarification regarding processing procedure and methodical choices

- expanding period of analysis from 10 to 20 years

- adding further quantitative insights

- providing more insights regarding robustness checks

- aggregation of results to global level

- adding more detailed discussion of model setup and parameter choices

We believe that these adaptations address the points raised and hope that the revised manuscript will now be considered suitable for publication in *ESD*. Please find below our detailed, point-by-point responses.

Reviewer #1

This study compares temperature and precipitation changes at 2°C warming in atmosphere model simulations with a slab ocean in which CO_2 increases at 1 %/yr and 4%/yr. The authors find no significant differences.
Whereas the title of the paper implied to me that the authors would perhaps compare simulations in an earth system model with different rates of CO_2 emissions but the same cumulative emissions, this is not the case, and the authors rely on simulations with a model with specified CO_2 concentration which does not simulate the response to emissions at all.

We fully agree with the reviewer and hope that we have made this clear by specifying the atmospheric general circulation model in the title. We also tried to make it very clear in the abstract and introduction, that our study is based on a concentration-driven setup of the GFDL-AM2 model without an interactive carbon cycle. This design allows us to prescribe CO_2 concentration pathways precisely and avoids the additional uncertainties associated with carbon-cycle feedbacks. Our study tries to complement other analysis in the field, as for example, the recent study by Hankel (2024), who investigated the role of CO_2 ramping rates on the Atlantic Meridional Overturning Circulation, herein omitting carbon cycle feedbacks while keeping oceanic responses. Our study was aiming to investigate if the atmosphere alone can bear memory - an unlikely but not impossible reaction to CO_2 forcing.

We understand, that our original title may have suggested an emissions-based framework and thus caused confusion. To address this, we have revised the title to *"Does the pace of carbon forcing matter* *in an atmospheric general circulation model?"* and clarified throughout the manuscript that our experiments are based on prescribed CO_2 concentrations rather than emissions. We thank the reviewer for pointing out the need to make this distinction more explicit.

Hankel, C.: The effect of CO2 ramping rate on the transient weakening of the Atlantic Meridional Overturning Circulation, Proceedings of the National Academy of Sciences, 122, e2411357 121, 2024. https://doi.org/10.1073/pnas.2411357121

Moreover the model has no dynamical ocean, and therefore does not realistically simulate the transient climate response to changing atmospheric forcing.

We fully agree with the reviewer that a slab ocean is a strong simplification compared to a fully coupled ocean and, by design, cannot provide a fully realistic representation of the transient climate response. However, this choice is deliberate: the simplified ocean allows us to focus on the role of the atmosphere while avoiding the complexity of full ocean dynamics. Still, the slab ocean does react to changing forcings, as there is a vertical coupling between the atmosphere and the ocean (or sea-ice and the ocean where applicable). Our study should be viewed as a targeted analysis of atmospheric responses under different forcing rates, rather than as a fully realistic Earth system simulation.

Given that the atmosphere-land system responds to changes in forcing generally on a timescale of months or years at most, there is no reason to expect that its response should be different in the different simulations the authors carry out.

We understand the argument to be that the temporal scale of the external forcing (CO_2 concentration increase) is slow, while the atmosphere–land system responds on relatively fast timescales. Thus the system will always have enough time to adapt. Indeed, the atmosphere-land coupling is reacting on very short time scales, with the atmosphere alone often responding within days to weeks.

Recent work (Katzenberger & Levermann, 2025), which has been published in the meantime, suggests that the atmosphere may possess a greater memory than previously assumed — as shown in the context of monsoon dynamics. Therefore, we think that the response-time scales may be slower than previously assumed, which would make it possible to observe differences between the simulations. Also dynamical feedbacks or threshold effects would open the possibility that differences between scenarios may emerge.

Katzenberger, A. and Levermann, A.: Monsoon Hysteresis reveals atmospheric memory, PNAS, Vol. 122 (19), e24180931, 2025. https://doi.org/10.1073/pnas.2418093122.

To add a historical perspective, the model set-up is similar to that used by Manabe and Wetherald (1975), albeit with a newer version of the GFDL model. Manabe and Wetherald (1975) explain that their model is only able to simulate the equilibrium climate response to changes in CO_2. Also, for example, the IPCC First and Second reports explain how an interactive ocean is necessary to simulate transient climate change.

Link to to paper mentioned by reviewer:
https://journals.ametsoc.org/view/journals/atsc/32/1/1520-0469_1975_032_0003_teodtc_2_0_co_2.xml

We thank the reviewer for this insightful historical context. Indeed, our model setup bears similarity to that of Manabe and Wetherald (1975), e.g. idealized topography and no heat transport via ocean currents. As we explain above, there is an ocean-(sea ice-)atmosphere coupling in our setup and the simplified slab ocean does have heat capacity - keeping the key characteristic as relevant for this research question.

We are aware that the current knowledge as e.g. stated in the first and second IPCC reports, assumes that a fully dynamic ocean is necessary to capture differences in the transient climate response because the timescales of the atmosphere are considered to be too fast. This is exactly what we want to test with our setup, in this study on a global level, motivated by findings in Katzenberger & Levermann 2025, as outlined above.

Our aim is to isolate the sensitivity of the atmosphere to different rates of CO_2 concentration increase under prescribed forcing. In this context, a simplified setup provides a controlled framework to examine how the pace of forcing can influence atmospheric states, without the added complexity and feedbacks of a dynamic ocean.

While the authors acknowledge that the model they use is not able to realistically simulate the transient response to changing levels of CO_2, they argue that the fact that the atmospheric response is not sensitive to the rate of increase in forcing requires confirmation (ln 13). The authors do not cite any references arguing that the atmosphere might exhibit such a sensitivity. Overall I disagree that this requires confirmation.

Many studies have examined how the climate responds to different magnitudes and types of forcing. However, relatively little attention has been given to its sensitivity to the *rate* of forcing change, despite this rate also being highly uncertain in future emissions scenarios. This gap in the literature has also been highlighted by Harger et al. (2025). Given its relevance for global climate, we are convinced that this is a sufficient motivation for this study.

On top, one central motivation for this study is a project in which we find that the atmosphere is able to store information on time-scales longer than previously thought as outlined above (Katzenberger et al. 2025). On a global level, such memory effects are not identified yet which is why we think this requires further analysis. As this project has been published in the meantime and is now citable, we added this part of our motivation in the introduction.