# Comment on bg-2023-146

Anonymous Referee #2

Chamberlain et al. present a set of idealized Earth system modelling experiments exploring the response of the Earth system to phasing out CO2 emissions. The experiments follow the "Zero Emission Commitment MIP" protocol but additional simulations (additional levels of emissions) are provided. The authors construct a simple slab (restoring) model where temperature is restored towards the equilibrium temperature of the model with two different time scales. They calibrate this model to their ESM and also compare results from three other ESMs with this slab model. The main finding that the authors emphasize is that the Southern Ocean continues to warm on centennial time scales after emissions ceased.

The response of the Earth system to phasing out emissions and the committed climate change due to prior emissions is a highly relevant research area, and there is generally a lack of ESM simulations that explore such scenarios. The model experiments presented here are therefore highly relevant and the model simulations are interesting and well designed. However, the manuscript reads in large parts like a technical summary of simulation results rather than focusing on new insights. Also, it remains unclear what new insights the two-slab model brings, particularly when it comes to the multi model comparison (see below for more details). Finally, the authors make no attempt to place their study in the context of previous literature, neither in the introduction nor in the discussion/conclusions section. Given these concerns, I would suggest substantial revisions of the manuscript before it might be suitable for publication in Biogeosciences. I cannot address all these points in depth in this review, but I will give some suggestions below.

Thank you for your review, fresh perspective and comments.
These have been useful to prepare to improve upon the submitted manuscript and clarify the presentation and discussion of the results.
Please see below for details (in blue) regarding how we can address these points.

Major points:

1) The abstract and the introduction contain too many technical details. For example, the abstract describes the ZECMIP simulation design (emission levels and the fact that the zero emission simulations are branched from the 1pctCO2 simulation), but is missing a summary of main results. The introduction is missing an account of previous literature (see below). In the results section, zonal mean section of salinity and oxygen are presented, but these results are never used or discussed (there are a few general sentences on biogeochemical changes in the conclusions section). It remains unclear what insights we gain from these figures, and how this relates to the main topic (the committed warming) of the paper. In general, the manuscript does not have a clear direction (at least it was not obvious to me). What do the authors want to address? Is it the fact that the ZEC750 simulation cools while the ZEC2000 simulation warms in their model? Is it a comparison with other models? Why do some models warm while others cool? What is the role of the Southern Ocean in this? It would help to formulate one or two clear research questions, and really set out to answer these.

This work presented is motivated by our recent participation in the ZECMIP and to understand why the model we used, ACCESS-ESM1.5 exhibited ongoing warming in some of the branches which was initially counter intuitive, but further analysis, with the help of the extra experiments presented, are able to demonstrate that it is the Southern Ocean driving a long-term global trend, which has not been featured much in the literature (but yes, as pointed out, the Southern Ocean is discussed by Gillet et al. 2011).

Salinity and other biogeochemical tracers were originally assessed when initially studying these experiments, testing if there were any indicative changes associated with the change in the sign of the ZEC values between the experiments, particular for salinity as there is potential to feedback on the thermohaline circulation.
Now these other tracers are secondary to the main results presented in the submitted manuscript, but they have been retained in the manuscript as further examples of changes to the climate state under zero-emission scenario.
Our motivations will be clarified by rewriting throughout the manuscript, including the abstract and conclusions.

2) Related to point 1, it also remains unclear what insights we gain from the application of the 2-slab model. I can see that the model is able to reproduces the global average surface temperature of the ZEC branches of the ACCESS model (no warming for ZEC750, increasingly more committed warming for higher emissions), but what does this mean physically? When it comes to the multi-model inter-comparison, the slab-model (as the authors present it) is not able to reproduce the cooling characteristics of the MIROC model, and the fit is not very good for the GFDL and UKESM models either. So again, what can we learn from this model then? As a side note, I believe that if there is something to learn from the slab-model, the model "ocean" time-scale would need to be fitted to the individual models (the authors only adjusted ECS). Would this improve the slab-model results? Would the slab model be able to reproduce the cooling in MIROC? As the results stand now, the slab-model seems to be able to reproduce the ACCESS ZEC-simulations more or less by chance, and it fails to reproduce relevant aspects of the zero emission commitment for the other models.

The purpose of the slab model is two-fold. Firstly, by replicating the global ACCESS-ESM results we show that the change in the global response can be understood by slow response of the ocean, the Southern Ocean in particular, to the climate forcing and no new processes need to be invoked. Secondly, by applying the slab model to $CO_2$ diagnosed from other models, we can pull apart the physical and biogeochemical responses of the climate systems under zero-emission trajectories.
No, the slab does not reproduce other models, it wasn't meant to as it was only tuned to the ACCESS-ESM and the slab emulates how the ACCESS-ESM would respond with the carbon-cycle response of the other models (the adjusting of the ECS is only to put the slab results on the same scale as the other models, as will be noted in a revised manuscript). That there are significant differences between the slab and the other models demonstrates it is differences in the physical models determining the overall zero-emission response, not the carbon cycle.
This may have been poorly communicated in the submitted manuscript, which can be modified to clarify these points, in the discussion here but also the abstract and conclusions.

3) This study is not the first to investigate the response of the Earth system to phasing out emissions, but neither the introduction nor the discussion/conclusions sections place the present manuscript in the context of previous literature. The first (to my knowledge) ZEC study with a full ESM was by Gillett et al. (2011), who also emphasize changes in the Southern Ocean. The study by Frölicher et al. (2014) finds a pronounced multi-centennial warming in one model, while a second model shows a cooling trend. Recently, Schwinger et al. (2022) have conducted a study, which also was based on the ZECMIP protocol, and they find a dominant role of AMOC decline and recovery for ZEC in their model. These studies come to my mind immediately, but there are probably more.

The work presented here is motivated by participation in ZECMIP which in itself included contributions from multiple models.
Thank you for indicating the other papers, these will be added along with another recent ZECMIP analysis paper to further improve the manuscript. The results in these are broadly consistent with our own results and our comparisons with other ZECMIP models and the inter-model variability found.
Schwinger et al 2022 in particular will be an interesting comparison which like experiments presented here, also explore a range of zero-emission branches (and overshoot scenarios) up to the emission of 2500 PgC

and test the climate response (and recovery) over multiple centuries, using the Norwegian ESM and a physical climate configuration that is sensitive to the AMOC (their supplementary Fig. S2 shows warming south of 40S after a couple of hundred years in all experiments, also inferring the Southern Ocean response, though this was not the subject of Schwinger et al. study).
See further comments regarding the simulation of AMOC responses below.

Other points:

Not sure if it is because I am not a native speaker, but I find the title of the manuscript not very intuitive to understand. I would encourage the authors to think about an alternative title.

The title is modified, "The Southern Ocean as the climate's freight train -- driving ongoing global warming under zero-emission scenarios with ACCESS-ESM1.5," so that the message is clearer, even if "freight train" is not.

The use of year 101 as start year for the simulations is confusing. I would suggest to set the nominal start year at year 1 in all tables and figures.

These had been the years from model time of the experiments, which was easier when writing.
But yes, these are easier for the reader by resetting the years as they are presented in the manuscript.

The abbreviations of the simulations is too similar to the abbreviation of ZEC values. For example, the authors use ZEC_200 for the temperature change after 200 years into the ZEC-simulations, and ZEC750 to denote the ZEC simulation with 750 PgC emissions. I would suggest to use a different abbreviation for the simulations.

After some consideration, these experiment terms are still used as is to be something short and clear to help readability of both the text and for use in figures. "*ZeroEmission750*" and "*Branch750*" are long, "*750PgC*" looks too much like a quantity rather than a label, "$A^{750}$" doesn't make much sense when B-style experiments are not presented, and *ZE750* is unappealing (and doesn't roll off the tongue as well?).
However, a paragraph is added in the Methods now to clarify how symbols and styles are used to describe these ZEC values and experiment labels.

Either in Section 3.2 or 3.3.1, it would be interesting to read something about the role of AMOC changes, which has been identified to play a dominant role in models with strong AMOC decline (Schwinger et al. 2022). Including AMOC strength in Fig 1 could be useful. In Fig 3b it looks like a signal of AMOC decline would be visible in the North Atlantic in the ZEC750 simulation?

Thanks for the comment, Schwinger et al. 2022 and NorESM2-LM results with a strong AMOC signal will be a good comparison.
Regional responses from participating ZECMIP models, including ACCESS-ESM1.5 and NorESM2-LM, are presented now in MacDougall et al. 2022, which highlights the significant regional variability between models and makes special mention of the different impact of AMOC from different models.
Some models show a strong cooling in the North Atlantic that may be associated with a slowdown in the AMOC, and Interestingly, NorESM2-LM is not one of them.
This discrepancy is possibly because the results in MacDougall et al. 2022, are from a lower branch, *ZEC1000,* and earlier in the experiments; $ZEC_{50}$ is before many of the signals in Schwinger et al. 2022 (or our results) become apparent.
Here in the results with ACCESS-ESM1.5, however, the North Atlantic is not standing out particularly in the results presented and we keep our focus on the Southern Ocean which has a strong signal.
The manuscript will be modified to acknowledge and discuss other literature and some of the differences between models, including AMOC.

Section 3.3.1: What is shown in the figures is the global meridional streamfunction not "the overturning". Overturning strength can be visualized through and calculated from the streamfunction. Please correct throughout the manuscript.
The terminology used in the manuscript is clarified as indicated.

Section 4.1: At least the main idea of the slab-model should be described in the main text, such that the reader can understand what the model is intended to do. Might be even easier to move equation A1 into the main text.
(See related comments to "Major Point 2" above)

line 22: I suggest to delete "of the global climate"
The phrase has been left in, it may be useful to the reader who may not be as familiar with the zero-emission commitment term.

line 24: "...the potential budget of carbon emissions permittable without exceeding any agreed thresholds of ``safe'' warming" is very complicated. "remaining carbon budget" has become an established term for this and could be used here.
The suggested terminology is adopted.

line 26: "carbon emission budget" -> "carbon budget"
The suggested terminology is adopted.

line 31: "This conclusion..." this is not a conclusion, it is an assumption.
The suggested terminology is adopted.

line 37: The acronym 1pctCO2 has not been introduced
This sentence with the *1pctCO2*, which is now in a ZECMIP subsection of the methods, is combined with the sentence that followed and has the description of the term.

line 35-41: Much of this paragraph could and should be moved to the methods section.
Moved.

line 70-75: The main issue with trends and biases for the kind of simulations presented here, is the switch from concentration to emission driven configuration. This could be made clear instead of the generic statement in the last sentence of this paragraph.

Thanks, this is a good point, and it is a good argument for the discussions I've heard regarding ZECMIP-style experiments being planned for CMIP7 that branch from a parent experiment that is an emission-based version of the *1pctCO2* experiment.
In the case of ACCESS-ESM1.5, checking output available from the ESGF for the *esm-piControl* that had an interactive carbon cycle, the drift in physical and biogeochemical states is still negligible for the first 300 years. Atmospheric $CO_2$ increases ~1ppm/100y and the magnitude of any trends in the average surface air temperature or sea surface temperature are less than 0.01 degC/100y.
This is now mentioned in the manuscript in this model description subsection of Methods.

line 77-86: The fact that the simulations presented here were run on different computer hardware is a technical detail that is not relevant for the results. This can be a footnote in Table 1 explaining why numerical values are slightly different from previously published results.
These technical comments have been moved from the text to the table caption as suggested.

lines 88-98: This general description of the ZECMIP experiments/protocol should come earlier.
The method section has been rearranged in response to this and other comments.

lines 100-106: Maybe a personal preference, but I don't think it is necessary to provide a summary of subsections at the start of a new section. Good descriptive titles of subsections are enough.
I find the brief high level summaries potentially helpful in communicating the work being presented, and the reader can skim over them easily.

line 109: "...gradient increase evenly" -> "... rate of surface air temperature change" or similar.
This sentence has been broken up and rewritten for clarity.

line 110: The numerical values presented here seem to contradict the values in Table 1? Please clarify.
I can see why this could be the case.
The manuscript is modified to clarify the values in the text are 'overall' values of the rates of change, and that the time series is 'approximately' linear.
Indeed, a quick look over Table shows temperature changes are not linear as $ZEC_{200}$ are not double $ZEC_{100}$, even allowing for uncertainty, as there is a stronger change in the first decades relative to later decades.

line 114-115: This sentence doesn't make sense, please consider rephrasing it.
Rephrased, "Most of the energy entering the climate from the imbalance at the top of the atmosphere (Fig. 1d) is taken up by the ocean of each experiment."

line 130: Unclear what does "the extension of this experiment refer to"? Please clarify.
This had been referring to the new experiments that had been integrated for longer, 300 years rather than 100 years when ACCESS-ESM originally produced results for ZECMIP.
The sentence has been rewritten for clarity…"For instance, while there is an overall global cooling in ZEC750, after 200 years from branching there is some warming in the same latitude band, 40–65◦ S, that stands out more clearly in ZEC1000 (Fig. 2 b and c)."

line 227: TCR is defined at year 70 (not 50) of the 1pctCO2 simulation (at doubling of atmospheric CO2).
Yes, thanks for catching this.

line 238-239: "... more than adequate" I don't think this statement is adequate. The simple model can reproduce certain aspects of the result.
The statement is rephrased to clarify it is only referring to "average temperatures" that are being discussed.

line 256-264: It remains unclear to me what the authors intend to say with this paragraph on tipping points. Please restructure/reword/clarify.
"Tipping points" are often used in describing significant potential impacts of climate change and rightly so. The idea of crossing some threshold of the climate system that triggers a new mechanism or process (e.g. ice sheet collapse, loss of rainforest) that drives the climate to a new state, whether at the local and global scale, is an effective way to get attention.
But after thinking about tipping points for some time, I find the concept is somewhat vague and poorly defined, and potentially missing other important processes.
For a time, our results from the original ZECMIP experiments hinted there may have been a tipping point in our simulations. However, results from our intermediate ZEC branches and then being able to replicate the global results with slab model indicate there is no evidence for any such tipping point driving the global response here.
And yet, here we have a process that indeed affects the climate for centuries, as simulated by ACCESS-ESM1.5, and the process is present in other models as well, albeit with varying impacts globally.
So, the intention here is to suggest that while this Southern Ocean response may not be a tipping point, it is worth being discussed with them…
Editting of the paragraph has been made to expand upon this and make the point clearer.
"While the Southern Ocean and its climate response may not fit an example of a tipping point, its potential to drive ongoing warming with potentially global impacts suggests it should be considered is discussions of regions and processes with potential to drive ongoing changes to the climate system."

line 283-286: There is no "contrast" here this just the different timescale (as the authors note). Please reword these sentences.
These sentences are pointing out how these models that have similar centennial responses but are different on shorter timescales, and sentences have been rephrased for clarity.

References:
Frölicher, T., Winton, M. & Sarmiento, J. Continued global warming after CO2 emissions stoppage. Nature Clim Change 4, 40-44 (2014). https://doi.org/10.1038/nclimate2060
Gillett, N., Arora, V., Zickfeld, K. et al. Ongoing climate change following a complete cessation of carbon dioxide emissions. Nature Geosci 4, 83,Äì87 (2011). https://doi.org/10.1038/ngeo1047
Schwinger, J., Asaadi, A., Goris, N. et al. Possibility for strong northern hemisphere high-latitude cooling under negative emissions. Nat Commun 13, 1095 (2022). https://doi.org/10.1038/s41467-022-28573-5
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