Comment on bg-2023-146

Andrew MacDougall

Review of: The Southern Ocean as the freight train of the global climate under zero-emission scenarios with ACCESS-ESM1.5

Overall assessment:

The paper documents experiments with ACCESS-ESM1.5 that expand upon the standard ZECMIP A-class experiments to better show the transition from negative ZEC at low cumulative emissions to positive ZEC at high cumulative emissions. Additionally that paper uses analysis of ACCESS-ESM1.5 and a slab ocean model to show that in ACCESS-ESM1.5 ZEC is dominated by Southern Ocean processes. While the paper is interesting, generally scientifically sound, and well written - some revisions are needed before publication.

Thank you for your review 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.

General Comments:

(1) The authors appear to be using two different algorithms to compute ZEC values. In the caption for Table 2 the authors indicate that they are using the standard algorithm outlined in MacDougall et al. 2020. That is "Values are the differences between 20-year averages centred at the year of the ZEC branch [...], relative to the 20-year average from the respective 1pctCO2 centred at the branch point." While the caption for Figure 2 says the regional ZEC is being computed as "Differences are with respect to the average of the first 10 years of each experiment, and smoothed with a 5-year filter."

I strongly recommend that the authors compute regional ZEC using the same algorithm as global ZEC, using maps of the 20-years average temperature from the 1pctCO2 experiment centred on the year emissions cease as the cessation temperature reference value. Using a different algorithm to compute regional ZEC risks making the results of this study incomparable with other similar studies.

These figures have been regenerated with 20-year averages, referenced to *1pctCO2*, as suggested, with no major changes in the results shown.
There are some minor changes to the regional/zonal temperature changes in the *ZEC750* panels of Fig. 2 and 3 in the Arctic where there was decadal variability seen in the original version of the figures.

Additionally, a description of the algorithm used to compute ZEC should be included in the methods section.

A description of the ZEC calculation is now included in a ZECMIP subsection of the Methods.

(2) Section 4.2 "Multi model Comparison" is the least convincing part of the study. From Figure 11 it is clear the using the slab model tuned to ACCESS-ESM1.5 does not capture MIROC or UKESM temperature trajectories well. While the match to GFLD is better both ESMs use the same ocean model (MOM) so a better match is to be expected. Additionally recent analysis of regional ZEC (MacDougall et al. 2022) showed that for a least some ESMs AMOC is dominating the ZEC response not the Southern Ocean, with some models having regional ZEC dominated by AMOC collapse (CESM2). Thus I suggest the existing section 4.2 be deleted and a more qualitative comparison be made to the regional ZEC effects shown in MacDougall et al. 2022.

The purpose of the slab model and this sub section will be clarified in the manuscript.
By forcing the slab model with CO_2 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 is showing how the ACCESS-ESM would respond with the carbon-cycle response of the other models. That there is a difference between models demonstrates it is differences in the physical models largely determining the 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.
Discussion is also added of other papers, in particular of the papers MacDougall et al. 2022 and also Schwinger et al. 2022 which has been brought to our attention and also discusses potential impacts of a collapse in the AMOC.

Aside, if the long-term ZEC response of the ESM is primarily determined by the physical component of the model, perhaps there is value in having a prescribed *ZEC1000* CO_2 time series, for example, to enable non-ESM climate models to run pseudo-ZEC experiments, much like a ZEC-scenario experiment.
This may assist in evaluating the relative roles of the AMOC and Southern Ocean and potentially other processes.

(3) Throughout the manuscript 4 digit model codes for time are used instead of years. Model years in all figures, tables and in text, should be given in standard Arabic numerals (no leading zero) with appropriate units (years). For figure captions please include a x-axis label of 'Model Years', to be clear that Gregorian calendar years are not being used.

Suggestions have been adopted.
Actually, the model does run with a Gregorian calendar (an unnecessary detail here), but yes, "Model Years" is a better way to label these axes.

(4) Please use consistent notation for the 1% CO2 experiment.

This experiment was referred 3 times in the submitted abstract; this will become once or twice after the abstract is revised to focus more on the conclusions rather than technical details.
We use a longer experiment name for readability in the abstract.
In the main text, the experiment should be referred to with the (somewhat) shorter format, *1pctCO2*.

Specific Comments:

Line 1: "Climate Projection Experiment" is an odd start. "Climate model simulations" would be more consistent with past terminology.

We use terms "simulation" and "experiment" almost synonymously, with some preference for "experiment." To me, "experiment" is more defined and "simulation" is somewhat generic, and for results presented here "experiment" feels better.

Line 5: Delete 'Multi'

Thanks, deleted

Line 16 to 20: Odd framing since ECS becomes a moving target as CO2 concentration will not stabilize for a very long time. Processes not included in ESMs such as the CO2 weathering feedback will cause a slow drawdown to close to pre-industrial over the next few 100,000 years (e.g. Archer 2005). Statement makes more sense after reading the paper but not best framing for an abstract.
At the time, ECS and TCR seemed to be a succinct way to argue that positive ZEC values are entirely reasonable.
However, agreed, this doesn't work as well when it requires substantial explanation and can be dropped as the abstract is reworked to better highlight the main conclusions of the work..

Line 24: Delete "safe".
Deleted

Line 26: Change "greenhouse gas" to 'forcing agent' (aerosols are not gases), and add 'unaccounted for' before 'climate feedbacks'
Manuscript modified as suggested.

Line 93: Seems to indicate the ACCESS-ESM1.5 does not have dynamic vegetation. Should say this in model description. Many of the models that participated in ZECMIP did have dynamic vegetation so could be re-written to say this is the implementation in ACCESS-ESM1.5 but other models have prognostically changing vegetation maps.

To simplify what is meant to be a brief description here of the set up of the *1pctCO2* experiment, "vegetation" has will be dropped and this statement will just refer to land use being set to preindustrial conditions.
Yes, there is no dynamic vegetation in ACCESS-ESM1.5, but ACCESS-ESM1.5 has prognostic leaf-area-index, (see the model description paper, Ziehn et al. 2020) so the influence of a vegetation type is able to increase or decrease, though these details are not necessary here.

Line 96: I never liked the description of this as 'unrealistic'. An asteroid strike, global thermonuclear war, or supervolcano eruption, would probably do the trick. Add 'baring global cataclysm' before 'a global instantaneous', and change 'unrealistic' too 'unlikely' to fix.

The aerosols kicked up by these catastrophes will also complicate climate forcing. Perhaps a zombie apocalypse would be a plausible way to trigger a cleaner zero-emission scenarios…

That said, the suggested text is adopted to clarify the applicability of the experiments presented.
The intention here is to highlight that while the experiments have an idealised nature, the results are consistent with other climate stabilisation experiments with varying combinations of climate forcing components, where the temperature of the branching is a key factor that determines the ongoing climate trajectory.
The language is softened and modified to point out that actually, the straight-forward nature of these zero-emission experiments is powerful in that it is easy to replicate with different models and compare with future generations of CMIP.

Line 109: Change 'about linear' to 'approximately linear'. Change 'gradient' to 'trend'. A gradient usually indicates a change in space, not time.
Modified to "The time series of surface air temperatures from each ZEC branch are approximately linear. The overall rates of change are…"

Line 110: Tipping points can be subtle transitions.
This phrase is removed from here.
There was a time when we first looked Antarctic sea ice extent of 100 years from the original *ZEC750*, *ZEC1000* and *ZEC2000* experiments, that there did seem to be step change that might have been a tipping point behind the global response.

But alas, the story got more complicated after running the infill experiments, running experiments for longer and the fact the global results can be reproduced with this slab indicate the no tipping point is required. However, none of this is necessary at this point in the manuscript and the term will be removed from the manuscript.

Line 117: change 'of average' to 'in average'
Modified as suggested.

Figure 1d: The preindustrial TOA energy balance seems to be negative. Is this plotted correctly? If so, is the model drifting?
You observe correctly, there is some energy leakage somewhere in the coupled model and has been discussed in the model description paper (Ziehn et al. 2020).
However, the physical climate configuration of the ESM has been stable for some time and there's a 1000+ model years of spinup so this imbalance doesn't drive significant drift in any of the climate components.
Text will be added to the manuscript to state there is no drift associated with the offset in Fig. 1d.

Line 141 to 149: Could compare results to MacDougall et al. 2022 here.
A paragraph making a comparison to the temperature change maps of MacDougall et al. 2022 will be added to the text here. This will include a mention that some of the models show AMOC responses and also point out that the Southern Ocean response we are presenting here is not apparent because they manifest after more than 100 years after branching and after the ZEC_{50} maps of MacDougall et al. 2022.

Line 256: May want to add a few sentences to discuss the potential impact of Ice Sheet loss on the Southern Ocean Feedbacks. I don't think ACCESS-ESM1.5 has a dynamic Ice Sheet model, so discussing what impact Ice Sheet loss may have is important.

No ACCESS-ESM doesn't have an active an ice sheet.
Sentences will be added to state this and the potential impact to further reduce overturning, as demonstrated in some recent ocean model results (Li et al 2023, https://doi.org/10.1038/s41586-023-05762-w) who tested the impact of freshwater fluxes from Antarctica in climate change scenarios which decreased overturning and warmed the subsurface.
These statements may be placed in the subsection presenting overturning streamfunctions.

Line 319: Spell out ESGF the first time you use it.
Done, though the first use will now occur earlier in the manuscript.

Line 341 to 342: Need to note that this is 2 of 4, not 2 of 9.
Text is modified as suggested: "…was one of only two full-ESMs that demonstrated significant positive ZEC values, or ongoing warming, out of the four ESMs that tested the zero-emission scenario after emitting 2000 Pg"

Line 343: Change 'about neutral' to 'approximately zero'
Changed.

References:

Archer D. Fate of fossil fuel CO2 in geologic time. Journal of geophysical research: Oceans. 2005 Sep;110(C9).

MacDougall AH, Frölicher TL, Jones CD, Rogelj J, Matthews HD, Zickfeld K, Arora VK, Barrett NJ, Brovkin V, Burger FA, Eby M. Is there warming in the pipeline? A multi-model analysis of the Zero Emissions Commitment from CO 2. Biogeosciences. 2020 Jun 15;17(11):2987-3016.