

Interactive comment on “Irreversible ocean thermal expansion under negative CO₂ emissions” by Dana Ehlert and Kirsten Zickfeld

Anonymous Referee #3

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The authors investigated the reversibility of the ocean thermal expansion (the thermohaline sea level rise) to idealized CO₂ forcing using a climate model of intermediate complexity. In their experiments, they first ramped up the CO₂ concentration by 1% yearly to quadrupling and then decreased it by 1% yearly back to the preindustrial value, after which the simulations were carried on for another 1000 plus years with fixed CO₂ concentration. They found that the thermohaline sea level rise is irreversible on human time scales and it continues to rise 80 years after the reversal of CO₂ forcing. They further reported that the rates of sea level rise/decline in their model generally increase with higher vertical diffusivity, with exceptions of overshoot of ocean circulations.

The manuscript deals with an important issue of climate change and could contribute to understandings of the reversibility of the climate system. The experiments and analyses are systematic and comprehensive. However, I feel that some issues should be addressed before it can be accepted. Please see my detailed comments below.

Response: We want to thank the reviewer for a helpful and positive review.

Major comments:

1. It would be useful if the authors can list the equilibrium climate sensitivity of the model they used, and briefly compare it with the current generation of climate models (the IPCC models). It may also help the readers if the authors can also compare the transient climate sensitivity across the sensitivity experiments with different mixing in their study. This can give the readers an idea of the effect of ocean mixing on the climate sensitivity.

Response: We included the equilibrium climate sensitivity of the UVic model. We also included a list of the transient climate sensitivity of all model versions and mention the IPCC AR5 and EMIC ranges.

2. The authors used a coupled model to carry out long simulations (longer than 1000 years). It is unclear whether the model is run with flux adjustment or the model is subject to large trend in the deep ocean. If there is any trend in the deep ocean, how does this influence the current results?

Response: The model does not require flux adjustments nor is there drift in the deep ocean as 6000 year spin-up simulations were performed for each model versions to eliminate drift in the ocean. Thank you for pointing to this gap in the model description. We included a mentioning of it in the simulation section.

3. It surprises me that the mean sea level change lags the CO₂ forcing by the same amount of time (80 years) in all the sensitivity experiments. It seems that the lag time is not dependent on the details of ocean mixing. Then, the question is what is setting this lag time? Is it possible that the lag time is model dependent? It looks to me that equation on Page 5 Line 20 is the place to start discussing this problem more carefully.

Response: Thank you for bringing this point to our attention. It seems the text might be a somewhat unclear. The lag is not always exactly 80 years for all model versions but ranges between 67 to 86 years. However, the lag is always on multi-decadal time scales. We adjusted the manuscript accordingly.

4. When reviewing previous studies on the reversibility of the climate system, I think Wu et al. (2010) is worth mentioning, in which paper the author described the hysteresis behavior of the hydrological cycle in response to a ramping down of CO₂ forcing.

Response: Thank you for pointing to this interesting study, we mentioned it in our manuscript.

5. Mignot et al. (2007) is one of the first papers discussing the subsurface warming and the overshoot of the AMOC, which process is closely related to the overshoot of the AMOC reported in the present manuscript.

Response: Thank you for pointing to this interesting article. We do not believe that the mechanism for the overshoot discussed in the Mignot paper and in our manuscript are the same. In the Mignot paper the reason for the overshoot is a subsurface warming induced by a complete shutdown of deep ventilation in the North Atlantic. In our case the AMOC does not shutdown at any point in the simulation and thus deep ventilation in the North Atlantic never shuts down completely either.

Minor comments:

1. Page 2, Line 22: Boutes et al. SHOULD BE CHANGED TO , Boutes et al.
Response: We assume this points to the usage of parenthesis. This is not a mistake. We refer to the initial description of the 2-layer model by Gregory 2000 and Geoffroy et.al. 2013 but then describe how Bouttes et.al. 2013 used it under declining radiative forcing. We rearranged the sentence to improve clarity.
2. Page 2, Line 26: are that is has SHOULD BE CHANGED TO are that it has
Response: This has been edited in the text. Thank you for pointing out this typo.