A warm stagnant ocean leads to deep-ocean deoxygenation without widespread anoxia - by Anthonisz et al.

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July 2026

The review

Summary

The authors investigate the response of ocean circulation and biogeochemistry to warm, highly stratified conditions using three MCO simulations with the ACCESS-ESM1.5 Earth System Model, forced with 1×, 2×, and 3× preindustrial atmospheric CO_2. In the two higher CO_2 experiments, the initially cold deep ocean leads to strong thermal stratification and a substantial weakening of deep and bottom-water ventilation. The authors apply Anderson Acceleration to efficiently spin up the marine biogeochemical component and examine the resulting $[O_2]$ distribution under these poorly ventilated conditions. Despite the strong reduction in deep-ocean $[O_2]$, the simulations do not produce widespread benthic anoxia, which the authors attribute primarily to the temperature-dependent shoaling of detritus remineralisation and the resulting reduction in organic matter export to the deep ocean. They therefore argue that enhanced stratification and stagnant circulation alone may be insufficient to generate widespread oceanic anoxia, and that additional processes such as enhanced nutrient supply may be required.

General comment

I would first like to thank the authors for this manuscript, which I found very interesting and engaging. The study presents a number of interesting results and provides valuable insights into the links between ocean circulation, stratification, biogeochemistry, and oxygenation under warm climate conditions. Overall, I support the publication of this manuscript after minor revision. My main general reservations concern the somewhat descriptive nature of the manuscript, particularly in parts of the physical and biogeochemical analysis, and, to a lesser extent, the tone of the manuscript, which I occasionally found somewhat defeatist. I think this is unfortunate because the study actually presents several interesting and potentially important results. I have divided my comments below into major and minor comments for clarity. However, I would like to emphasise that I provide these comments primarily as suggestions for improving and strengthening the manuscript, rather than as strict requirements for publication. None of the comments below should be considered individually mandatory for the manuscript to be accepted. Nevertheless, I hope that the authors will consider and discuss as many of them as possible.

Major comments

1) Section 3 is, in my opinion, overly descriptive. The use of MLD provides useful information, but it does not by itself provide a sufficient explanation of the mechanisms driving the major

changes in ocean circulation. In particular, why does the MOC change so dramatically between the experiments? Is this primarily a direct consequence of temperature effects on convection, or does warming act indirectly through changes in sea ice, precipitation, surface freshwater fluxes, wind stress, or some combination of these processes?

If the objective of this section is to describe the physical state of the different experiments (which I understand to be its main purpose) then I think the authors should investigate these mechanisms in greater detail. This would allow the discussion to move beyond a description of the model state and provide a physically based explanation for the collapse of the SMOC and the associated reorganisation of the overturning circulation. I would therefore suggest shortening Section 3.1 and retaining primarily the results that are directly relevant to the analysis presented in Section 3.2, while replacing some of the descriptive material with a more mechanistic analysis of the circulation changes.

At the same time, I suggest making the titles of Sections 3.2 and 3.3 more explicit. At present, "Ocean dynamics in MC1" and "Ocean dynamics in MC2 and MC3" do not fully convey the distinct scientific questions addressed in these sections. From my understanding, Section 3.2 primarily examines the consequences of the modified paleogeography and the opening of seaways for ocean circulation, whereas Section 3.3 focuses more specifically on the additional effects of increasing atmospheric (pCO_2). The section titles could reflect this distinction more directly, for example by using titles such as "Influence of seaway opening on ocean circulation" and "Response of ocean circulation to pCO_2".

2) Figure 3: I would ask the authors to provide further justification for the duration of the Anderson Acceleration (AA) step. From panels (a), (b), and (f), I can accept that the duration of the acceleration is sufficient for MC1, as the dashed curves appear to have stabilised by the end of the AA period, suggesting that the biogeochemical state is approaching equilibrium. However, I find this much less convincing for MC2 and MC3, where the system does not appear to have reached a similarly stable state by the end of the AA step. Furthermore, why does the transition between the end of AA and the subsequent 1100 years of DI of MC2 and MC3 suddenly produce a stable equilibrium in [O_2] while it was absolutely not suggested by the dashed curves (this was very surprising to me)?

I think that a valid interpretation is the possibility that a longer AA period could have allowed the biogeochemical system to evolve further towards substantially lower deep-ocean [O_2], potentially leading to more extensive O_2 depletion (as suggested by the trend observed during the AA spin-up stage). This point seems particularly important given that the authors themselves are surprised that widespread deep-ocean anoxia does not develop despite the strong weakening of the SMOC in MC2 and MC3. I therefore encourage the authors to justify why the chosen AA duration is sufficient for MC2 and MC3, and, if possible, to provide evidence that extending the acceleration period would not significantly alter the resulting [O_2] distribution.

3) Lines 175–176: I find the conclusion that the circulation differences are "likely due to the increased temperatures" somewhat too strong given the evidence presented. How can we be certain that temperature itself is the primary cause, rather than changes in paleogeography that modify ocean circulation patterns?

If Figures A2 and A3 represent the system at the end of the simulation, then they cannot by themselves establish the direction of causality between temperature and circulation. Further explanation is therefore required to demonstrate what actually causes the temperature increase and how this warming subsequently affects the circulation.

I think the authors could already clarify this issue around line 153, where they explicitly state that MC1 has globally higher SAT and SST than PI, but do not explain the mechanisms responsible for this warming.

4) Lines 360–362 and the framing of the main result. I find the phrasing in this part of the manuscript somewhat overly negative, particularly given that the authors demonstrate a very interesting and potentially important result. The effect of warming and circulation changes on deep-ocean [O_2] is substantial. From Figures 11b and 12b, [O_2] in the deep ocean appears to decrease by approximately 150 μmol.kg^{-1}, with the reduction seemingly linked primarily to the substantial weakening and/or shutdown of deep-ocean ventilation.

I understand that the authors may have expected to obtain a more strongly oxygen-depleted state, such as widespread hypoxia or anoxia, and that the absence of such conditions is central to their conclusions. Nevertheless, the magnitude of the deep-ocean deoxygenation observed here is itself remarkable. The results provide strong evidence that changes in ocean circulation and ventilation can be major drivers of ocean deoxygenation, even if circulation changes alone do not necessarily produce widespread anoxia in this specific experimental setup.

I therefore think the authors could frame the results more positively and place greater emphasis on this finding. The manuscript does a good job proposing additional mechanisms that may be required to explain the full development of widespread anoxia, but the authors should also emphasise that their simulations demonstrate a very substantial deoxygenation response. This is key, as the absence of widespread anoxia could all come down to the choice of initial conditions, as already mentioned by Reviewer 1, or a too short AA duration (see major comment 2).

5) Figures: I suggest adding concise titles or descriptive headings to the figure captions to summarise the main information shown. This would make the figures easier to interpret independently of the main text.

For the figures presenting anomalies, I suggest adding contours corresponding to selected anomaly values in addition to the filled colour contours. In particular, explicitly showing the zero-anomaly contour would make it much easier to identify regions where the sign of the response changes, and additional contours at selected values, for example -150, -100, -50, 50, 100, and 150, could also help the reader quantitatively interpret the magnitude and spatial extent of the anomalies.

Minor comments

- Line 26: I would be careful with the use of the term "persistent." To complement Reviewer 1's comment, these hypotheses appear to be primarily associated with the onset of organic carbon burial and anoxia rather than specifically with the duration of anoxic events. I would therefore suggest replacing "persistent" with a term that more directly reflects the link with the onset or development of anoxia, as the current wording may unnecessarily suggest that the authors are addressing the duration of OAEs.

- Line 60: Why not use "ESM" here, since the term was already introduced earlier in the manuscript?

- Lines 79–81: I suggest rephrasing this sentence as follows: "This result partially aligns with Lee et al. (2025), which found that warmer initial conditions can weaken the AMOC, whereas colder initial conditions can enhance the AMOC, using a different model." The authors should then specify which model was used in Lee et al. (2025). It is currently unclear to me whether this model is one of the 14 models included in the MioMIP1 analysis mentioned on lines 67–68, or whether it is a separate modelling study. This should be clarified.

- Lines 89–91: I would be careful with the phrase "geological time scales." I would suggest explicitly stating the timescale being referred to. This would make the comparison with the much more rapid timescale of anthropogenic climate change more precise.

- Lines 123–133: I found the experimental setup and, in particular, the application of Anderson Acceleration, interesting and innovative compared with approaches I am familiar with. To better demonstrate the practical benefits of this approach, I suggest reporting the computational cost of the simulations, particularly the relative computational time required for conventional direct integration versus the accelerated spin-up procedure.

- Figure 6: I find the notation for the units of Sverdrup somewhat unusual. I suggest using a more direct notation, for example: $1\ \mathrm{Sv} \equiv 10^6\ \mathrm{m}^3\ \mathrm{s}^{-1}$.

- Figure 7: I suggest specifying explicitly that the figure represents meridional boundaries to avoid confusion. I would also recommend providing the full explanation for why the MOC cannot be calculated across the open boundaries in this context. Since this figure is central to the interpretation of the circulation changes, the explanation should be sufficiently complete for the reader to understand the limitation without having to reconstruct the argument from the main text.

- Figures 7 and 8: I suggest improving the visual delimitation of the continents in these figures. At present, the continents have a colour similar to the neutral colour of the colour bar, which can make the figures somewhat misleading and difficult to interpret. I recommend using a distinct colour for the continents and/or adding clear continental boundaries, for example with thin black outlines.

- Table 3: I guess that these thresholds for oxygen-rich, hypoxic and anoxic are common, but I would nevertheless suggest adding a reference to support these choices.

-Section 4.2: I suggest moving the detailed description of the temperature-dependent remineralisation scheme to the Methods section. As already noted by Reviewer 1, this information is introduced too late in the manuscript, despite being fundamental to the interpretation of the main result.

- Personal interest: Do the authors have an explanation why it is the SMOC that collapses and not the AMOC in their setup? Is it mainly due to internal temperature or salinity rearrangement, or is the combination of the two required in order for the SMOC to collapse? Could you comment on this?

I would like to thank the authors again for their work and for presenting what I consider to be an interesting and valuable study. I hope that my comments will help strengthen the manuscript and further highlight the interesting results obtained in this work. Overall, I think the authors have done a very good job!