

Response to Review by Justin Gérard

We thank Dr Gérard for reading our paper in full and in detail. We greatly appreciate the constructive feedback. Below is our response to this review and relevant changes we have made to the paper. Line numbers are given with reference to the new version of the manuscript (without tracked changes). Some edits shown below were made in response to the review by Dr Pohl. We use the following colours to highlight the reviewer comments and our responses:

Blue text denotes reviewer comments;

Black text denotes our response to the reviewer comments;

Olive text denotes existing text from the manuscript;

Red text denotes new additions to the manuscript.

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 CO₂. In the two higher CO₂ 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 [O₂] distribution under these poorly ventilated conditions. Despite the strong reduction in deep-ocean [O₂], 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.

We thank Dr Gérard for this suggestion. We agree that a more thorough analysis of the drivers of circulation changes would be interesting. Unfortunately, such analysis would require a range of additional sensitivity simulations, which are not within the scope of this paper, nor would they be feasible within the time span of an honours thesis. Dr Hutchinson is currently working on this analysis and the results will be presented in a follow-up paper.

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 ($p\text{CO}_2$). 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 $p\text{CO}_2$ ”.

We agree with Dr Gérard and change the titles to the following:

3.3 Ocean dynamics in MC1 under mid-Miocene Paleogeography

3.2 Response of ocean dynamics in MC2 and MC3 under mid-Miocene Paleogeography to increased $p\text{CO}_2$

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 [O₂] 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 [O₂], potentially leading to more extensive O₂ 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 [O₂] distribution.

We acknowledge that the trend was not fully equilibrated during the AA phase. However, the apparent slope of the trend during the AA appears ‘steep’ because AA causes an approximate 8x speed-up in the BGC evolution. When we return to DI, the trend is immediately flatter because there is no longer such acceleration - effectively like ‘dilating’ the time axis. Immediately after DI recommences both MC2 and MC3 have a negative trend. Subsequently, deoxygenation slows for MC3, and in MC2 actually begins to reverse. This reversal is indicative of the physical circulation slowly increasing.

Furthermore, Anderson Acceleration is most effective on deep ocean changes and is made less effective by variability. This is most obvious when comparing its effects on shallower waters with greater interannual variability (e.g NPIW, Figure 3d in the manuscript) to the ocean below 3000 m (Figure 3b) where it dramatically accelerates deoxygenation for MC2 and MC3.

If AA had been run for longer, it is possible that the end point would be different for the deep ocean. This suggests that the more robust conclusion is not the absolute value of oxygen concentration in the deep ocean, but rather the large reduction in oxygen content of the ocean as a result of rapid warming. Furthermore, the deep ocean environment of almost no ventilation, but also no oxygen consumption is stable for over 1000 years in the deep ocean of MC2 and MC3. The stability of a given oxygen concentration with essentially no circulation is maybe more surprising than a lack of anoxia. We hope that this is an interesting example of both the utility of AA, but also how AA decouples physical and biogeochemical evolution.

A reframing of our results and a shift in focus is warranted.

To that end we make the following changes:

Lines 13-14:

Surprisingly, ~~we do not find large scale deep anoxia or hypoxia~~ we find that a stagnant ocean leads to large reductions in oxygen but not necessarily lead to anoxia or hypoxia, due to a shoaling of the remineralisation depth.

Lines 158:

~~Given the trending physical simulation, AA could have been engaged later to integrate biogeochemistry under better equilibrated physics, but with limitations in computational resources and time, engaging AA sooner saves time and allows for a longer post-AA integration to test the stability of the biogeochemical state produced. Uncertainty in how the physical simulation will evolve makes it difficult to optimise the spin up strategy *a priori*.~~

We add a new section to the discussion to make clear AA's impact beginning on line 391:

4.4 Impact of spinup strategy

AA accelerates consistent changes very effectively, such as the slow deoxygenation below 3000 m in the elevated $p\text{CO}_2$ simulations (Figure 3b), but prevents changes due to physical evolution (e.g NPIW in MC3, Figure 3d). AA also accelerates slow changes that occur in a evolving physical system. For example, the reduction in oxygen below 3000 m in the MC2 simulation continues after DI recommences but eventually reverses as ventilation in the MC2 simulation strengthens (Figure 3b,f). Our use of AA, which dramatically accelerates slow but consistent oxygen loss in the deep ocean for MC2 and MC3, creates a biogeochemical simulation with very little drift in a particular physical state.

Due to its proximity to this new section we now rename the subsequent section:

4.5 ~~Limits of Anderson acceleration~~ Physical ocean model acceleration

We also further emphasise that our “equilibrium” oxygen concentration consists of very slow changes, rather than opposed restoring forces and emphasise that a more complex biogeochemical model may not produce the same state on lines 363-366:

~~Our MC2 and MC3 simulations maintain a delicate balance between near zero detritus transport and near zero oxygen transport for over 1000 years in the deep ocean. Any transport of detritus, even if it is weak and intermittent, would alter the oxygen profile of the deep ocean, highlighting the need for more sophisticated biogeochemical models to generate robust insights into oceans that are very different to those that biogeochemical models are optimised for.~~

We also following changes to lines 379-382 to emphasise that our results suggest that circulation shutdown can coincide with a lack of anoxia, rather than circulation shutdown not resulting in anoxia:

~~Given that our simulations are essentially as unventilated as possible in the deep ocean, to force achieve ocean anoxia with our background conditions under the preservation hypothesis alone would either require similar stratification at lower temperatures, as this would allow~~

more detritus to reach the deep ocean before being remineralised, or a different temperature-related behaviour of remineralising bacteria.

We also make the following to changes lines 388-389:

~~The preservation of oxygen concentrations in our simulations failure to produce anoxia for over 1000 years, despite near-zero benthic oxygen transport supply,~~ highlights the need for a better understanding and a better representation of detritus sinking and remineralisation in BGC models.

We make further changes to lines 324-326: Overall, our results suggest that while stagnant circulation patterns do create global reductions in dissolved oxygen, they ~~are not sufficient to generate~~ **do not guarantee** a large-scale OAE within the framework of our model and boundary conditions, because of the temperature-driven shoaling of remineralisation depth.

We also somewhat re-frame the interpretation of our results in the conclusion.

We make the following changes to lines 432-435:

We find that the combination of a hot ocean with high remineralisation rates and high levels of stratification produces ~~a delicate equilibrium between near zero detritus flux at depth and near zero oxygen supply to the deep ocean. Our stratified simulations maintained showed low benthic oxygen concentrations without~~ **but no benthic anoxia for over a millennia in the absence of oxygen supply from the surface.**

We also make changes on lines 438-441:

Although not tested explicitly by our work, our simulations support the findings of other simulations, which have suggested that increased nutrient content in the ocean is essential to triggering an OAE (Gérard et al., 2025; Cui et al., 2025; Monteiro et al., 2012), as we ~~are unable to produce~~ **find anoxia is not guaranteed** without enhanced nutrient runoff.

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.

We thank Dr Gérard for pointing out this issue with our presentation of the physics of the MC1 simulation.

We now alter lines 185-186 as follows:

There are key differences in strength and location of deep water formation, **likely due to the altered paleogeography and** increased temperatures in the MC1 simulation compared with PI (Figures A2, A3).

We also acknowledge that we have not provided a sufficient explanation of warming in the MC1 simulation and add explanation on lines 160-163:

The MC1 simulation has globally increased near-surface air temperatures (SATs) and sea surface temperatures (SSTs) compared to PI (Figures A2, A3). This is partially a result changes to imposed ice sheets in the MC1 simulation, which reduce albedo but could also be a result of changes to circulation and therefore heat transport. The multi-model comparison of Burls et al. (2021) found that surface temperatures increased by 2°C as a result of implementing MCO boundary conditions.

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 [O₂] is substantial. From Figures 11b and 12b, [O₂] in the deep ocean appears to decrease by approximately 150 $\mu\text{mol.kg}^{-1}$, 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).

We agree with Dr Gérard's assessment here. We have already re framed out results to the focus to highlight the persistence of an ocean state with very sluggish circulation and a very small vertical supply of detritus to the deep ocean, rather than whether the final state is anoxic or not (See changes made in response to the previous comment). We now also refer to the loss of oxygen at the beginning of the discussion and mention the deoxygenation in the abstract.

We make changes to the conclusion on lines 437-438 to further reframe our findings:

~~Even so, our results suggest that when deep ocean ventilation shuts down, anoxia onset might not occur, or would at the very least be delayed~~ We find that circulation shutdown results in dramatic deoxygenation of the ocean, but that this deoxygenation may take many millennia to occur.

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.

We considered this suggestion, but decided to retain the original style in which the captions describe the details of the data.

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.

We thank Dr Gérard for this suggestion. We have added the zero contour in addition to selected value contours (denoted by tick marks on the colour bar) to our anomaly plots to help readers in the interpretation of these plots.

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.

Changes made to this section in response to Reviewer 1 address this. Line 28 now reads:

There are two hypotheses for ~~persistent anoxia at depth~~ the deposition of shale which signals an OAE.

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

We thank Dr Gérard for pointing out this error and make the following change on line 63:

Simulations of the MCO ~~with using ESMs earth-system models~~ struggle to recreate the temperature patterns evidenced by proxies, with significant cold-biases in the high-latitudes (Burls et al., 2021), (Figure A1).

- 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.

We now make clear that that study used the integrated water isotope enabled Community Earth System Model version 1.2 which was not one of the 7 models used to run the 14 simulations of the MioMIP1 on Lines 83-86:

This result partially aligns with Lee et al. (2025), a different MCO model study which found warmer initial conditions can weaken the AMOC while colder initial conditions can enhance the AMOC using the integrated water isotope enabled Community Earth System Model version 1.2 (Lee et al., 2025). Note that Lee et al. (2025) was not part of the original MioMIP1 ensemble.

- 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.

We make the following changes on lines 95-96:

In the past circulation would have changed on geologic time scales over many millennia, whereas in the future changes may happen within a century much faster (Drijfhout et al., 2025)

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.

This has now been addressed in response to review 1, See changes on line 467-481:

Compared with the climate model used in this project, the AA algorithm has very little computational demand. (Khaliwala, 2023) For example, to run a year of an ACCESS-ESM1.5 simulation with BGC in the configuration used here takes ~ 1 hour 15 minutes and uses 384 CPUs. The AA script, which in our implementation must be run once for every 10 model years, uses only 1 CPU and runs for less than 5 minutes, making it a insignificant saving in resources compared to the background cost of running the full model.

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

We change the captions of Figures 6 and 7 to define the unit.

Sverdrups ($1 \text{ Sv} \equiv 10^6 \text{ m}^3 \text{ s}^{-1}$)

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.

We thank Dr Gérard for the input of this figure. We now clarify that the integral cannot be calculated over an open boundary.

The latitudinal extent and depth of open-boundaries ocean gateways for the Atlantic in the MCO (CAS and Tethys) are blocked out in grey as the stream function cannot be calculated over an open boundary. and the latitude range is restricted to exclude the open Southern Ocean and ACCESS-ESM1.5's tripolar grid region (north of 65° N).

- 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.

A modified version of these figures will be present in the final manuscript

- 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.

When checking our references and double checking our code to see where we got this definition from we realised that, like our reference material, we had calculated anoxia as $\leq 1 \text{ mmol/m}^3$ rather than $\leq 10 \text{ mmol/m}^3$ as quoted in the manuscript. We thank Dr Gérard for helping us identify this error and we now use quotation marks around “oxygen rich” to denote that this is not a common definition and reference the study from which we take our definitions of Anoxia and Hypoxia in the caption of Table 3:

Fraction of whole ocean volume that is “oxygen-rich” ($\geq 240 \text{ mmol/m}^3$), hypoxic ($\leq 62 \text{ mmol/m}^3$) and anoxic ($\leq 10 \leq 1 \text{ mmol/m}^3$) for the PI control simulation, MC1, MC2 and MC3. Our definitions of hypoxia and anoxia were taken from Duboc et al. (2024).

-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.

We thank Dr Gérard for this suggestion and note that as suggested by Dr Pohl in his review we have now moved the description of remineralisation from section 4.2 (beginning at line 322).

- 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?

This and other circulation questions will hopefully be answered in future work lead by Dr David Hutchinson, as outlined in our response to other comments and is beyond the scope of this paper. However for the reviewers personal interest we include the following interpretation:

The Southern Ocean deep water formation (as we know it today) is colder and fresher than AMOC deep water formation, the later being warmer but saltier. In our elevated CO₂ simulations there is both warming and freshening, both of which can reduce the density of shallow waters, leading to a deep water formation “off state”.

The Southern Ocean gets fresher and warmer, without the salinity component to counteract the warming and freshening. David's hypothesis is that freshening (from enhanced precipitation vs evaporation) around Antarctica might be the key in the warmest case. But both are important, warming and freshening. This question will be addressed in much further detail in future work.

We again thank Dr Gérard for his thorough review of our work. His feedback and constructive criticism has helped us to greatly improve the manuscript and its presentation.