

## Response to Review by Alexandre Pohl

We thank Dr Pohl for the positive feedback and exceptionally constructive criticism as it has greatly improved the manuscript. Below is our response to this review and relevant changes we have made to the paper. Line numbers are given relative to the new version of the manuscript (without tracked changes). 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.

Some sections quoted in this response include changes made as a result of Dr Gérard's review, all changes are denoted in red.

### Paper summary:

The authors produce 3 biogeochemistry-enabled simulations under Miocene boundary conditions at 1, 2 and 3 times the preindustrial CO<sub>2</sub> concentration, using the complex model ACCESS-ESM1.5. They analyze ocean dynamics and ocean oxygenation in these 3 simulations, always using a preindustrial simulation for reference. Because Miocene simulations were initialized with a cold ocean derived from another model, they all undergo thermal stratification. They authors force an accelerated version of the biogeochemical component using these dynamical fields to simulate the redox landscape resulting from a sluggish ocean. Their main message is that, although warming induces an increase in primary productivity, anoxia does not expand in the deep ocean because the dependence of remineralization on seawater temperature induces a shift in the bulk of remineralization to the shallower ocean, hence inducing a relatively smaller oxygen consumption in the deep ocean compared to the colder state.

### General comment:

I did enjoy reading this manuscript. It is very descriptive, which makes it a bit tricky to read the first time, but it provides a good, richly-illustrated and rigorous analysis of the model simulations. More importantly, I think that it addresses an interesting question: what are the consequences of a reduction in ocean circulation on the redox landscape? The experimental setup is a bit unusual, but very clever in the sense that it permits to address this question at moderate computational cost using a robust model, by deliberately inducing thermal stratification. I never read any study using such approach and think the latter could be re-used in the future. I strongly support publication, after minor revisions. My comments below are mostly intended to help improve the presentation of the results. There is notably a series of typos, which is always a bit annoying to see when spending time evaluating a manuscript. I

do not have any strong criticism regarding the scientific content.

We thank the reviewer for the overall positive assessment and appreciation of the manuscript.

### Main Comments:

1. Importance of background conditions. I think the finding of a lack of expansion of anoxia is critically dependent on the background (pre-warming) oxygenation. Indeed, the authors do simulate a strong decrease in benthic dissolved oxygen concentrations in their warmer simulations (Fig. 10). This deoxygenation would induce a major spread of anoxia, if the benthic ocean was less oxygenated before warming.

Therefore, I suggest to state up front, in the title, that these findings apply to the Miocene only; I expect these results would not stand under a deeper-time period with reduced atmospheric  $pO_2$  for instance, or under alternative configurations of the continents that would be less prone to benthic ventilation such as the early Paleozoic at least in some models (Pohl et al., 2022). Thus, the findings are interesting, but time-specific.

We appreciate that the title is too general for the experiment that we are running, are therefore change the title to

“A warm stagnant **Miocene** ocean leads to deep-ocean deoxygenation without widespread anoxia”

We agree that our findings do not necessarily transfer to pre-Cenozoic continental configurations usually involved in studies of OAEs, however, our paleogeography is much more similar to present than those studies. We now make reference to this on line 428:

The highly stratified ocean states in our simulations provided an opportunity to test ocean anoxia onset under the preservation hypothesis with a relatively high resolution ESM and paleogeography more similar to present than most studies of ocean anoxia.

Further, We more explicitly state that the background atmospheric oxygen conditions are identical to pre-industrial in the methods section:

Line 113 changed to:

All simulations were run with a fixed, prescribed atmospheric  $pCO_2$  and  $pO_2$ .

I think the authors should discuss the nutrient inventory used in these simulations; nutrients are a key modulating parameter of background oxygenation but the values imposed are never explicitly discussed. Justifying the boundary conditions used, in terms of atmospheric  $pO_2$  and nutrient inventories, and ideally ground-truthing the resulting ocean oxygenation conditions, is important considering the key role played by background  $[O_2]$  in the results.

Clarification has been given above to the  $pO_2$  boundary conditions. As for nutrients we clarify on lines 132-133:

After 1000 years, initial BGC conditions for WOMBAT, including nutrient inventory, were taken from the PI control run

We choose PI conditions to initialise from as we do not have estimates of MCO ocean nutrient inventory available and we intended to compare to the PI simulation.

I also suggest to emphasise the importance of background oxygenation in appropriate locations of the manuscript, notably in the abstract (lines 13–14) and throughout the main text (notably lines 309–311).

We make changes to the abstract on lines 13-14:

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

And make 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. Had we initialised our simulation with lower oxygen concentrations then it is possible that widespread anoxia would have occurred.

Since the authors discuss their results with regards to OAEs, it may be good to discuss a few additional aspects: OAEs usually are protracted events (Reershemius and Planavsky, 2021)

We now make explicit reference to this on lines 371-374:

Given that the deep ocean continues to gain heat in both simulations, and that MC2 begins to show increasing circulation towards the end of its integration, the stratified states represented in our simulations are likely unstable on time scales of multiple millennia, as has been found for a stratified state using a lower resolution model (Hülse et al., 2021). This contrasts proxy evidence for the duration of OAEs, which usually suggests durations in excess of 100kyr (Reershemius and Planavsky, 2021).

The rates of carbon injection during these events was lower than today (I suggest to provide key numbers; I think rates were  $\frac{1}{2}$  during the Permian-Triassic and  $\frac{1}{10}$  during the PETM) (Cui et al., 2021; Turner, 2018). Numbers could also help support statements like the one on lines 89–91.

Thermal stratification could have sustained over long periods as long as warming continues, notably in the case of a volcanic trigger (Gernon et al., 2024), but then additional feedbacks may enter into play such as atmospheric pO<sub>2</sub> increase due to organic carbon burial, and nutrient loading due to increased continental weathering (e.g., Newby et al., 2025; Zhang et al., 2026).

We now mention that the rate of carbon change (much like the change in climate) would have changed much slower in the past, adding to line 95-96:

whereas ~~in the future~~ changes may happen much faster within a century (Drijfhout et al., 2025). Similarly, past changes to climate occurred as a result of much slower carbon

injection (e.g.  $\sim 4$  Gt of carbon per year during the end Permian Mass extinction; Cui et al., 2021) than current human emissions ( $\sim 38$  Gt of Carbon in 2025; IEA, 2026)

We have also added a clarifying point on line 375:

possibly because a stratified state could be sustained by long term warming unlike the rapid change in our experiments (Gernon et al., 2024).

Line 26: another aspect that is poorly constrained is the depth at which deep-ocean anoxia occurred, since ancient deep-sea sediments were destroyed during subduction. See also lines 402–403. These results of a shoaling of remineralization suggest that deep-ocean anoxia may well have been primarily affecting shallow water environments – although, again, background deep-ocean [O<sub>2</sub>] conditions can alter this statement.

We agree that this is an interesting point, and mention its implications in the conclusion as Dr Pohl points out.

Better phrasing of the overall approach. As for now, the manuscript reads like the authors unfortunately ended up simulating a stratified ocean due to inappropriate initial conditions, and therefore decided to take the opportunity to exploit this sluggish ocean circulation to conduct this study. Although I suspect it faithfully summarizes the situation, and I do completely understand that science works this way based on opportunities and backup plans, I think the manuscript would benefit from a rephrasing. I think it would be more attracting to the reader if the authors simply stated up front that they generated an artificially sluggish ocean circulation by imposing thermal stratification to then quantify the consequences on ocean oxygenation. This is really just a matter of rephrasing key sentences, notably in the abstract (lines 7–9) but also on lines 96–98 and 315–319, and I think it would permit better exploiting the novelty/strength of this work. I think such approach is very interesting, but its novelty is not clearly put forward for now.

We thank Dr Pohl for this suggestion. He is correct in his assessment of the history of Mr Anthonisz's honours project. We thankfully take this suggestion on board and have made following adjustments:

We replace lines 7-9:

When  $p\text{CO}_2$  was raised to  $2\times$  or  $3\times$  PI  $p\text{CO}_2$ , circulation became dramatically altered due to cold initial conditions, resulting in a temperature-driven stratification which was stable for over 2000 years of integration.

With:

In the elevated  $p\text{CO}_2$  simulations, the rapid increase in temperature induced temperature-driven stratification which was stable for over 2000 years, allowing us to explore ocean oxygenation in a stratified physical state.

We also re-word lines 103-105:

Our stratified simulations ~~do however~~ provide an opportunity to test the preservation hypothesis of OAE formation in a highly stratified simulation using a relatively high resolution ESM

We also note that lines 331-338 have been significantly modified in response to Dr Gérard's review and now read:

The unventilated ocean state, caused by a cold start initialisation in our spin-up, is also most likely not representative of an MCO equilibrium climate, for which we had initially intended to simulate biogeochemistry. Our unusual physical simulation highlights the need for warm initial conditions to reach a steady state quickly, as has been found in other paleoclimate modelling efforts (Lunt et al., 2017). This possibly transient state is caused by rapid warming of the upper ocean, whereas a fully-equilibrated Miocene deep ocean may have better ventilation. Work examining the sustainability of a very stratified ocean using end-Permian boundary conditions found that the overturning circulation shutdown after rapid warming may only persist for thousands of years before giving way to more vigorous circulation than before the warming event (Hülse et al., 2021). Our simulations might, however, beAs such, our simulations are likely representative of a transient behaviour of the ocean when forced with rapidly increasing greenhouse gas concentrations.

The temperature-dependence of remineralization is key and yet only properly introduced in the discussion (section 4.2). Before that, the reader has to extract this info from lines 230–231. I think it is important to describe the temperature-dependence of remineralization before showing its impacts, hence much sooner in the manuscript.

We thank Dr Pohl for this constructive feedback, We now move the description of remineralisation from section 4.2 to section 2, alongside the model description:

In WOMBAT, remineralisation rate  $R$  is dependent on detritus concentrations and an exponential of temperature and is represented as:

$$R = \mu_d D$$

where:

$$\mu_d = 0.048 b^{cT} \text{ day}^{-1} \quad \text{above 180m}$$

and

$$\mu_d = 0.024 b^{cT} \text{ day}^{-1} \quad \text{below 180m}$$

where  $D$  is detritus concentration in  $\text{mmol/m}^3$  and,  $c = 1.0^\circ\text{C}^{-1}$ ,  $b = 1.066$  and  $T$  is Temperature in  $^\circ\text{C}$  (Oke et al., 2013; Ziehn et al., 2020) . This parametrisation was optimised for the Southern Ocean under modern conditions (Law et al., 2017) . All of our MCO experiments, particularly the elevated  $\text{CO}_2$  simulations, are warmer then the conditions for which WOMBAT was optimised

Need to better compare with existing work. I think some key references are missing. Notably:

- Key references discussing the importance of temperature-dependent remineralization to capture past ocean biogeochemistry (Crichton et al., 2021; John et al., 2014) – since temperature-dependent remineralization is key in this study.

We thank Dr Pohl for directing us to these useful sources which highlight the importance of temperature dependence on remineralisation.

We add the following after the description of remineralisation in WOMBAT on line 110:

Accurately representing the temperature dependence of remineralisation and other biogeochemical processes is critical for effectively representing ocean biogeochemistry for both near present and paleo oceans (Crichton et al., 2021; John et al., 2014).

Previous work looking at the behaviour of the biological carbon pump (and notably the POC rain flux to the deep ocean) during warming events (Wu et al., 2025).

Thank you for pointing us to this interesting work, which has relevance to our experiment. We now refer to this work in our discussion on line 348-349:

... reducing export production and transport of nutrients out of the photic zone (Kvale et al., 2015). This aligns with recent work simulating the carbon pump during Phanerozoic hyperthermals, which found that rapid microbial respiration in a warmer ocean may produce a positive feedback loop by reducing carbon sequestration (Wu et al., 2025). Although our experiments are run with a prescribed  $p\text{CO}_2$  they demonstrate rapid remineralisation in warmer waters.

Previous work simulating a stagnant ocean using a short model integration starting from cold initiation conditions (Penn et al., 2018). The authors might be interested in discussing the findings of another paper that identified the transient nature of the model response analyzed in the previous reference (Hülse et al., 2021; see their supplementary section “Ocean circulation response to instantaneous warming”).

We thank Dr Pohl for directing us to these references. We now refer to Penn et al. (2018) when introducing the reader to the importance of ocean oxygenation in the introduction on line 21-22:

Declining oxygen concentrations can change marine community structure, function, animal behaviour and cause mass mortality events (Breitburg et al., 2018). Changes in oxygen concentration, as a result of rapid warming, were a contributing factor to the end-Permian (~252 million years ago Ma) mass extinction (Penn et al., 2018).

We further refer to Penn et al. (2018) in our conclusion on lines 428-432:

The highly stratified ocean states in our simulations provided an opportunity to test ocean anoxia onset under the preservation hypothesis with a relatively high resolution ESM and paleogeography more similar to present than most studies of ocean anoxia. Work using a lower resolution ESM and different boundary conditions has identified that oxygen loss due to warming and circulation shutdown was a contributing factor to the largest ever mass extinction (end-Permian, ~252 Ma). Therefore, examining a stratified simulation is critical for understanding our potential futures (Penn et al., 2018).

We also thank Dr Pohl for directing us to Hülse et al. (2021). We agree Supplementary Figure 8 in that paper is very interesting and demonstrates how a lower resolution model can investigate long term changes to circulation in a way that is quite difficult with our more expensive CMIP6 ESM. We now make reference to this work in our discussion from line 336:

Work examining the sustainability of a very stratified ocean using end-Permian boundary conditions found that the overturning circulation shutdown after rapid warming may only persist for thousands of years before giving way to more vigorous circulation than before the warming event (Hülse et al., 2021). Our simulations might, however, be As such, our simulations are likely representative of a transient behaviour of the ocean when forced with rapidly increasing greenhouse gas concentrations.

And on lines 372-373:

...the stratified states represented in our simulations are likely unstable on time scales of multiple millennia, as has been found for a stratified state using a lower resolution model (Hülse et al., 2021).

5. Starting from appropriate initial conditions is key. I think that this study could be a good opportunity to stress the importance of running simulations conducted with complex models (hence of short duration) from warm conditions, in order to enhance ocean mixing and permit to rapidly reach steady state. This aspect is, I think, largely considered in the paleoclimate modelling community, but never properly mentioned.

We thank Dr Pohl for this point. Experimental design, especially when computing costs are high, is difficult. We now make reference to this in the methods at line 127-129:

Beginning simulations from the endpoint of other similar simulations is a common strategy in paleoclimate modelling to reduce spin up cost (e.g Burls et al., 2021), however Thisour approach resulted in a “cold start” initialisation, where the surface of the ocean heats faster than the deep ocean, due to the higher climate sensitivity of ACCESS-ESM1.5.

We now also make reference to this in the discussion on lines 331-334:

The unventilated ocean state, caused by a cold start initialisation in our spin-up, is also most likely not representative of an MCO equilibrium climate, for which we had initially intended to simulate biogeochemistry. Our unusual physical simulation highlights the need for warm initial conditions to reach a steady state quickly, as has been found in other paleoclimate modelling efforts (Lunt et al., 2017).

## Other comments:

Line 26: I think the two hypotheses traditionally apply to organic carbon burial, not anoxia

We edit lines 28-34 to more properly reflect this:

There are two hypotheses for persistent anoxia at depth the deposition of shale which signals an OAE. One is the preservation hypothesis, where increased stratification prevents circulation of oxygenated waters and allows the formation of large areas of anoxic bottom waters, particularly where circulation is restricted by topographic barriers (Sooraj et al., 2024). The second is the productivity hypothesis, where increased biological activity and export production elevates oxygen demand and results in the formation of oxygen deficient zones (ODZs) and allows for the accumulation of organic matter that has not been consumed underneath them.

Line 60: “Earth” (capital is missing).

Now corrected

Line 61: there and throughout, I suggest to keep the word “significant” for statistical tests and otherwise replace it with “substantial” or equivalent.

We make the following changes:

Line 63:

~~substantial~~significant cold-biases

Line 178:

indicating ~~strong~~significant throughflow

Line 179:

between 400 and 1400 m also suggest ~~significant~~transport of younger

Line 186:

reduced strength and ~~visible~~ significant changes

Line 222:

due to ~~substantial~~ significant lateral flow

Line 226:

there ~~are~~ is a significant number of ~~multiple~~ grid boxes cells

Line 255:

is due to ~~an~~ significant increase in nutrients

Line 362:

would result in ~~very significantly~~ different transport

Line 385:

and this could be ~~an important~~ significant factor

Line 61: comma to be deleted after the reference.

Now corrected

Lines 65–66: but see also limitations on the reconstruction of SST gradients on the data side (Pohl et al., 2025; Sarr et al., 2024).

This is interesting and worth noting in our introduction although we cannot make a deep dive into temperature reconstructions for this BGC paper. We make the following changes to lines 66-68:

reconstructed during the MCO using stable oxygen isotopes: underestimating warming at the poles and overestimating warming in the tropics (Burls et al., 2021; Hossain et al., 2023). We note, however, that this proxy method has limitations, and may overestimate SST by as much as  $\sim 18^{\circ}\text{C}$  for a continental configuration close to ours (Pohl et al., 2025).

Line 68: “between models” (plural)

Now corrected

Lines 75–77: please clarify the wording.

We make the following changes on lines 77-81

Modelling studies comparing MCO circulation patterns under different paleo-bathymetry reconstructions such as Hutchinson et al. (2025) and Liu et al. (2024) have found that relatively small changes to the seaway connecting the Arctic to the North Atlantic ocean can

have a crucial influence on the North Atlantic salinity balance, and hence the presence of an AMOC (Hutchinson et al., 2025; Liu et al., 2024) for which. In these studies the difference in the amount volume of fresh water supplied to the Pacific vs the Atlantic appears to be a key control on the relative strength of the PMOC and AMOC (Naik et al., 2025).

Line 88: “the rate of change in boundary conditions”. I would tend to consider boundary conditions as fixed values during the simulation, and the authors may rather want to refer to forcing here?

Dr Pohl is correct that this is the wrong terminology to use here. We were trying to emphasise that a step wise change in  $p\text{CO}_2$  (which in our simulations is a boundary condition) is not representative of how changes have occurred for the last 66 million years. We make the following adjustments to line 93:

The dependency on initial conditions and the rate of change in boundary conditions slower speed at which climate change occurred in the past is important to consider

Lines 88–91: this part I think essentially refers to the key notion of steady-state vs. transient behaviour. I suggest to be more explicit.

The edits to this section made to address other comments clarify this; it now reads:

The dependency on initial conditions and the rate of change in boundary conditions slower speed at which climate change occurred in the past is important to consider

Line 95: “suggests” (missing s).

We make the following changes to line 102:

record, which that suggests sustained Southern Ocean deep water formation

Line 114 and throughout. I do not think that the word “topography” is appropriate. Please consider replacing “topography” by the appropriate word everywhere, referring to the palaeogeographical reconstruction as a whole, to bathymetry. . .

We make the following changes

Line 120:

and palaeogeographytopography-driven changes

Line 237:

To isolate the impact of modified topography palaeogeography

Line 261:

To summarise, changing from PI to MCO palaeogeographytopography

Line 368:

and palaeogeographytopography that are more

Line 121: “simulations” (plural)

Now corrected.

Lines 121–122: clearly state this is because the GFDL model leads to colder conditions?

Other changes made to this section now address this issue, it now reads:

Beginning simulations from the endpoint of other similar simulations is a common strategy in paleoclimate modelling to reduce spin up cost (e.g Burls et al., 2021), however

Thisour approach resulted in a “cold start” initialisation, where the surface of the ocean heats faster than the deep ocean due to the higher climate sensitivity of ACCESS-ESM1.5.

Fig. 2: all acronyms should be defined in the caption.

Now corrected where not otherwise defined.

Lines 127–130: I suggest to expand the description of the acceleration, otherwise this does not make sense to the reader who did not read the Appendix in detail.

We make the following changes to lines 135-138:

We first perform a 100 year spinup of WOMBAT using DI, followed by 600 years using AA, which treats a year or a series of years as a single time step and uses an accumulated history of tracer evolution to accelerate the changes produced by a time step. We used a cycle length of 10 years during the acceleration as a compromise between accounting for interannual variability and leveraging the acceleration algorithm.

Line 137: delete space between “3000 m” and the dot.

Now corrected.

Line 153: add space between “(SATs)” and “and”.

Now corrected.

Figure 5: I think an additional figure showing raw fields of MLD would be helpful in the supplement.

Full fields of MLD will be in the appendix of the revised manuscript

Line 163: please consider replacing “concentration” by something more appropriate.

We make the following changes on line 173:

simulation features a ~~distinct~~concentrated maximum

Figure 7: I think it is not allowed to calculate a MOC in regions where the ocean boundaries are not closed – are the calculations shown in Fig. 7 robust, then? I do get that the authors masked their results using grey rectangles, but still, this seems unexpected to me.

For the Atlantic as a whole, since the integral to calculate the streamfunction,  $\Psi$ , is in the  $z$  and  $x$  direction, latitudes where there is no open boundary are of no concern. For the areas beneath the grey rectangles: Although not theoretically robust, there is not a noticeable break in the stream function (as was the case at the depths for which the seaways are open). We chose to include this component of plot as we believe it is illustrative and not misleading but agree that there should be a clarification in the figures caption.

We therefore make the following changes to the caption of Figure 7:

The latitudinal extent and depth of open boundaries for the Atlantic in the MCO (CAS and Tethys) are blocked out in grey. Streamfunction values calculated below grey rectangles, while technically calculated with an open boundary, are included for illustrative purposes. ~~and the~~Latitude range is restricted to exclude the open Southern Ocean and ACCESS-ESM1.5’s tripolar grid region (north of 65° N).

Lines 200–202: reference?

This is actually not from another paper but from our own analysis of data from the CMIP6 experiments but we forgot to refer to our figure. We add the following reference to lines 210-213:

High emissions experiments using ACCESS-ESM1.5 display a near identical loss of deep mixed layers in the Ross and Weddell seas and loss of the negative overturning cell south of 60° (Figure A11).

Line 227: I think  $O_{2sat}$  should be replaced with  $O_2$

Now corrected.

Figure 8: I do not understand the notion of “artificial age limiter”. Please be more specific.

“Artificial age limiter” is probably not the best way to describe this. By design, water age cannot be greater than the amount of time the simulation has been run for. The spin up of the PI control run was completed in two separate 1000 year sections, and water mass age was reset to zero halfway through that simulation. The maximum water age is therefore the length of the second spin up period. In practice this means that there are large areas of the ocean where water mass age  $\simeq$  simulation age, indicating they are not representative of equilibrium values. We wanted to point this out, especially as the PI control run is often implicitly viewed as equilibrated. We now change the caption of Figure 8 to focus on the interpretation of the figure rather than the history of how the ACCESS-ESM1.5 PI control experiments were run:

~~The area enclosed by the black line in subplot d) was affected by an artificial age limiter in the PI control run, equilibrium age values are likely higher in this area includes large regions in the PI control with the same age as the simulation length, equilibrium ages in this area would likely be much higher.~~

Line 238: “lower [...] than” (instead of “that”).

Now corrected.

Lines 239–240: I think Fig. 9 suggests it can be primarily attributed to changes in ventilation, not solubility

Looking at the top row of Figure 9, there are zones where ventilation age is no different, but there is reduced oxygen concentrations due to increased AOU and reduced  $O_{2sat}$  (e.g.  $\sim$  500 m depth, 50°N), there are also zones where there is little change in age AOU or  $O_{2sat}$ . We decide to remove this sentence as it appears to be at least half untrue and its content is not useful. Line 244:

~~The reductions in oxygen can be primarily attributed to reductions in oxygen solubility (Figure 9).~~

Line 263: I think the key point is then changing remineralization depth. Although I see what the authors mean when referring to changing export production, I think they need to be more specific since I think in MC3 export production at 50 m increases. See also elsewhere regarding the possible confusion arising from the reference to export production at different depths, reflecting changing remineralization depth. I also realize that an additional figure showing depth profiles of POC remineralization could be helpful.

We unfortunately do not have the necessary output to plot remineralisation, however we concur with the key point being shoaling of remineralisation depth. We make the following changes on lines 275-276:

This implies that depths greater than  $\sim 1500$  m reductions in AOU are attributable to ~~reductions in export production~~ shoaling of remineralisation depth rather than increased circulation.

Section 3.4.3. I think that the findings that MC3 induces a spatially more uniform / homogeneous [O<sub>2</sub>] would deserve to be put forward. I think it is an interesting aspect.

We agree that this is an interesting aspect, probably a result of a lack of circulation which would usually form tongues of more and less oxygenated water. We now mention this on lines 306-308:

Oxygen concentrations in the MC3 simulation are noticeably more uniform than in the MC1 simulation (Figures 13, 14). This is likely a result of the loss of ventilation, which produces more structure in the oxygen profiles of the MC1 and PI simulations.

Lines 321–322: to be explained/described.

Our comparison to Lago and England, and to the CMIP6 SSP585 experiment was meant to demonstrate that while our experiment is clearly a flawed representation of the MCO, it may be more analogous to the future because of this. We add to line 342:

The similarity between these high emissions scenario experiments and our own experiments suggests that the Earth's future may have more in common with our experiments than a past hothouse climate.

Section 4.2:

please plot the resulting curve of remineralization as a function of seawater temperature.

As we have already provided the equation for such a curve, and as this is not a model description paper, we find it unnecessary to add another figure to our text, which already includes many figures.

Size-dependent sinking speed would also possibly impact the conclusions (Kriest and Oschlies, 2008).

While implicitly discussed we now refer to size dependency explicitly on lines 434-437:

It is plausible that oxygen could be pushed towards anoxia with a more complex representation of biogeochemistry, particularly the representation of ~~detritus sinking and~~ remineralisation and ~~detritus~~ sinking dynamics, such as size dependant detritus sinking speeds.

Organic matter sulfurization could also later the POC rain flux to the seafloor (Hülse et al., 2019).

We thank Dr Pohl for pointing us to this study. This is a process that is not represented in the BGC model used in our work. We now make reference to this study on line 359-360:

Further the sulfurization of organic matter, which likely had a substantial impact on organic matter burial during OAE-2 ( $\sim 94$  Ma, Hülse et al., 2019) is not included in our BGC model.

Line 332: “be” to be deleted.

Now corrected.

Line 344: “npCO2”. Typo.

Now corrected.

Line 346: the authors refer in several places to the computational weight of the model, here even referring to “unfeasible” simulations. Please properly quantify this aspect by stating how many model years per wall-clock days are achieved on how many cores, for the online model, the online model with WOMBAT, and the AA scheme. It would be interesting for the reader. See also line 428: numbers would help support such statement.

We choose to include greater detail for the computational demands of ACCESS-ESM1.5 compared to AA in Appendix A1. We make changes to lines 467-471:

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  $\sim 1$  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.

Lines 349–350: does the model include a refractory POC pool? How is the latter handled?

WOMBAT includes three types of organic carbon: a single functional type of zooplankton, phytoplankton and detritus. It does not have a refractory POC pool.

Lines 352–354: or different background oxygenation conditions, see previous comments.

We now make reference to this point on lines 379-380:

Given that our simulations are essentially as unventilated as possible in the deep ocean, to achieve force 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.

Line 354: I do not think this is true. Some studies suggest the spread of anoxia in response to global climate cooling, for instance during the Late Ordovician (Pohl et al., 2021) or possibly the Late Devonian.

We thank Dr Pohl for pointing out this blind spot in our reading and accordingly change our language on line 382:

However, in proxy records, many OAEs are always associated with elevated  $p\text{CO}_2$  and temperature (Rampino et al., 2023).

Lines 355 and 360: please stay consistent in the spelling of “runof”.

We now use “runoff” on lines 384, 386 and 388.

Line 372: “surface flux regimes”. What do the authors mean?

We were trying to communicate the inability of our model to be run with both an “active” and “passive” temperature and salinity, however this sentence is no longer needed and this section has been changed to reflect progression. Beginning from line 399 it now reads:

In the context of model spin up, AA has to date only been applied to dynamically passive tracers. While it is not necessary for the physical ocean model to be in equilibrium (Khatiwala, 2024), variability in circulation has been found to degrade the performance of the AA algorithm (Khatiwala, 2023b). This complicates spin up strategy and necessitates a trade off between equilibrating biogeochemistry for a frozen"circulation regime or allowing it to evolve in tandem with the physical simulation. It has been suggested that AA could also be extended to dynamic tracers such as temperature and salinity, by using iterative passive tracers (Khatiwala, 2024), creating a passive temperature and passive salinity tracer, that can be accelerated towards equilibrium using a given (repeating) physical circulation (Khatiwala, 2023b). After a suitable interval (e.g. 50 years), the accelerated passive tracers could then be used to re-initialise the dynamic temperature and salinity, with alternation between active (online) and passive (AA) spinup stages. Preliminary experiments with implementing such an alternating passive and active spin-up strategy in MITgcm by S. Khatiwala have yielded promising results, and work is currently underway by D. Hutchinson to apply it to ACCESS-ESM1.5. If successful, this could provide major benefits in accelerating the spin-up of ocean physics in a fully-coupled earth system model.

We also take the time to edit our sections describing AA and it's implementation on line 45:

Here we use Anderson Acceleration (Anderson, 1965; Khatiwala, 2023b, 2024)

And on line 50:

Anderson Acceleration (AA) is a form of sequence acceleration. It is a "black box" method, meaning it can be adapted to a model with relative ease (Khatiwala, 2023b, 2024)

And in the conclusion on Line 420:

The use of AA here is one of the first practical applications of acceleration methods in Earth system modelling. More effective ways to harness these methods are almost certainly possible.

And on line 472:

than ~~receiving an input~~ providing an input and receiving an output from the model.

And line 473:

be applied to ~~almost~~ any biogeochemical model ~~that uses iterative time steps~~.

We also update our code and data availability statement to better direct readers to code that may be of interest to them on lines 448-449:

Both were used in their non CMORised format. Anderson Acceleration code is additionally available at (Khatiwala, 2023a).

Section 4.4. What about ocean acceleration techniques?

For this work we focused on applying Anderson Acceleration to accelerate biogeochemistry in an Earth System Model. See also our changes outlined above.

Lines 401: see also the seminal OAE2 study of Monteiro et al. (2012) or the Permian-Triassic simulation anchored in abundant proxy data of Hülse et al. (2021).

We thank Dr Pohl for directing us to this work, which we now cite on line 439-440:

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)

Water mass age: I think this quantity should be defined upon first use.

We make changes to lines 241-243:

AOU is influenced by circulation (~~indicated here by water age~~), detritus concentrations and seawater temperature. We use water age as a proxy for circulation. Water age is a tracer which increases by 1 every year and returns to zero upon contact with the surface; it is used as a measure of a parcel of water's last contact with the surface.

We again thank Alexandre Pohl for his time and effort in reviewing our paper. His contributions have both corrected errors, improved the presentation of the paper and helped to contextualise it within the wide field of paleoclimate science.