Review of:

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

For ClimPast

Dijon, July 7, 2026

Alexandre Pohl (CNRS, Dijon, France)

Paper summary:

The authors produce 3 biogeochemistry-enabled simulations under Miocene boundary conditions at 1, 2 and 3 times the preindustrial CO_2 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.

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 dissolve 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.

- 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.
- I also suggest to emphasize 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).
- 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)
    - The rates of carbon injection during these events was lower than today (I suggest to provide key numbers; I think rates were ½ during the Permian-Triassic and 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_2 increase due to organic carbon burial, and nutrient loading due to increased continental weathering (e.g., Newby et al., 2025; Zhang et al., 2026).
    - 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_2] conditions can alter this statement.

2. 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.

3. Temperature-dependent remineralization. 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.

4. 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.
- 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).

- 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 ").

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 modeling community, but never properly mentioned.

Other comments:

- Line 26: I think the two hypotheses traditionally apply to organic carbon burial, not anoxia.
- Line 60: “Earth” (capital is missing).
- Line 61: there and throughout, I suggest to keep the word “significant” for statistical tests and otherwise replace it with “substantial” or equivalent.
- Line 61: coma to be deleted after the reference.
- 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).
- Line 68: “between models” (plural)
- Lines 75–77: please clarify the wording.
- 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?
- Lines 88–91: this part I think essentially refers to the key notion of steady-state vs. transient behavior. I suggest to be more explicit.
- Line 95: “suggests” (missing s).
- 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…
- Line 121: “simulations” (plural)
- Lines 121–122: clearly state this is because the GFDL model leads to colder conditions?
- Fig. 2: all acronyms should be defined in the caption.
- 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.
- Line 137: delete space between “3000 m” and the dot.
- Line 153: add space between “(SATs)” and “and”.
- Figure 5: I think an additional figure showing raw fields of MLD would be helpful in the supplement.
- Line 163: please consider replacing “concentration” by something more appropriate.
- 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.
- Lines 200–202: reference?
- Line 227: I think O_2^sat should be replaced with O_2.
- Figure 8: I do not understand the notion of “artificial age limiter”. Please be more specific.
- Line 238: “lower […] than” (instead of “that”).
- Lines 239–240: I think Fig. 9 suggests it can be primarily attributed to changes in ventilation, not solubility.

- 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.
- Section 3.4.3. I think that the findings that MC3 induces a spatially more uniform / homogeneous [O_2] would deserve to be put forward. I think it is an interesting aspect.
- Lines 321–322: to be explained/described.
- Section 4.2:
    - please plot the resulting curve of remineralization as a function of seawater temperature.
    - Size-dependent sinking speed would also possibly impact the conclusions (Kriest and Oschlies, 2008).
    - Organic matter sulfurization could also later the POC rain flux to the seafloor (Hülse et al., 2019).
- Line 332: “be” to be deleted.
- Line 344: “npCO2”. Typo.
- 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.
- Lines 349–350: does the model include a refractory POC pool? How is the latter handled?
- Lines 352–354: or different background oxygenation conditions, see previous comments.
- 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.
- Lines 355 and 360: please stay consistent in the spelling of “runoff”.
- Line 372: “surface flux regimes”. What do the authors mean?
- Section 4.4. What about ocean acceleration techniques?
- 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).
- Water mass age: I think this quantity should be defined upon first use.

References cited

Crichton, K.A., Wilson, J.D., Ridgwell, A., Pearson, P.N., 2021. Calibration of temperature-dependent ocean microbial processes in the cGENIE.muffin (v0.9.13) Earth system model. Geosci. Model Dev. 14, 125–149. https://doi.org/10.5194/gmd-14-125-2021

Cui, Y., Li, M., Van Soelen, E.E., Peterse, F., Kürschner, W.M., 2021. Massive and rapid predominantly volcanic CO_2 emission during the end-Permian mass extinction. Proc. Natl. Acad. Sci. U.S.A. 118, e2014701118. https://doi.org/10.1073/pnas.2014701118

Gernon, T.M., Mills, B.J.W., Hincks, T.K., Merdith, A.S., Alcott, L.J., Rohling, E.J., Palmer, M.R., 2024. Solid Earth forcing of Mesozoic oceanic anoxic events. Nat. Geosci. 17, 926–935. https://doi.org/10.1038/s41561-024-01496-0

Hülse, D., Arndt, S., Ridgwell, A., 2019. Mitigation of Extreme Ocean Anoxic Event Conditions by Organic Matter Sulfurization. Paleoceanography and Paleoclimatology 34, 476–489. https://doi.org/10.1029/2018PA003470

Hülse, D., Lau, K.V., van de Velde, S.J., Arndt, S., Meyer, K.M., Ridgwell, A., 2021. End-Permian marine extinction due to temperature-driven nutrient recycling and euxinia. Nature Geoscience 14. https://doi.org/10.1038/s41561-021-00829-7

John, E.H., Wilson, J.D., Pearson, P.N., Ridgwell, A., 2014. Temperature-dependent remineralization and carbon cycling in the warm Eocene oceans. Palaeogeography, palaeoclimatology, palaeoecology 413, 158–166.

Kriest, I., Oschlies, A., 2008. On the treatment of particulate organic matter sinking in large-scale models of marine biogeochemical cycles. Biogeosciences 5, 55–72.

Monteiro, F.M., Pancost, R.D., Ridgwell, A., Donnadieu, Y., 2012. Nutrients as the dominant control on the spread of anoxia and euxinia across the Cenomanian-Turonian oceanic anoxic event (OAE2): Model-data comparison. Paleoceanography 27. https://doi.org/10.1029/2012PA002351

Newby, S.M., Young, S.A., Them, T.R., Gill, B.C., Owens, J.D., 2025. Balancing Redox Budgets: Mechanisms for Prolonging Anoxia During Major Carbon Burial Events. American Journal of Science 325. https://doi.org/10.2475/001c.143665

Penn, J.L., Deutsch, C., Payne, J.L., Sperling, E.A., 2018. Temperature-dependent hypoxia explains biogeography and severity of end-Permian marine mass extinction. Science 362, eaat1327.

Pohl, A., Lu, Z., Lu, W., Stockey, R.G., Elrick, M., Li, M., Desrochers, A., Shen, Y., He, R., Finnegan, S., Ridgwell, A., 2021. Vertical decoupling in Late Ordovician anoxia due to reorganization of ocean circulation. Nature Geoscience 14. https://doi.org/10.1038/s41561-021-00843-9

Pohl, A., Ridgwell, A., Stockey, R.G., Thomazo, C., Keane, A., Vennin, E., Scotese, C.R., 2022. Continental configuration controls ocean oxygenation during the Phanerozoic. Nature 608, 523–527. https://doi.org/10.1038/s41586-022-05018-z

Pohl, A., Wong Hearing, T.W., Brayard, A., Grossman, E., Joachimski, M.M., Le Hir, G., Letulle, T., Lunt, D.J., Martinez, M., Pucéat, E., Suan, G., Valdes, P., Donnadieu, Y., 2025. Spatial biases in oxygen-based Phanerozoic seawater temperature reconstructions. Earth and Planetary Science Letters 663, 119418. https://doi.org/10.1016/j.epsl.2025.119418

Reershemius, T., Planavsky, N.J., 2021. What controls the duration and intensity of ocean anoxic events in the Paleozoic and the Mesozoic? Earth-Science Reviews 221, 103787. https://doi.org/10.1016/j.earscirev.2021.103787

Sarr, A.-C., Poulsen, C., Do, E.L., 2024. Revisiting the early Late Cretaceous Equable Climate Problem through a model-data perspective. https://doi.org/10.22541/essoar.173385826.62535324/v1

Turner, S.K., 2018. Constraints on the onset duration of the Paleocene–Eocene Thermal Maximum. Phil. Trans. R. Soc. A. 376, 20170082. https://doi.org/10.1098/rsta.2017.0082

Wu, Y., Song, Haijun, Chu, D., Cui, Y., Dal Corso, J., Luo, G., Song, Huyue, Tian, L., Song, Hanchen, Jia, E., Tong, J., 2025. Microbial metabolism amplified warming in three Phanerozoic hyperthermal events. Nat Commun. https://doi.org/10.1038/s41467-025-66388-2

Zhang, F., Wei, G.-Y., Maffre, P., Li, Z., Zhou, J., Pohl, A., Lin, Y.-B., Elrick, M., Cheng, K., Pogge Von Strandmann, P.A.E., Shen, S., 2026. Lithium isotope evidence for enhanced continental weathering triggering the late Paleozoic greenhouse-to-icehouse climate transition. National Science Review 13, nwag168. https://doi.org/10.1093/nsr/nwag168