Review of “Spatially contrasted response of Devonian anoxia to astronomical forcing”. Justin Gérard et al. 2025, for Climate of the Past

First Reviewer: Anonymous Referee #1

Apologies to the author for taking so long to reply to the corrections! I am generally satisfied with the responses made by the authors and think that the manuscript can be accepted for publication after a few supplementary amendments that can probably be overseen by the editor without a new round of review.

We thank the reviewer for its time and the consideration of our answers.

First, I praise the authors for performing the additional sensitivity simulation to alternative surface forcing fields (wind stress and albedo) obtained from another HadCM3 simulation with a different set of orbital parameters. However, I disagree that the simulated impact is of secondary importance compared to that of the direct, orbitally-modulated, PO4 weathering. The numbers obtained for region SA (as an example) are 16 mmol.m-3 in this new simulation vs 45 mmol.m-3 in the main experiment. But the 45 mmol.m-3 represent the amplitude of change across two precession extremes, whereas, if I read the prescribed sets of orbital parameters correctly, the 16 mmol.m-3 represents the amplitude of change between approximately an extreme of precession and the mean. The appropriate numbers for comparison would therefore likely be close to roughly twice this latter value, i.e., 32 mmol.m-3 (although I agree this is rather speculative), and thus would be of the same magnitude as the change imposed by orbitally-paced PO4 weathering. Note that this is even more obvious for some of the other regions discussed (Si, NP, LG). In my opinion, this supplementary simulation rather suggests that orbitally-paced circulation changes are to the very least of similar amplitude (but see below) as those induced by PO4 weathering and I think this should be reflected explicitly in the manuscript.

We thank the reviewer for this comment and for re-engaging with the comparison between our sensitivity experiment and the main simulations.

First, we would like to clarify that the proposed extrapolation, consisting of doubling the reported anomalies, is not straightforward. This implicitly assumes a symmetric and linear response of the system to orbital forcing, which is not supported. For instance, Sarr et al. (2022) showed that variations in ocean anoxia between low- and high-eccentricity states can be of similar magnitude to those obtained between two precession extremes. Based on this, the amplitude obtained from our experiment would not necessarily increase when compared to another precession extreme, and therefore cannot be simply scaled by a factor of two.

More importantly, the values obtained in this sensitivity experiment are not directly comparable to those from the main transient simulations. The orbital configuration used here (eccentricity = 0.08) is intentionally extreme (for the purpose of the emulator) and unrealistic for Late

Devonian conditions. Because the response to eccentricity is non-linear, this implies that the resulting [O_2] changes are likely strongly overestimated.

For these reasons, we maintain that the effect of ocean circulation is secondary to that of nutrient weathering in cGENIE. Nevertheless, we acknowledge that the quantitative comparison presented in the previous version of the manuscript is not appropriate and may be misleading. As we are not able to provide more robust estimates within the current experimental framework, we consider that removing this comparison from the manuscript improves clarity and avoids potential misinterpretation. Instead, we now emphasize that assessing the relative contributions of circulation changes and phosphate weathering would require a dedicated set of experiments based on consistent and realistic orbital configurations. We also explicitly note that this perspective is supported by the results of Sarr et al. (2022).

Lines 359-364: "...was derived from HadCM3L simulations conducted by Sablon et al. (2025). Hence, further assessments of the impact of dynamically evolving wind stress and albedo fields under astronomical forcing, through dedicated experiments specifically designed to isolate and quantify these effects, would be valuable, particularly using higher-complexity models (e.g. IPSL). This is further encouraged by the results of Sarr et al. (2022), who showed that astronomically driven changes in ocean circulation and ventilation can exert a strong control on oxygenation, with regional variations reaching up to 40 μmol.kg^{-1} in a Late Cretaceous context. More generally, the emulator accounts for ..."

Second, there is something confusing to me in the response of the authors regarding the sensitivity of cGENIE to surface forcing (wind stress and albedo). Their Figure R1 (and the corresponding text in the response) suggests that the global ocean circulation in cGENIE is poorly sensitive to moderate changes in albedo and wind stress (obtained from different FOAM simulations in this example). Yet the sensitivity test to the wind stress and albedo obtained from a HadCM3 simulation with a different set of orbital parameters suggests an O2 variability of similar amplitude as the orbitally-paced PO4 weathering (see above), which is therefore assumed to be significant. It is unclear to me what should be concluded from this, given that the forcing fields are not shown. Does FOAM exhibit very small changes in wind stress and albedo across the range of tests performed, which would explain the weak sensitivity of cGENIE in Figure R1? Alternatively, is it because Figure R1 shows sensitivity tests to CO2 rather than orbital parameters? Or is it because the ocean circulation in cGENIE is too weakly sensitive to its surface forcing? If the latter is true, I think it warrants a more general discussion about the potentially overly large sensitivity of cGENIE to nutrient inputs compared to ocean circulation, although this may be out of the scope of this manuscript, as, in this case, it would not be limited to the results of this study.

In the new Fig. R1 (this document), we show that variations in the global overturning circulation remain small across both the main simulations and the additional sensitivity experiment. This indicates that the differences in [O_2] previously reported are not primarily driven by changes in large-scale overturning circulation. Instead, this suggests that the response is likely dominated by surface circulation processes. More generally, this behaviour points to a limited sensitivity of the large-scale ocean circulation in cGENIE to changes in surface wind stress and albedo under the range of forcings explored here. We agree with the reviewer that explicitly acknowledging this aspect improves the consistency of the manuscript and provides a more transparent interpretation of our results.

Lines 427-430: "...configurations (extracted from the 1.1 Myr transient simulation). This small variability likely reflects the absence of dynamical changes in ocean circulation associated with orbital variations across the simulations. It may also indicate that the large-scale ocean circulation in cGENIE is not sufficiently sensitive to variations in surface albedo and wind stress forcing."

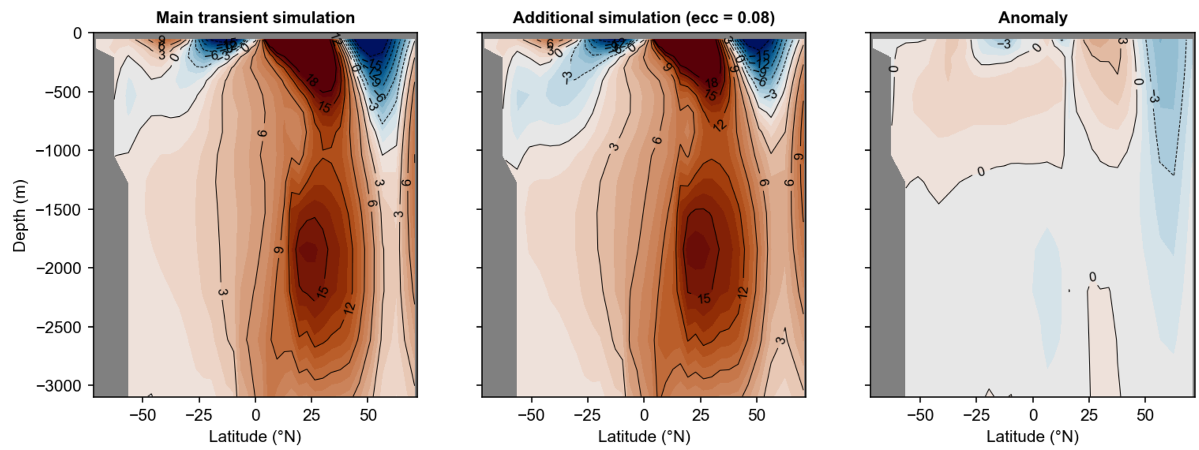

Figure R1: Global meridional overturning circulation (expressed in Sv; 1 Sv $\equiv 10^6$ $m^3 s^{-1}$) simulated during the main transient simulation (left), the additional simulation performed with the wind stress fields and albedo profile obtained with an eccentricity of 0.08 (middle), and the anomaly between the two (right).

l. 296-298. The phrasing is a bit odd because the authors did not demonstrate that the low O2 state of the Late Devonian is an emergent property of the Devonian paleogeography. Rather, they prescribed conditions that actually lead to a low O2 state in the simulations. Suggested rephrasing: "Our simulations show that prescribing plausible atmospheric O2 conditions for the Late Devonian (80% of the modern, add references) lead to an oceanic background state already depleted in dissolved O2, which..."

We fully agree with the reviewer and changed the manuscript accordingly.

Lines 296-298: "Our simulations show that prescribing plausible atmospheric pO_2 for the Late Devonian (80% of the modern, Krause et al., 2018, Mills et al., 2023) leads to an oceanic background state already depleted in $[O_2]$, which likely contributed to the increased frequency of anoxic events observed throughout this period (Becker et al., 2020)."

Appendix A. Perhaps worth mentioning again that the negligible changes in ocean circulation are due to the absence of dynamical changes linked to orbital variations across the simulations. In any case, it could be interesting to look at how the circulation is modified in the sensitivity test (to the extreme orbit wind stress and albedo) that was performed.

We agree with the reviewer to explicitly mention this in Appendix A. The changes were already implemented in the response to the second main comment.

Lines 427-430: "...configurations (extracted from the 1.1 Myr transient simulation). This limited variability likely reflects the absence of dynamical changes in ocean circulation associated with orbital variations across the simulations. It may also indicate that the large-scale ocean circulation in cGENIE is not sufficiently sensitive to variations in surface albedo and wind stress forcing."