Simulating the climate response to atmospheric oxygen variability in the Phanerozoic

Recommendation: Accept after moderate revisions provided that the authors can adequately answer to my 2 major comments.

Anonymous

Summary:

Wade et al. quantify the climatic impact of changing atmospheric oxygen concentrations (pO_2) using two ocean-atmosphere general circulation models in the Holocene, the Cretaceous and the Permian. They systematically conduct their simulations at 3 pO_2 levels (10, 21 and 35 ‰), which are shown to reasonably cover the pO_2 changes reported during the Phanerozoic. In their model, higher pO_2 values (and associated greater atmospheric mass) lead to two competing effects: an increase in Rayleigh scattering that induces an increase in albedo and surface cooling, and an increase in greenhouse effect that leads to surface warming.

The authors first run the two models on the preindustrial Holocene configuration. Interestingly, the state-of-the-art IPCC-class model (HadGEM-AO) and the version of the model designed for deep-time studies (HadCM3-BL) provide climatic responses that agree at first-order, thus supporting the robustness of the subsequent deep-time HadCM3-BL integrations. In their Holocene simulations, the mean annual global climate response to an increase in pO_2 is a warming, with varying regional patterns. The warming is particularly strong in the northern high latitudes, especially during the cold month. A cooling is simulated at low latitudes, which is especially strong and extends to most continental areas during the warm month. Higher pO_2 values tend to flatten the equator-to-pole temperature gradient. They also lower the climate sensitivity to atmospheric carbon dioxide.

Then the authors run the HadCM3-BL model in the Cretaceous and two Permian time slices. They show similar climatic behaviors and discuss two specific points related to each case study: the response of the terrestrial vegetation to changing O_2 levels during the Permian and the impact of changing O_2 levels on the capacity of their model to simulate the low latitudinal temperature gradients traditionally reconstructed for the Cretaceous based on proxy data. They notably show that changing oxygen concentration only slightly improves model-data agreement in the Maastrichtian.

Last but not least, they propose a quantification of the uncertainty in global temperature resulting from uncertainties in the pO_2 during the Phanerozoic. They show that the temperature bias associated with poorly constrained pO_2 levels is significantly lower than the uncertainty associated with the lack of constraints on the pCO_2, with a notable contribution of pO_2 during the Permian though.

It should be noted that Wade et al.'s implementation of O_2 forcing leads to results that agree at first-order with most previous attempts, but differ in sign with the simulations of Poulsen et al. (2015; 10.1126/science.1260670). Analysis of the model runs led the authors to suggest that Poulsen et al.'s implementation may not be totally coherent.

General comments:

I think that Wade et al. provide a very interesting and innovative study that shades new light on the

poorly explored question of the potential impact of changing pO_2 levels on deep-time climate. The results are based on numerous general circulation model simulations using two generations of climate models. The manuscript is relatively well organized (an exception if the methods section, see comments) and richly illustrated with high-quality figures and abundant information embedded in tables. The manuscript is lengthy (as testified by the length of my summary above). This is essentially due to the large amount of diagnostics provided by the authors but I also suggest below deleting the section of the manuscript relative to the impact of wind stress, which I think is not very useful and relatively badly integrated in the manuscript (see hereafter).

The discussion of the discrepancy with Poulsen et al.'s (2015) results is well conducted. Indeed, Wade et al. not only compared their results with the diagnostics provided by Poulsen et al. but also downloaded and analyzed the climatic simulations of the latter, by repeating key diagnostics that they previously provided for their own model runs. This effort deserves to be acknowledged. As a reviewer of this paper, I would be happy to have Poulsen et al.'s response, be it as another review or at least as a comment on the ClimPast Discussion forum. Therefore, I encourage the Editor to contact Poulsen et al. I also encourage the authors to make their implementation of O_2 forcing available online (as numerical – fortran – code or as equations) in order to allow other research groups to conduct similar experiments using other climate models, thus permitting to determine to what extent the discrepancy between Poulsen et al.'s results and theirs is model-dependent (and conversely, implementation-dependent; see major comment).

Most of my comments are intended to help the authors sharpen and clarify their manuscript. The only (other) major (potentially critical) comment I have regards the robustness of the analyzed climatic simulations. I suggest accepting this manuscript with moderate revisions, provided that Wade et al. can demonstrate that their climatic simulations are robust (i.e., sufficiently close to equilibrium).

Please note that the text refers in several places to supplementary figures. I did not find any SOM.

A. Major comments:

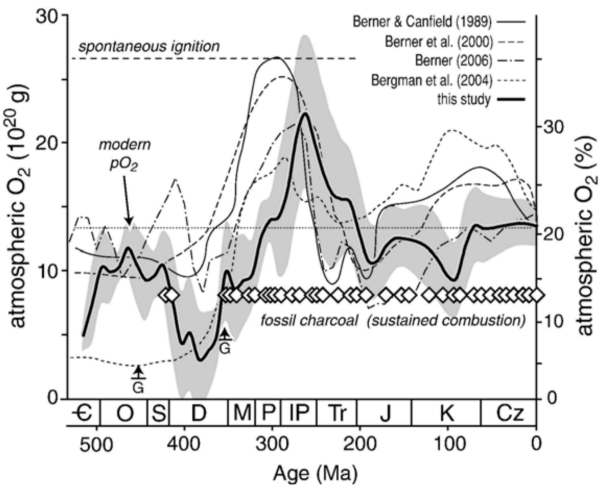
- On the discrepancy with Poulsen et al.'s results. Since the current study casts doubts about the Poulsen et al. implementation of O_2 forcing, I suggest making the implementation of Wade et al. available online to allow other modelers to repeat such experiments using alternative climate models – using GENESIS in particular. I think that such common effort will allow improving the implementation of oxygen forcing in a collaborative and efficient way.

- On the robustness of the climatic simulations. I recently had the opportunity to attend a presentation by Dan Lunt showing that the climatic simulations published by Lunt et al. (doi:10.5194/cp-12-1181-2016), run for 1422 years, did not reach equilibrium. A longer duration in the order of 10 kyrs is necessary to reach deep-ocean equilibrium, with the global mean SST simulated at the end of the longer simulation significantly differing (several °C) from the SST simulated after 1422 years of model integration time. Therefore I logically wonder if the climatic results used in the present manuscript based on the 1422-year long model integrations (see page 6, line 15) can be trusted. To what extent are the model runs equilibrated? I encourage the authors to clarify this point. Otherwise, the subsequent publication of longer model runs may significantly question the robustness of this entire study.

  ALSO: Page 8, line 6. "iterated for 100, 1000 and 100 years". What's the justification for the 100-year integration time used for two of the 3 experiments? I doubt that such duration is sufficient to reach equilibrium under a doubled CO2 level.

B. Other comments:

- Title: I would suggest revising the title to clearly indicate that several case studies are considered – maybe something like: "Simulating the climate response to atmospheric oxygen variability in the Phanerozoic – Holocene, Carboniferous and Permian case studies". In my opinion, such title would be more instructive, notably permitting readers interested in these 3 key time slices to more easily find this paper.
- Page 1, Line 1. "10 %": Fig. 1 suggests that it could have reached lower values.
- Page 1, line 5. "during different climate states" > "under different...".
- Page 1, line 15. "increasing oxygen content leads to a **slightly** better agreement".
- Fig. 1. Please show the different time slices used in each case study using for instance vertical lines.
- Fig. 1 caption. "High and low limits on atmospheric oxygen are indicated by horizontal grey dashed lines". What does that mean? Please clarify. I guess those horizontal lines indicate the 2 end-member O_2 levels considered in the deep-time case studies. In this case, the lower line is wrongly placed in the figure (this is not 10 %).
- Page 3, line 1. "to 20–35 % in the Permian **and subsequently stabilized at levels around 15–30 % from the Mid Triassic onward**" or similar.
- Page 3, line 6. See studies by Dahl et al. (doi:10.1073/pnas.1011287107) and Lu et al. (doi: 10.1126/science.aar5372) though, which provide very interesting insights into the evolution of pO_2 during the Phanerozoic. The authors may want to refer to these studies.
- Page 3, line 12. "visible life". OK, but I'm pretty sure it refers to the ocean realm, not to terrestrial life. Similarly, I think that the most prominent change between the Precambrian and the Phanerozoic is the advent of complex forms of life **in the ocean** during the Cambrian Explosion and subsequent Ordovician radiation.
- Page 3, line 15. "**possibly** led to the Ordovician glaciation" (there are a lot of alternative hypotheses and the spatial cover and thus climate impact of the primitive Ordovician vegetation remain poorly constrained).
- Page 3, lines 17–18. "which is consistent with a long-term sensitivity of the Earth system to CO_2". I do not understand what the authors want to convey here, please rephrase.
- Page 3, line 21. "continuously since the late Silurian". Fig. 5 of Algeo and Ingall (doi:10.1016/j.palaeo.2007.02.029) (below) suggests that the charcoal record is more or less continuous since the latest Devonian or so.



- Page 3, lines 24–34. In my opinion, this paragraph is off topic or, at least, should not be included here.
- Page 4, line 12. "Cenomanian (mid Cretaceous, **~95** Ma)".
- Page 4, line 31. "Changes to the incoming solar radiation **[reference?]**".
- Section 2 "Methods & Simulations". This section should be better organized. I suggest using subsections. Here are suggestions:
    - Page 5, line 4. "2.1. Models"
    - Page 6, line 16. "2.2. Experiences" or "2.2. Boundary conditions"
    - Page 9, line 1. "2.3. Data"
    - Page 9, line 8. "2.4. 1D energy balance model"
    - Page 9, line 26. "2.5. Climate sensitivity"
- Page 5, lines 13–14. "A fixed vegetation distribution of plant functional types is employed". Which one? A present-day one?
- Page 5, line 33. "increases in **thickness**"
- Fig. 2. Temperature unit?
- Page 6, line 5. "limited to 4 m **thick**"?
- Page 6, lines 18–19. "as it is possible to alter the model topography and bathymetry". Please delete.
- Table 1.
    - Please explain how the experiments name is built. As it is, the reader has to figure it out himself. The use of "2x" and "4x" in particular, is not obvious. This is placed at the beginning or in the middle of the experiment name and does not refer to any CO_2 level but rather seems to multiply the CO_2 value used in the baseline runs. Please, explain all this, for instance in the caption of Table 1. Also, what's the "*"?
    - What's the horizontal bar delimiting the 2 parts of Table 1? I guess that "baseline runs" and "sensitivity tests" may be included to refer to each part.
    - Here and throughout (Table 2, Fig. 6, Fig. 9, Fig. 12, Fig. 16 etc.), I would prefer to see the unit in parentheses rather than with a "/": "CO_2 / Pa" > "CO_2 **(Pa)**". The use of "/" is confusing when it does not represent a ratio.
- Fig. 3. Precipitation unit? + "Continental outline is represented with the thick black line" or similar.
- Page 7, lines 2–4. "The annual average … Figs. 2 and 3." Please move these lines and figures into the results section.
- Page 8, line 4. I guess this is "**O_2** content".
- Page 8, line 11. This sounds unlikely, see for instance Fig. 1 of Royer et al. (doi:10.1130/1052-5173(2004)014<4:CAAPDO>2.0.CO;2).
- Page 9, line 3. "heuristically". Well, this is obviously "by hand".
- Page 9, line 24. What's $\tau_{s,ebm}$ referring to?
- Page 10, line 5. Please define "CS" and "CRE".

- Page 10, lines 14–21. Here and throughout: the text is sometimes difficult to follow because the authors do not refer to figure panels. Please explicitly include "(Fig. 4b)" etc. when appropriate. Also page 13.
- Page 10, line 23. "(Figure 4 centre)" > "(Fig. 4 middle column)"
- Page 10, lines 25–27. "These could be … reduction with height". Is this effect really significant? This could be tested with a flat Earth simulation.
- Fig. 4a. Is Panama really open?
- Fig. 4. Please define the "cold month" and the "warm month".
- Table 2.
    - Missing data for 4xPI-GEM.
    - The authors may want to include data for their EXP^{21}_{10} experiments in brackets next to their EXP^{35}_{10} results to permit the comparison with Poulsen et al.'s results.
- Page 12, line 9. "Comparing the surface temperature **(Fig. 4a,b)** ~~and precipitation~~ response". Precipitation is showed in the next paragraph.
- Page 12, line 12. "air temperature ~~and precipitation~~ anomalies". Precipitation is showed in the next paragraph.
- Page 12, line 12. "Fig. S1". Missing supplementary figures? Please check throughout, including on page 15, line 8 + page 18, lines 7–11.
- Page 13, line 1. "representation of polar climate processes between the two models" + amplification by polar ice feedbacks.
- Page 13, line 12. Bjerkness compensation.
- Page 13, line 15. "suggests that pO_2 could mediate monsoon climate". Please check in the model output.
- Fig. 6 caption. "Global mean values (mm/day) are offset". Please rephrase. As it is, this suggests that values are really offset, which would be annoying. The text label is offset.
- Fig. 7. and Fig. 8:
    - Bottom left: What are the dashed lines?
    - Bottom right: Grey line is missing in the legend.
- Fig. 9, caption. "top-of-atmosphere radiative **im**balance".
- Page 18, line 11. "numerically unstable". Any idea why?
- Section 3.4.
    - I suggest changing the title for something more specific like "Response of Permian vegetation to changing O_2 levels" since this section really deals with the Permian case study.
    - The temperature and precipitation dependence of the dominant PFT simulated in the Permian should also be considered. To what extent are the changes in vegetation cover and type due to changes in temperature and precipitation? Changes in precipitation in the Wu-CM runs (Fig. 6e), in particular, seem to spatially correspond to the expansion of the BLT PFT (Fig. 10). I would like to see a short analysis of the environmental affinities of the main PFTs shown on the maps in Fig. 11. I suspect

that temperature and precipitation threshold values may play a more important role than changing O_2/CO_2 ratios.

- Fig. 11. The color map is reversed from a to b, which makes it difficult to read. Please revise.
- Page 19, line 15. "expansive". What does than mean?
- Page 20, line 8. I guess it means that the simulated changes in carbon storage on land do not impact the pCO_2 level? It would be good to clearly state what the authors mean by "not interactive".
- Page 20, lines 20–22. "however, it is likely … equator-to-pole temperature gradient". Please provide references to support this statement.
- Section 3.6 "Importance of Wind Stress". I get that atmospheric mass impacts wind stress, which in turn impacts the ocean circulation and the heat transport (see page 4, line 20). Unless I get it wrong, those effects are included in the coupled ocean-atmosphere simulations conducted by the authors, which is a good point. However, I do not understand why the authors test the impact of removing wind stress. In my opinion, this section is off topic and should be deleted and possibly kept for another contribution, which would simultaneously shorten the present manuscript and leave the possibility to conduct a robust analysis of the climate response (including the response of ocean dynamics, the analysis of which is essentially lacking so far). (For this reason, I did not include the minor comments relative to this section in this review).
- Fig. 12, caption. "Proxy data locations (**Upchurch et al., 2015**) are indicated".
- Fig. 14, caption. What's the unit of precipitation in panel c? Is this an annual mean?
- Page 24, line 1. "**mainly** due to".
- Page 24, lines 2–3. "The pre-industrial Holocene … of the Archean". Please support this statement with appropriate references.
- Page 24, lines 13–15. So, the implementation of pressure broadening is not the same as in Poulsen et al.? Page 7 line 2 suggests that O_2 forcing is analogous to Poulsen et al. Please clearly state what's common between both studies and what's different. Also, I encourage the authors to make their numerical code available for future work (see major comment).
- Page 24, lines 18–21. Since sub-daily model output was not written on disk in Wade et al. model runs and thus not made available for analysis, I suggest deleting this comparison that is not that instructive.
- Page 24, lines 22–24. Please be cautious: even in a slab model, the continental configuration can impact the ocean heat transport due to the varying ocean area and global climate and thus temperature (which is also impacted by the continental configuration). Another, maybe more robust argument to support the comparison, is that (i) both reconstructions are not so different at first-order and (ii) both simulations provide a relatively close global climate state (compare the mean annual SAT in Poulsen et al. 21% and this study 21% – ca. 18°C vs. 22°C).
- Page 24, lines 25–31. The contribution of changing ocean dynamics / deep circulation to the simulated climate changes is addressed for the first time here. I suggest either deleting this unsupported statements or providing clear diagnostics of the changes in ocean dynamics.
- Page 25, lines 1–2. The authors may want to refer to Pohl et al. (doi:10.5194/cp-10-2053-2014), who demonstrated the importance of ocean dynamics to simulate Ordovivician climate changes.
- Page 25, lines 5–6. "Increases … high latitudes". Please provide a reference.

- Page 25, lines 3–14. The authors may also want to refer to the climatic mechanism demonstrated by Rose and Ferreira (doi: 10.1175/JCLI-D-11-00547.1), which was subsequently invoked by Ladant and Donnadieu (doi:10.1038/ncomms12771) to explain the climate changes observed in their Cretaceous model runs.
- Page 25, line 16. "While subsequent experiments have put this in doubt". Please support this statement with a reference.
- Page 25, lines 24–25. "although there is evidence of vegetation which causes C4-like fractionation". During which geological period?
- Page 25, line 31. "Other approaches such as trait based methods". The authors may want to cite Porada et al. (doi: 10.1038/ncomms12113) who applied a trait-based model to simulate the impact of the Ordovician primitive terrestrial vegetation on weathering.
- Page 25, line 32 to page 26, line 2. I suggest deleting this paragraph.
- Figure 16. The authors previously demonstrated that Poulsen et al.'s implementation of O_2 forcing may not be robust. I think this is thus relatively unexpected that they here use those results in their Phanerozoic calculations, even if this may constitute a conservative estimate. I suggest using the results of the current study instead.
- Page 27, lines 17–18. "If pCO_2 and pO_2 … in the Phanerozoic". Why? Why would cooler climates be associated with higher pO_2 levels? I cannot imagine any clear and straightforward explanation to this.

C. Minor points:

- Page 3, line 2. "(grey shading **in Fig.** 1)".
- Page 5, line 13. "which simulate**s**".
- Page 6, line 12. Reference formatting: "(Valdes et al., 2017)".
- Page 6, lines 20–21. Reference formatting.
- Page 7, line 3. Please use correct experiment names instead of 4 x PI-CM[21].
- Page 8, line 17. "monotically increasing ozone column". Please rephrase.
- Page 8, line 29. Reference formatting.
- Page 10, line 19. "**Wu**-CM" (lower-case).
- Page 10, line 21. "This suggests that … but is non-linear". Please revise.
- Page 12, line 14. ", which **are** strongest".
- Fig. 5, caption. "4XPI-GEM(red)": missing space. See also multiple occurrences on page 18, lines 1–5.
- Fig. 6 color bar labels. Font size issue leading to overlapping text.
- Page 19, line 6. "**Fig. 11a**".
- Page 19, line 16. "reduces" > "reduce" (2 occurrences on the same line).
- Page 20, line 13. Please delete question mark.
- Page 20, lines 25–26. "These show **that** across both CO_2 contents **that** increasing". Please rephrase.

- Page 22, lines 4–5. “has the capacity to alter the radiative budget of the atmosphere and therefore ~~on~~ Earth’s climate”.
- Page 22, line 6. “with increasing pO_2**:**”.
- Fig. 15, caption. Missing space.
- Page 25, line 3. “~~also~~ contribute”.
- Page 25, line 9. “compared”.
- Page 25, line 13. “which may increase lead to”. Please revise.
- Page 27, line 7. “PAL”. Please write in full or provide meaning.
- Page 27, line 16. “When pO_2 **was** higher”.