Replies to Reviewer Comments

David Wade on behalf of the authors

We would like to extend our gratitude to Jim Kasting and the anonymous reviewers for the time and care they took in reviewing the paper. The comments will be dealt with in turn and changes to the updated manuscript will be described. In all cases, reviewer comments can be identified by the red text and the author reply in the black text. We've provided a tracked changes document for the editor where the new additions (bold black text here) are added in blue and text we have deleted is scored out in red for clarity.

Anonymous Reviewer #3

The manuscript "Simulating the Climate Response to Atmospheric Oxygen Variability in the Phanerozoic" by Wade et al. presents results from two ocean-atmosphere global circulation models to test the response of temperature, precipitation, and climate sensitivity to variable oxygen levels in earth's past. The primary results are that increasing oxygen levels causes global temperature to increase, precipitation to decrease, and climate sensitivity to change slightly. These results lead the authors to conclude that oxygen is a secondary factor (to CO2, though presumably also to solar luminosity and paleogeography) in earth's climate history. The study is mostly very well done, interesting, and well presented. My comments are mostly minor, and should not impede the eventual publication of the manuscript in Climates of the Past.

The use of two climate models is a strength of this paper, and I commend the authors for the extra effort. However, without a more in-depth discussion of how the models are different and how the differences lead to the responses reported in the paper, the effort falls a little short. It is worth noting and discussing that both models Edwards and Slingo (1996) radiation scheme. What about other physics schemes? Would other non-Hadley models that don't share the same physical parameterizations be expected to have larger differences than these two models?
While the two models are part of the Met Office Unified Model family of models, the two share few physical schemes except for the use of the Edwards and Slingo radiation scheme, although different versions, and the convection scheme is similar (but with a number of updates between HadCM3-BL and HadGEM3-AO). However, there are a number of large differences. It should be noted that the models do not share the same dynamical core and the cloud and precipitation schemes are different. In addition, the ocean model used is different. Whether other models would be expected to have larger differences will depend on the main driver of those differences. If the radiation scheme would cause the greatest difference, this would not be captured here, for instance. Deconvolving the drivers of differences between the model results to individual physics schemes would require a considerable model development effort and the authors are not aware of any effort to perform such a task in the paleoclimate community. While it is possible to rationalise the

differences between models (e.g. Lunt et al 2012 doi:10.5194/cp-8-1717-2012), ascribing these changes to particular model components is much more challenging and beyond the scope of our study.

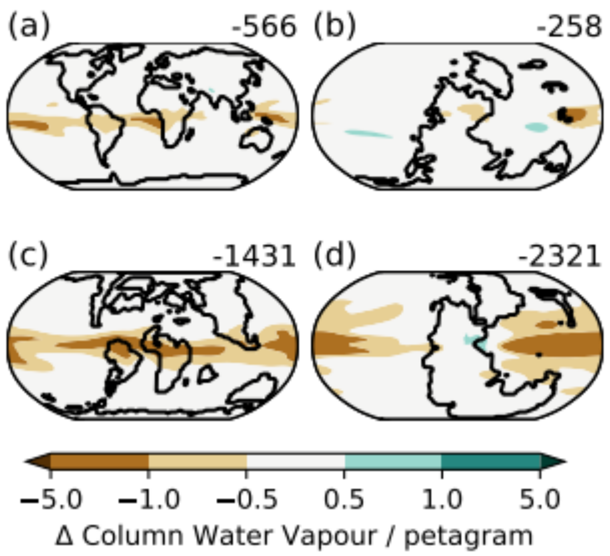
We therefore propose to update the description of model comparison as follows:
"~~, also suggesting this is not a model dependent result~~. It is worth noting that HadCM3-BL and HadGEM3-AO are not completely distinct climate models, for instance sharing the Edwards and Slingo 1996 radiation scheme, so this is unlikely to capture the full variability in possible climate model responses. **That the results are** ~~This is~~ in reasonable agreement with the 1-D results of Payne et al. 2016**,** who simulated a temperature response between +1.05 and +2.21 ^{o}C depending on assumptions about atmospheric ozone**, gives some confidence in the HadCM3-BL and HadGEM3-AO results.**

One of the most interesting results in the study is the difference in response with geography, and specifically the fact that the Wuchiapingian simulations show a temperature response that is opposite of the other runs. This is especially interesting in light of the conflicting results from previous models. The authors need to include an analysis and explanation of this result.

The response seen in the Wuchiapingian simulations combined with the lower (less positive) temperature anomaly for 4xPI-GEM$^{35}_{10}$ motivated the transient CO_2 doubling experiments which permit an interrogation of the climate sensitivity and the components that contribute towards these. These are provided in the section *Climate Sensitivity* and suggest a higher climate sensitivity at low pO_2 which is consistent with the order of temperature anomalies across the experiments (higher in cooler climates, smaller to negative for warmer climates). In addition to this, we have investigated a potential cause of the change and propose that the increase in convection at low pO_2 increases atmospheric moistening (see figure overleaf) which has a warming effect analogous to Rose and Ferreira 2013. This would serve to explain not only the changes in climate sensitivity but also the temperature response in the Wuchiapingian.

Changed around p18 l10:
"which tended to cool the low pO_2 (**Fig. 11**).
Unlike the clear sky shortwave effects, the longwave cloud radiative effects seem consistent across the three experiments.
It should be noted that attempts were made to simulate..."

Added final paragraph of *Climate Sensitivity* subsection:
"**The increase in climate sensitivity appears to be linked to the reduction in temperature anomaly in a warmer climate state. We propose that this is due to more vigorous convection at low pO_2 (Goldblatt et al. 2009) leading to an atmospheric moistening (Fig. 12) which causes warming analogously to Rose and Ferreira 2013. This is consistent with the increases in climate sensitivity observed - in a warmer climate the atmosphere can hold more water vapour, so any changes to water vapour will be amplified in their impacts on the radiative budget of the atmosphere. This water vapour feedback is also consistent with the weaker clear sky shortwave radiative effect observed in 2xMa-CM and the temperature response observed in the Wuchiapingian simulations.**"



Caption: “Change in column water vapour in (a) PI-CM$^{35}_{10}$, (b) As-CM$^{35}_{10}$, (c) Ma-CM$^{35}_{10}$ and (d) Wu-CM$^{35}_{10}$. Note the atmospheric drying at high pO_2 is enhanced in the warmer climate states of the Wuchiapingian and Maastrichtian and more subdued in the cooler climate states of the Asselian and Holocene.”

The manuscript tries to do too much. Section 3.4 is one example (3.5 and 3.6 are others). The discussion of the earth system feedbacks is interesting, but I would have preferred to see it in a standalone study that could do it justice and allow for a fuller discussion of the results and limitations. One shortcoming that the authors do not address is the physiological response of plants to changes in CO2 and O2. How the model handles these changes needs to be described. How well do we know how plants today and in the past responded to changes in atmospheric composition? Recent literature also indicates that changes in soil respiration may be as important as changes in plant respiration. How is this handled in the model?

The use of modern plant functional types for past climates is a key limitation of this section (as openly stated on p19 l2-4 of the OM), however is a necessary evil given the nature of this type of climate modelling. State-of-the-art offline methods (not coupled to climate models) use trait-based approaches (e.g. Porada et al. doi: 10.1038/ncomms12113, recommended by AR#2) that model plant physiological strategies. This is becoming the recommended way to simulate vegetation over the traditional PFT framework (see e.g. https://doi.org/10.1177/0309133315582018), however this will take time to filter into coupled climate models. Soil respiration is not treated in the model, however it is worth noting that due to the carbon cycle not being interactive it would not affect the results of the study (treatment of soil carbon in MOSES, now JULES is relatively recent, see doi:10.5194/gmd-10-959-2017). In MOSES2.1, atmospheric oxygen affects the photorespiration compensation point so the text has been updated to specify this:

“accounting for a number of factors including atmospheric oxygen content**, which affects the photorespiration compensation point (Clark et al. 2011).“**

Section 3.5 and the discussion of other mechanisms for producing warm climates is really a distraction from the main focus of the paper. The model-data comparison is not particularly rigorous and not necessary, and the discussion of warming mechanisms is incomplete and doesn’t reference many important studies. Both sections should be

deleted.
We thank the reviewer for this suggestion and have removed the model-data section from the revised manuscript. We have also removed the paragraph beginning “Increased oxygen content may also contribute to explaining…”

Section 3.6 on the influence of wind stress is interesting, but not very insightful without a proper analysis of the explanation for the differences between runs. This section should be removed or (preferably) expanded. How does the total heat transport differ between these runs with and without wind stress?
We thank the reviewer for this suggestion and have removed this section from the revised manuscript.

One of the main results of the paper is that the response to changes in O2 is very much a function of cloud feedbacks (e.g. Section 3.2). How robust then are the results? How do cloud feedbacks in HadCM and Had GEM3 compare to each other and to other models? This major point is not discussed in the Discussion or presented in the Conclusions.
The cloud feedbacks due to pO_2 changes have not been assessed in other climate models. Studies of cloud feedbacks in the context of climate models mostly relate to CO_2 forcing. The closest analogue to the radiative changes associated with pO_2 variability would be solar geoengineering due to the offset of shortwave and longwave radiation. A slightly earlier version of the HadGEM3 model (HadGEM3-ES v6.6.3 vs HadGEM3-AO v7.3) has longwave cloud feedbacks broadly in line with other climate models (Russotto et al. 2018, see https://doi.org/10.5194/acp-18-11905-2018)

P. 3, L. 17. “which is consistent with the long-term sensitivity of the Earth system to CO2 changes. . .” I don’t understand this comment. The fact that the CO2 range is constrained should not have an influence on the climate system sensitivity to CO2.
We have removed this last part of the sentence starting on line 16 page 3.

P. 16, L. 6-7. Please state the climate sensitivity of HadGEM3-AO and HadCM3-BL.
This information and references to the values have been added to the text as “For reference, HadGEM3 has a climate sensitivity of +3.6 ^{o}C (Nowack et al. 2015) and HadCM3 has a climate sensitivity of +3.1 ^{o}C (Johns et al 2006).”

P. 18, L. 1. “The clear-sky longwave radiative flux changes are higher in PI2X-CM. . .” That’s not what I see in Fig. 9a. Is there a typo here, or am I misinterpreting something?
The change is quite small, so this has been reworded to “radiative flux changes are **slightly** higher”.

P. 18, L. 8. “For Ma-CM, this value is much larger.” This is an interesting result that is not intuitive. The authors should provide a fuller explanation of the large change in sensitivity with this paleogeography and include the figure in the main text.
These figures have moved to the main text. The cause of this difference is addressed in the comment above.