

Interactive comment on “Simulating the Climate Response to Atmospheric Oxygen Variability in the Phanerozoic” by David C. Wade et al.

Anonymous Referee #3

Received and published: 3 February 2019

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

C1

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?

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 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?

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.

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

C2

should be removed or (preferably) expanded. How does the total heat transport differ between these runs with and without wind stress?

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.

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.

P. 16, L. 6-7. Please state the climate sensitivity of HadGEM3-AO and HadCM3-BL.

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?

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.

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2018-149>, 2018.