

Interactive comment on “Climate and marine biogeochemistry during the Holocene from transient model simulations” by Joachim Segschneider et al.

Anonymous Referee #2

Received and published: 11 February 2018

This paper provides a very worthy contribution to this special issue in honour of Ernst Maier-Reimer, who himself was a pioneer in modelling of global ocean biogeochemical cycles.

In this paper the Kiel Climate Model, coupled to PISCES is forced by accelerated and non-accelerated orbital parameters and atmospheric CO₂ for the last 9,500 years. This paper is the first to report on changes in the strength of the carbon pumps that drive the ocean-atmospheric CO₂ flux and dynamics of oxygen in seawater, including the oxygen minimum zones, in response to these forcings. The authors state as the most significant result that they find a substantial increase in the volume of the eastern equa-

[Printer-friendly version](#)

[Discussion paper](#)



torial oxygen minimum zone into the late Holocene, but only in the non-accelerated simulation, concluding that non-accelerated experiments are required for analyses of marine biogeochemistry in the Holocene. One obvious question would be whether this conclusions extrapolates further back in time (glacial-interglacial timescales; ocean anoxic events...).

The manuscript is well written, and an important contribution to improve our knowledge of the processes involved with carbon and oxygen cycling in the ocean. Below are some comments /suggestions, followed by minor typos: 1. In the conclusion the authors acknowledge the fact that most ocean-atmosphere coupled models do not simulate the mid-Holocene climate optimum under the applied astronomical and CO₂ forcings, and suggest that perhaps full scale ESM, including a land biosphere and free carbon cycle, may resolve this. Although the authors have been very careful in their wording, I do wonder whether it would be helpful to describe their model simulations as sensitivity tests to certain forcings over this time period. This doesn't take away the novelty of the results, but emphasizes the limitations. 2. In the experiment set-up only CO₂ is allowed to change, whereas methane and nitrous oxide concentrations were kept constant. According to Fluckiger et al. (2002) especially methane fluctuated considerably more during the Holocene than CO₂. Would this not influence the greenhouse forcing? 3. Are planetary and cloud albedo included in the radiation calculations? 4. Comparison with proxy reconstructions is a bit thin: Inferences of AMOC: it would be nice to see the model simulations compared with proxy reconstructions (for example: Hillaire-Marcel et al., 2001; Hoogakker et al., 2011, 2015; Thornalley et al., 2013). Volumes of oxygen minimum zones: while the authors refer to the review of Moffit et al. (2015), it would have been nice to see how changes in oxygen concentrations compare with local continental margin Holocene nitrogen isotope records of for example the Arabian Sea and eastern Pacific Ocean.

Minor comments: P 15: line six, should this be -0.4 GtC/yr? P 16: line 20, double relevance. P 19: line 24: sea-ice not seaice. P 22: line 28: affect instead of effect,

BGD

Interactive
comment

Printer-friendly version

Discussion paper



or impact? P 24: line 23: physical instead of physical, line 24: extremes rather than extrema?

Interactive comment on Biogeosciences Discuss., <https://doi.org/10.5194/bg-2017-554>, 2018.

BGD

Interactive
comment

Printer-friendly version

Discussion paper

