Climate of the Past Discussions

Clim. Past Discuss., referee comment RC2
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Comment on cp-2021-180

Anonymous Referee #2

Referee comment on "Climate and ocean circulation in the aftermath of a Marinoan snowball Earth" by Lennart Ramme and Jochem Marotzke, Clim. Past Discuss., https://doi.org/10.5194/cp-2021-180-RC2, 2022

Review for COP

Title: Climate and Ocean Circulation in the Aftermath of a Marinoan Snowball Earth

Authors: L. Ramme and J. Marotzke

In this paper, the question of the ocean destratification is addressed using an Earth System Model (ICON-ESM). Based on extended set of simulations, this study demonstrates that the post-melting stratification cannot length more than a few kyrs, so one order of magnitude shorter than expected (Yang et al. 2017).

Scientific interest

The paper is very well written and considerable care has been taken to the modeling approach. Moreover, the authors have clearly identified the fundamental processes hidden behind the "fast" freshwater stratification break-up (section 7.3), a tendency enhanced by the super-greenhouse climate (section 6). If some minor points require clarification, this manuscript deserves to be published.

Here are some minor recommendations that the authors may consider to improve the paper (ranked by order of importance)

1) The conclusion is a bit weird compared to the findings highlighted in the core of the MS. Statements presented here are speculative or not enough constrained. I encourage the authors to rewrite this section.

lines 436-440: this statement seems to be very speculative. My main concern regards the assumption made for the post-snowball sea surface temperature. Indeed by using 15 000ppmv as a melting threshold, the authors probably underestimate the pCO_2 at the end of the snowball earth event, so the Earth's climate is not drastic enough (the used CO_2 threshold is more in agreement with the water belt solution (Abbot et al. 2011)).

lines 441: In addition to underestimate the pCO_2, this study also assumed an instantaneous ice sheets melting, so this paragraph needs to be rephrased. (we could speculate that this melting occurs on a very short period of time, 2 kyrs as defended by Hyde et al, 2000 but this behavior seems to be inconsistent with Benn et al. 2015, ice sheet-climate simulations suggesting a decreasing of the ice sheet volume with the rising of the CO_2 above 0,02bar).

2) lines 360-374: Here the authors try to infer the time scale and environment for cap dolostone using their results about the ocean destratification. This approach is interesting but suffers a major flaw caused by the use of a uniform ocean depth (and held constant to 3500m) as a boundary condition. In my view the surface salinity simulated in the vicinity of continents (fig.7) cannot be considered as representative of coastal areas where cap dolostones were formed.

- Abstract (lines 14): without an accurate bathymetry, results of this study are not robust enough to support this conclusion.

- line 364 - I don't understand why the authors used Allen and Hoffman, 2005 here. This paper is explicitly focused on giant ripples recorded in cap dolostones. This paper is related to the topic but seems to be more appropriated to explore wind speeds during the sea level rise.

3) lines 422-425: According to my understanding, the main reason of the circumpolar existence seems to be the singularity of the Marinoan paleogeography, the northern hemisphere being characterized by the absence of continents above 50° of latitudes (fig.1).

Minor points

- table 2. TSI ☐ Total Solar Irradiance reduced by …

- what's difference between aureal and austral winter?

- fig. 6a "global MOC" is misleading and could be replaced by MOC at 30°S (to be consistent with the caption). fig 6b, c, d global values or zonally averaged ? (if yes you need to precise the location)