

Response to Anonymous Referee #2

- Referees comment
- Authors response

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 pCO₂ at the end of the snowball earth event, so the Earth's climate is not drastic enough (the used CO₂ threshold is more in agreement with the water belt solution (Abbot et al. 2011)).

Yes, the CO₂ concentration we apply during the supergreenhouse climate is at the lower end of the estimates, but still a possible scenario. Furthermore, we discuss the effect of potentially higher CO₂ concentrations in Sec. 7.2, where we derive that even at 10⁵ ppmv moderate temperatures are conceivable near the poles. Accordingly, our statement in the conclusions says that the impeding effect of temperature was only potentially severe with very high pCO₂. Hence, we do not claim that the moderate temperatures found in our study are representative for the snowball Earth aftermath in general, but that large parts of the possible range of CO₂ concentrations would result in climates exhibiting significantly large regions with still moderate temperatures. We rephrased this part of the conclusion to make our point more clear.

lines 441: In addition to underestimate the pCO₂, 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 CO₂ above 0,02bar).

In the mentioned paragraph, we state that the thermal expansion of seawater is small compared to the possible sea-level changes attributed to the glacio-isostatic adjustment. The melting of large continental ice sheets is of course the dominant contributor to the sea-level rise in the first several thousand years after the snowball Earth. However, including this in our model would not change the conclusion that also in the long-term the contribution of the thermal expansion of seawater is small. Therefore, we now name the melting of the continental ice sheets in addition to the glacio-isostatic adjustments as being the dominant contributors to sea-level changes. Apart from that, we do not see a reason to reformulate this paragraph.

We do not see a conflict in the conclusions of Hyde et al. (2000) and Benn at al. (2015). Hyde et al. (2000) states that the melting of the continental ice sheet was very rapid (<2000 years) once the deglaciation started. This is what is causing the inflow of

freshwater important for the ocean stratification in the snowball Earth aftermath. The simulations of Benn et al. (2015) are about fluctuations in the continental ice sheet mass before the start of the rapid deglaciation. The reductions and oscillations they find have a much longer timescale ($>10^4$ years) and the possible freshwater input would probably have been mixed with the deeper ocean through the dynamic circulation of the snowball ocean (Ashkenazy et al., 2013, DOI: 10.1038/nature11894), before the rapid deglaciation started.

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.

Indeed, our simplified bathymetry does not allow for a proper reconstruction of the conditions during the deposition of cap dolostones. However, we can make constrictions based on our finding of a rapid removal of the oceanic freshwater layer. As Liu et al. (2014, DOI: 10.1016/j.epsl.2014.06.039) state that there are signs of deposition in two chemically distinct fluids, we can infer that these dolostones must then have been deposited during the deglacial period, because after that, the ocean circulation would have removed any freshwater layer quickly. We reformulated the paragraph and hope that it now gives a better explanation of the conclusions that can be drawn from our study.

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

We changed the wording in this sentence, as well as in the first bullet point of the conclusions, to be in lines with the reformulated paragraph mentioned above.

- 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.

We agree that this citation might not be the correct reference to say that cap dolostones were deposited in coastal areas, as we did in the text. The revised paragraph does not include this specific statement anymore and the citation is removed likewise.

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).

This is correct, but lines 422-425 do not discuss the reason for the existence of the circumpolar current. In the text above, around lines 405 and 419, we refer to “the location of continents during the Marinoan...” to explain the strong circumpolar current. To be more clear here, we changed this wording to “The absence of continents north of 51°N during the Marinoan...”.

Minor points

- table 2. TSI → Total Solar Irradiance reduced by ...

We changed the caption of table 2 accordingly.

- what's difference between aureal and austral winter?

“aureal” was a mistake, “austral” is correct. Thank you for spotting the error.

- 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)

We adapted the label of figure 6a and clarified in the figure caption what properties are shown in figures 6b, c, d. Values in 6c and d are indeed global means and the transport shown in fig 6b is through a meridional section in the circumpolar current.