Response to Anonymous Referee #1

- Referees comment
- Authors response

The motivation in terms of cap carbonate formation, effects on ocean life, comparison with previous estimates of the de-stratification timescale of the fresh water layer is very helpful and well written.

The model description is especially helpful. The authors seem to have identified all weaknesses in their experiment design, anticipated all possible caveats/criticisms and addressed them very well. As part of this they discuss the coarse atmospheric and oceanic resolution, flat ocean topography, the unavoidable arbitrariness of the vertical mixing scheme that is addressed by sensitivity experiments, inability of the model to simulate full-thickness ice layer that is addressed via a readjustment of the salinity stratification upon melting of sea-ice to 35% area extent, the inability of the model to represent sea ice/glacier dynamics for a thick ice cover (sea ice dynamics is appropriately turned off then, leaving only the thermodynamics active), our incomplete knowledge of CO_2 after snowball events that is addressed via 3 different sensitivity scenarios, and more.

Figure 2: perhaps show also the overturning for the fully glaciated state (and the temperature and salinity for both states), so that it can be compared with the following simulated times shown later.

We added the requested panels to figure 2 and modified it to have the same style as figure 5 for an easy comparison. We also removed the uppermost row in figure 5, which showed the state of the control climate again, as this information is now fully included in figure 2. We adapted the text to fit the changed format in figure 2 and added a short paragraph about the ocean circulation in the snowball state.

Figure 3: may want to show sea ice instead/also in units of thickness/equivalent sea level, volume seems less easily interpreted here.

We now show sea-ice thickness in figure 3, as this is indeed easier to interpret. The plot of ice volume was removed, because it does not provide any additional information. We chose to show mean ice thickness instead of water equivalent, to avoid confusion about whether the value refers to a global or a hemispheric sea-level equivalent, since the plot shows the ice thickness for both hemispheres individually.

I agree with the authors that their circumpolar current is a weak point, and that this is a result of the flat topography. A mid-ocean ridge across the circumpolar opening would indeed have helped. It seems to me that the authors address this deficiency reasonably well in their analysis and discussion.

The thick snowball sea ice cover is sometimes referred to as sea glaciers, to distinguish it from the very different present-day sea ice. I don't know that this terminology is necessarily better than sea ice, admittedly.

We stick with the phrase „sea ice" when talking about the sea ice simulated in our model, because the model really only includes a formulation that was developed for the present-day sea ice. However, there are a few occasions in the manuscript where we speak about the general thick snowball sea ice cover. We adapted those formulations to „sea glacier" or similar phrases.

Lines 240-245: Interesting finding of distinct MOC cells in the freshwater and salty layers.

Figure 5: given the focus on stratification/re-stratification, it would make sense to show the temperature and density too. Perhaps another column of panels for temperature, with density contours over both temperature and salinity.

Around line 255: the density is not shown, but sloping and then vertical circumpolar salinity contours suggest that the circumpolar current is initially baroclinic and then mostly barotropic once adjusted, likely a result/artifact of the flat bottom as the authors mention. Is it? The relevance of this baroclinicity is mentioned below.

We agree that showing temperature and density is useful and adapted figure 5 accordingly. This point, regarding the sloping isolines and the baroclinic current, in combination with the clarifications later in the referees comment, is very helpful. We restructured the description of the results in figure 5 and added a few sentences.

Section 6: nice analysis of the overall warmth of climate and a useful comparison to previous studies.

Lines around 320 and 395: a very important and helpful discussion of the de-stratification timescale, and its causes, and a useful contrast with previous 1D vertical model results. This seems one of the highlights of this work. A comment on this: the important part is not the strength of the circumpolar current but its baroclinicity, given the thermal wind balance: drho/dy~du/dz. The sloping iso-halines shown at some stage of the deglaciation suggests a baroclinic current and later barotropic. The sloping lines should help the de-stratification process. Would be interesting to compare the top-to-bottom vertical shear in the circumpolar current simulated here vs in present-day and thus the implications on the sloping isolines of salinity and their contribution to the destruction of the fresh water layer.

We are thankful for this interesting and helpful comment. The referee is right that the circumpolar current is initially baroclinic and later barotropic. The strong shear in the current, together with the associated sloping isopycnals, is likely a major contributor to the fast destratification found in our model. To adequately represent this importance, we added a paragraph, including a figure showing the vertical profile of the zonal velocity in the current, to the discussion in section 7.3.