

## ***Interactive comment on “A conceptual model of oceanic heat transport in the Snowball Earth scenario” by D. Comeau et al.***

### **Anonymous Referee #1**

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Review of "A conceptual model of oceanic heat transport in the Snowball Earth scenario" by D. Comeau, D. A. Kurtze and J. M. Restrepo

The authors use a low-order climate model to study the role of the ocean circulation and ocean heat transport for the initiation of hard Snowball Earth episodes (i.e., global sea-ice cover). Besides the investigation of the large-ice cap instability associated with a Snowball Earth, the authors further study the small-ice cap instability. To this end, they develop a simplified coupled atmosphere-ocean-sea-ice model in which the radiative effect of the atmosphere on the surface temperature is represented by a prescribed effective emissivity and the ocean is represented by four boxes with heat transport between them. The ice representation is the most complex part of the model as it includes flow of thick sea ice under its own weight (i.e., sea glaciers). The authors use the model to demonstrate that 1) ocean heat transport works against Snowball Earth

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initiation since a Snowball Earth results when they shut off the ocean circulation, 2) the heat exchange at the ocean-ice interface has a strong impact on the ice edge in this model, and 3) the Snowball ocean circulation can either be directed poleward or equatorward. I find the paper very interesting, well suited for Earth System Dynamics and a valuable contribution to the literature on Snowball Earth climate dynamics. I do have a couple of suggestions and comments though that I hope the authors will be able to address before publication.

Major comments:

1. Atmospheric component of the model: there seems to be no representation of atmospheric heat transport, in contrast to the classic EBMs of Budyko and Sellers. Indeed, the Budyko and Sellers models only have multiple stable states because of atmospheric heat transport (Held and Suarez, 1974, Simple Albedo Feedback Models of the Ice Caps; Fig. 3). This seems worth pointing out because it implies that the bistability found in the model used here is different from the stability found in atmosphere-ice EBMs without ocean heat transport. It also makes me wonder to what extent the solutions would differ if a representation of atmospheric transport was included. I.e., would there be more equilibrium solutions, less, or the same? Answering this might require much work and might go beyond what is possible in the revision, but I would appreciate if the authors devoted some discussion to these points.

2. The model is hemispherically symmetric and has no cross-equatorial ocean flow, but much of the ocean heat transport is achieved by the cross-equatorial AMOC in the present-day climate, with upwelling in the Southern Ocean and downwelling in the North Atlantic. So are comparisons between the Sv of the model's ocean circulation and present-day observations really meaningful?

3. Conductive heat flux at ocean-ice interface, Eqs. 3 and 4: the model includes heat conduction proportional to the temperature difference between the ice freezing temperature and the ocean temperature. But for salinities greater 24.7 the density

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maximum of sea water is at its freezing point and the formation of sea ice at the surface must therefore be preceded by convection due to stability reasons (e.g., Washington and Parkinson 2005; Voigt and Marotzke, Climate Dynamics 2010). So shouldn't this term then not always be zero, since I expect salinities are above 24.7 psu. Apparently it is not, as is shown by the importance of that term, but it remains unclear to me why. Maybe for a partially ice covered box the ocean temperature will be above freezing temperature since otherwise the box was completely ice covered, but then it seems physically dubious to use the mean ocean temperature to parametrize the ocean-ice heat flux as this relies on the temperature of the ocean region where there is no sea ice, and hence no ocean-ice flux. How does this affect the result on the importance of the ocean-ice flux?

4. Global-mean ice-accumulation rates: In equilibrium the global-mean ice-accumulation rate must be zero. In Fig. 2d, the global-mean of the red dashed line seems to be positive if looking at the blue axes, but now that I notice the red axes and that it might indeed be zero. So the blue and the red axes should have the same zero position. Related to this, the global-mean accumulation rate in a Snowball Earth state must be zero, but in Figs. 3d, 4d it is positive everywhere, and so the ice is still growing and the model is not yet in equilibrium. I suggest to run the model into true equilibrium. The final ice thickness will be set by the value of the geothermal heat flux.

5. It's very interesting to see that the model can sustain a stable ice edge at 10-15 deg of the equator. This is similar to the Jormungand state (Abbot, Voigt, Koll, JGR 2011; Voigt and Abbot, Climate of the Past 2012) but must be for a different reason. In Abbot et al. and Voigt and Abbot, the stable low-latitude ice edge was made possible by the low albedo of snow-free sea ice and ocean transport was not important. Here, in contrast, the stability must be due to ocean transport, and a more detailed discussion of the physics that are involved in the stabilization of the ice edge by the ocean transport would be desirable.

6. In the abstract the authors state that the presence of ocean heat transport works

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against Snowball initiation. But the impact seems to be non-monotonic according to Fig. 5c: I.e., an intermediate circulation strength ( $D=0.05$ ) prevents global ice cover, but for both a lower and a higher value of  $D$  and thus a weaker and a stronger circulation the model falls into a Snowball.

7. The fact that the Snowball ocean can flow in both directions is interesting, and while it is mentioned in the text I feel that having a figure illustrating this should help. Maybe a more general schematic that describes the connection between ocean circulation and ice edge in the different climates?

Minor comments:

1. Use of colatitude instead of latitude: I suggest the authors change to latitude as this is more familiar for climate scientists, and when looking at the plots I had to sometimes remind myself that 0 degree is not the equator but the pole.

2. I suggest to integrate Eq. 7 with respect to longitude as the model is zonally-symmetric

3. page 13, line 17: long wave → longwave

4. page 16: longitudinal domain → latitudinal domain?

5. page 16: for documentation purposes I recommend including a plot for the control climate described here

6. Heading of Section 4.2: Circulation constant → Hydraulic constant?

7. page 23: broken sentence on lines 14-16

8. Fig. 7: include the melting occurring when  $\epsilon=0.4$ ; this is discussed in the text but not shown in the figure

9. Figs. 8 and 9: include arrows to further indicate the direction in which the experiments are performed; also for the values of  $\epsilon$  and  $D$  for which the blue crosses

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and red circles overlap I suggest to plot alternating crosses and circles, otherwise it is difficult to see that there are two experiments leading to the same climate

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