

## Answer to reviewers

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We are thankful for the reviewers' constructive comments that helped to considerably improve and clarify the manuscript. We hope that its revised version answers their concerns. In the following we illustrate how we took the reviewers' comments into account. Each reviewer is addressed individually, with the reviewer's comments in italic font, our answers in normal font. We also made changes to the manuscript that are independent of the reviewers' comments. These changes are presented in section 4.

### 1 Reviewer 1

#### 1.1 Major comments

*(i) The paper is polemic. It sets up the notion that climate modeling studies have determined that the snowball Earth hypothesis is implausible, and then aggressively refutes this strawman argument. Of course this is not quite the state of snowball Earth modeling. Since the original 1-D EBMs, a hierarchy of models has been used to test the idea of a snowball Earth. These models have obtained different solutions, but the important contribution is that through these studies a much richer understanding of the dynamics and physics has surfaced, including the roles of sea ice and sea glacier dynamics, ice albedo, ocean dynamics, Hadley circulation, and clouds. In addition, many previous snowball Earth studies have carefully pointed out that large uncertainties remain in components of the climate system that may be important to snowball Earth simulation.*

We beg to differ. The cited studies of Chandler and Sohl (2000), Poulsen et al. (2002), Poulsen (2003) and Poulsen and Jacob (2004) have reported difficulties in Snowball initiation, and Chandler and Sohl (2000) and Poulsen et al. (2002) speculated about missing processes, such as carbon dioxide condensation, whose inclusion might be needed for easier Snowball initiation. To illustrate this

point, we cite from Poulsen et al. (2002): “In comparison to these uncoupled atmospheric GCM studies, FOAM exhibits a reduced climatic sensitivity to reductions in solar luminosity and pCO<sub>2</sub>, largely as a result of ocean dynamics [Poulsen et al., 2001]. It is unlikely that a further reduction of  
25 pCO<sub>2</sub> in the FOAM Neoproterozoic experiments will result in an ice-covered Earth, since radiative forcing has a logarithmic dependence on CO<sub>2</sub>. This raises the possibility that alternative triggers are required to initiate a snowball Earth.”

In the broader community, this has led to the notion that Snowball Earth initiation is difficult in climate models, with a particular role of the ocean. This can be seen from the following quotes from  
30 publications cited in the introduction of our manuscript:

1. From Kerr (2010): “Some more-recent paleoclimate modeling, however, suggests that the leap from lowlatitude glaciation to a hard snowball may be difficult or even impossible. “We can get ice on land,” says climate modeler Mark Chandler of the Goddard Institute for Space Studies in New York City. “It’s the ocean we can’t freeze over.” Model oceans can hold lots of heat and move it  
35 around in currents, frustrating a complete freeze-over, Chandler says. A few years ago, “the pattern was that the more sophisticated the model, the less likely you’d get a hard snowball result,” he says. Discouraged, Chandler and others moved on to other projects.”

2. From Lubick (2002): “Climate modellers are also reluctant to embrace snowball Earth. “It’s very, very difficult to simulate,” says Chris Poulsen, a modeller at the University of Southern Cali-  
40 fornia in Los Angeles. Last year, he published a simulation showing that the ice sheets would have stopped at northern Europe during the late Neoproterozoic. One problem, say the modellers, is that oceans contain too much heat for them to freeze over completely.”

Therefore, we do not agree with reviewer 1 when saying that the paper is polemic. In contrast, we consider it a main result of our paper that Snowball Earth initiation might be much easier than  
45 previously reported. Nevertheless, we have revised the manuscript to carefully point at model uncertainties and the need for future studies with different AOGCMs. These issues are now addressed in the abstract, the introduction, and the discussion. In the introduction, we also indicate that the model hierarchy has led to important insight into the processes important for Snowball Earth initiation as mentioned by the reviewer.

50 The introduction now also addresses the point that ocean dynamics have been made responsible for difficulties with Snowball initiation. In the discussion, we note that our study shows that Snowball initiation is not necessarily more difficult in models with ocean dynamics compared to models without. We also replaced “extreme forcings” by “much stronger forcings” in the abstract, the introduction, the discussion and the conclusion, and we have changes “Despite severe limitation of  
55 their model” to “Despite limitations of their model,”

*(ii) This study does not contribute substantially to our understanding of the dynamics and physics of snowball Earth initiation, and fails to indicate why this particular model simulates global sea-*

ice cover at 94%. What is happening at the sea-ice line that causes it to advance in the Marinoan case relative to the pre-industrial case? This is the salient problem. The global energy balance analysis using a 0-D EBM does not speak to this issue, consumes too much of the paper, and provides little insight into the GCM behavior. What is it about the ECHAM/MPI-OM model that facilitates snowball Earth simulation? The fact that it is a state-of-the-art model is not sufficient explanation.

We acknowledge the fact that the temperature balance in the surface ocean layer and lowest atmospheric level at the sea-ice line is an interesting issue because it illustrates which processes promote or impede sea-ice formation at the sea-ice line by exerting positive or negative temperature tendencies (vertical mixing and horizontal advection in the ocean, cloud radiative forcing,...) as done in Poulsen et al. (2001). Nevertheless, we are convinced that model behavior away from the sea-ice line is equally important for Snowball Earth initiation because of the interaction of different latitudes through advection.

This implies that a more global view on the difference between two climate simulations is helpful. This issue is exactly addressed in our analysis with the 1d-EBM by analyzing the effect of differences of planetary albedo, effective emissivity, and heat transport on zonal mean as well as global mean surface temperatures. It is therefore a very helpful tool. The 0d-EBM is not at all used for this purpose but to estimate the Snowball Earth bifurcation point and the transition times. We are convinced that a careful description of the 0d-EBM helps the audience to apply it to their simulations (if they intend to do so). That the 0d-EBM is of interest to others is confirmed by reviewer 3.

To answer why ECHAM5/MPI-OM freezes over much more easily than FOAM, three of us (D. S. A, R. T. P., and A. V.) are involved in the SNOWMIP project cited in the manuscript. Its somewhat unfortunate that the results of SNOWMIP will only be published next year, and we have revised the corresponding part of the discussion section to give a more concise description of the main SNOWMIP results. These results help to make clear why ECHAM5/MPI-OM has less initiation trouble than FOAM. Nevertheless, more AOGCMs are needed to put our results into perspective. This is also stated in the revised discussion section of the manuscript.

Our model is the most sophisticated climate model that has ever been used for Snowball initiation as is reasoned in the introduction. We are therefore convinced that the term “state-of-the-art” is appropriate. However, the revised manuscript now alludes to the fact that our model incorporates neither sea-ice glaciers nor land glaciers.

(iii) The authors conclude that low-latitude continents cause cooling (and facilitate global sea-ice cover) due to an increase in surface albedo. This is rather obvious. Of course surface albedo will be greater in the Marinoan case than the pre-industrial case because the sea ice area is larger. As in (ii), the important question is why the sea-ice expansion is greater with low-latitude continents. The paper doesnt address this.

We do not share the reviewer's opinion that this result is obvious. While we, in agreement with Lewis et al. (2003), find that low-latitude continents cool the climate, Pollard and Kasting (2004) and Poulsen et al. (2002) found opposite behavior in their models. This is stated in the manuscript and shows that while our result is what one might expect (see manuscript), other models have arrived at a different conclusion.

Moreover, it is important that MAR and PI use the same global mean background surface albedo fixed in MAR and PI. This fact is now repeated in the discussion section. The higher surface albedo of MAR, resulting mainly from the increased sea-ice cover in MAR, is a consequence of the shift of continents, which redistributes background surface albedo across the globe. In contrast to the reviewer's comment, the manuscript addresses why MAR is so much colder than PI. This is done by the 1-d EBM in section 3.2, showing two thirds of this cooling can be attributed to increased planetary albedo, the remaining one third to a weaker greenhouse effect.

## 1.2 Minor comments

1. p. 1862. *T should be used for surface temperature, rather than  $\tau$ . Later in the paper, T is used for ocean potential temperature and  $\tau$  is a time constant. Please make these symbols consistent throughout.*

For the 0d-EBM in Sect. 5, the time constant is now denoted by  $\gamma$  instead of  $\tau$ . We keep  $\tau$  for the surface temperature of the 1d-EBM in Sect. 3.2 since this is a widely used symbol for surface temperature without any danger of confusion (e.g., see Heinemann et al. (2009)).

2. p. 1863. *...effective emissivity decreases... due to larger longwave cloud radiative forcing (not shown)... The authors should expand and explain this point. How are the clouds changing? Which clouds?*

We have rephrased this paragraph to better describe the global role of clouds and to stress the particular role of clouds for planetary albedo between 20N and 45N as well as for effective emissivity in the tropics. To this end, we have also added global mean cloud shortwave and longwave forcing in Table 3, and we note that in the tropics, cloud cover around 300 hPa is higher in MAR than in PI (see section 3.2 in the revised manuscript).

3. p. 1872. *... we point out that not only ocean dynamics and sea-ice and snow albedo parameterizations but also differences in the simulation of the atmospheric circulation and clouds must contribute... Where? Other snowball Earth studies have done this and should be cited here, but this is exactly what's missing from this study.*

We have rephrased this paragraph too make clear that this has been shown in the SNOWMIP project described in Pierrehumbert et al. (2010). To our knowledge, SNOWMIP is the first study

that compares Snowball initiation in two different AGCMs in a controlled setup.

4. p. 1865. *the sea-ice line has stabilized at 30N ... Previous studies have indicated that the sea-ice line de-stabilizes once it enters the Hadley realm. ECHAM seems to show a similar behavior.*

130 The Hadley cell argument cannot explain why some AGCMs (Chandler and Sohl, 2000; Micheels and Montenari, 2008) do not get unstable once the sea-ice enters the Hadley cell realm. That said, the Hadley cell argument can not explain why Chandler and Sohl (2000) and Micheels and Montenari (2008) find stable states with sea-ice close to the equator because their models also include Hadley cell dynamics. We therefore do not mention it in the manuscript.

135 5. p. 1872, 1874. *Discussion of land glaciers on a slushball Earth. Pollard and Kasting (2004) have shown that the simulation of land glaciers is sensitive to details of continental paleogeography.*

We thank the reviewer for pointing this out. In the revised manuscript, we are more cautious about the implications of our results for slushball solutions in section 6. We state that our land glaciers might be able to form once topography is included, and we cite Pollard and Kasting (2004) and Liu and Peltier (2010) to note that the formation is sensitive to details of continental paleogeography.

140 6. *Not all of the figures are necessary. The land/ocean mask in Fig. 1 can be seen in Fig. 7. The horizontal grid distance in Fig. 3, while an interesting technical point, is not necessary. Fig. 8 is also unnecessary, and can be described in the text. These figures could be replaced with figures that show the surface temperature of the open ocean for the MAR and PI case. In addition, figures should be added to address points (ii) and (iii) above.*

145 We keep the land-sea mask as seen by the atmosphere model ECHAM5 (Fig. 1) since it is an important boundary condition and warrants an individual plot. However, we have dropped the plot of the ocean grid (Fig. 3) and refer to Voigt (2010) for this figure. We have included a plot of the surface temperature of simulation TSI96, i.e., the state with maximum stable sea-ice cover. This plot illustrates how far tropical land temperatures are from allowing perennial snow cover and hence, in principle, the formation of land glaciers.  
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## 2 Reviewer 2

*It would have been nice to know what physical feedback leads to the different model results here, but I fear this may not be simple to decipher. It is not impossible that a different GCM applied to the same problem will give different results and it will be difficult to tell what difference between the two models is responsible for the different behavior. This is because the different model feedbacks (sea ice albedo, clouds, snow...) are all coupled, and if one of them is, in fact, responsible for the different model response, the others react and could mask the original cause. While this is somewhat disappointing, it simply means that we need many more studies with state-of-the-art models such as*

*used here to put the results of the present model in perspective.*

160 We have taken these points into account by changes to the abstract and the discussion section as described in the answer to reviewer 1.

### **3 Reviewer 3**

*My first comment is about the slushball. Pollard and Kasting in an AGU monograph have done some simulations using an atmospheric GCM coupled to a slab ocean in which they test the conditions*  
165 *required to have land ice down to the sea level at the equator with an ice-free equatorial ocean. Using an offline ice-sheet model, they find that high topography closed to the ocean may allow to initiate ice-sheet on the equatorial continents. I know that there are many differences between the Pollard and Kasting model and the Voigt et al. model. Nevertheless, it may be fair to relate these results in the paper in order to counterbalance the discussion.*

170 We have taken the results of Pollard and Kasting (2004) as well as newer results of Liu and Peltier (2010) into account and are more cautious about the implications of our results for the Slushball Earth hypothesis in the revised manuscript. Corresponding changes have been made to the discussion section as described in the answer to reviewer 1.

We also thank the reviewer for his comment on the large igneous provinces though we do not  
175 discuss this further in the manuscript.

### **4 Changes to the manuscript that are independent of the reviewers' comments**

1. After submission of the paper in September, we have performed one more simulation with TSI set to 94% of its present-day value and carbon dioxide doubled with respect to its pre-industrial concentration (simulation TSI94-2CO<sub>2</sub>). This simulation does not result in a Snowball Earth. We now  
180 give a CO<sub>2</sub> range of one to two times the pre-industrial concentration whereas the original manuscript provided a range of one to four times the pre-industrial concentration. Corresponding changes have been made in the manuscript in the abstract, section 4, and table 2, and figure 5 of the revised version.

2. After submission of the paper, we have learned about a 0d-EBM by Galeotti et al. (2004) for the K-T boundary. We cite their work and reason why our 0d-EBM is a significant improvement of  
185 their work in section 5.

## References

- Chandler, M. A. and Sohl, L. E.: Climate forcings and the initiation of low-latitude ice sheets during the Neoproterozoic Varanger glacial interval, *J. Geophys. Res.-Atmos.*, 105(D16), 20737–20756, doi:10.1029/2000JD900221, 2000.
- 190 Galeotti, S., Brinkhuis, H., and Huber, M.: Records of post-Cretaceous-Tertiary boundary millennial-scale cooling from the western Tethys: A smoking gun for the impact-winter hypothesis?, *Geology*, 32, 529–532, doi:10.1130/G20439.1, 2004.
- Heinemann, M., Jungclauss, J. H., and Marotzke, J.: Warm Paleocene/Eocene climate as simulated in ECHAM5/MPI-OM, *Clim. Past*, 5, 785–802, doi:10.5194/cp-5-785-2009, 2009.
- 195 Kaufman, A. J.: Palaeoclimate: Slush find, *Nature*, 450(7171), 807–808, doi:10.1038/450807a, 2007.
- Kerr, R. A.: Snowball Earth has melted back to a profound wintry mix, *Science*, 327(5970), 1186, doi:10.1126/science.327.5970.1186, 2010.
- Lewis, J. P., Weaver, A. J., Johnston, S. T., and Eby, M.: Neoproterozoic “snowball Earth”: Dynamic sea ice over a quiescent ocean, *Paleoceanography*, 18(4), 1092, doi:10.1029/2003PA000926, 2003.
- 200 Liu, Y. and Peltier, W. R.: A carbon cycle coupled climate model of Neoproterozoic glaciation: Influence of continental configuration on the formation of a “soft snowball”, *J. Geophys. Res.-Atmos.*, 115, D17111, doi:10.1029/2009JD013082, 2010.
- Lubick, N.: Palaeoclimatology: Snowball fights, *Nature*, 417(6884), 12–13, doi:10.1038/417012a, 2002.
- Micheels, A. and Montenari, M.: A snowball Earth versus a slushball Earth: Results from Neoproterozoic climate modeling sensitivity experiments, *Geosphere*, 4(2), 401–410, doi:10.1130/GES00098.1, 2008.
- 205 Pierrehumbert, R. T., Abbot, D. S., Voigt, A., and Koll, D.: Climate of the Neoproterozoic, submitted to *Annu. Rev. Earth Pl. Sc.*, 2010.
- Pollard, D. and Kasting, J. F.: Climate-Ice Sheet Simulations of Neoproterozoic Glaciation Before and After Collapse to Snowball Earth, in: *The Extreme Proterozoic: Geology, Geochemistry and Climate*, Geoph. Monog. Series, 146, edited by: G. S. Jenkins et al., 91–105, AGU Washington, DC, 2004.
- 210 Poulsen, C. J.: Absence of a runaway ice-albedo feedback in the Neoproterozoic, *Geology*, 31(6), 473–476, 2003.
- Poulsen, C. J. and Jacob, R. L.: Factors that inhibit snowball Earth simulation, *Paleoceanography*, 19(4), PA4021, doi:10.1029/2004PA001056, 2004.
- 215 Poulsen, C. J., Jacob, R. L., Pierrehumbert, R. T., and Huynh, T. T.: Testing paleogeographic controls on a Neoproterozoic snowball Earth, *Geophys. Res. Lett.*, 29(11), 1515, doi:10.1029/2001GL014352, 2002.
- Poulsen, C. J., Pierrehumbert, R. T., and Jacob, R. L.: Impact of ocean dynamics on the simulation of the Neoproterozoic “snowball Earth”, *Geophys. Res. Lett.*, 28(8), 1575–1578, doi:10.1029/2000GL012058, 2001.
- Voigt, A.: Snowball Earth – Initiation and Hadley Cell Dynamics, *Reports on Earth System Science*, No. 220 83/2010, 129 pp., ISSN 1614-1199, Max Planck Institute for Meteorology Hamburg, Germany, 2010.