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Climate of the Past
Discussions

Interactive comment on "The initiation of Neoproterozoic "snowball" climates in CCSM3: the influence of paleo-continental configuration" *by* Y. Liu et al.

A. Voigt (Referee)

aiko.voigt@lmd.jussieu.fr

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The paper describes a series of simulations with the atmosphere-ocean general circulation model (AOGCM) CCSM3 to study the initiation of Snowball Earth events using two different paleo-continents. Because only relatively few AOGCM studies have been performed on this topic so far, the paper could substantially add to the scientific debate, in particular because it analyses the effect of the continental distribution and aerosols. Personally, I find the results that low-latitude continents warm the climate particularly interesting as it is opposite to what I have found in the AOGCM ECHAM5/MPI-OM. However, I recommend that the presentation and some parts of the analysis should be substantially improved before publication. At several places, the authors make claims that need to be made more substantial. I therefore recommend major revision.

Main comments:

1. I generally like the introduction but was surprised that the authors did not mention the Jormungand hypothesis. The Jormungand hypothesis is complementary to the hard Snowball, soft Snowball and thin-ice hypothesis and would allow to explain at the same time the CO2 hystersis and the survival of life (Abbot et al., 2012). The Jormungand hypothesis clearly must be mentioned in the introduction.

2. The authors argue that four factors explain why the configuration with low-latitude continents is always colder than the one with southern hemisphere continents. I have to admit that I am not entirely convinced by their arguments, apart maybe from the argument of stronger sea-ice dynamics in the case with southern hemisphere (SH) land (570 Ma). For example, the low heat capacity of land explains the colder *winter* temperatures in the SH land case, but this alone would not necessarily lead to colder *annual* temperatures as the land gets warmer in summer for the same reason. Notwithstanding non-linear rectification effects, the heat capacity alone should have no effect on the annual temperature. The authors do not discuss such non-linear effects but use the winter temperature to argue about the annual temperature (page 3636, starting at line 10). Regarding the albedo difference of sea ice vs. land: this argument only holds if the sea ice is bare, but I would expect it to be covered by snow if it has not entered the subtropics, which it does not in the present study (e.g., Voigt& Abbot, 2012). In the case of the cloud forcing: the cloud forcing can be misleading as it is correlated with the surface albedo. If there is land in the tropics, the diagnosed cloud forcing will be smaller because of the higher surface albedo. This seems to explain the less negative tropical cloud forcing in 720 Ma in Fig. 12. For the sea-ice argument, it might be worthwhile to mention that the stronger NH sea-ice transport in 570 Ma is consistent with the stronger NH Hadley cell because one expects a stronger Hadley cell to transport more sea-ice towards the equator as described in Voigt& Abbot (2012) (see their Fig. 13).

The authors should reconsider these factors, and better explain them if they believe they explain the effect of the continents.

3. I strongly recommend that the authors apply the diagnostic energy balance model developped by Heinemann et al. (2010) and used in Voigt et al. (2011) to decompose the temperature difference between 720 Ma and 570 Ma into contributions from the clear-sky albedo, all-sky albedo, clear-sky emissivity, all-sky emissivity and heat transports. The EBM only needs time-mean zonal-mean top-of-atmosphere radiative fluxes, which are standard output of climate models. The EBM is easy to use and quantifies (in deg K) how much of the temperature difference is caused by these individual factors. This would substantially improve the manuscript and make it more convincing.

4. The way the authors refer to the different continental configurations is confusing. In the abstract, they refer to the 720 and 630 Ma continents, but later use the label "570 Ma" for the 630 Ma continents. This should be changed. Why not only talk of the 720 Ma and 570 Ma continents? They never actually use 630 Ma continents for reasons they give on page 3623.

5. The absence of soft Snowball states in this model is only mentioned in passing but clearly is one of the most important results of the paper and needs to be discussed properly. Previous studies (Yang et al., 2012a and 2012b) with CCSM3 (i.e., the same model as used here) found soft Snowball states when using modern continents. Why are they missing in the present study? The abstract must clearly point out the absence of soft Snowball states as they are intensely debated in the literature. The authors should also give the critical values of the sea-ice cover as the Snowball bifurction point is characterized by both the critical CO2 and sea-ie cover.

6. Model description: A major part of the paper is concerned with the description of the model setup. While this is clearly warranted in a study that applies a climate model to deep climate questions, I found that the description often is too much focused on CCSM3. For expample, the soil color is described in terms of a scale from 1-8 (page

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3624). For people that are not using CCSM3, this information is not helpful. Instead, it would be desirable to characterize the soil in terms of physical quantities such as albedo, roughness length, and water holding capacity. These values, however, are not given. On a similar thread, it is not clear to me what the real meaning of tauback is. Is it the global-mean total optical depth of atmospheric aerosols? If so, please name it like this and use the variable τ. What are the scattering and absorbing properties of the aerosol, and what is its asymmetry factor? The absorption and scattering properties are clearly important to how aerosols affect the gloabl temperature and hence Snowball initiation. For example, if the aerosol is mainly scattering, then increasing its atmospheric loading clearly makess Snowball initiation easier because it cools the climate. If the aerosol is mainly absorbing, this issue is less obvious. Since the effect of aerosols on the Snowball initiation forms a major result of the paper, these aerosol properties should be given.

7. Similarly, I felt that the discussion of how the authors stabilize the ocean model is very distracting and technical. The authors spend more than wo pages (page 3624-3627) discussing this issue and further analyze how the control runs change depending on whether the RIDGE or DIFFUSION method is applied. Later in the paper, when they discuss the Snowball bifurcation point, the authors argue that the choice of RIDGE vs. DIFFUSION should not affect the bifurcation point (page 3632, line 25). But then why do they spend so much time on RIDGE vs. DIFFUSION? I recommend removing the corresponding paragraphs. Another possibility would be to only show results from the DIFFUSION method in the main part of the paper, and move the comparison to RIDGE to an appendix.

8. The authors talk about low clouds on page 3638 and that they expect the effect of an increase low clouds near the ice edge to be robust across climate models. They cite Voigt&Marotzke (2010) but we did not make such a statement in Voigt&Marotzke (2010); this citation needs to be removed. Also, low clouds are the hardest clouds to model in global climate change projections (e.g., Bony and Dufresne, 2005), so why

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should they be be robust in deep climate states for which the models have neither been developped nor tested against observations (which for obvious reasons do not exist) or large-eddy simulations? The authors also give no explanation of the increased low cloud coverage. I recommend removing the entire paragraph.

9. Global-mean surface albedo of the two paleo-continents: Do the 720 Ma and 570 Ma continents have the same global-mean surface albedo (neglecting snow and sea ice, and not weighting the surface albedo by incoming shortwave irradiance when computing the global-mean)? I am asking because an important aspect in Voigt et al. (2011) was that the global mean surface albedo (not weighted by solar insolation and assuming zero snow and sea-ice cover), was the same in their present-day and Neoproterozoic setup. This allowed Voigt et al. (2011) to investigate how a shfit of high surface albedo regions (i.e., continents) from high- to low-latitudes affects the climate. As described by Voigt et al. (2011), the shift leads to a cooling because the high surface albedo more incoming shortwave irradiance in low latitudes and hence leads to an increased reflection (in terms of Wm^{-2}). In the present study, the two land maps might have a different global-mean surface albedo. This might contribute to the difference in their climates.

Minor:

Abstract, line 8: What do the authors mean by "most recent continential configuration"? Do they mean the 630 Ma (or better the 570 Ma) reconstruction. Please rephrase.

Abstract, line 24: I might be worthwhile to add that the cooling due to the absence of vegetation is due to an increase in the surface albedo.

Page 3623, line 5: The use of the word "confirm" suggests that the authors find soft Snowball states. But indeed, they do not (see one of my main comments). Please rephrase.

Page 3623, line 21: How do the authors arrive at the estimate of 0.5 deg C? Is this

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a lapse-rate argument, or did they actually run their GCM with different elevations? Please clarify.

Page 3619, line 15-16: Add Voigt&Abbot (2012) to the list of AOGCM studies that determined the Snowball bifurcation point.

Page 3621, line 20: It would be helpful to more clearly mention the following: Voigt et al. (2011) and Voigt&Abbot (2012) called their continents Marinoan, but the continents used by them are more similar to the Sturtian continents (720 Ma) of the present study. This migth avoid confusion.

Page 3624, line 26: Please give values for CH4 and N2O here. I couldn't find them above.

Page 3627, section 3: The sudden switch to modern continents was confusing. I do not understand why these sensitvity runs were not performed with one of the paleo-continents.

Table 1, Fig. 2: I suggest that the results of Fig. 2 are included in Table 1 by adding a column with the final surface temperature. I found it very hard to read Fig. 2 using the labels run 1, 2 etc. Fig. 2 could then be removed.

Page 3633, lines 1-7: The role of the tropical wind-driven ocean circulation in working against Snowball initiation has been discussed in Poulsen Jacob, Voigt&,Abbot (2012), and Yang et al. (2012a,b). Maybe these studies could be mentioned here?

Page 3636, lines 5-7: The sentence seems to be broken.

Page 3636, line 17: Does a negative sensible heat flux means more flux into or out of the surface? I do not know which sign convention is used.

Page 3641, lines 1-5: The authors argue about the different magnitudes of their four factors, but they give no justification why one factor should be strogner than the other. I again recommend the use of the EBM (see above), which allows a straight-forward quantification.

Page 3641, lines 6-19: I recommend moving this paragraph into the introduction. CO2-feedbacks have not at all been adressed in the paper, so I do not think they should deserve an extended and prominent discussion in the conclusion.

Table 1: Run 14 and 15 are not presented in Fig. 2. Why? Also please give surface albedo instead of soil color and and soil texture. These seem to be CCSM3-specific variables and are of limited use to the reader.

Figs.,5,6: Are these figures needed? See my main comment on the RIDGE vs. DIFFUSION issue.

Fig. 7.: It might be worthwhile to use the same colors for the same CO2 value in panels a and b.

Fig. 8: Why does the 110 ppmv run in panel a has a dip at year 14450?

Fig. 10: I would prefer plotting sea-ice fraction on the y-axis, and to reverse the direction of the x-axis (lowest CO2-values on the left).

Fig. 11 and 12: The colors of the 720Ma and 570Ma runs seem to be reversed with respect to Fig. 10.

Fig. 13: Include sea-ice margin in plot. Are these RIDGE or DIFFUSION simulations?

In general, I do not like captions like "similar to Fig. X". They forces me to go back and forth within the paper, and make the paper hearder to read. Every caption should contain the information to read a figure independently from the other figures.

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