Reply to Reviewer's Comment

Main changes made in this revision:

- **Moved most of the original section 4 and the original Figs. 1, 4 5 and 6 to Appendix A**
- **Completely re-written the discussion (new section 5)**
- **Substantially changed the original Fig. 13 (new Fig. 12) and added Four new figure (Figs. 1, 9, 11 and 13).**

Response to Referee #1 (A. Voigt):

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 CO_2 hystersis and the survival of life (Abbot et al., 2012). The Jormungand hypothesis clearly must be mentioned in the introduction.

The Jormungand hypothesis was proposed in Abbot et al. (2011) and we agree that it should be mentioned. However, we do not think it can be considered as a complementary hypothesis to the hard snowball, soft snowball and thin-ice hypotheses unless it can be confirmed to exist in a fully coupled AOGCM. This has yet to be demonstrated.

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).

The new analyses performed with the 1-D EBM has enabled us to explain more clearly the nature of the influences which determine the annual mean surface temperature.

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).

The reason for the difference between snow covered land and snow covered sea ice is explained more clearly in the revised manuscript (see the new fig. 11). The major reason is that sea ice cover (and the depth of snow upon it) has a more significant seasonal variation than that of snow on land in the mid- to high latitudes (>40°S).

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.

The low-cloud coverage and forcing are explained more fully in the revised manuscript. The low-cloud coverage is clearly shown in the new Fig. 12c to be much lower over land than over ocean, and this is consistent with observations for present day climate (see Figure 1 below. A further reference is also provided in the paper.). Therefore we expect the argument, that there will be enhanced negative cloud forcing in the vicinity of the ice edges if there is more ocean in the region, to be robust although the exact magnitude may vary considerably among GCMs.

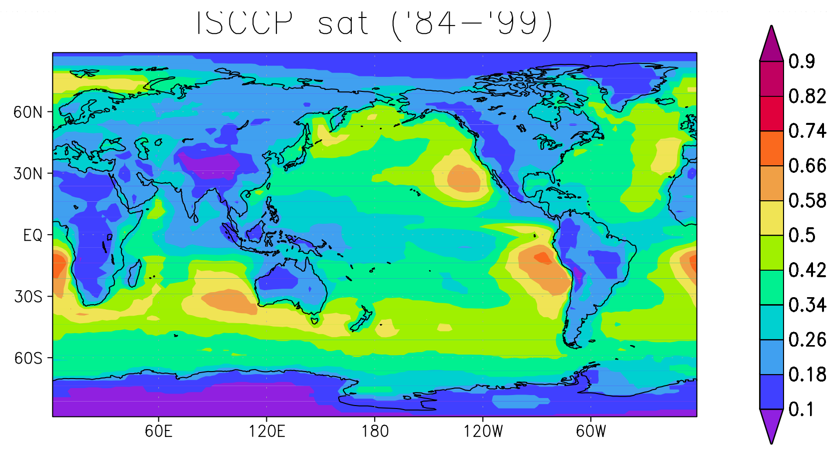

Figure 1 Satellite observed low-cloud coverage from 1984-1999, plotted from ISCCP data sets. Thanks to M. Zhao at GFDL, Princeton, NJ for providing the figure.

For the sea-ice argument, it might be worth-while mentioning that the stronger NH sea-ice transport in the 570 Ma model 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 sea ice edges are not really within the influence of the Hadley cell, and our new analyses of the forcing on sea ice (see wind stress in new Fig. 13) confirms that the Hadley circulation doesn't have any significant influence on the sea-ice transport. The analysis, however, supports the idea that the very strong eastward ocean/sea ice current in the 570 Ma continental configuration strongly promotes the sea ice transport towards the equator due to the action of Coriolis force (Ekman flow).

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 trans- ports. 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.

The 1-D EBM has been fully applied in the revised version of the manuscript and the results obtained are shown in the new Fig. 9.

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.

The description has been modified so as to enhance the clarity of the discussion.

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.

We don't think it appropriate to more fully discuss the existence of soft snowball states in the context of this study as we don't have land ice sheets in the model. A further study similar to that of Hyde et al. (2000) will need to be completed in order to comment more fully on this issue. This is underway and will be reported upon elsewhere

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?

Again we don't think either the work of Yang et al. (2012a, b) or our current work contains sufficient model ingredients to answer the question concerning the existence of soft snowball Earth solutions. A soft snowball Earth has always been, in our opinion, considered to be a state in which most of the continents are covered by ice sheets and most of the oceans probably covered by sea ice but the tropical oceans remain ice free, since the motivation of this hypothesis is to explain the observation of low-latitude glaciogenic deposites at sea level without requiring the ocean to completely glaciated. Such states have never been defined in terms of the fraction of the ocean that should be covered by sea ice in a soft snowball Earth. In particular such states do not require that a stable sea ice front exist within 25-39 degrees of the equator.

The abstract must clearly point out the absence of soft Snowball states as they are intensely debated in the literature.

No. See above.

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.

These values are included in the abstract, but as explained above we do not think they are sufficient to establish the existence or non-existence of soft snowball Earth

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 (page3624). 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.

Since soil color only determines the soil albedo and the properties associated with soil color 4 have been described on the same page (p3624 of the original manuscript), we think the description of soil color should be sufficient.

The roughness length of soil is not altered by either soil color or soil texture in CCSM3, therefore it is not described in the paper. However, the determination of three roughness lengths (momentum, sensible heat flux and water vapor) of soil is described in detail in (Oleson et al., 2004, on page 58) which we have referenced.

The change of soil texture/type affects many properties of the soils such as porocity, hydraulic conductivity, thermal conductivity and heat capacity etc. We think the readers should refer to the technical note describing the Community Land Model ((Oleson et al., 2004) since these properties are all functions of the composition of the soils which were already provided in the text, but we have added this reference in additional points where it's needed in the revised manuscript.

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 global temperature and hence Snowball initiation. For example, if the aerosol is mainly scattering, then increasing its atmospheric loading clearly makes 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.

Yes, it is the total optical depth of atmospheric aerosols (which is just sulfate aerosol in this case), and it is the same everywhere. We have used 'τ' in the revised manuscript following the reviewer's suggestion. The single scattering albedo and asymmetry factor are functions of the wavelength of the solar radiation, and can be seen from Fig. 3 of (Kiehl and Briegleb, 1993). It has almost zero absorption within the spectal band 0.3-1.1 μm, so it has a cooling effect when the concentration increases (through increasing the optical depth in the model). Both this reference and further discussion of the properties of the aerosol have now been added to the revised manuscript.

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 two 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.

We have provided this information for completeness but the reviewer's suggestion of moving it to the appendix has been followed in revising the manuscript.

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.

It was our inference that such a statement was implied in Voigt and Marotzke (2010) but we stand corrected and have removed the reference.

Also, low clouds are the hardest clouds to model in global climate change projections (e.g., Bony and Dufresne, 2005), so why 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.

We continue to consider the feature of low-cloud coverage (and the sign of the associated cloud forcing) described in the text to be robust, although we of course agree that the magnitude of the forcing is poorly modeled by current GCMs. Please see our response to the reviewer's question #2 above.

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 Neo- proterozoic 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 sur- face 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.

The area of the 570 Ma and 720 Ma continental configurations we adopted are ~130

and ~110 million km^2, respectively (see Liu and Peltier, 2010; 2011 for a more detailed description of the two reconstructions), so they do not have the same global-mean surface albedo. However, we don't think this will matter in our model settings as the land surface albedo is lowered dramatically due to its water content (wetness) so that the surface albedo of bare land is much less important than that of clouds. This is demonstrated by comparing the zonal mean surface albedo and planetary albedo shown in Figs. 9b and 9c, respectively, in the revised manuscript. This might be different in other GCMs.

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.

Yes, we meant the 570 Ma reconstruction and this sentence has now been rephrased.

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.

Yes, this has now been added

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.

We have rephrased these comments but the referee appears to be employing a definition of a soft snowball state that differs from our own. See above.

Page 3623, line 21: How do the authors arrive at the estimate of 0.5 deg C? Is this a lapse-rate argument, or did they actually run their GCM with different elevations? Please clarify.

We carried out simulations with both 100 m and 400 m continental elevations for the 570 Ma continental configuration at the beginning of this project. The time series of global mean surface temperature for the two simulations are shown in Figure 2 immediately below.

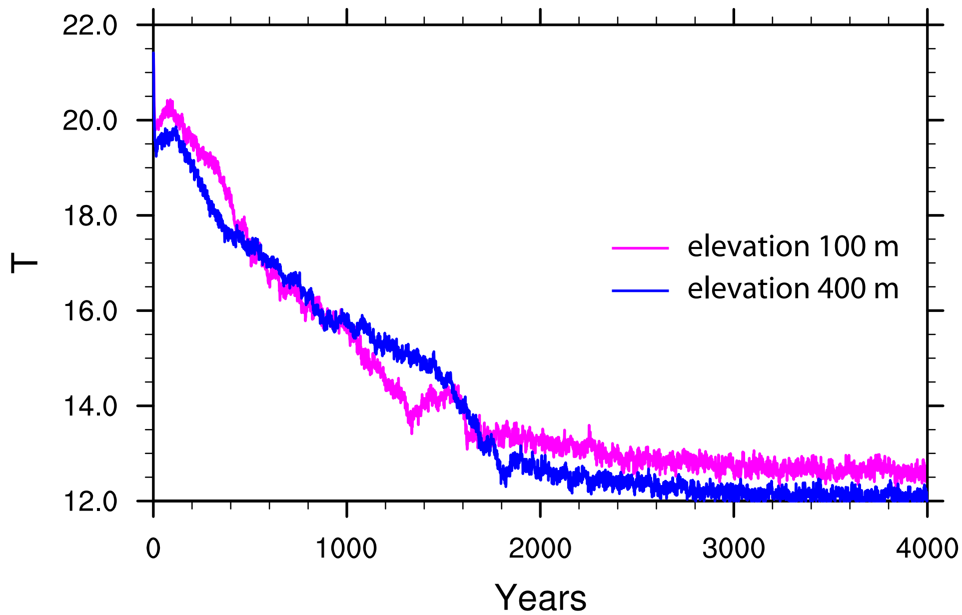

Figure 2

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

This was in the original sentence but we have made certain that this is the case as there was no intention to exclude this reference..

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.

Yes, the sentence has been modified to eliminate confusion.

Page 3624, line 26: Please give values for CH_4 and N_2O here. I couldn't find them above.

Right, the values have now been added.

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.

Further discussion has now been added to the text. The present-day continents are

used because they are host to more realistic vegetation, soil types, ozone, CFCs etc. It doesn't make much sense for us to attempt to create these boundary conditions "realistically" for 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.

We have now added the values for mean surface temperature over the final 10 years of the simulations in Fig. 2 to Table 1, but we would like to keep the figure as it shows the timescale of the global mean temperature evolution which is useful. However, we have modified the legend of the figure believing that this will aid in understanding the figure contents.

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?(Voigt and Abbot, 2012)

These references are added elsewhere to the revised manuscript.

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

The whole paragraph has now been rewritten

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.

Here the negative sensible heat flux means that the atmosphere is losing heat to Earth's surface. These discussions have been completely eliminated from the revised manuscript

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.

We have employed the EBM to analyze the results and removed the assertion concerning their absolute magnitudes as these may vary among GCMs

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.

Moved to the introduction section following the reviewer's suggestion.

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.

Showing results for runs 14 and 15 was not necessary since the same parameter as that in Runs 13 and 16 was being varied. We have now chosen to remove runs 14 and 15 from Table 1 and Figure 2, and to rename the original run 16 run 14 in the revised manuscript.

Soil color is directly related to the surface albedo, while the soil texture is the composition of the soil which determines the heat capacity, thermal conductivity and porosity etc. of the soil. The default soil color as well as soil texture in the CCSM3 model is geographically variable, we don't think it's very useful to provide the detailed values in the table. The point of Table 1 and Figure 2 is to demonstrate in a simple manner that our choice of soil color and texture doesn't change significantly the mean properties of the present-day bare land. The word "soil texture" is somewhat equivalent to "soil type" and we have added that in the revised manuscript

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

Moved to Appendix A

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

Yes, the figures are modified.

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

The reason is unclear. We had repeated the simulation before the paper was submitted and the dip was reproduced. So it was not due to a restart error (the simulation is resubmitted every 20 model years until it is complete). We did not analyze the reason for the dip because we thought it more likely due to temporary numerical instability.

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).

The sea-ice fraction is also included in this revised figure.

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

The colors are now consistent.

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

In the main text of the revised manuscript, all the results are for the DIF method except for Fig. 2 in which results for both methods are shown.

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 independenly from the other figures.

All figures now have independent captions except Fig. 4 which is similar and close enough in position to Fig. 3 to warrant use of a simplified caption.

References:

Abbot, D. S., A. Voigt, and D. Koll, 2011: The Jormungand Global Climate State and Implications for Neoproterozoic Glaciations, Journal of Geophysical Research - Atmospheres, 116, D18103, doi: 10.1029/2011JD015927.

Response to Referee #2 (Anonymous):

The puzzle represented by the Neoproterozoic global glaciations continues to capture the interest of researchers a decade and a half after the term “snowball Earth” was popularized by Hoffman et al. (1998), and rightly so. The descent into extreme cold climate, followed by recovery to more clement conditions via unknown processes, stretches our understanding of how Earth’s climate system ought to operate, at a time that is also tantalizingly close to the first appearance of advanced multicellular life, leading to speculation on the role of climate driving evolution. However, quite a few snowball modeling efforts over the years have focused more on trying to model the idea of what Hoffman et al. envisioned, i.e. the total glaciation of the “hard snowball” scenario, rather than trying to capture reality to the best of our ability to reproduce. The result has been some provocative theoretical work which is interesting, but doesn’t advance well the geology or paleobiology communities’ understanding of what may have actually occurred.

Thanks.

This reviewer was therefore pleased to see Liu et al. explore the parameter space of continental configuration and associated 3D ocean circulation patterns from a more realistic perspective. The choices made regarding land surface characteristics, etc. are reasonable, the rationale is clearly laid out, and the consequences of those choices and the subsequent results are discussed in a thorough manner.

My main criticism of the paper centers on the glossing over of the choice of continental configurations, specifically the 570 Ma configuration for the Gaskiers glaciation, because in doing so I think the authors are missing an opportunity to highlight still more strangeness regarding Neoproterzoic climates. I understand that by going with the 720 Ma (Sturtian) and 570 Ma (Gaskiers) reconstructions, the authors gain some variety in land distribution that using a 635 Ma (Marinoan) reconstruction wouldn't provide. But the Gaskiers glaciation is emphatically not a Marinoan event, falling as it does in the Ediacaran period (635-542 Ma), and moreover, it was not a snowball glaciation. Thus the exercise of trying to identify a snowball bifurcation point for the 570 Ma time period doesn't really make sense in terms of the goal of increased modeling realism, even though the continental reconstruction might make for an easier comparison to earlier work by the authors.

Since the reviewer has apparently noticed the advantages of employing the 570 Ma continental configuration listed in the paper, we need not repeat them here. It is being employed solely in order to have a more polar continental distribution as a point of constrast with the Sturtian model, the best available Marinoan model being very close to that for the Sturtian event. The authors understand that the Gaskiers glaciation was most probably not as global as was the Marinoan. We take the present work to be a point of departure towards a more sophisticated modeling study in which realistic land ice sheets are included and this further complexity will be described elsewhere in future.

That the CCSM3 as configured actually generates a colder world for the 570 Ma non-snowball glacial event vs. the 720 Ma snowball event is a really interesting outcome, however, and deserves a rather more extensive discussion as to why the model results don't appear to be lining up with the geologic record. It may be the result of the experiment configuration, or perhaps something in the way the model is parameterized; maybe the authors themselves are uncertain as to the source of the discrepancy. But in this reviewer's opinion, it would be more useful to the paleoclimate community at large to discuss the model/data split, and provide some informed speculation and specific suggestions for further exploration, than it would be to simply report another set of bifurcation points. In this way, the authors would be helping to foster more conversation between the data world and the modeling world.

The results from 570 Ma continental configuration may not be compared directly with the geologic record because in reality, the solar luminosity should have been much larger at 570 Ma than at 720 Ma (by ~1%according to (Gough, 1981), which is equivalent to ~3.5 W m^{-2} in terms of global mean). This difference is more than enough to make the 570 Ma climate warmer than the 720 Ma climate at the same *p*CO_2.

A couple of minor points:

Fig. 4 is referenced re the continental configurations used once before Fig. 1, and twice before Figs. 2 and 3; perhaps these can be re-ordered, or a new Fig. 1 created indicating just the continental/bathymetric features used.

A new Fig. 1 has been produced as suggested by the reviewer.

Run 11 as shown in Fig 2 does not appear to have achieved equilibrium at the end of 2000 years. – do the authors know how much more run time is required for that run to come into equilibrium?

Run 11 was not run to equilibrium because it was already sufficient to demonstrate how cold the climate can be relative to run 10 where the only difference is the parameterization of aerosols. According to our experience, the run will reach equilibrium after another 1000 years. Since the temperature of run 11 is decreasing by only ~2 °C/kyr at the end of 2000 model years, we expect the global mean surface temperature will further decrease by less than 2°C at equilibrium.

Added Reference

Gough, D. O.: Solar Interior Structure and Luminosity Variations, Sol Phys, 74, 21-34, 10.1007/Bf00151270, 1981.

Hyde, W. T., Crowley, T. J., Baum, S. K., and Peltier, W. R.: Neoproterozoic 'snowball Earth' simulations with a coupled climate/ice-sheet model, Nature, 405, 425-429, 2000.

Kiehl, J. T., and Briegleb, B. P.: The Relative Roles of Sulfate Aerosols and Greenhouse Gases in Climate Forcing, Science, 260, 311-314, Doi 10.1126/Science.260.5106.311, 1993.

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, 10.1029/2009jd013082, 2010.

Liu, Y., and Peltier, W. R.: A carbon cycle coupled climate model of Neoproterozoic glaciation: Explicit carbon cycle with stochastic perturbations, J Geophys Res-Atmos, 116, 10.1029/2010jd015128, 2011.

Oleson, K. W., Dai, Y., Bonan, G., Bosilovich, M., Dickinson, R. E., Dirmeyer, P., Hoffman, F., Houser, P., Levis, S., Niu, G.-Y., Thornton, P., Vertenstein, M., Yang, Z.-L., and Zeng, X.: Technical description of the Community Land Model (CLM), NCAR Technical Note NCAR/TN-495+STR, 174, 10.5065/D6N877R0, 2004.

Voigt, A., and Abbot, D. S.: Sea-ice dynamics strongly promote Snowball Earth initiation and destabilize tropical sea-ice margins, Clim Past, 8, 2079-2092, 10.5194/Cp-8-2079-2012, 2012.