1 Reply to the second referee's comment

Please find our replies to the referee's comments below the quotes from the review.

> *This manuscript highlights the impact of the albedo and heat transport on the early Earth climate for different pCO2 (0.4, 0.6, and 0.8bar). The sections 3.5 to 4.1 are clearly the most interesting parts of the manuscript and are potentially important to understand the effects of the ocean dynamic on the early Earth climate (faster rotation rate, a reduced continental crust, ...). This study demonstrates that in warmer conditions, a supposed characteristic of the early Earth climate, the meridional heat transport tends to decrease, the oceanic overturning being partially governed by the absence of continents.*

While the advantage of applying the ocean GCM is most obvious in Sect. 3.4 to 4.1 of the paper, we strongly disagree with the statement that the other parts are of less interest. The scope of the paper is to provide a comprehensive picture of the simulated early Archean climate states (and, of course, additionally providing the evaluation of the uncertainties as well as the suggested parameterisation of the albedo for 1d-models), and we would like to keep this scope instead of primarily focussing on the two individual parts of the paper named by the referee. Also, keep in mind that the more complex ocean model affects the atmospheric properties (e.g., via its impact on sea-ice growth). However, we will extend the section on ocean dynamics (see our reply to the third reviewer below).

> *If this paper has the potential to become a valuable contribution, this manuscript will require several major improvements before it is ready for publication. The main issues are:*
>
> *Scientific interest*
>
> *The scientific relevance of this paper must be defined more clearly.*

See our comment above.
We believe that the scientific scope is well defined in the Introduction (see p.527, l.15-17, and the last paragraph).

> *Moreover, results described in the section 3 are partially published in Kienert et al. (2012).*

This comment by the referee is unjustified and misleading. We clearly cite the information which is already published ('As found in Kienert et al. (2012) ...', p.536, from l.19) and it is necessary to present it for a proper understanding of the paper. We must explain the relevance of the pCO_2 of 0.4, 0.6 and 0.8 bar so that it is clear why the climate states with these CO_2 partial pressures are chosen for the investigation in this paper. The fact that the partial pressures are higher than previously estimated as well as the small difference between the pCO_2 required for the critical state and the 288K-state are an important motivation for a closer look at the underlying origins – which include the details of albedo and heat transport in Section 3.
While it increases the readability of the paper to have this information at the beginning of Section 3, we will move it to the Introduction and refer to it only as shortly as possible at its current place.

> *To improve the paper, a very significant rewriting is necessary. The paper should be focused on the ocean dynamic (unfortunately too shortly discussed in the present version of the manuscript).*

See our comment above. As written, we will extend the section on ocean dynamics, but we disagree with the reviewer regarding what the focus of the paper should be.

> *A second problem concerns the systematic comparison with the preindustrial climate. The comparison with preindustrial climate does not appear, in most of the sections, relevant to decipher the effects of the albedo, topography, CO2, or of the faster rotation rate. Indeed boundary conditions, between a pre-industrial run and an early Archean run, are clearly too different to be easily compared.*

In some of the cases (e.g., latitudinal profile of surface albedo, Sect. 3.1.1) the comparison with the present-day (PD) climate clearly allows to conclude the main origin of the differences. However, the referee is obviously right that in other cases the differences to the present-day state cannot easily be explained as they are caused by a complex interplay of several differences in boundary conditions.
While we do aim at presenting the characteristics of the simulated early Archean states, it is not our aim to explain the origins of the differences to the PD climate system for all climate variables (which might also be confusing and distract from the main objective). Nevertheless, we believe that it is valuable to include the PD state in the figures (and to give some comparison) since it improves the intuitive understanding of the properties presented for the early Archean.

To explore the potential impact of each factor, sensitivity experiments should be conducted by replacing one element of the reference run (ex : EA, pCO2=0.6bar). Hence, the new version of the manuscript should contain sensitivity runs showing (1) the ocean dynamic behavior as a function of the Earths rotation and (2) the oceanic dynamic behavior as a function of the topography. This addition is needed because major results concerning the effects of the CO2 are already published in Kienert et al. 2012.

Please see our answer above regarding the reviewer's misleading and unjustified comment on the results that have been published before in our GRL-paper and to which we refer.
We will provide (in Sect. 3.5) a figure comparing ocean meridional heat transport for the following two pairs of simulations which all have pCO_2=0.6 bar and early Archean solar luminosity: (1) 'present-day topography and rotation rate' compared to 'early Archean topography and present-day rotation rate' in order to demonstrate the impact of the topography. (2) 'early Archean topography and present-day rotation rate' compared to 'early Archean topography and rotation rate' in order to demonstrate the impact of the rotation rate.

Presentation of the manuscript

The manuscript is well written but not easy to follow because the text is not well organized. Indeed the section 3 associates results along with discussion. That disrupts both the reading and the argumentation (ex: section 3.1.2). Results and discussion must be separated in distinct sections. A section about results must clearly announce new findings.

In the original version of the manuscript, we have slightly departed from the typical structure in order to present the results as a coherent storyline. We realise that this has caused some confusion. We will rephrase the parts discussing the results and move them into the last Section, then called 'Discussions and Conclusions'. This especially applies to the discussion on lowest stable sea-ice latitudes (Sect. 3.1.2) and the discussion at the end of the section on clouds (Sect. 3.1.3).

I also suggest that the section 2 includes a paragraph and a table in which the set of simulations performed will be clearly presented.

We will include a short overview of the simulations that were performed in a new Subsection 2.3, including a list or table.

> *The literature review is up-to-date albeit some references to previous work (especially Jenkins and al.) must be added (ex: line1 p540). The new version of the manuscript should refer more clearly to these previous studies (i.e Jenkins 1993, Jenkins et al 1993).*

We will add the Jenkins et al. (1993) reference to the paper. Regarding the effect of reduced land fraction, we will further add a reference to Jenkins (1993b) (Global and Planetary Change). However, we do not see why Jenkins et al. (1993) should be cited on p.540, l.1, because no experiments in that paper (or the other papers by Jenkins) seem to have varied the CO_2 concentration (in a sufficient amount of steps) to investigate what the lowest stable sea-ice latitude is.

> *Suggestions to improve the manuscript*
>
> *In addition to my general comments, I make some recommendations that the authors may consider to improve the paper.*
>
> *(1) p527 line 8-10 'A few early studies of the Archean climate (Jenkins, 1993, 1996) have applied 3-dimensional models but were highly simplified (e.g. without a full ocean model).'*
>
> *I disagree with the sentence (notably 'highly simplified'). The authors have to demonstrate why the AGCM used by Jenkins (1993) seems to be simpler than an EMIC (here CLIMBER3).*

When writing 'highly simplified' in our manuscript, we had in mind the very simple treatment of the ocean by 'calculating sea surface temperatures from the surface energy balance for an ocean with no heat capacity' (Jenkins, 1993a) (and thus also the resulting consequences for the atmospheric boundary conditions). This also implies that no sea-ice model is applied, but instead only a parameterisation (under annual mean forcing) of the albedo in dependence of this surface temperature. However, the treatment of the surface albedo (due to sea ice and snow) is fundamental for the application to the faint young Sun problem since it directly enters the radiation balance. Even the differences between a purely thermodynamic sea-ice model (which omits sea-ice dynamics) and a more complete model is likely to be significant (cf. Section 3.1.2). However, it is of course correct that the treatment of atmospheric dynamics in the AGCM used by Jenkins (1993a) is more fundamental than in our model - we apologize for being unclear about this! We will rewrite this part in a more detailed and therefore precise way and delete the general statement 'highly simplified'. When extending the reference to the papers by Jenkins, it must also be noted that only a limited number of simulations were performed in those studies so that no pCO_2 for the critical state, the state with present-day temperature or a just ice-free state were determined. Therefore, no systematic analyses of these states was done. More importantly, Jenkins (1993a) applied a solar constant reduced by only 10% and 15% compared to today's value which does not correspond to the Archean, especially not the early Archean. The lower solar luminosity and higher pCO_2 are crucial aspects in the analysis of the Archean climate (see also our comment below on the comparison of our results with those by Jenkins).

> *(2) At the end of the section 2, please add a table wherein you will present the numerical experiments you performed (in the manuscript a part of simulations performed are described in sections 3.1.1 and 3.1.2, so after the first results).*

As written above, we will add the short Subsection 2.3 with an overview of the simulations.

> *(3) A very useful addition to this work would be to simulate the climate using Jenkins boundary conditions published in 1993 (i.e no land, solar constant=1233W.m-2, and pCO2 = 330ppm and 2640ppmv) to compare climates simulated by CLIMBER and an AGCM (the real-time computing to perform this new set of simulations should be reasonable).*
>
> *p529 lines 16-18 'In total, the model simulates approximately 200 model-years per day of integration on a single CPU, which makes it possible to perform a large number of ensemble simulations until they approach equilibrium after 5000 yr in this study.'*
>
> *However I realize that the authors may not want to undertake this comparison I have outlined. In that case they should better explain the agreements/disagreements with the conclusions of Jenkins concerning the early Earth climate. At the first order, the authors could assume that the CLIMBER run EA, pCO2=0.6bar corresponds to the run SCRC (Jenkins 1993). Do the same with the run EA, pCO2=0.4bar and the run SCR (Jenkins 1993).*

The reviewer suggests to use Jenkins' boundary conditions which, considering a solar constant of 1233 W m^{-2} instead of 1024 W m^{-2}, correspond to a time about 1.2 billion years ago (2 b.y. according to Jenkins, but based on a crude approximation to the evolution of solar luminosity; see Figure 1 in Feulner, 2012) instead of 3.8 billion years, thus long after the end of the Archean eon and even closer to the present-day solar constant! This is also the main reason why the two exemplary CO_2 concentrations applied by Jenkins are lower by more than two orders of magnitude than the pCO_2 applied in our simulations. Therefore, including simulations with those boundary conditions which are so different from the early Archean climate state discussed in our paper would cause confusion rather than give any additional insight.

> *(3a) section 2.2.4. Results in agreement with Jenkins et al. (1993), please refer to this study*

We will include a reference to Jenkins et al. (1993).

> *(3b) fig.5. 8. 10. 11. 16. These results could be compared with Jenkins results (1993)*

For the reason of the different solar constant and pCO_2 mentioned above, we would find it misleading to systematically compare all the results presented in these figures with the results by Jenkins (1993a), treating those as a reference case. However, since the simulations by Jenkins are based on a similar rotation rate ($\Omega = 1.7$ compared to $\Omega = 1.6$) and fraction of emerged surface (0% compared to 1%) as our simulations, it will definitely be an improvement of the manuscript to compare some basic characteristics of the results with those by Jenkins, and we will include such comparison in the revised version of the manuscript.

> *(4) p536, line 19 p538, line 11. Results already published in Kienert et al, 2012. I suggest to rewrite or remove this part.*

We believe that there is a typo by the referee and that he refers to the brief introduction to Section 3 which ends on page 537, line 11. It is short, necessary and properly cited – however, we will move this part to the Introduction. Please see our comment above.

> *(5) p540 – lines 12-18. The authors must explain why the impact of topography on the sea- ice decreases (PD to EA) when the rotation rate increases (table 2)?*
> *(5bis) table 2. Add the clouds response and all boundary conditions used*

Our focus regarding table 2 is on the reduction of the critical sea-ice latitude due to the increase in rotation rate. We will provide an explanation for the effect of the topography change in the revised version of the manuscript and add the boundary conditions to the caption of the table.

> *(6) fig.6 (and fig.19a). The authors must explain how the surface albedo could be higher than the planetary albedo (fig.6 see blue and green curves in high latitudes and fig.19a red, blue lines below 265K)*

This is the normal situation for very high surface albedo and the reason is the absorption of solar radiation by the atmosphere. Imagine, hypothetically, that the surface albedo approaches 1 – there will still be absorption by the atmosphere so that the planetary albedo must have an upper limit. (Keep in mind that about 23% of the incoming short-wave radiation is absorbed by the atmosphere in the present-day climate, so that the planetary albedo cannot exceed 0.77 when those conditions are fixed.) We will put the explanation into the manuscript.

> *minor points*

> *(7) p528 lines 9-12 'In the early Archean, the emerged surface area was much smaller than today (Flament, 2009) so that similarities in the dynamics of the climate system with aquaplanet (an idealised planet fully covered by oceans) states are expected.' The assumption of Flament et al. is not an aquaplanet sensu stricto, this study suggests that continents were mostly flooded until the end of the Archean, which means that the continental crust significantly affects the bathymetry.*

That is true, of course. On the other hand, surface boundary currents would require emerged continental crust, and the surface type is an important aspect for the atmospheric boundary conditions, too. Also keep in mind that we apply only 20% (mainly submerged) continental crust which is randomly scattered in the reference simulation. We will weaken the statement which motivates the comparison with aquaplanet simulations, writing 'some similarities might be expected' and clarifying the aspect of submerged continental crust.

> *(8) fig.9 and lines 2-5 p543. The authors must explain why the same pCO2 are not used for all runs?*

As written on page 543, we wanted to compare the two 'critical states' as well as the two states with a mean temperature of 288 K rather than comparing those with the same pCO_2. We wanted to show the (almost absent) effect of the changes in rotation rate and topography without a possible distortion due to an indirect change going along with a difference in temperature. We will add a clarifying sentence.

> *(9) fig.18. Continents are supposed to have a positive topography. Please correct this point.*

We will correct this.

> *(10) Here is a reference to add to complete the list*
>
> *–Precambrian climate – the effects of land area and Earths rotation rate. Jenkins, GS., Marshall, HG., Kuhn, WR. JGR-atmosphere, volume: 98, issue: D5, pages: 8785- 8791, 1993 DOI: 10.1029/93JD00033*

We will add the reference.

2 Reply to the third referee's comment

Please find our replies to the referee's comments below the quotes of his review.

> *The authors investigate the role of albedo and of heat transport in the context of the Early Archean. First, the manuscript is very poorly organised and major revisions are required before being published, both in terms of treatment and organization of the text. The paper consists in a set of sensitivity experiments to various parameters, which can be either related to the state of the Earth during the Early Archean or related to model parameterisations. In addition, results already published in Kienert et al. (2012) are mixed with new results.*

Please see our reply to the second referee above. The brief description of results from Kienert et al. (2012) to which we refer is properly cited and necessary for the understanding of this paper. We will move it to the Introduction and refer to it in an even shorter way at the beginning of Section 3.

Finally results are shown and discussed in each section. A final discussion is needed. It will help the readers to better understand the role of each factor. In conclusion, I strongly recommend a complete rewriting of this paper before publication.

As written above in response to the second referee, we will provide a 'Discussions and Conclusions' section into which we move the discussions that are so far in Section 3.

Major points:
P527-528: Introduction
A clear review of the state of the art must be done in the introduction.
P527-L13: I do not consider the model CLIMBER as a true 3-D model. The model Climber 3α consists of a 2.5-dimensional statisticaldynamical atmosphere module coupled with a general circulation model for the ocean component (MOM3) (Eby et al., 2012). Thus, I do not believe that the AGCM used by Jenkins' work is a 'highly simplified 3-D models'.

Regarding the reference to Jenkins' study, please see our response to the second referee above who raises the same point. Regarding the strengths and weaknesses of our model for this study, also confer to our reply to D. Abbot's review. When referring to 3d-models, this is not about a distinction between the atmospheric dynamics of an AGCM and parameterised dynamics, but it is about including genuinely 3-dimensional effects as, e.g., the simulation of sea-ice (and thus surface albedo) and meridional heat transport. Please keep in mind that these aspect were not taken into account in the 1-dimensional radiative-convective studies of the faint young Sun problem (see our Introduction).

P528-L10: the area of emerged continental crust is due to both crustal growth and a change in hypsometry.

Correct, as described by us in Section 2.2.1. We will now add the information already at this place in the Introduction.

P529-530: The authors should add a sentence to explain how the values of model parameters were fixed.

These are effective parameters representing basic processes, and the parameters depend in parts on the model and its resolution, so that they must be tuned. They are the same as in the standard version of the model, and they are the result of tuning the simulated climate system to the present-day state. We will clarify this in Section 2.1.

> *P530-L13: The model includes an isothermal stratosphere. This is very surprising because the atmosphere is free of oxygen during the Early Archean. Thus there is no ozone layer.*

We did not include the removal of ozone in our simulations. However, compared to the other effects, we do not expect it to be a major contribution (2 K global SAT increase in the simulations by Jenkins, 1995). We will make a comment regarding the absence of ozone in the revised version.

> *P533: longwave parameterisation*
> *The authors have fixed a parameter 'a' which fits well with the results obtained with the MTCKD parameterisation (section 2.2.2 and fig.3). On fig.3, two other parameterisations are shown (CA and GBKM) which have never been cited either in the main text or in the figure caption.*

The results (in Fig. 3) from both parameterisations are taken from the implementations by Halevy et al. (2009) and are cited accordingly in the caption of Fig. 3. We will add the references to Segura et al. (2007) and Meadows and Crisp (1996); Halevy et al. (2009) follow their approaches. We will further explicitly refer to the GBKM and CA parameterisations in the main text of Section 2.2.2.

> *A set of experiments has been done (section 4.2-P550) to test its impact on CO2- induced greenhouse warming. The explanations seem to confirm what is already known about the importance of the parameter 'a'. The section 4.2 should be transferred into section 2.2.2 or section 2.3.*

We know from the beginning that the radiative transfer scheme is important (Fig. 3), but we do not know the effect on the critical CO_2 partial pressure. This cannot be deduced from Fig. 3 without actually doing the simulations (under early Archean boundary conditions!). So, it is a proper new result and belongs into Section 4.

> *P533 : atmospheric meridional cell strength*
> *The authors adjust the Ci factors using the temperature and velocities fields from aquaplanet simulations (Marshall et al., 2007).*

Marshall et al. (2007) have fixed present day values for CO2 (and orbital parameters). The change in topography influences drastically the Ci factors. How do changes in pCO2 or rotation rate act on Ci parameters?

We would not consider the changes in Table 1 to be 'drastic' in the light of their impact and overall uncertainties. However, the effect of changes in topography is by far the dominant cause for changes in the C_i because they actually parameterise the loss of kinetic energy due to friction. So, we do not expect rotation rate or pCO_2 to have a significant impact on them, but it would of course be desirable to have results from an ocean-atmosphere GCM for comparison. We will add a corresponding comment to Section 2.2.3.

P534-L23: equation (2)
The parameters of the equation (2) must be explicitly described. The authors should explain how the parameter R (or Ω) is influenced by a change in pCO_2? ΔT is the fractional change in potential temperature from equator to pole (Held and Hou, 1980), which varies in function of several parameters (not only rotation rate).

We will explicitly describe the parameters. As one can see from Fig. 5 and Eq. 2, the effect of ΔT is very small for the states close to the 288K-state. While the effect is not insignificant for the critical state, it is still smaller than the impact of the rotation rate on R. The change in the rotation rate (which has the same magnitude for all climate states under investigation) thus has the most important effect on the cell boundaries, and we neglect the dependency on ΔT in our simulations. We will explicitly comment on this in the revised version of the manuscript.

P536: overall impact of technical modifications
The additional experiments that have been done can be explained in this section.

We are not sure which additional experiments are meant by the reviewer.

P537 and p550: Figure 7 shows the surface and planetary albedo. The authors must explain why surface albedo exceeds planetary albedo at high latitudes (in case of sea ice). This remark is also true for the section 5.

This is normal for a very high surface albedo because of atmospheric absorption of short-wave radiation (see our comment above to the same question by the second referee). We will add the explanation to the manuscript.

> *P539: Two sensitivity experiments are performed to decipher the role of rotation rate and topography. Theses results are not discussed further.*

Please see our reply above regarding table 2. Furthermore, we will provide a figure on the changes in heat transport due to (1) the change of topography and (2) the increase in rotation rate which will be based on the results of these experiments.

> *P541: planetary albedo and clouds*
> *The figure 7 must be simplified. It is not useful to show the results for clouds from all CMIP5 models (only refer to the work by Taylor et al., 2012). I suggest to make comparisons between runs (present day and preindustrial) and the observations in the section 2.3 because this only concerns the ability of the model to simulate present day climate.*

We will move Fig. 7 and the corresponding text to Sect. 2. An important part of the message is that clouds are a significant source of uncertainty in all model simulations, also in those based on very complex models. Making that point only in writing might not illustrate the magnitude of the effect to the non-expert. And since the figure comparing the performance of our model with observations is there anyway, there is no cost of providing the comparison with CMIP5-results in addition. So, we would prefer not to remove them from the figure.

> *P545: atmospherics dynamics*
> *Figure 12 represents the mean zonal winds as a function of latitude, not height.*

The maxima are taken with respect to height, and they are shown as a function of latitude. We will rephrase this.

> *P546: ocean dynamics*
> *This section is interesting but must be more detailed. The surface velocities for 3 CO2 levels must be shown. How do the ocean dynamics respond to sea ice (and reciprocally)?*

We will follow the advice and show the surface velocities for the other pCO_2 and thus extend the discussion on ocean dynamics in Sect. 3.4.

> *The authors compare Early Archean and preindustrial runs but these comparisons do not permit to analyse the respective effects of pCO2 and rotation rate on ocean dynamics. This comment is also valuable for heat transport.*

As written in response to reviewer 2 above, we will provide an additional figure in Section 3.5. We will show heat transport (oceanic, but also atmospheric and total) for (1) a state with PD rotation rate and PD topography (but reduced solar luminosity and increased pCO_2), (2) a state with early Archean topography and PD rotation rate as well as (3) the early Archean state.

> *P548 : impact of uncertainties in topography and radiative transfer.*
> *This section must be shortened and discussed earlier in the paper. The impact of topography should be shown before the effect of pCO_2 and rotation rate rather than the opposite. Concerning LWR parameterisation, the authors should discuss this point in section 2.2.2.*

As written above, Section 4.2 on the uncertainty due to the LWR parameterisation contains original modelling results (the changes of the critical partial pressure) and should not be moved to Section 2.2.2. We will more strongly emphasize this result in addition to the conclusion of a 'significant impact'. We also believe that the evaluation of the uncertainties is a significant part of the scientific relevance of this paper, and we would like to keep it bundled at the end of the paper. As written in our response to D. Abbot's review, we would like to extend this Section by a short Subsection 4.3 on the impact of the uncertainty in sea-ice and snow albedo.

> *Minor points:*
> *P528-L27: add a reference*

We will add the reference to Wang and Stone (1980), who have suggested such a parameterisation for near present-day conditions, at this place already.

> *P531-532: the section about topography can be shortened.*

That is a question of the perspective of the reader. The faint young Sun problem is a quite interdisciplinary subject. And those with a stronger focus on crustal development would be probably more interested to see our justification for the area-per-height distribution that is applied. Also, it might be useful for those who plan to do similar studies with different models – when the question arises which topography to apply. We would prefer to keep the details.

> *P537: It seems that orbital parameters have not effect on the symmetry (or asymmetry) of temperature (fig.5). Is it correct or is it due to the scale of the figure 5?*

There is only one potential asymmetry which we believe the referee must have had in mind. Depending on the orientation of the Earth's axis with respect to its orbit and of the eccentricity of the orbit, the summer in one of the hemispheres is in almost all cases longer and milder than in the other one (and vice versa for the winters). While the received solar radiation is identical when integrated over time (due to Kepler's second law), there might be nonlinear reactions of the climate system (e.g., clouds or heat fluxes) which could cause a difference between the hemispheres. However, we can expect such an effect to be much smaller than the one due to asymmetries in our early Archean topography field, and it is not visible in Fig. 5. Asymmetric effects of orbital forcing as known from the Milankovitch cycles strongly depend on the asymmetric topography of the Earth.

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