Please find our reply to the reviewer's general comments below, as well as specific replies to the points that he asks to be addressed in the revised version of the manuscript.

> *Overview: This paper rigorously investigates the CLIMBER climate model in Archean configuration. The paper is fairly well-written, and has relatively few grammatical and typographical errors. I recommend that the paper be published, but only after the limitations of CLIMBER are made more clear to the reader. When simulating a climate vastly different from our own, we should try to use models with as much basic physics and as few empirical parameterizations as possible. Empirical parameterizations are less likely to be valid when used to extrapolate to a very different climate, whereas basic physics should still hold. We know what the equations for atmospheric dynamics are, and can get a fairly good picture of atmospheric behavior when we use them even in a fairly coarse atmospheric GCM. But CLIMBER instead employs empirical relationships for atmospheric dynamics that are unlikely to be valid in different climates. The total number of simulations done here is not excessive (there is no real parameter sweep across uncertain parameters), so I dont understand why a coupled GCM couldnt have been used. We would still have plenty of uncertainty from the parameterized cloud scheme of a GCM, but at least wed have a more realistic picture of the effect of changes in rotation rate and atmospheric pressure on things like the atmospheric circulation and vertical temperature structure. This is critical because the vertical temperature structure determines the radiative forcing you get from adding $CO_2$ and because the surface winds force the ocean circulation. Resolving these dynamical effects is the main reason you would do a 3D study, as opposed to the old 1D radiative convective studies. I am very sympathetic to the idea of using simple models to gain a better qualitative understanding, but its not clear to me that CLIMBER really delivers more qualitative understanding than a coupled GCM does. These limitations of the model are not made clear enough in the current draft of the paper. This is important since many people may read this paper who are not climate dynamicists and might not immediately recognize that CLIMBER is very different from a modern coupled GCM.*
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> *Comments: 1. Issues with Model: As outlined above, CLIMBER is simply not up to the task of accurately calculating changes in*

*atmospheric circulation and lapse rate (and therefore the essential question of radiative transfer for this project) in a vastly different climate from modern. The atmospheric component of CLIMBER is essentially a sophisticated energy balance model that approximates atmospheric heat and moisture transport by eddies as a diffusive process (Petoukhov et al., 2000). An empirical parameterization (Eq. (3) of the paper) must be used to calculate the lapse rate. Since we expect the lapse rate to be driven by convection to the moist adiabait in the tropics, this parameterization can presumably do a reasonable job there. But in the extratropics eddies are critical for determining the lapse rate (e.g., Schneider, 2006), and large inversions will develop over ice in the winter hemisphere (e.g., Pierrehumbert, 2005). The empirical parameterization used here is certain to simulate lapse rates incorrectly in these regimes. This is absolutely critical for the present study because the radiative forcing you get from adding $CO_2$ to the atmosphere is highly dependent on the lapse rate. Another issue is the atmospheric circulation pattern. As described in section 2.2.3, rough parameterizations must be used to calculate atmospheric cell positions and strength. This is important because it will lead to surface winds, which drive the much more sophisticated ocean model. If the atmosphere is doing something screwy, the ocean will be too and cant be trusted. Why are these issues particularly relevant for the present study? First, because the rotation rate is changed, which will clearly affect the dynamics. As the authors note in section 2.2.4, they simply move the cell boundaries to where they think they should be based on some previous work. Given this, its hard to claim that the model has really calculated the effect of changing rotation rate (since a big part of it was really imposed). Furthermore, the effect that changing rotation rate would have on eddy behavior and therefore extratropical lapse rate appears to be completely neglected. A second issue is that the authors change the atmospheric pressure. This will not only affect Raleigh scattering, which the authors do include, but should also tend to increase atmospheric heat transport, all else being equal, and counteract the effects of increased rotation rate on the meridional temperature profile. Such effects cannot be calculated using an empirically based model like CLIMBER.*

We obviously do not claim that CLIMBER's atmosphere is as good as a GCM and we openly discuss the parameterisations involved. But the progress when using a 3d-model compared to 1d-models for studies of the faint young Sun problem (especially, including sea-ice changes and heat transport) in a comprehensive study is the essential point, and the simulations with CLIMBER are thus a significant step forward compared to previous research. It should be kept in mind that earlier modelling studies of the faint young Sun problem with radiative-convective models used a constant planetary albedo, for example. Furthermore, as will be explained below, the parameterisations are more realistic than assumed by the reviewer. However, we definitely encourage simulations with atmospheric GCMs for comparison (to the extent to which they are computationally feasible).

The total number of simulations has actually been very high so that no atmospheric GCM could have been used for this study. The identification of the $CO_2$ partial pressures for the three analysed states was a prerequisite and took about 20 simulations. But also within the study itself, the sensitivity test with respect to the LWR schemes (Sect. 4.2) required more than 30 simulations. Also, 7 additional topographies were tested (Sect. 4.1), and 15 simulations enter the suggested ice-albedo parameterisation for 1d-models (Sect. 5). Furthermore, several dozen simulations were performed for analysing the sensitivity of the critical $CO_2$ partial pressure to sea-ice and snow albedo (see below). All of these simulations were performed for 5000 model years until they approach equilibrium. Such a comprehensive study would have been very difficult with an atmospheric GCM.

Regarding transparency, we think that we have been very clear about the parameterisations required by CLIMBER in contrast to an atmospheric GCM. In Section 2.1, we have introduced CLIMBER's atmosphere as statistical-dynamical, and we have elaborated on it by stating that it '*assumes a linear decrease of temperature in the troposphere with a lapse rate calculated by the model as well as an isothermal stratosphere above*' and that '*instead of explicitly resolving synoptic scale processes, their statistical behavior is modeled*'. The entire Subsections 2.2.3 and 2.2.4 cover details of the lapse-rate and meridional-cell parameterisations and how we adjusted them to be appropriate for the Archean climate. Nevertheless, we will of course follow the reviewer's advice and put additional emphasis on the difference between CLIMBER's atmosphere and a GCM (see below).

The reviewer specifically mentions several deficiencies of CLIMBER's atmosphere, especially with respect to vastly different climates. While a more physical representation of theses properties and processes would of course be desirable, note that temperatures in our simulations are not too different from today's for which the model is validated. They range from a sea-ice boundary at 34°N/S to a state just warm enough to be ice-free. The situation would be different when simulating, e.g., deglaciations of snowball Earth states or a hot Archean climate with temperatures up to 80°C. The large differences in the climate state compared to today with respect to topography and rotation rate have been discussed in the paper, and appropriate model modifications have been applied.

The reviewer especially points out that our parameterisation of the vertical temperature distribution $\Gamma$ in the free atmosphere would incorrectly simulate the lapse rate in the extratropics (because of the role of eddies) as well as over ice due to inversions which occur. However, while the parameters $\Gamma_0$, $\Gamma_1$ and $\Gamma_2$ of our lapse-rate parameterisation are fixed in the original model, we have modified them (Sect. 2.2.4) so that the parameterisation takes into account the changes of eddies with the rotation rate $\Omega$ in a physics-based manner. The introduced dependency on the rotation rate is based on Monin-Obukhov-Kazanski similarity theory (see, e.g., Zilitinkevich, 1969). According to this theory, the vertical turbulent fluxes of wind stress, heat and humidity and the corresponding vertical profiles of horizontal velocities, temperature and water vapor in the upper part of the planetary boundary layer (PBL) and the lowermost part of the free atmosphere are determined by the near-surface friction velocity $u^\star$, temperature $T^\star$ and specific humidity $q^\star$ scales, as well as the Rossby number Ro, the 'roughness parameter' $\mathrm{C_r} = \mathrm{L} \times \mathrm{z}_0^{-1}$ (where L is the Rossby radius for the macroturbulent eddies and $\mathrm{z}_0$ is the effective roughness length for the surface friction) and the temperature and specific humidity vertical stratification parameters in terms of the effective static stability parameter $S_\star$. The latter three dependencies (on Ro, $\mathrm{C_r}$ and $S_\star$) are a consequence of the application of the baroclinic theory to the description of the ensembles of the extratropical macroturbulent eddies. Then, the assumption of a quasi-linear vertical temperature profile throughout the free troposphere leads to the dependency of $\Gamma$ on $\Omega$ given in eq. (4) of our manuscript. Thus, extratropical eddies are implicitly taken into account in our lapse-rate parameterisation. We will point this out in the modified version of the manuscript. We note in this context that even with the original parameterisations of the lapse rate, and the macroturbulent eddy fluxes of heat and humidity described accordingly by eqs. (2), (27)-(29) and (33) in Petoukhov et al. (2000), these variables are actually intimately coupled in our model as $\Gamma$ explicitly enters the coefficients of macroturbulent eddy diffusion in eqs. (27) and (33). This latter process in turn efficiently regulates the longitudinal and meridional distribution of $\mathrm{T_a}$ in the extratropical latitudes, thus affecting $\Gamma$ there according to eq. (2). This provides quite realistic values of $\Gamma$ in the extratropical latitudes for present-day and close-to-present-day climate conditions as shown in Petoukhov et al. (2000) and Ganopolski et al. (1998, 2001).

In the module for the calculation of the vertical sensible and latent heat fluxes at the Earth's surface in our atmosphere model, these fluxes are described applying Hansen et al. (1983) bulk parameterisations. In doing so, the very high values of the negative (downward) sensible heat flux over one or another geographic region – and the accompanying temperature inversion within the bulk (lower and middle part) of the PBL – are automatically accounted for in our model. Such an effect takes place basically in the grid cells with extremely cold surface temperature. This situation is explicitly treated in our model when calculating the surface $T_g$ and near-surface air $T_s$ temperatures, as well as the vertical temperature profile within the bulk of the PBL. As far as these variables enter the formula (25) in Petoukhov et al. (2000) for the calculation of $T_a$, the lapse rate $\Gamma$ responds correspondingly to the above-mentioned effect according to eq. (2). So, temperature inversions over sea ice are actually taken into account in our lapse-rate parameterisation. We do however apply a cap at 3 K/km; but this only affects the coldest stable states and only in a limited latitude range where the latitudinal lapse-rate profile is already quite flat (cf. Fig. 10).

The prescribed shift of the annual mean cell boundaries is numerically justified in Sect. 2.2.4, but we will add a short physical explanation. Note however, that the effect of these changes on the climate system under early Archean boundary conditions is obviously not prescribed, but is part of the results of our simulations.

> *The path to an acceptable publication that I see is to discuss these issues more clearly and frankly, to make sure the reader has no misconceptions about whats been done. Specifically, I think you need to:*
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> *1. Revise the abstract so that you describe CLIMBER a bit, rather than just saying you used a 3D model. I would write something like: We use CLIMBER, an intermediate complexity climate model with a sophisticated energy balance atmosphere and some dynamics based on empirical parameterizations coupled to an ocean GCM.*

We think that the key information in the abstract should be that the model includes the processes neglected in 1d-models (i.e., that it is 3d) and that we adjusted the boundary conditions (including, e.g., differences in topography or rotation rate). We will now additionally refer in the abstract to the simplified atmosphere according to the reviewer's comment. We believe that the term '*sophisticated EBM*' would be misleading because the model explicitly describes the large-scale fields, and the parameterisations involved take into account the macroturbulent eddies. Therefore, we think that the term ‘*statistical-dynamical atmosphere model*’ is more appropriate, but we will also refer explicitly to the fact that parameterisations are used. We will add the following sentence to the abstract behind ‘early Archean climate system’: ‘*In order to do so, we have appropriately modified an intermediate complexity climate model that couples a statistical-dynamical atmosphere model (involving parameterisations of the dynamics) to an ocean general circulation model and a thermodynamic-dynamic sea-ice model.*’

> *2. Stop talking about 3D models like theyre all the same thing. For example, on the last line of page 527 you allude to ECHAM/MPI-OM as if its just another 3D model, when actually it is a coupled ocean-atmosphere global climate model that is in a completely different class from CLIMBER. I think you need to go through your paper critically and remove misleading statements like this.*

When referring to the GCM studies as being 3d, our perspective was that all the models take into account sea ice, an ocean and heat transport which is to be seen in the context of previous research on the faint young Sun problem. For the revised version, we will go through the paper and distinguish explicitly between GCMs and CLIMBER where we refer to GCM results (as in case of ECHAM/MPI-OM referred to by the reviewer).

> *3. Discuss openly the issues I’ve raised above about the atmospheric dynamics and lapse rate in CLIMBER. I think you need to add this to the relevant subsections of section 2 and you need to emphasize this in the conclusions. You particularly need to note in the conclusions that the model requires empirical parameterizations to calculate the lapse rate, and that the radiative forcing associated with an increase in $CO_2$ will depend strongly on these assumptions. I also suggest reiterating the call made by (Feulner, 2012) for the application of state-of-the-art climate models to this problem in the conclusions.*

Subsection 2.2.3: We will add a sentence right at the beginning: ‘*The dynamics of the atmosphere are important for heat transport as well as for driving the ocean circulation.*’

Subsection 2.2.4: It is discussed in detail in this subsection why we prescribe the cell boundaries of 22.5° and 52.5°. Therefore, we think that it is clear that they are prescribed instead of calculated, and we believe that this prescription is reasonable. We will add a short physical explanation for the reduction of the Hadley cell width in case of the higher rotation rate.

Subsection 2.2.4: We will add information on the physical basis of the dependency of the lapse rate on the rotation rate, thus pointing out that and how eddies enter into its parameterisation (see our comment above). We will also note that temperature inversions over sea-ice are taken into account in the parameterisation, but that a 3 K/km cap is applied (see our comment above). We will further add: '*Note that model-simulations with CLIMBER-3α that include the unmodified lapse-rate parameterisation have been validated for the present-day climate (and its seasonal cycle), and they involve a significant range of surface air temperatures from those over Antarctica to those in the equatorial region.*'

Subsection 2.2.5: After commenting on the adjustment of Rayleigh scattering to the differences in total atmospheric pressure we will add: '*..., but deviations in the total atmospheric pressure from the present-day value are neglected in the calculation of atmospheric heat transport.*'

Conclusions: After the first sentence (which states that we have modified a 3d climate model), we will add the following sentence: '*The main model modifications concern the topography, the long wave radiative transfer scheme, the parameterisation of the lapse rate and the prescribed atmospheric meridional cell boundaries. These properties and parameterisations are fundamental for the energy balance, including the greenhouse effect of* $CO_2$*, and for heat redistribution.*'

Conclusions: As the last sentence, we will add: '*It is desirable to compare our results with future studies using different models. Within the limits of computational feasibility, a comparison with simulations relying on more complex climate models which include a state-of-the-art atmospheric GCM would be valuable.*'

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