The authors wish to thank the reviewer for their constructive and insightful comments which have greatly improved the manuscript. We address the comments as detailed in the following text and in the revised manuscript. The reviewers' comments are in bold underline and the normal text is our responses.

"- equation (1): Ls should be replaced by Lf (Latent heat of fusion, described later on).":

This mistake has been amended in the revised manuscript.

"- the description of the Hadley cell circulation is a bit difficult to follow with the use of many intermediate notations for heat fluxes. Maybe a final summary of FT and FQ as functions of Ftotal , FT_eddy and FQ_eddy would be useful (equations 18a, 19a)."

We have redefined Eq. 18 as FT = FT_eddy + FT_HC = FT_eddy [long form defined in Eq. 14] + FT_HC [long form defined in Eq. 16].. and same for FQ in Eq. 19...

"- there is apparently no vegetation on land and the surface albedo is controlled only by snow... I am wondering to what extent this explains some biaises of the model, or if this is negligible when compared to other factors (cloud cover, fresh snow versus ice albedo, ...). More importantly, it is not clearly explained in the manuscript how land evaporation is computed: there is a "surface water availability" parameter in equation (7), but no information is given on what it actually means. A bit more discussion on land cover (or lack of) would be appreciated."

As pointed out by the reviewer, we assume a fixed uniform surface albedo over bare ground (of 0.15), unaffected by different vegetation types. In the EBM of Bintanja (1997) land is divided into three surface types (grass, forest and permanent land ice) – all fixed to present-day distributions. The simplification in ZEMBA, specifying one surface albedo for bare land, could lead to discrepancies in the surface albedo for simulations of the pre-industrial climate, particularly over regions with dense forest cover. Including a present-day vegetation distribution and albedo would improve the PI simulation. However, vegetation feedbacks would not be included unless dynamic vegetation cover is included. Given that our study focuses on the feedbacks due to snow, sea ice and ice cover changes we have chosen to exclude changes in vegetation. In future work, it would be interesting to study the impact of dynamic vegetation changes over the Quaternary. In the revised manuscript [**line 265**], we have added the following text to the "Land Surface Albedo" section of the Methods:

"The assumption of a uniform α_{bg} albedo overlooks the important influence that different vegetation types have on land albedo. In contrast, Bintanja et al., (1997) divides "ice-free" land into present-day distributions of grass and forest cover, though these proportions are held constant over time. Thus, in both approaches, these potentially significant vegetation feedbacks are excluded from Quaternary climate simulations. While including present-day vegetation distribution could improve pre-industrial simulations of ZEMBA, we see limited added value in doing so for studies of orbitally-driven climate change. Nonetheless, we recognize that these simplifications in land albedo may affect the strength of albedo feedbacks over land, which could be explored in future applications of the model."

As for evaporation, we have included information on the surface water availability parameter which is 1.0 over ocean, and 0.7 over land, reflecting reduced water availability over land (as in Bintanja, 1997). The following text has been added to the Turbulent Heat Fluxes section of the Methods [**line 162**]:

"Following Bintanja et al., 1997, W is set to 0.7 and 1.0 over land and ocean, respectively, to reflect reduced water availability over land."

"- Table 1 lists only a small set of the parameters used in the model. It would be useful to have a more extended list…Examples:"

We have included a table of all parameters listed in the text in the appendix.

"line 177: Ta' is Ta corrected with a lapse rate of -6.5K/km. This information is useless if we don't know the height at which Ta' is evaluated."

At the beginning of the results section, we state "zonal mean elevations over land taken from ICE-6G_C" dataset for the LGM and present-day conditions (also outlined in Table 2). In the revised Methods section, when describing the Hydrological Cycle, we have added a note to make this clear [**line 182**]:

"In the current version of the model, this zonal-mean elevation is prescribed (see Section 2.4 and Table 2). In the future, we intend to make the zonal-mean elevation dependent on a coupled ice sheet model."

"Line 309: constant sea ice thickness…"

We have clarified that sea ice thickness is set to 2 m (which has been given a symbol – d_{si}), both in the text and the table of important model parameters. In the revised text, the following has been added [**line 326**]:

"Sea ice volume is then converted into sea ice areal extent by assuming a constant sea ice thickness (d_{si}) which is set to 2 m".

- cloud cover is taken from NorESM and is shown of Fig.1 along some other fixed parameters (ocean circulation, Hadley cells). There is some discussion in the paper of the impact of ocean circulation… but little discussion on the Hadley cell parameters, and none on the clouds. This should certainly be addressed in a revised version of the paper.

Anonymous Referee #2 has also commented on the need for more discussion on the sensitivity of ZEMBA to cloud cover and other key parameters. We have included a new set of model simulations, to addresses these concerns (as detailed in the reply to reviewer #2). To summarise briefly, we perform an additional set of sensitivity experiments attached in the appendix (Fig. 1), which replicates the large perturbations to model parameters performed by Bintanja (1997). Overall, ZEMBA shows a heightened sensitivity to cloud parameters, and especially the globally averaged cloud optical depth parameter. As for the Hadley cell parameterisation, we modify λ- representing the fractional difference between the upper branch's uniform moist static energy and the surface moist static energy at the equator. While changing λ affects zonal precipitation in the tropics, the total atmospheric energy transport remains governed by meridional gradients in moist static energy, leaving global mean temperature largely unaffected. For more details, see the appendix of the revised manuscript.

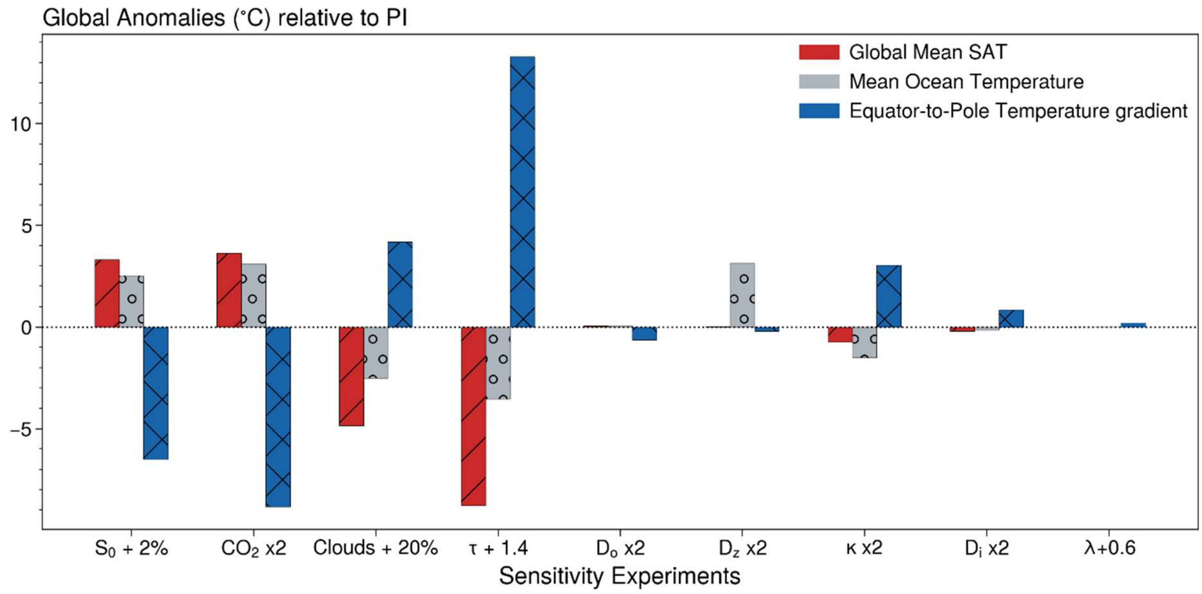

Figure 1: Anomalies in global mean surface air temperature, global mean ocean temperature and the air temperature difference between the equatorial (0◦-10◦) and polar regions (80◦-90◦) for changes in the solar constant (SO), atmospheric CO2 level (CO2), cloud amount, cloud optical depth (τ), ocean diffusion coefficient for horizontal eddy/gyre heat transport (Do), ocean diffusion coefficient for vertical heat transport (Dz), coefficient for turbulent heat fluxes (κ), sea ice thickness (dsi) and a Hadley cell parameter (λ).

- line 174: fsf is the fraction of precipitation that falls as snow. Usually, this is understood as a statistical, or possibly as a time fraction. But later on (line 233) this is used as a geographic fraction, which might be something quite different… Is this really justifiable?

The expression for F_{sf} is taken from Harvey et al., (1988) as the fractional area of the grid box over which (prescribed) precipitation falls as snow, based on meteorological station data from 30N to 90N. In Bintanja et al. (1997), they take this same expression to represent the fractional area of snow present on land for each grid box as a function of surface air temperature. In our case, we use this expression as in Harvey et al. (1988), but instead representing the fractional area of precipitation that falls as snow based on simulated (and not prescribed) precipitation. Given the simplicity of the model, this rate of precipitation is assumed to be uniform for each grid box, thus the geographic fraction of snowfall is equal to the total fraction of precipitation that falls as snow (as presented in the Hydrological cycle). We acknowledge that this is a simplification in the model and have made our approach clearer by modifying the text in the Hydrological Cycle subsection of the *Methods* [**line 183**]*:*

"The expression for f_{sf} is taken from Harvey (1988) as the fractional area of a grid box over which precipitation falls as snow, based on meteorological station data. Therefore, rather than assuming a uniform distribution of snowfall across each grid box, this parameterization allows for only a portion of the land or ocean surface to be snow-covered. As precipitation is assumed to fall uniformly over each grid box, however, this geographic fraction also represents the overall proportion of precipitation that is converted into snow."

- the results in terms of snow cover (Figure5, lines 385) are a bit disappointing. Concerning a future application of this model to the question of ice ages (like a coupling with an ice-sheet model), this might be a severe limitation. Some discussion on this point would be useful… Can really the model be used for such a purpose?

We acknowledge that the representation of seasonal snow cover in the simple EMBA is challenging. Indeed, for the coupling of ZEMBA to an ice sheet model this needs to be further investigated and improved. However, we find the annual-mean snowfall rates over the NH (Fig. 3) to correspond well with NorESM2 and ERA5 output given the simplicity of the model. In the revised Discussion section (in the paragraph concerning model limitations), we have rewritten the discussion of seasonal snow cover as follows [**line 543**]:

"In addition, one of the challenging aspects of a simple model such as ZEMBA is the underestimation of the winter maximum in snow cover over land (Fig. 5a), resulting in a smaller seasonal amplitude in snow cover. However, we note that the annual mean rates of snowfall correspond well with NorESM2 and ERA5 1940-1970 over the northern high latitudes (Fig. 3a)."

.**- almost all model outputs are compared to GCM outputs, except for the meridional heat fluxes on Figure 9c and 9d. This is a bit surprising and, if possible, it should be corrected.**

We opted not to include ERA5 meridional heat transports as the time required to process the large amount of data for comparing to ZEMBA was beyond the scope of this paper. However, we note that the NorESM2 heat transport corresponds nicely to ERA-Interim reanalysis estimates of heat transport averaged from 2000 to 2014 (Trenberth and Fasullo, 2017). In the revised manuscript (in the Pre-Industrial Simulation subsection of the Results section) we have included the following text [**line 426**]:

"The simulated northward heat transport via the atmosphere and ocean is depicted in Figure 7 in reference to NorESM2. We note that NorESM2 heat transport values replicate those estimated from 2000 to 2014 using ERA-interm reanalysis (Trenberth and Fasullo, 2017), including total heat transport exceeding 5.5 PW in each hemisphere and ocean heat transport peaking around 2 PW at 15N."