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Climate of the Past Discussions

Interactive comment on "Weakened atmospheric energy transport feedback in cold glacial climates" *by* I. Cvijanovic et al.

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We are very grateful to the reviewer for the detailed suggestions that have helped improve the paper.

Major comments:

page 1238, line 19-24: It is unclear for which climate state (PD or LGM) the freshwater hosing experiments are performed. So a better description of the used simulations is needed. I guess that the authors use only simulations for one climate state, e.g. PD. This assumes that the reaction of the climate system on freshwater hosing is independent from the climate state, a rather strong assumption. The authors need at least discuss this issue.

We have addressed this issue in our revised manuscript by adding the following sentences in Section 2 (Experimental configuration):

ECBilt-CLIO was spun up for 1500 years using present day orbital forcing and pre-industrial GHG levels and this state was used as a starting point for the freshwater hosing experiments.

....

The same SST anomalies are applied in both sets of experiments. We limit this study to the mechanism of the atmospheric response under the two climates while potential differences in the oceanic response between PD and LGM are not considered here.

page 1240, line 11-12: How are the transports calculated. Using 6-hourly data, daily means, monthly means ??? Please be more specific.

We have added /edited the following sentences explaining the transport calculations:

In these calculations, monthly mean surface and top-of-atmosphere fluxes and precipitation values are used. Whilst the total atmospheric transport is obtained by integrating the atmospheric net energy budget over the latitudes (from the south pole to the given latitude), latent energy transport is calculated considering the fresh water budget obtained as surface evaporation minus precipitation.

....

These calculations have been verified by direct calculation using the individual time step values of vertical integrals of meridional advection of temperature, water vapor and geopotential (vT, vq and vΦ), but as these fields were not obtained for all the experiments we primarily use the approach described above.

page 1242, line 14-16: the DES is a residual, so how could this decomposed?

We have edited the following sentence:

Through direct transport calculations using vertical integrals of meridional advection of temperature and geopotential, DSE anomalies have been decomposed into their sensible and potential components (not shown).

page 1242, line 22-25: the authors need to explain in more details how the fluxes are calculated. E.g., it is unclear if the prime denotes deviations from the monthly mean the seasonal mean and if so which season.

The following paragraph was added to our manuscript:

Eddy momentum diagnostics presented here are obtained from the monthly model output, using the decomposition of the total flux into a mean meridional circulation term and stationary and transient eddy flux terms (Peixoto and Oort, 1992). In this manner, total flux and mean meridional circulation terms are obtained directly from the model output, the stationary eddy flux is calculated by subtracting the zonal means while the transient eddy flux is given as the difference between the total flux and the other two terms. We have verified this approach by calculating the transient eddy flux from the daily data (by subtracting the 8 day means from the daily values, but using a smaller sample of years) and arrived at the same conclusions.

section "conclusions": I think the authors use a coarsely resolved model configuration. Clearly atmospheric eddies are resolved but underestimated in a T42 resolution. Moreover, 18 levels also affect the atmospheric waves and therefore the atmospheric energy transport. So at least the authors should discuss the issue of how the rather coarse resolution may affect the results presented. Given the rather coarse resolution of simulations I encourage the authors to include observations, e.g., it would be nice to see where the observation (just one point) lies in Fig. 2.

Because several studies have already investigated the performance of the CCM3 at T42 resolution, we did not try to do the same by comparing our PD control simulation with the observations; instead we have chosen to refer to those. We find your comment

very useful because these references do suggest that there are such areas where the winter transient eddy heat fluxes are overestimated so one should be cautious with the interpretations. We have added the following paragraphs to Sections 2 and 3.2.1, respectively:

Section 2: The performance of the model dynamics was analyzed by Hurrel et al. (1998) and it was found that CCM3 reasonably well depicts the main storm tracks and transient eddy flux fields. Differences occur in the winter over the North Pacific, where the simulated transient eddy heat flux maximum is too strong and displaced to the north. The simulated transient kinetic energy is somewhat underestimated, especially over the summer hemispheres. Magnusdottir and Saravanan (1999) analyzed the meridional heat transport in the CCM3. Both in terms of peak value and the latitude of maximum atmospheric transport do their results compare well with those obtained by Trenberth and Caron (2001) based on reanalysis data.

Section 3.2.1: Caution should be taken given that CCM3 overestimates the winter maximum in transient eddy heat flux over the North Pacific (Hurrel et al. 1998). Nevertheless, our analysis focuses on relative changes (compared to the base state) and is in line with existing studies (Cheng et al. 2007; Li and Battisti, 2008; Donohoe and Battisti, 2009) suggesting that the found response is robust.

How does the PD simulation compare to observation for atmospheric transport (Fig. 3, suggestion to show the difference to observations) stationary and eddy heat flux anomalies (Fig. 4). The authors could also include observations in Fig. 6, 7, and 8 and discuss the differences to give the reader the ability to assess the findings in the light of observations.

The studies by Hurrell et al. (1998) and Magnusdottir and Sarvanan (1999) discuss this in detail so we have mentioned their main findings. Figures 6, 7 and 8 represent the anomalies so we can not make a direct comparison to observations, while for the base state comparisons we refer to the paper by Hurrel et al. (1998).

Minor comments:

We have implemented most of the suggested comments into our revised manuscript. We discuss below only the points where we did not follow your suggestions completely or the ones that require further explanation:

page 1239, second paragraph: Please define summer - It sounds like JJA, so why is September not included? You might also show September, as this is the month with the least sea ice extent (at least for PD).

We have clarified the definition of summer in our manuscript - it is JJA. This season was chosen because the effect of the applied warming anomalies was more visible than in the other seasons. Thanks to your comment we realized that it would be more obvious to plot the land mask together with sea ice changes, therefore we have adjusted Figure 1 b and c to show the both.

page 1239: NHTG: Gradient is a bit misleading here as one would expect the unit K/km.

We have changed NHTG into NHTD (Northern Hemisphere temperature difference) in order to keep the temperature units (K).

page 1240, last sentence: It would be nice to see this, so maybe the authors could do a similar figure as Fig.2 but for 45N and 60N. So the reader could actually see a peak around 45N.

We believe that this could be concluded from figure 3 a and d, and instead of adding additional figures, we have edited the text to more clearly refer to Figure 3:

The PD simulations show weaker response at the equator with sensitivity increasing northward and peaking in the mid-latitudes (as it can be seen from Figure 3 a and d showing the meridional transport anomalies).

Page 1244, line 24-28: I think it would be helpful for the reader to see the changes

of the Hadley Circulation.

We are concerned that this would confuse the reader, since it is not only the Hadley cell strength but also the gross moist static stability that has to be taken into account. But we try to be more careful and describe in more detail what the main changes in the Hadley cell structure are:

In our experiments, the Hadley cells are not necessarily located over the same areas in the different simulations. Similarly to the described ITCZ shifts, the meeting points of the northern and southern Hadley cell move more in LGM than in PD simulations, and locations of the Hadley cell maxima can be at different heights or latitudes in different simulations. Such is the case for the annual PD and LGM northern Hadley cells maxima that are located close to 15° N but at 700 hPa and 500 hPa, respectively. Therefore, instead of specifying a certain latitude and height, we simply consider the point of the maximum of the mass stream function. We then calculate the ratio of the northward energy transport at this latitude and the stream function maximum itself.