Reply to Reviewer #2

General Comments

The goal of this paper is to investigate whether teleconnections from the Tropical Pacific to North America and the Gulf of Mexico (via the Pacific/North American pattern) are maintained during the Last Glacial Maximum when large ice sheets covered much of North America. The analysis is performed using PMIP2 and PMIP3 simulations, the NCEP/NCAR reanalysis and some low-resolution simulations performed using CCSM3. I think this is interesting and novel, and the authors' results are supported in the datasets they analyse. However, the authors make a few methodological choices that make me wonder about the general applicability of their results, especially to historical climate conditions.

We thank the reviewer for the careful reviews, which are important for us to improve the paper. Replies to the comments are as follows. All our replies are in blue.

1. Most of the datasets that the authors use are old. Firstly, the sensitivity experiments are performed with a PMIP2-era climate model, CCSM3. While the dynamical phenomena that the authors are investigating are not likely to be strongly compromised by this choice, their choice to use a lower resolution with this model than was even used for PMIP2 is puzzling, unless it's a dataset of opportunity. This resolution choice can have important implications for the results they present, since the representations of stationary wave patterns under glacial boundary conditions are known to degrade at lower resolutions (cf Lofverstrom and Lora, 2018). Additionally, the use of the ICE-5G ice sheet reconstruction for their LGM boundary conditions is problematic, as the dome in this ice sheet reconstruction is so much larger than current estimates would predict. If the authors want to suggest that their results have applicability to the actual conditions at LGM, then it would be helpful if they present information on which sensitivity experiment best corresponds with current estimates of true LGM conditions.

We started this work a few years ago when there was only PMIP2 data, and PMIP3 data was not available yet. We found that the PNA is distorted in PMIP2 simulations. Then, we performed the low-resolution sensitivity simulations, with ICE-5G. The low-resolution simulation results were also used in a different work (Lu et al., 2016).

As PMIP3 data became available, we found the similar results in PMIP3 simulations. Especially, the LGM simulation of CCSM4 shows consistent result with that of CCSM3. Therefore, we feel that the result of distorted PNA path at LGM is not dependent on model versions.

ICE-6G vs. ICE-5G: To answer the question about the thickness difference of the Laurentide ice sheet between ICE-6G and ICE-5G, we plot vertical cross sections of the ice sheet thickness along 45 °N and 60 °N in Figure R1 below. It can be seen that the thickness of ICE-6G is close to 80% of ICE-5G in general. ICE-6G is even higher than 80% ICE-5G in some regions. The shape of 80% ICE-5G over North America does not well match the twin-peaks of ICE-6G at 45 °N. However, the shape of 80% ICE-5G matches that of ICE-6G reasonably well at 60 °N, except for the region between 200 ° and 230 ° in longitude where 80% ICE-5G is even lower than ICE-6G. Figure 2e shows that as 80% ICE-5G is applied, the PNA path is distorted toward Arctic, and that the present-day PNA no longer exists.

The PNA is a large-scale atmospheric circulation system. It may not be very sensitive to the small-scale structures of the ice sheet, we feel.

In the revised manuscript, we will explicitly point out the differences between ICE-5G and ICE-6G. Figure R1 will be added to the Supporting Information. In the conclusion section, we will add a few sentences to point out how the PNA path changes with increasing ice-sheet thickness. For example, the present-day PNA path remains for ice sheet thicknesses no more than 60% ICE-5G (Figs. 2a-d). However, the PNA is distorted as ice sheet thickness reaches 80% ICE-5G (Figs. 2e-g).

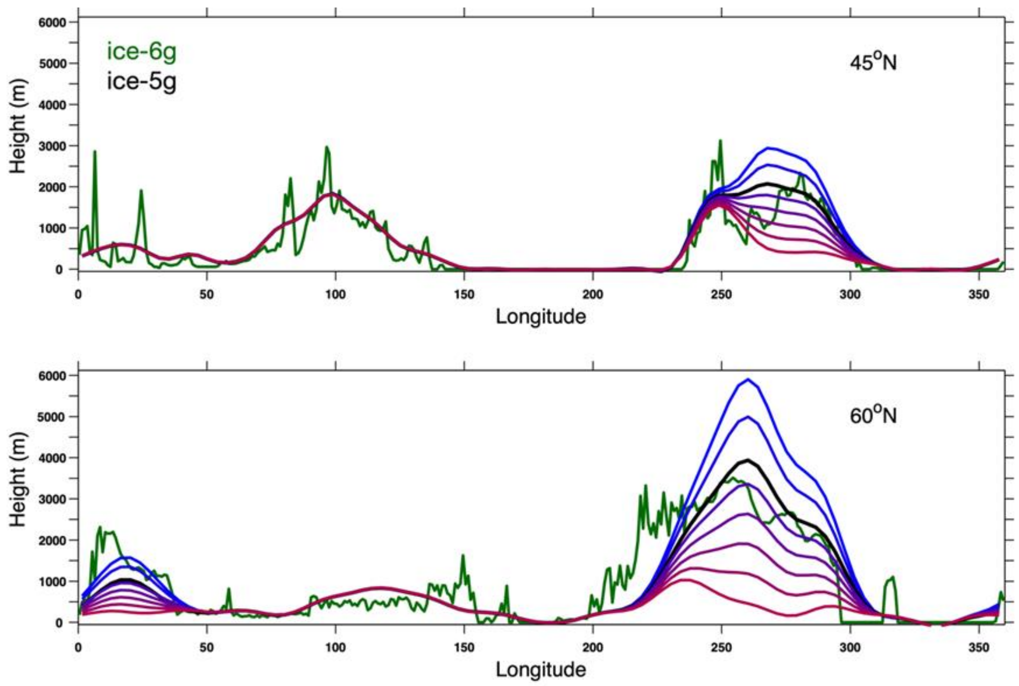

Figure R1. Vertical cross sections of ice sheet thicknesses of ICE-5G and ICE-6G at 45 °N and 60 °N. Different ice sheet thicknesses in our sensitivity experiments (from 0% to 150%) for ICE-5G are all plotted.

2. The authors use a point-based definition for the PNA rather than a principle component-based definition. Given the locations of modes of variability can change under different boundary conditions, restricting themselves to fixed locations in space seems limiting. The authors attempt to compensate for this choice by including a buffer zone around each centre of action, but it feels like the analysis is more convoluted as a result, requiring multiple sets of correlation figures with different centres of actions to explain their results. I would like to see the analyses repeated using PCA for at least one set of model data to see whether that alters the interpretation of their results at all. It should also help with separating the signal they are investigating from the subtropical wave train.

We have done analysis, using different methods, such as EOF and rotated EOF (REOF). The results are almost the same as the correlation analysis. Figures R2-4 shows the REOF results of 500 hPa height in NCEP/NCAR reanalysis, CCSM3-PMIP2 PIC and LGM simulations. The 2nd REOFs in the NCEP/NCAR reanalysis and the CCSM3 PIC simulation well represents the loading pattern of the present-day PNA (Figures R2 and 3).

In contrast, the 2nd REOF in the CCMS3 LGM simulation does not show the PNA pattern (Figure R4). The 3rd and 4th REOFs demonstrate connections between North Pacific and Arctic, and between North Pacific and the southern part of North America.

The reason why we stay with the point-based method is because the four base-points demonstrate the traditional view of the PNA path. Moreover, it is easier for us to quantify how far the PNA path is distorted away from the present-day PNA path, as shown in Figure 3.

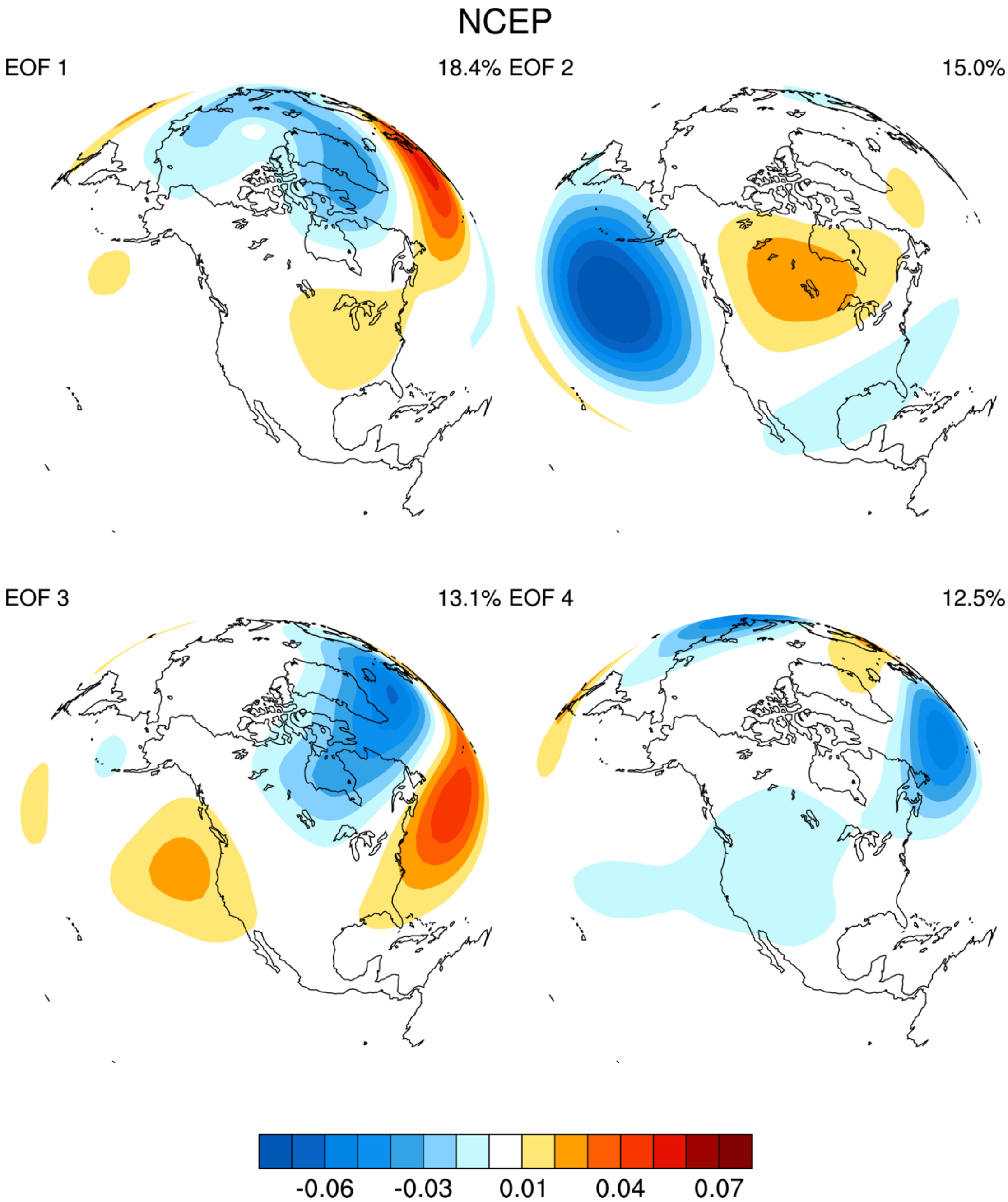

Figure R2. Spatial patterns of the Rotated Empirical Orthogonal Function (REOF) analysis of 500 hPa height in NCEP/NCAR reanalysis.

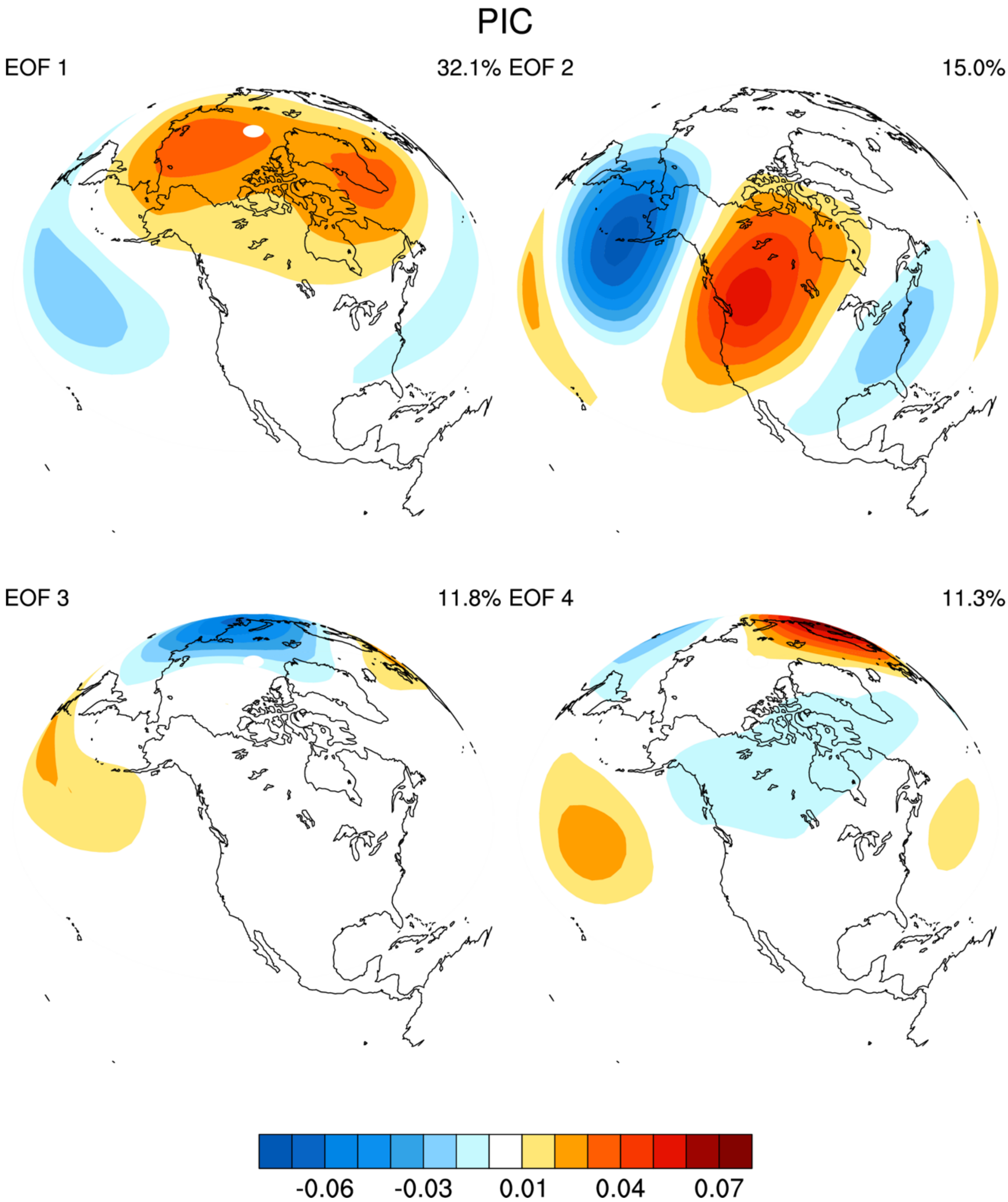

Figure R3. Spatial patterns of REOFs of 500 hPa height in the PIC simulation of CCSM3.

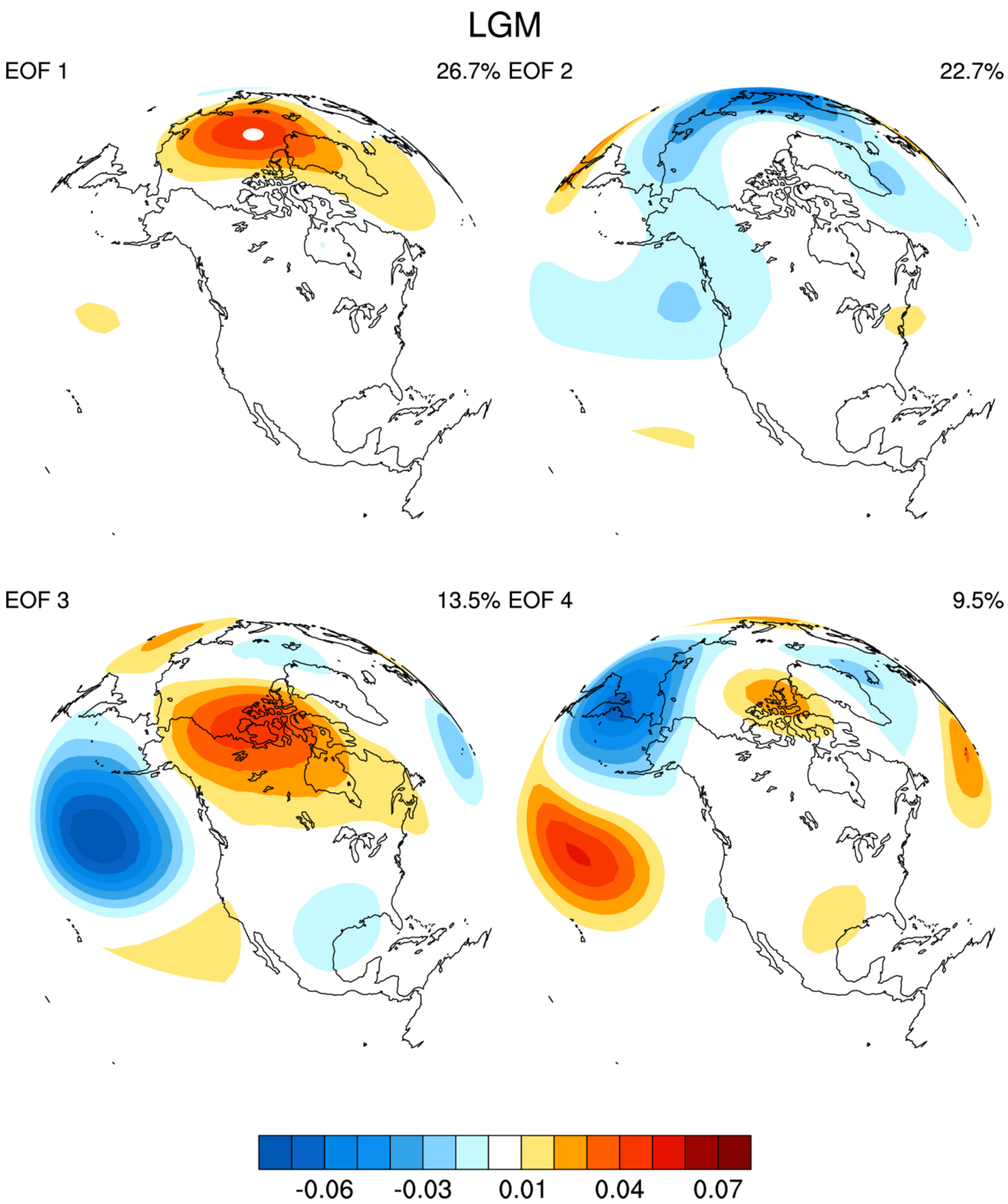

Figure R4. Spatial patterns of REOFs of 500 hPa height in the LGM simulation of PMIP2 CCSM3.

Finally, I find these results interesting from the perspective of altered atmospheric dynamical regimes and altered atmospheric variability in the presence of large ice sheets. I don't understand the authors' interpretation that a rerouting of the teleconnection pattern and reduced strength of the present-day pattern of the PNA makes it "broken". What's so special about Alberta and the Gulf of Mexico? Isn't it also interesting that a re-routed teleconnection means that regions of the Arctic are now being affected more directly by tropical Pacific variability? Also, a discussion of how the tropical variability itself might be different at LGM (weaker, as I understand it) would help contextualize the work better. As it is, it makes me curious whether there is an implication for this result they are working toward that isn't communicated in the manuscript.

Thanks for the suggestion!

When we use the word "broken", it means breaking of the present-day PNA teleconnection. We agree with the reviewer that "distorted PNA" is good enough. Therefore, "broken" will be removed. We shall also focus on the distorted PNA path in the revised version, emphasizing the connections toward Arctic and southern part of North America.

Yes, previous works showed weaker ENSO at LGM (Zhu et al., 2017). We will add brief discussion in the revised version.

Scientific Comments

I feel like insufficient information is provided about the datasets provided, particularly for the reanalysis. What years were used? What is its resolution and the resolution of the model results presented?

We use the recent 30-year NCEP/NCAR reanalysis from 1988 to 2017. Information of horizontal resolutions of reanalysis and models will be added.

The reanalysis seemed to be used as a proxy for observational conditions. How well does this reanalysis reproduce observed PNA variability? There is observational data for both the pattern and time series of the PNA from 1950 to compare against.

Yes, reanalysis cannot be considered "real" observational data. At present, most modeling works are compared with reanalysis by taking the advantage of its easier access.

The 2nd REOF in the NCEP/NCAR reanalysis in Figure R2 is almost the same as that given by the Climate Prediction Center of NCEP (https://www.cpc.ncep.noaa.gov/products/precip/CWlink/pna/pna_loading.html).

At present, there are three different time periods being presented in the plots in Figures 1, 3 and in the supplement: transient years 195? to 200? in the reanalysis, and fixed boundary conditions under preindustrial and LGM conditions. While it's unlikely that a simulation that doesn't generate a realistic PNA pattern under preindustrial conditions will produce a realistic PNA under late 20th century conditions, it is not accurate to treat the reanalysis and PIC simulations as representing the same climate state. Since the historical experiment is a Tier 1 experiment, results that do match the reanalysis time period should be available for all of the PMIP models presented here.

We agree that the PIC simulations of PMIP2 models are different from the NCEP/NCAR reanalysis that includes climate changes. However, the datasets from PMIP2 simulations are only available for the PIC and LGM experiments, not including historical simulations.

In the present paper, our key point is to address the difference of the PNA path between two very different climate states: LGM vs. present. Therefore, NCEP/NCAR reanalysis is not much different from the PIC simulation in this context.

I would like to see a discussion of how the significance of correlations was determined.

We used 30-year data for the reanalysis (1988-2017), all models of PMIP2 and PMIP3, and our sensitivity simulations. The degree of freedom is 30. For a two-tailed test, the critical value of the correlation coefficient is 0.35 for the 95% confidence level. We will explicitly point out this in the revised version.

Be more precise about criteria for considering a PIC simulation to have represented the PNA successfully. Do there have to be significant correlations between Hawaii and within 10deg of every other centre of action or also between each of the other centres of action? I understood the criteria to suggest that the all regions had to be significantly correlated with Hawaii, but a visual inspection of Figure S2 suggests that some of the "well-performing" runs do not capture the Gulf of Mexico centre of action within 10degrees and the defined significance thresholds.

First, we pointed out that our definition is a "loose definition". Such a loose definition is to figure out how much the PNA at LGM is distorted away from its present-day path. The quantitative results is shown in Figure 3. It can be seen from Figure 3e that the correlation coefficient just reaches the criteria at the Gulf Coast for the PIC simulation of HadCM3M2 and CNRM-CM33 models. Figures S2b and c show two small shallow blue areas that are just at the margin of the 10 degree circle.

Ln 196 The authors claim that FGOALS-1.0g, IPSL-CN4-V1-MR and MIROC3.2 are unable to reproduce the North Pacific centre of action correlations with Hawaii, but only FGOALS-1.0G appears to have insignificant correlations at this site in Fig 3c. Why the claim that they are not reproducing it, then?

Agree. Changes are made. IPSL-CN4-V1-MR and MIROC3.2 have insignificant correlations at the Gulf Coast instead of the North Pacific.

Ln 242-243 The authors state there are two jets at LGM: a subtropical jet at 30N and a subpolar jet at 63N. Do they actually intend to say that the southward branch is actually a subtropical jet or a subtropically-located eddy-driven jet?

Yes, the southward branch is the subtropical jet.

ln 247-248 It is true that the latitudinal temperature gradients are sharper at 35-50N, but not much at 70N, where the subpolar jet the authors are discussing arises, unless you include the temperature gradient associated with the ice sheet surface. Due to the lack of evident meridional gradients in temperature here, I question their interpretation. What about the role of katabatic winds or non-linear interactions of the winds with the ice sheet at their westernmost interaction point?

Agree.

Following the suggestion, we have replotted Figure 6. The bottom panels of temperatures (Figs. 6d-f) are replaced with meridional temperature gradients, which are shown below. Meridional temperature gradients show a local maximum at about 70N, right over the northern side of the ice sheet.

Katabatic winds are mainly near the surface. Here, the subpolar jet is located between 400 and 300 hPa.

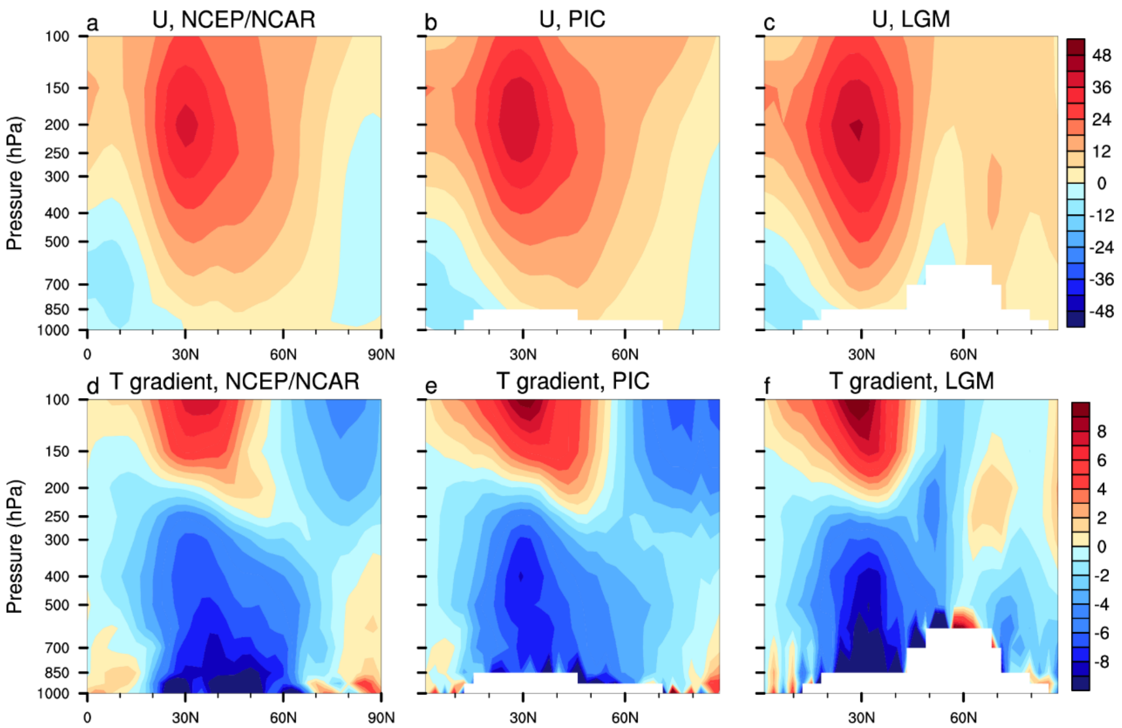

Fig. 6. Vertical cross sections of DJF zonal winds and meridional temperature gradients along the longitude of 100 °W in the NCEP/NCAR reanalysis and PMIP2 CCSM3 simulations. Top panels: zonal winds, and bottom panels: temperature gradients. Left panels: NCEP/NCAR, middle panels: PIC, and right panels: LGM. Zonal-wind unit is ms^{-1}, and temperature gradient unit is K/(1000 km).

Ln 260-261 How much does the core of the jet shift southward as the ice sheet height increases in supplemental figure 4e? It doesn't appear to be more than a couple of degrees and is barely discernible from these plots. The more apparent feature is that the core of the jet becomes much narrower as it strengthens, while the 12 m/s isoline initially expands northward and eventually breaks away from the rest of the jet.

Agree. The subtropical jet shifts southward by about 3 degrees. In the revised version, we will point out that the jet core becomes narrower with increasing ice sheet thickness.

Technical Details

Given the authors are analysing CCSM3 simulations at different resolutions, it would be helpful to specify which resolution version they are referring to in plots and discussions.

We will add more specific information of data resolutions in the revised version.

In Figures 3c and d, it would be helpful for interpreting the results if PMIP2 and PMIP3 models from the same model tree were given the same symbols (where possible).

We have updated Fig. 3.

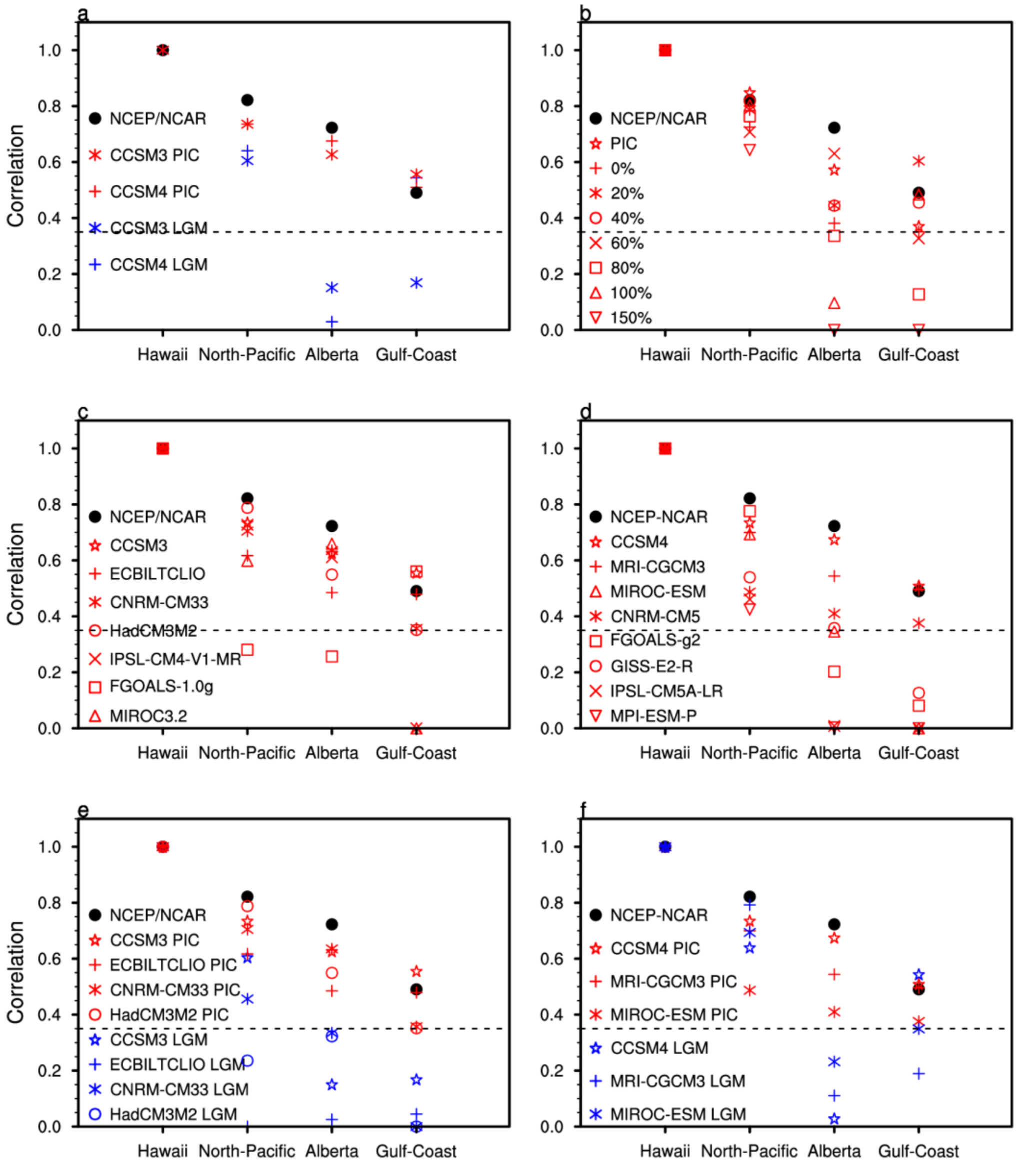

Fig. 3. Correlation coefficients at the four PNA action centers in PIC and LGM simulations for PMIP2 and PMIP3 models, with the base point near Hawaii. The negative values over Alberta and the Gulf Coast are reversed to positive. The dashed lines correspond to 0.35, which represent the 95% confidence level. (a) CCSM3 and CCSM4, (b) sensitivity simulations, (c) PIC simulations of PMIP2 models, (d) PIC simulations of PMIP3 models, (e) LGM and PIC simulations for well-performing PMIP2 models, and (f) LGM and PIC simulations for well-performing PMIP3 models.

Figures 3e and f caption was difficult to understand without reading a few times and figuring out from the plots themselves. A modification as simple as "LGM and PIC simulations for well-performing PMIP2 models" would get rid of this problem.

Thanks, changed.

Ln 198 typo "FGOAL-1.0g" to "FGOALS-1.0g" ln 202 typo "Albert" to "Alberta"

Thanks, changed.

ln 203-205 missing key point in the text that it is at LGM that these simulations are unable to reproduce correlations of PIC.

Added.

Ln 240 "North American" to "North America"

Revised.

ln 261 "Significant jet split" to "Significant jet splitting"

Revised.

ln 271 "westerly jet act as wave guides" to "westerly jet acts as a wave guide "

Revised.

ln 339 "We have showed" to "We have shown"

Revised.

ln 340 "forced jet split" to "forced jet splitting"

Revised.

ln 341-342 double negative makes this sentence say the opposite of what you're trying to say "ENSO would have little direct influence"

Thanks, revised.

Figure 7 Overall, I find this plot very effective at illustrating the critical latitudes. However, the presentation of the results in units of m□1 rather than the number of wavelengths per latitude circle (e.g. a wave 1 field would have one complete wavelength around the hemisphere) makes it difficult to get meaning from the colour contours.

Thanks for the suggestion. We prefer to keep the unit because it is the standard unit. The stationary wavenumbers are calculated following equation 6.29 in Held (1983).

Figure 8 and S5 Showing the zonal anomalies of geopotential heights would make the author's argument clearer without being limited to the height scale capturing the background zonal gradient.

We feel that Figure 8 and S5 can give readers better intuition on how atmospheric circulation is forced by the large ice sheet. We prefer to keep the two Figures.

None of the data used in this study was acknowledged. Acknowledging data sources is good practice, and it is also stipulated as a condition of usage in some cases. CMIP data archives also require users to include a table listing information about each simulation used in their publications. The supplement is fine for this, I think.

Thanks for the reminder! All the data sources used in the paper will be acknowledged.

References:

Held IM (1983) Stationary and Quasi-stationary Eddies in the Extratropical Troposphere: Theory. in B. J. Hoskins, Pearce RP (eds.) Large-scale Dynamical Processes in the Atmosphere. Academic Press, pp. 127–168.

Lu Z, Liu Z, Zhu J (2016) Abrupt intensification of ENSO forced by deglacial ice-sheet retreat in CCSM3. Climate Dynamics 46:1877-1891.

Zhu J, Liu Z, Brady E, Otto-Bliesner B, Zhang J, Noone D, Tomas R, Nusbaumer J, Wong T, Jahn A, Tabor C (2017) Reduced ENSO variability at the LGM revealed by an isotope-enabled Earth system model. Geophysical Research Letters 44:6984-6992.