Clim. Past Discuss.,
https://doi.org/10.5194/cp-2019-92-RC2, 2019

Climate of the Past Discussions

Interactive comment on "Distorted Pacific-North American Teleconnection at the Last Glacial Maximum" *by* Yongyun Hu et al.

Anonymous Referee #2

Received and published: 17 October 2019

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.

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.

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.

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.

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?

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.

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.

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

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.

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?

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?

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?

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.

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.

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).

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.

Ln 198 typo “FGOAL-1.0g” to “FGOALS-1.0g”

ln 202 typo “Albert” to “Alberta”

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

Ln 240 “North American” to “North America”

ln 261 “Significant jet split” to “Significant jet splitting”

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

ln 339 “We have showed” to “We have shown”

ln 340 “forced jet split” to “forced jet splitting”

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

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.

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.

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.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2019-92, 2019.