Review for the manuscript

Asymmetric changes of temperature in the Arctic during the Holocene

based on a transient run with the CESM

by Hongyue Zhang et al.

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General

The revised version has been improved compared to the first submission and comments are mostly sufficiently addressed, especially in the context of adding and analyzing currently available Holocene transient simulations. Still, I am critical as to whether the impact of acceleration is as minor as the authors suggest, especially for long term changes on sea ice or other quantities. For example, Fig. 12 in the reply even shows a nice example that the acceleration technique has consequences on the evolution of patterns, such as the temperature profile.

Reply: Thank you for your recognition of the progress of our manuscript. We have carefully considered the comments and tried our best to address every one of them. We are concerned with Arctic temperature change, which primarily involves climate change processes on the land surface and sea surface at high latitudes. As expressed in the reference we exemplify, the accelerated model result has minor impact on the problem we are studying compared the unaccelerated model. Our conclusion is also supported by Figure 12 (Lu et al. 2019). Figure 12 shows the time series of the vertical temperature profiles. Y-axis is depth (m) and x-axis is accelerated year, and the contour is the anomalous temperature (°C). Black lines connect the maximum signals in the ocean. The acceleration causes longer adjustment times in the deep ocean compared to the surface ocean, leading to weakened and delayed responses in the deep ocean. However, our study is mainly concerned with the variation of the shallow ocean temperature. If we focus only on the ocean temperature in the range of 0-50 m, it can be seen from Fig. 12 that there is little difference in the comparison of temperature diffusion without and with acceleration by a factor of ten.

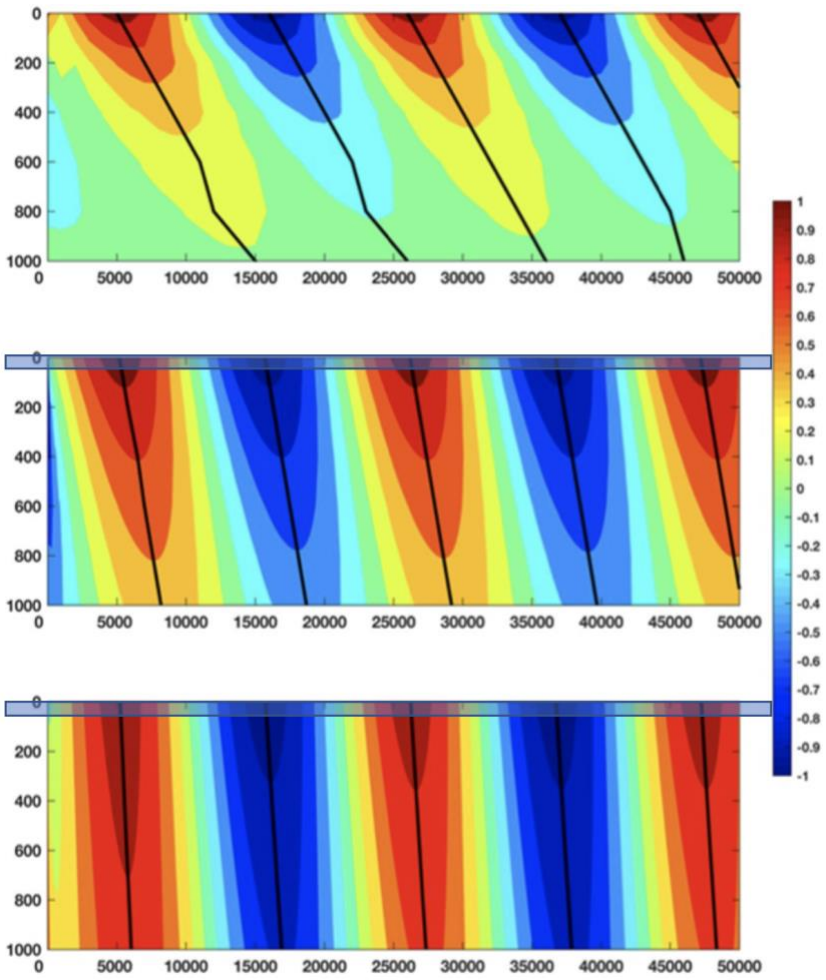


Fig. 12 Time sequence of the vertical temperature profile in a simple diffusion model under three acceleration scenarios: (upper panel) 100-fold acceleration, (middle panel) tenfold acceleration and (bottom panel) non-acceleration. (from Lu et al. 2019)

I also very much appreciate the additional work and investigations, especially related to the implementation of statistical tests. However, in the revised version the tests are poorly introduced and implemented and I encourage the authors to be more specific in the final version of the paper which statistical tests are applied and if they are appropriate for the respective purpose. For instance, the effect of serial correlation is still not addressed. In addition, it is important to select the level of confidence before the statistical test is carried out (typically alpha=0.05 for a two sided test) and eventually test if the numbers lie within or outside the confidence interval. For the trend test it is not mentioned at all which test has been applied (e.g. Mann-Kendall Test or any other Bootstrap method). To facilitate the description of statistical tests used, the authors should include a short caption into their Methods section and briefly describe which statistical tests are applied, including their specific setup and potential shortcomings (e.g. when the number of degrees of freedom is very small or the effect of serial correlation is not accounted for).

Reply: Thank you for your recognition of our work. The significance test used in the paper is mainly the students t-test, and the significance level is chosen as alpha=0.1 for two-tailed. In the revised manuscript, we have added a subsection in Section 2 specifying which statistical tests were applied as well as the EOF analysis method.

We have revised this description as:
"2.3 Analytical and Statistical Methods
We focus on long-term temperature changes in the Arctic during the Holocene. The significance test used in this study was calculated according to the two-tailed Students t-test at the 90% (alpha = 0.1) or 95% (alpha = 0.05) confidence level. The Students t-test was used to compare the means of two groups and determine if the difference in means is statistically significant and was also used to test the statistical significance of each grid in the figures below. The sample size of the Pacific Arctic region in the temperature proxy data is small and thus a small degree of freedom. We apply empirical orthogonal function (EOF) analysis, also known as principal component analysis (PCA), to sea level pressure changes in the Northern Hemisphere. EOF analysis is a standard analytical technique used in climate science to study patterns of spatial variability. EOF is obtained by computing the eigenvectors and eigenvalues of the spatially weighted covariance matrix of the temperature field. Applying EOF to the Northern Hemisphere sea level pressure is a common method to study the Arctic dipole mode. The objective is to show the variation of the Arctic dipole mode during different periods of the Holocene (0-2 ka BP, 5-8 ka BP). As described in Section 3 below, the second mod of EOF for the Holocene 5-8 ka BP period explains 11.5% and that for 0-2 ka BP period explains 16.3% of the sea level pressure variation."

In general, I think the manuscript should be published, but I still vote for "major revisions" until the statistical tests are carried out and described more thoroughly to back up the robustness of according results. For the re-revised version I list some minor comments below

Reply: Thank you for your comments. We have carefully considered the comments and tried our best to address every one of them.

Specific

Abstract:
The abstract is still in its original form. The authors should include the additional conclusions taking into account the new simulations and also mention the drawbacks and shortcoming of their study using the acceleration technique in the very beginning.

Reply: Thank you for pointing out our oversight. The new abstract was rewritten to add the shortcomings of the acceleration technique and the results of the verification of the Arctic temperature asymmetry using unaccelerated simulations. The results of the PDO section were removed in new abstract.

Introduction:
When presenting the additional simulations it would be also helpful to make the reader aware that the EC-Bilt model is an Earth System Model of Intermediate Complexity and therefore falls into another category of climate models. The comprehensive Earth System Model simulations with CCSM3 and IPSL are therefore better suited for a consistent comparison with the accelerated simulations the authors carry out with the CESM.

Reply: Thank you for your suggestion. We have added a description of EC-Bilt as an Earth system model of moderate complexity in the introduction section.

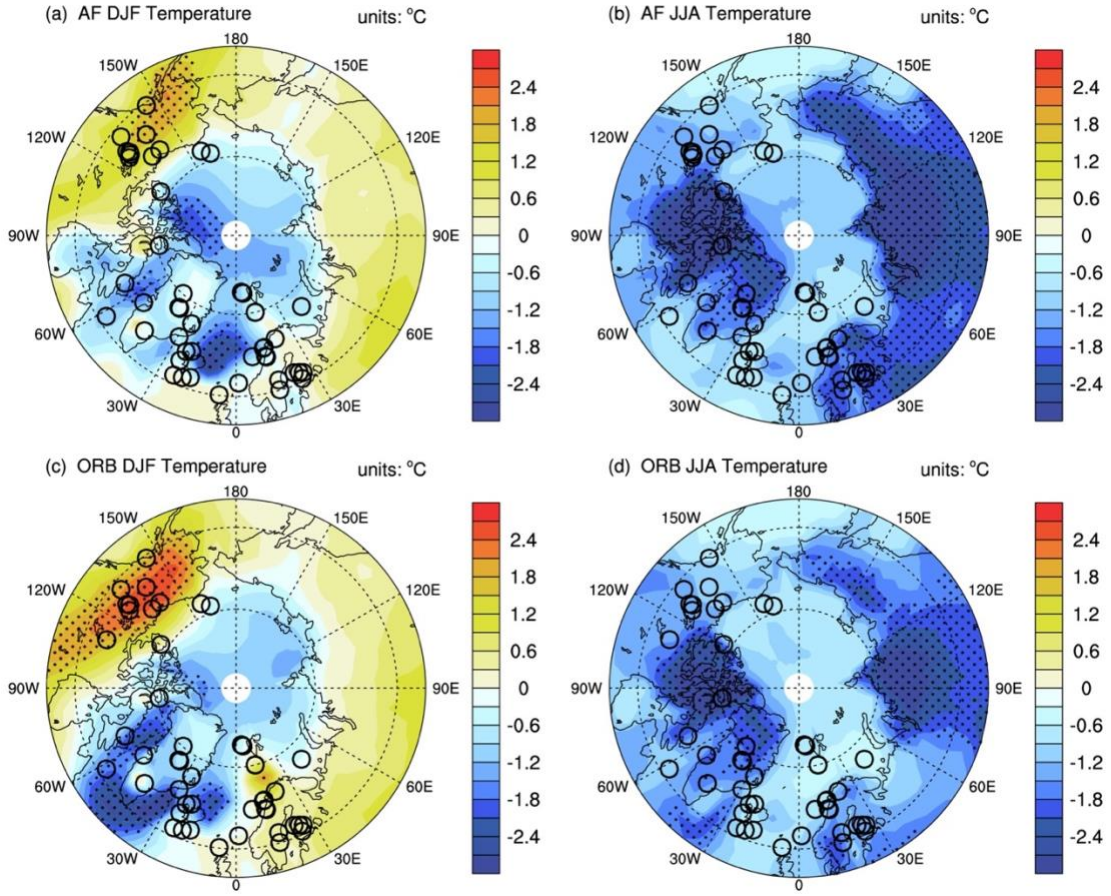
2.1 The CESM Model and the transient simulations
In the last paragraph the authors list studies covering transient Holocene simulations. In this list the more recent unaccelerated MPI-ESM (Bader et al., 2020) and IPSL (Braconnot et al., (2019) simulation are missing and should be added.

Reply: Thank you for your comments. We have added the information on non-accelerated models (IPSL and MPI-ESM) in the last paragraph of this section for the reader to have a more comprehensive understanding.

We have revised this description as:
"There are, to our knowledge, 5 sets of climate simulations published so far covering the entire Holocene period, namely ECBilt-CLIO [1], FOAM [2], TraCE-21ka [3], FAMOUS [4] and LOVECLIM [5]. Except for TraCE-21ka, these simulations are accelerated by different factors. Some unaccelerated simulations have also been published in recent years, such as those covering the Holocene 8ka BP and 6ka BP based on the MPI-ESM [6] and IPSL [7], respectively. The external forcings considered in these simulations are generally a part of the combination of the Orbital Forcing (ORB), the Greenhouse Gases (GHG), the continental ice sheets (ICE), the Meltwater Flux (MWF), the Volcanic Forcing, the Landuse Forcing and Ozone Forcing."

3.1 Arctic Temperature Change
Fig. 2 and following Figures: The temperature/sea ice/sea level pressure differences should be flagged with a symbol (e.g. hatching) for those areas being statistically significant different at the 5% level. Although the numbers mentioned in the Figure captions should contain an information on their statistical significance.

Reply: Thank you for your very helpful suggestions. To better illustrate the statistically significant areas on the figures, we performed a significance test using the Students t-test with a significance level of alpha=0.1 for two-tailed. The regions that passed the significance test under this calculation are marked with a dotted conformation. As shown in Figure 2 below, we modified Figures 2,4,5,6,7.



3.2 Sea Ice Change (Aice, March) and SLP Change

ll. 439 new version: the authors mention some physical mechanisms linking the change in radiation to albedo and heat storage changes, quoting Dai, (2021). The study of Dai (2021) used however a profoundly different setup (500 year CO2-surface albedo vs. 2000 year control) with CESM 1.0. My suggestion is to use the results based on the accelerated CESM simulations the authors carried out, if mechanisms are investigated and interpreted in the context of Holocene climate change.

Reply: Thank you for your valuable advice. We want to emphasize the enhancement of positive feedback related to changes in sea ice, and the enhancement of temperature asymmetry caused by radiation to heat changes. You are correct, Dai et al. (2021) involves research on heat storage and CO2-surface albedo. Due to the complexity of thermal storage and other forcing radiative effects and the fact that they are not the primary objective of our consideration, we did not perform calculations for CESM data. We have modified the description in 439.

ll. 488 new version: As stated in the previous comments, I still have concerns with the formulation “The results of the ORB simulations are more significant than those of the AF, […]”. Conceptually, a result can be statistically different to a reference/control on a certain level of confidence or not. Maybe authors can clarify this statement whether

this is linked to a statistical statement or in the context of a comparison that one simulations shows larger/lower values compared to another one.

Reply: Thank you for your valuable comments. In the new revised manuscript we clarified that the ORB simulations compared to AF simulations are values comparisons. We modify the statement that ORB is more "significant" in the manuscript because we did not perform a significance test for the difference between the two Arctic regions for two simulations.
We revise line 488 to read as follows "Compared with the AF results, the ORB simulation show that the sea level pressure changes in the two regions are more contrasting and the difference is larger. This suggests that orbital forcing plays a contributing role in generating this asymmetry than other forcings."

Fig. 5: I assume the numbers at the color bars are still in units of Pa, although the labeling has changed to hPa. Authors should also change the numbers displayed at the colorbar for consistency.

Reply: Thank you for pointing that out. We have revised Figure 5.

3.3 EOF of SLP and UV wind regression
Unfortunately, even authors state in their reply that the motivation for using EOF/regression was implemented in the revised version, it is still not presented. For instance, hemispheric EOFs can be misleading in their pattern structure (cf. Discussion Ambaum, 2001 on the physical plausibility of the Arctic Oscillation concept (1 EOF SLP Northern Hemisphere). Besides that, authors should explain why EOF analysis presents a meaningful tool, even on hemispheric scales for investigating SLP changes (e.g. because of potential representation of real teleconnection patterns) and why the according PCs could be used as a basis for regression. It should be tested or at least mentioned if this teleconnection structure represented by the EOF is actually real or just an artefact of the Eigenanalysis. In the present form results are just presented without introducing any general background shortcomings of the methods.

Reply: Thank you for mentioning this point. In the manuscript above, we pointed out the asymmetric variability characteristic of Arctic temperature and the similar asymmetry of sea ice and sea level pressure variability during the Holocene. Therefore, we would like to further illustrate the connection between them. Thus we explore the physical processes between them by analyzing the Arctic dipole. The EOF method is commonly used to study the Arctic dipole in the past studies (Wu et al., 2006; Wang et al., 2000; Skeie et al., 2000). This is exactly our motivation for using EOF. Our EOF for the SLP in the manuscript is not on the hemispheric scale. The first leading mode of the EOF corresponds to the Arctic Oscillation pattern, and the second mode that corresponds to the Arctic dipole. So in the manuscript we focus on the second mode. Regression of the PC time series shows how the corresponding temperature, sea ice, and wind change under the action of the Arctic dipole, thus helping us to understand

the physical processes involved.

l. 550: A hypothesis can not be verified. It can only be falsified, whether it is consistent or not with a null hypothesis on a certain level of confidence. In addition, similar to previous chapters, all numbers mentioned in the text and figure captions need to be tested on their statistical significance.

Reply: Thank you for your comments. The hypothesis we describe in line 550 of the manuscript is not a statistically significant hypothesis. Rather, it refers to a conjecture we made in line 292 about the contribution of sea ice and sea level pressure changes to temperature asymmetry. We have revised this "hypothesis" in the manuscript. We have also added a statistical test description to this paragraph.

3.4 The connection between Arctic Dipole pattern and PDO
I suggest to completely leave out the entire section. The PDO is never mentioned in the introduction or elsewhere and the analysis does not add anything to the conclusions presented in the manuscript. There is still no motivation given for the analysis (Why separate positive and negative PDO years and perform an EOF ?!?). In addition, the interpretation of according results is more than speculative.

Reply: Thank you for your important suggestion. We removed this section as you suggested. Our ideas in this section demonstrate that different phases of the PDO in the Holocene are associated with Arctic dipole modes. We separate PDO in different phases and perform EOF to explore the relationship between PDO phase and the Arctic Dipole. We conclude that the positive phase of the PDO is dominant in the early-mid Holocene and the Arctic Dipole is weak. In the Late Holocene, the PDO negative phase is dominant, and the Arctic dipole is stronger. We argue that The potential phase of the PDO dominates the SLP to form the Arctic dipole mode. The PDO is also closely related to the orbital forcing. Therefore, it is speculated that the ORB orbital forcing affects the Arctic dipole by modulating the PDO phase, and then contributes to the asymmetry of the Arctic temperature.

4 Discussion
1st paragraph, l. 699 new version: “are responsible” – I suggest to reformulate to “are an important factor”
Reply: Thank you for your suggestion. We've modified it.

l 777: please re-formulate “new insight” to “additional insights on Holocene time scales” – The study is not the first one addressing climateteleconnections in the Arctic realm.
Reply: Thank you for your comments. We've modified it.

l. 780: please remove the sentence “These results can have useful implications on predicting…” - The study has no dedicated chapter on predictability of the Arctic climate. The basic processes authors motivate have been already published. (e.g. the importance of sea ice and atmospheric circulation on regional Arctic processes).

Reply: Thank you for your helpful comments. We've modified it.

Supplementary:
Fig. S3. Please also carry out according statistical significance tests for the plots and the numbers mentioned in the Figure caption.

Reply: Thank you for your suggestion. We've modified it.