Replies to Editor:

Thank you very much for these constructive comments. We have revised the manuscript carefully based on these suggestions. The followings are our point-to-point replies.

1. *To ensure that the comments of reviewer 1 have adequately been addressed, I still think some more information could be provided as to the role of tropical diabatic heating for the NH response. For the SH response you show these experiments that highlight the role of the tropical SSTs. I wonder if you could just emphasize whether or not these experiments reveal a role for topical SST changes for the NH. If not, then a simple statement to that effect may help in the NH section (I don't think you need to show if there's no effect).*

Responses: Thank you very much for this suggestion. The tropical diabatic heating plays a negligible role in the NH response. We would like to emphasize that the strong NH response is caused by the TP perturbation (Fig. R1a, c). We have done an AGCM experiment to show the NH response to the tropical forcing (Fig. R1). Given the tropical SST anomaly in the AGCM (Fig. R1e), there is nearly no response in the NH (Fig. R1b, d), suggesting the tropical SST has a trivial role in the NH response. We added this statement in line 381 of the revised manuscript "*We want to emphasize that the tropical SST changes contributes little to the response in NH.*"

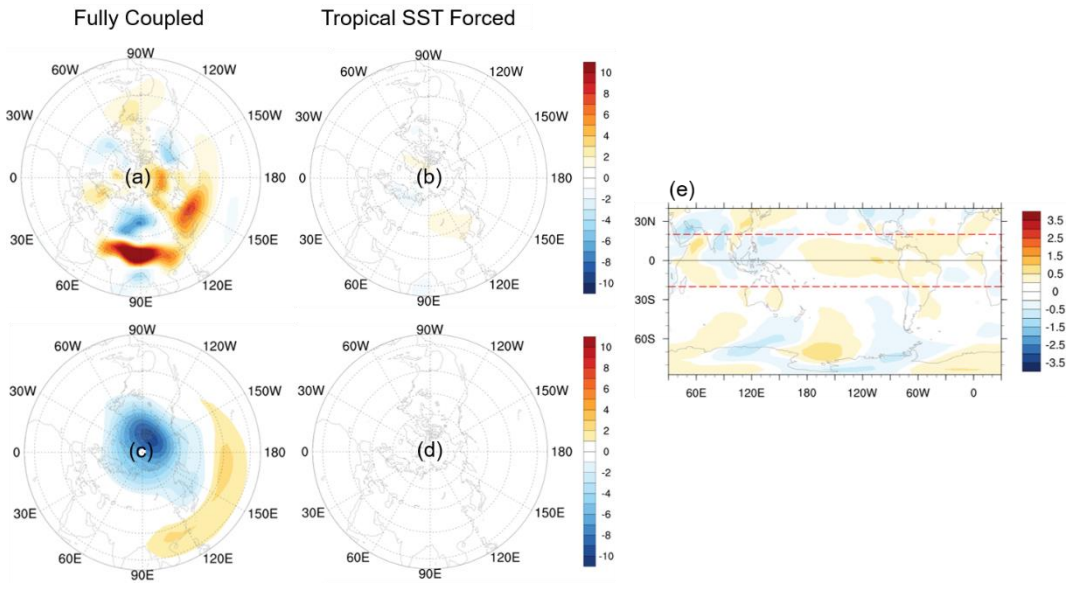

Fig.R1 Changes in (a)-(b) SAT and (c)-(d) temperature averaged over 100-200 hPa from the fully coupled runs (a, c) and tropical SST-forced AGCM runs (b, d), averaged over years 1-100. Dashed red lines in (e) shows the tropical SST forced region.

2. *l36 & 37: You state that the pathways are northeastward at the upper levels while they are eastward and northward at the lower level, as though these two things are different. It may not be totally clear to readers what the difference is between "northeastward" and "eastward and northward". Suggest some rewording to capture whatever the important difference is here between the upper and lower levels.*

Responses: Thank you very much for this suggestion. We have revised the pathways in upper level is northeastward directly, while in lower level, is eastward and then northeastward.

3. *l120/122/125 and possibly elsewhere: "sensitive" --> "sensitivity"* Revised.
4. *l150: "experiments has" --> "experiments have"* Revised.
5. *l167-168: You say that for visual clarity you don't show significance testing. But I think you should probably make some statement about whether all of the features considered are significant. Most other studies are able to produce figures that are visually clear and can show the significance, so I think a preferable option would be to actually show the significance.*

Responses: Thank you very much for this suggestion. We show the significance in Fig.2 and Fig.8 in the revised manuscript to show the response in most NH and SH region is significant at 95% confidence level, and for visual clarity we do not show the significance in other figures in each section.

We add statement in line 201-203 "*In Fig. 2 most changes are significant at the 95% confidence level, suggesting the robust connection between the TP and the Arctic. For visual clarity, we do not show significance test in other figures in this section.*"

Line 360-362 "*Changes in the SH austral winter are significant at the 95% level, based on the Student t-test, while they are not significant in the austral summer. For visual clarity, we do not show significance in other figures in this section.*"

6. *l189: I think some of reviewer 1's main comments stemmed from the fact that they did not realize that the longer timescale features of these particular experiments have already been discussed in prior studies. But I think that there are still places where you could be clearer about this. For example, l189: you say that these aspects were reported in detail in Yang and Wen (2020) but it's not clear that Yang and Wen (2020) discussed these particular experiments. Suggest "was reported for these same experiments in detail in Yang and Wen (2020)" (or something to that effect).*

Responses: Thank you very much for this suggestion. We have revised line 189 as "*which was reported for these same experiments in detail in Yang and Wen (2020).*"

7. *l285: this sentence is incomplete*

Response: Sorry. We have revised this sentence "*First of all, in the winter pattern of atmospheric circulation changes, there is a large-scale anomalous low above the TP, accompanied by an anomalous high downstream of the TP*"

8. *l385: "panes" --> "panels"* Revised.

9. *l486: Again, to address reviewer 1's concerns, perhaps here in the summary and discussion you could re-emphasize that the slower ocean processes and equilibrium responses are investigated in these same experiments in other papers.*

Responses: Thank you very much for this suggestion. We re-emphasized in Line 491-493 "*The slower ocean processes and equilibrium responses are investigated in these same experiments in our previous papers (Yang et al. 2020; Yang and Wen. 2020)*"

10. *l557-566: I'm still not really satisfied with your response to my comment on this aspect. This linkage to the response to global warming seems like a wild extrapolation to me and I suggest it be removed. You don't really know what the thermal effects of the Tibetan plateau are in isolation here - the mechanical effect must be huge. It seems like too much of a leap to make such claims about what will happen under global warming. This doesn't seem like a key point to your manuscript, so rather than making unsubstantiated claims, I suggest removing this, unless you can provide better justification for it.*

Responses: Thank you very much for this suggestion. We removed the line 573-582 and rewrote the line 44 as "which may help to understand the TP role in the future climate changes in polar regions."

Replies to Reviewer #3:

Thank you very much for these constructive comments. We have revised the manuscript carefully based on these suggestions. The followings are our point-to-point replies.

1. *In the numerical experiments — "OnlyTibet" and "NoTibet" with and without orography of TP, a large region (including some parts of East Asia, Russia, Mongolia) is considered as TP. However, the TP does not, by definition, include those regions. So, instead of taking the large extended region as the Tibetan plateau region and it is better to stick with the definition of TP and accordingly perform the simulation to examine the objectives.*

   One severe implication of this is — previous studies have shown that Mongolian mountains have an effect on the upper-level winter jet stream. The Mongolian mountains influence stratospheric flow twice as much as the Tibetan plateau and Himalayas (White et al.2018). Since the considered region also include Mongolian mountains, therefore, the impact also might include a major contribution from Mongolian mountains.

Responses: Thank you very much for this suggestion. Two years ago, we had carried out an experiment with only TP removed, called No_TibetOnly (Fig. R2a), while the Mongolia Plateau (MP) is unchanged. We found that the circulation change in the NH upper level is similar to that in NoTibet, for example, the strengthened westerlies, the cooling center in upper level over Arctic and the deepening of polar low (Fig. R2c, d). What's more, the reduced upward wave propagation in No_TibetOnly is nearly identical with that in NoTibet (Fig. R3), suggesting that the TP plays a major role in upper-level winter circulation in our model experiments. We had also examined the TP-Antarctic teleconnection in No_TibetOnly and the results is consistent with that in NoTibet (Fig.R4). Therefore, result from our experiments is mainly caused by TP.

We know some previous studies pointing out the important role of MP on upper-level wintertime jet stream (Shi et al. 2015; White et al. 2017, 2018). White et al (2018) used the WACCM within the CESM1.0.6 framework, with a horizontal resolution of 2.5°×1.9°. We use the different model version (CESM 1.0) and resolution (3.75°×3.75°). More importantly, previous studies used orography experiments from AGCMs, while we use fully-coupled models with full ocean dynamics. This may lead to different model results. We examined the ocean circulation in NoTibet and No_TibetOnly (Fig. R2b) and we found the AMOC changes in these two experiments are similar, suggesting that the TP is important to the AMOC. For the PMOC, although the TP is the

most important, the MP does play a role, because MP can affect wind-driven part of the PMOC through its role in the Pacific atmospheric circulation, which is consistent with White et al. (2017). We want to emphasize that in the fully-coupled experiments, it is the TP that plays the most important role in the polar regions and ocean circulation, and the MP plays a minor role. We can still use these idealized orography experiments to analyze the TP's influence. We will study the role of MP thoroughly in coupled models in the near future. Anyhow, more experiments should be used to identify the different role of TP and MP in the future

In line 572-577, we added "*Some previous studies figure out the MP plays a vital role in the NH upper-level wintertime circulations (White et al. 2017) using AGCM. We have performed coupled model experiments with only TP region removed (60°-130°E, 20°-45°N), in which the response is similar to that in NoTibet, suggesting the TP's dominant role in polar region. Note that previous studies used AGCM and we use coupled models with full ocean dynamics, which can better study the TP's role on global climate.*"

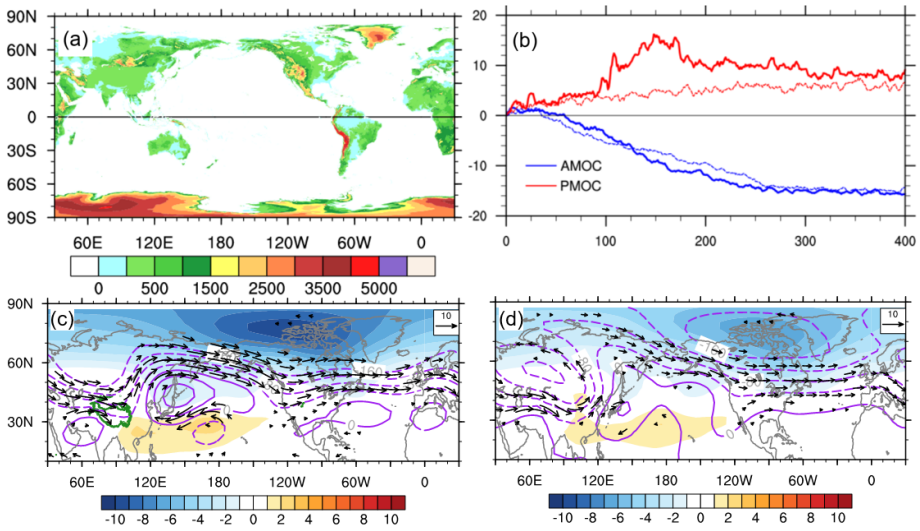

Fig.R2 (a) Topography configuration without Tibetan Plateau (60°-130°E, 20°-45°N) (**No_TibetOnly**). (b) Temporal evolutions of the PMOC (red) and AMOC (blue) in NoTibet (solid curves) and No_TibetOnly (dashed curves). (c) Winter circulation changes (years 1-100) averaged over 100-200 hPa in NoTibet with respect to Real. (d) is for No_TibetOnly. Contours represent geopotential height change (units: m), shading is for temperature change (units: °C), and vector is for wind change (units: m/s).

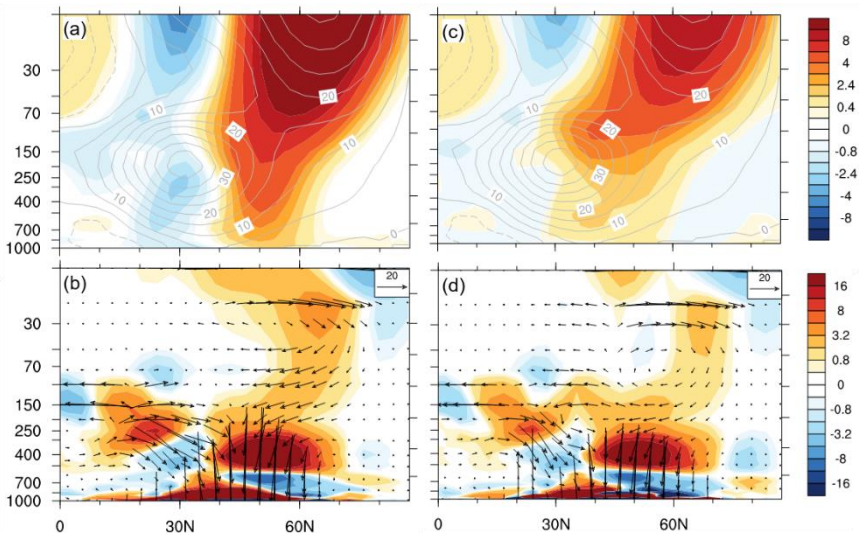

Fig.R3 Quasi-equilibrium winter changes in (a) zonal mean zonal winds (color; m/s) and (b) EP flux (vectors; m^2s^{-2}) and its divergence (color; ms^{-1}) averaged over years 1-100. (a, b) is for NoTibet and (c, d) is for No_TibetOnly.

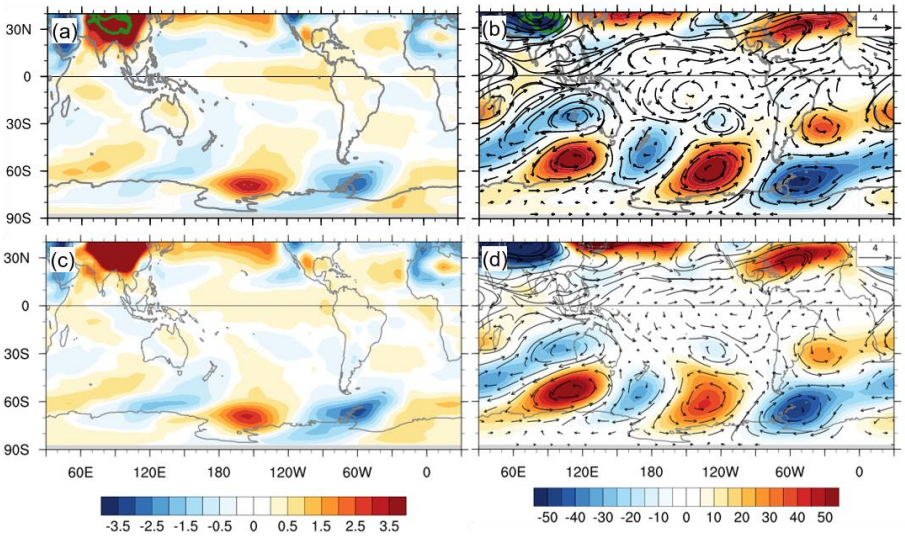

Fig.R4 Quasi-equilibrium winter changes in (a) SAT (color; °C) and (b) wind (vector; m/s) and eddy geopotential height (color; m) at 200 hPa averaged over years 1-100. (a, b) is for NoTibet and (c, d) is for No_TibetOnly.

2. *As far as I understand, all figures in this manuscript are produced with model resolution (3.75°×3.75°), using some interpolation method. What is the resolution of topography in Figure1? Please clarify.*

Responses: Thank you very much for this comment. In Fig. 1 we use a high-resolution topography data with horizontal resolution of 0.17°×0.17°, to make the topography look clearer.