Replies to Reviewer #1

Thank you very much for your encouraging comments and specific suggestions. The reviewer has three main concerns as follows.

"(1) dedicating over one third of your results section to results submitted in more detail elsewhere seems unreasonable. Many previous studies are cited that have looked at the effects of the Tibetan plateau, largely on Asian climate, (2) but few details are given on what those studies show, and how these results build on this literature. Lastly, despite strong claims that these results can be connected to the real World, (3) there is no discussion of the biases in the CTL simulation of the model, of consideration of how such biases may affect the results, or on whether the results may be sensitive to the rather low resolution of the model. I would recommend publication only if the following revisions can be completed satisfactorily."

We have revised the manuscript carefully based on these suggestions. Please see our point-to-point replies below.

Major comments.

1. *As with all models, the CESM has inherent biases. You need to provide either a section that compares your CTL simulation to observations, or a citation to a study that does. In either case you should discuss the performance of the model for metrics of interest (e.g. stationary wave strength, AMOC strength, deep water formation regions, cross-equatorial heat transport separation between atmosphere and ocean, a double ITCZ bias…), and discuss how you anticipate these biases may impact your results.*

Response: Thank you very much for this suggestion. We now discuss briefly the model bias and the resolution problem in the discussion section. **(1)** The model bias of CESM with respect to the observations has been examined comprehensively in previous studies (https://journals.ametsoc.org/topic/ccsm4-cesm1). **(2)** In our previous studies using CESM, we also examined the model bias carefully. We are sorry that we did not present such comparison in the manuscript. **(3)** In general, compared to the observations, the CESM can simulate a pretty good mean climate and ENSO variability. The following figures provide comparison between CESM results and observations in several aspects.

Figure R1 shows the meridional overturning circulation (MOC) in the Atlantic and Pacific from our CESM control simulation, and the MOC from ECCO-v4 ocean reanalysis. We can see that they are mostly consistent. For both datasets, the AMOC has a maximum value of about 15-20 Sv, at depth of 1000-1500 m; the shallow MOC, or the wind-driven subtropical cell in the Indo-Pacific basin has symmetric structure and a maximum value of more than 30 Sv at about 100 m. Note that the vertical coordinates in Figs. R1a and b have different scales from that in Fig. R1c.

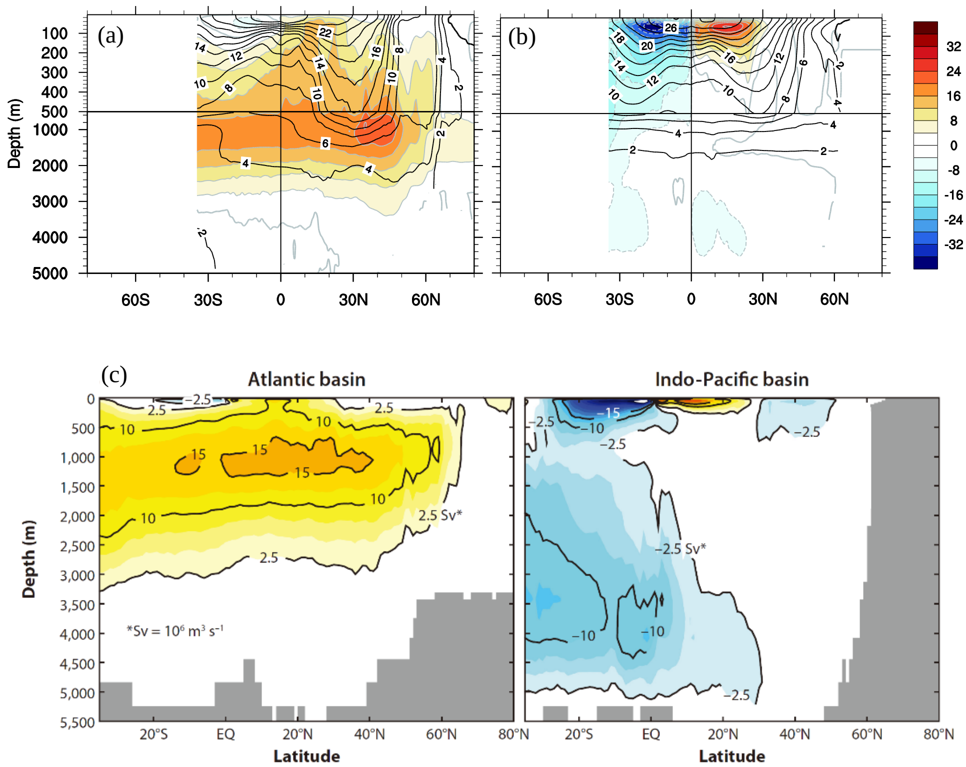

Figure R1 The meridional overturning circulation (MOC) (color shading; units: Sv) and potential temperature (contour interval=2°C) for (a) the Atlantic and (b) the Indo-Pacific from the CESM control simulation with grid of f19_gx1v6. (c) MOC for the Atlantic (left) and for the Indo-Pacific (right) from the ECCOv4 ocean reanalysis (Forget et al., 2015; Ferreira et al., 2018)

Forget G, Campin JM, Heimbach P, Hill C, Ponte R, Wunsch C. 2015. ECCO version 4: an integrated framework for nonlinear inverse modeling and global ocean state estimation. Geosci. Model Dev. 8:3653–743.

Ferreira, D., P. Cessi, H. K. Coxall, and et al., 2018: Atlantic-Pacific asymmetry in deep water formation. Annual Review of Earth and Planetary Sciences, 46, 327-352.

Figure R2 shows an excellent agreement between the meridional heat transports calculated from CESM control simulation and the observation (dotted curves). The Earth's climate system is maintained by a hemispherically antisymmetric poleward heat transport with the peak value of about 5.5 PW (1 PW = 10^{15} W) at 40°N/S. The atmosphere heat transport (AHT) dominates poleward of about 30°N/S while the ocean heat transport (OHT) dominates in the deep tropics. These features are well recognized, and have been documented in numerous studies (e.g., Trenberth and Caron, 2001; Held, 2001; Wunsch, 2005; Czaja and Marshall, 2006)

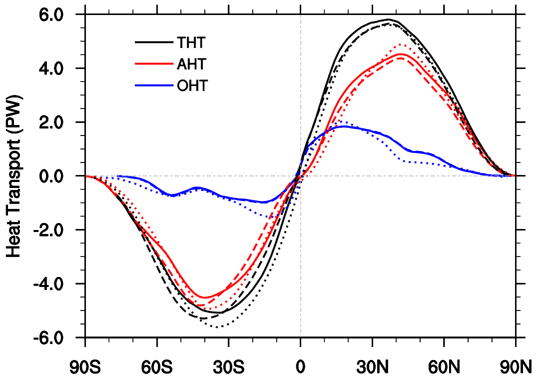

Figure R2 The mean total meridional heat transport (THT, black), the AHT (red) and global OHT (blue) (units: PW). The solid lines show the heat transport calculated directly from the velocity and potential temperature fields, while the dashed lines show those deduced indirectly from the net heat flux. The dotted lines show the observations (Trenberth and Caron, 2001). Adopted from Yang et al. (2015).

*Trenberth, K. E., and J. M. Caron, 2001: Estimates of meridional atmosphere and ocean heat transports. J. Climate, **14**, 3433–3443.*

The mean SST pattern from CESM control run (Fig. R3a) is generally in good agreement with that from ERSST-v4 (Huang et al., 2015) (Fig. R3b). The SST bias in CESM is also clear; for example, the warm pool in the western tropical Pacific is weaker and the cold tongue in the eastern tropical Pacific is stronger (Fig. R3a), when compared to the observations (Fig. R3b).

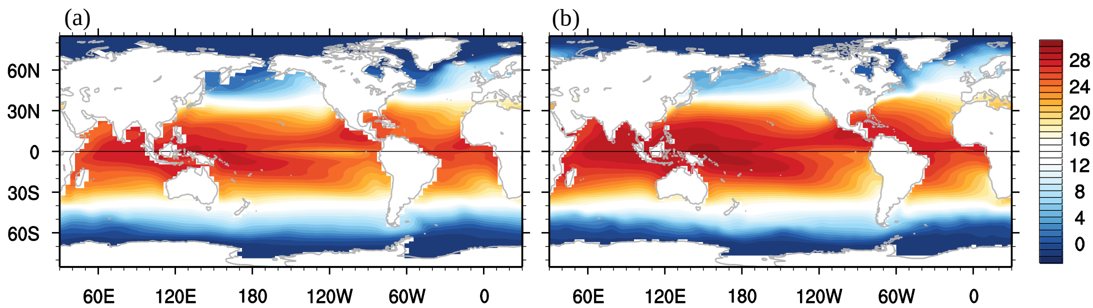

Figure R3 Spatial patterns of mean SST (°C) from (a) CESM control run and (b) ERSST-v4, averaged between 1980 and 2018 (Huang et al., 2015).

*Huang, B., and Coauthors, 2015: Extended reconstructed sea surface temperature version 4 (ERSST. v4). Part I: upgrades and intercomparisons. J. Clim., **28**, 911-930.*

We also compare the tropical climate *interannual variability* from CESM with observations (ERSST-v4) in the period of 2000-2018. Figure R4 shows the standard deviation of SST in the tropical

Pacific, which can be also seen as the ENSO variability. The magnitude of ENSO variability in CESM (Fig. R4a) and that in ERSST are comparable, with the former slightly stronger than the latter. The pattern of CESM ENSO is narrower and more concentrated on the equator, than that of the observation. In general, the CESM can well simulate the ENSO variability. More information on CESM ENSO variability can be found in in Fig. R7.

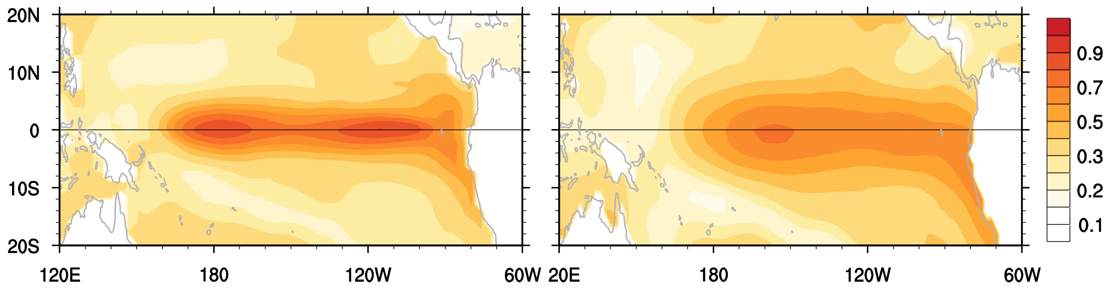

Figure R4 Spatial patterns of standard deviation of tropical SST (°C) from (a) CESM control run and (b) ERSST-v4 for the period of 2000-2018.

To know more about CESM performance and bias, we also performed a historical run of CESM, forced by the historical change of the atmosphere (including CO2, aerosol, volcanic eruption, etc.). Figure R5 shows the temperature change since 1850 from CESM and observation (Hadley Centre SST dataset: HadSST3.0, https://www.metoffice.gov.uk/hadobs/hadsst3/). We can see that the simulated global mean surface temperature (GST) and land surface air temperature (SAT) agree well with the observations. The effect of volcanic eruption on surface temperature is well reproduced in the new CESM simulation. In addition, the simulated trends of surface temperature (blue line) in several different periods are also consistent with those of the observations.

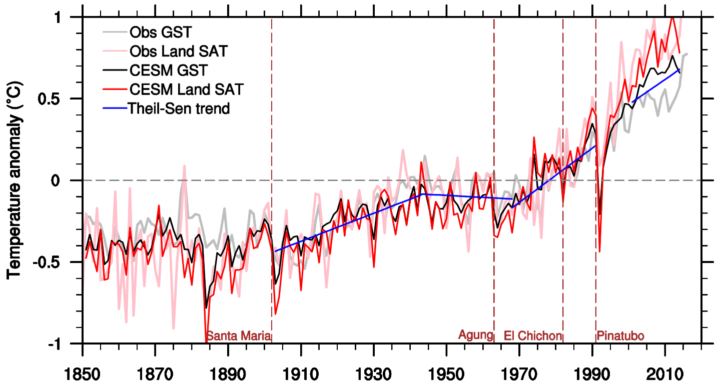

Figure R5 Evolution of global surface temperature since 1850 from the historical simulation of CESM and observation of the Hadley center SST dataset.

We also examined the model bias in simulating Arctic sea ice. Please see the reply to the next question (Fig. R6b).

2. You use a relatively low resolution model. I recognize that running such coupled simulations at higher resolution may not be possible, but you should at least acknowledge the low resolution, and discuss whether you think changing the resolution would impact your results (perhaps drawing on previous work with models at different resolutions?).

Response: Thank you very much for this concern. In one of our previous papers (Yang et al., 2015), we used the same CESM model with high resolution (1.9x2.5_gx1v6, i.e., the atmosphere model has the finite volume nominal 1.9°×2.5° in the horizontal; the ocean model has uniform 1.125 °spacing in the zonal direction, and non-uniform spacing in the meridional direction, which is 0.27 °near the equator, extending to the maximum of 0.65 °at 60 °N/S and then shrinking gradually to the poles). We compared the mean climate states of high-resolution model and low-resolution model, and found no significant differences between them, including the mean AMOC, the mean global SST and SAT, the mean meridional heat transport, and so on. It is true that the sea-ice cover in low-resolution version (red line in Fig. R6a) is more extensive than that in the high-resolution version (black line) in the Pacific and Nordic Sea. However, they are comparable in the subpolar North Atlantic and the Southern Ocean. The Labrador Sea is critical for the deep-water formation and thermohaline circulation, which is not affected by the model resolution (Fig. R6a).

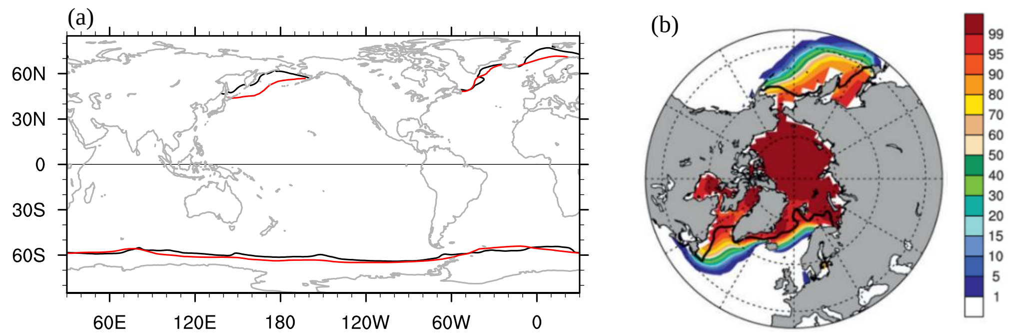

Figure R6 (a) Sea-ice extent in CESM control simulations with f19_gx1v6 (black) and T31_g37 (red). (b) Sea-ice fraction (%) for historical run of CCSM4 T31 in the Northern Hemisphere boreal winter. SSM/I observations for 10% sea-ice concentration are shown as a heavy black line for reference.

Shields et al. (2012) compared the sea-ice extent from CCSM low-resolution simulation results with the observations of SSM/I (Fig. R6b). They showed some problems with Northern Hemisphere (NH) Arctic locales where sea-ice extent and thickness were excessive, and improved with higher resolution. The relatively poor performance of the model in representing the NH sea ice compared to the Southern Hemisphere sea ice is due to different processes involved. In the NH, it is accomplished by coastal boundary currents, which are neither resolved nor parameterized. This leads to a too-small poleward heat transport in the Arctic. With higher resolution, these coastal currents are resolved, which leads to a redistribution of heat and a reduced sea-ice bias in the NH (Jochum et al., 2008).

Jochum, M., G. Danabasoglu, M. Holland, Y. O. Kwon, and W. G. Large, 2008: Ocean viscosity and climate. Journal of Geophysical Research Oceans, 113(C6).

Shields, C.A., D.A. Bailey, G. Danabasoglu, M. Jochum, J.T. Kiehl, S. Levis, and S. Park, 2012: The Low-Resolution CCSM4. J. Climate, ***25****, 3993–4014, https://doi.org/10.1175/JCLI-D-11-00260.1*

Yang, H., Q. Li, K. Wang, Y. Sun and D. Sun, 2015: Decomposing the meridional heat transport in the climate system. Climate Dynamics, *doi: 10.1007/s00382-014-2380-5,* ***44****: 2751-2768*

3. *If the effect of the TP on the AMOC is in press in other publications, as you suggest, then you should substantially shorten the section on this impact.*

Response: Thank you very much for this suggestion. The manuscript has been shortened substantially. In many places, we cut back the detail on the TP effect on the AMOC. In this revised manuscript, we cite "Yang and Wen (2019)" only in three places.

4. *The structure of the paper could be significantly improved to help the reader. Try to present a story, rather than a collection of separate results. It is a rather long paper, which may reduce the impact of the more interesting results? consider shortening by removing some more minor results, particularly those that simply confirm previously published results.*

Response: Thank you very much for this suggestion. This manuscript has been shortened substantially. (1) We took out one figure, and reduced the subplots of each figure by more than 40%, to focus on the effect of TP removal. (2) In section 5, we reordered the analyses on the ocean buoyancy change, and started the section with a bigger picture of general ocean change. Original Figures 11 and 12 were combined into one (Fig. 10) in the new version. (3) Following your suggestion, this revised manuscript focuses more on major results that are not reported in previous publications, and many minor results are removed.

5. *You have no metrics for significance or robustness of your results. Particularly for some of the smaller signals (e.g. over the tropical pacific), or for results that differ between your two experiment*

sets (e.g. southern ocean changes), you should know if these are statistically significant-use the control simulation to calculate significance and provide this as shading or masking in your plots.

Response: Thank you very much for this suggestion. We have checked carefully the significance of our results by using the Student *t*-test. Most changes caused by the TP are significant at the 99% level. Some changes, for example, changes in the Southern Ocean, need more investigation. During this revision, we focused on the linear components of changes (please see Reply to Question #8), which have strong signals. The weak signals, or the nonlinear changes, are not discussed in this revised manuscript.

6. *There are a number of causal statements which I do not feel are sufficiently backed up by the results presented. You should either prove the causal connection, or weaken your statements. See specific comments below.*

Response: Thank you very much for this suggestion. We have revised this manuscript substantially, and tried our best to avoid causal statements. We provided more details on causal connections.

7. *Details are missing on how metrics and variables were calculated - it is not necessary to include full derivations, but you should clarify what you have calculated and how.*

Response: Thank you very much for this suggestion. Please also see our replies to your specific comments. We have provided detailed information for figures and calculations.

8. *Please be consistent with whether you are talking about the effect of the TP, or the effect of removing the TP - pick one, and use it throughout. It will substantially help a reader to follow your arguments. Similarly, consider multiplying your "Flat-OnlyTibet" experiment result by -1.0, so they will match the "Real-NoTibet" figures where the response is linear. This will allow a reader to more easily see where there are differences. You could also consider putting the "Flat-OnlyTibet" figures into supplemental information, except for times when you really want to highlight differences between them (and then you should know whether those differences are internal variability, or statistically significant differences due to nonlinear interactions with other orography).*

Response: This is a great suggestion. To avoid misleading our readers and for the sake of simplicity, we focus on the linear response of global climate to the TP removal during the revision. Since the climate change in NoTibet is opposite to that in OnlyTibet, and they have roughly the same magnitude, we define the linear (or symmetric) and nonlinear (or asymmetric) changes due to the TP removal, respectively, as follows:

$$\text{Linear Change} = [(\text{NoTibet} - \text{Real}) - (\text{OnlyTibet} - \text{Flat})]/2 \qquad (1)$$

$$\text{Nonlinear Change} = [(\text{NoTibet} - \text{Real}) + (\text{OnlyTibet} - \text{Flat})]/2 \qquad (2)$$

In the revision, we focus on the linear climate change due to the TP removal. The nonlinear change is not discussed in this version. We have checked carefully and found that the nonlinear changes are very small in most regions. Also, we focus on the quasi-equilibrium (QE) response of global climate, defined as the climate change averaged over the last 100 years of the 400-year integration, namely, only annual mean climate change is investigated.

Minor comments

9. Title: 'Portraying the Impact of the Tibetan Plateau on Global Climate' reads better.

Response: Thank you very much for this suggestion. Revised.

Abstract

10. Line 22-35. Be careful with your wording here - your results are for one model, at relatively low resolution - how do you know this represents reality?

Response: Thank you very much for this suggestion. The abstract has been rewritten.

11. Line 25. Local response is really just describing the height of the TP, which isn't necessarily that interesting.

Response: Thank you very much for this suggestion. We removed this statement.

12. Line 35. I was surprised by the sudden inclusion of the South Pole - if the TP significantly impacts the climate of the South Pole then that deserves more attention than you have given it. If the impact is not statistically significant (and preferably understood and explained), then it should not be mentioned in the abstract.

Response: Thank you very much for this suggestion. We removed this statement. We found the TP does have a big impact on the climate in the Southern Hemisphere high latitudes (maybe not on the South Pole). We are actually very interested in this remote impact. We will investigate this connection in depth in our future work.

Section 1

13. Line 57-59. What is this connection between the TP and ENSO? Does the presence of the TP affect ENSO or the PDO in your simulations?

Response: Thank you very much for this concern. We do find connection between the TP and ENSO in our experiments, but the connection between the TP and PDO does not seem to be obvious. The following figure is from our next paper (***in preparation***), showing that the ENSO variability would be enhanced significantly if there were no TP (red curve in Fig. R7a), when compared with the CESM control run (black curve) and the observation (green curve). The basic idea is that removing the TP would result in weakened trade wind in the tropical Pacific, leading to surface warming (cooling) in the central-eastern (western) equatorial Pacific (Fig. R7b), a flattened equatorial thermocline and an eastward shift of atmospheric convection center. These background changes in the tropical air-sea system will enhance the Ekman pumping feedback and the thermocline feedback between the lower atmosphere and upper ocean, leading to stronger ENSO variability.

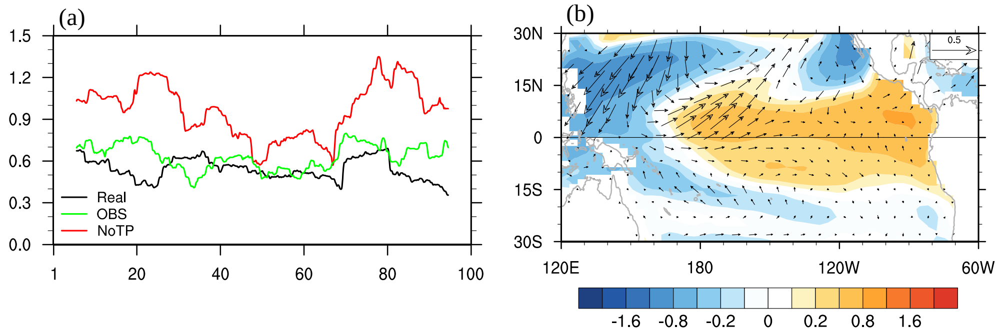

Figure R7 (a) Time series of standard deviations (SDs) of sea surface temperature (SST) anomalies from ERSST (1901-2000, green), Real (black) and NoTibet (red). The SST anomalies are averaged in the Nino-3.4 region (170°-120°, 5°S-5°N,) and filtered with a 5-85 month band-pass filter. The standard deviation is calculated from SST anomalies by applying a sliding window of 11 years. Climatological monthly mean is derived from the data for years 1971-2000 in ERSST and from model outputs from the last 100 years. (b) Quasi-equilibrium changes in SST (°C) and surface wind stress (dyne/cm^2) in NoTibet, from which the mean states of Real are removed.

14. *Line 60-61: you suggest that the teleconnection in previous studies is connected with the TP specifically - is this not what you are trying to look at?*

Response: Thank you very much for this comment. Liu et al. (2013) only suggested a connection between the East Asian monsoon and the North Atlantic. So, we removed this reference here. Cui et al. (2015) studied the impact of the SST change in the North Atlantic on the heating over the TP in a qualitative way, and focused on atmospheric processes at the seasonal timescale. In this work, we want to quantify the TP effect on the North Atlantic, and emphasize that the atmospheric processes are important at short timescale (within 100 years), while the ocean dynamics become critical at longer timescale (longer than 100 years).

15. *Line 72-84. Note that Shi et al. 2015 (Climate Dynamics), Sha et al. 2015 (JGR) and White et al. 2017 (already cited) demonstrate that the Mongolian mountains to the north of the Tibetan plateau may be as, if not more, important that the Tibetan plateau itself for wintertime stationary waves and Asian monsoons. This might be worth mentioning since these regions uplifted at different times.*

Response: Thank you very much for this comment. We have read their papers carefully. (1) They all used ***atmospheric general circulation models*** (AGCMs), not coupled models. (2) They all concluded that the Mongolian Plateau (MP) is important to ***winter*** atmospheric circulation over Asia. (3) For ***summer*** atmospheric circulation, the TP is more important. Some details from their papers are given as follows.

"Shi et al. (2015) found that the MP, despite its smaller size, exerts a great influence on the planetary-scale circulation and subtropical westerly jet. Sha et al. (2015) used the outputs from the experiments of Shi et al. (2015) and found that the MP plays a significant role in the strengthening of the East Asian winter monsoon. White et al. (2018) also found that the MP and nearby mountains have an impact on the upper-level wintertime jet stream, which is much stronger than the TP and Himalayas to the south. However, these studies all showed the impact of the MP on the ***winter*** atmospheric circulation over East Asia, and their conclusions were all obtained based on the experiments from standalone ***AGCMs***. For the ***summer*** monsoon circulation over the Euro-Asian region, they all agreed that the TP plays a more important role.”

Our conclusions are obtained based on ***coupled*** model experiments. We focus on the TP effect on ***global-scale*** atmospheric and oceanic circulations. In this revision, we added discussion on their works in section 6.

16. Line 85-93. This is a confusing section - are you suggesting that changes in the local TP climate may have subsequent changes on the global climate through the mechanisms you study in this paper? It would be a very small impact compared to the removal of the orography completely, so I’m not sure how you can relate these.

Response: Thank you very much for this comment. To avoid confusion, we moved this paragraph to the section of “Conclusions and discussion.” As you mentioned, we would like to say that the changes in the local TP climate under the global warming may cause subsequent global climate changes through possibly similar mechanisms we study in this work, although the magnitude of current climate change is very small compared to the removal of the TP. Since the TP is very sensitive to the global warming, and the warming rate over the TP during the past half century is much higher than that in the other regions, the TP’s global impact for future climate change may need more attention.

17. Line 110. Your results are interesting, but I think it is overstating the significance to suggest it will help understand future climate changes, unless we’re looking at geological timescales when the orography has changed significantly.

Response: Thank you very much for this comment. We have toned down this statement. We would like to say the global consequences of the strong warming in the TP region need to be considered seriously. This work may provide a clue to the question of how the TP can affect the global climate in the future. This is important for a better projection on future climate changes over the TP and the globe.

Section 2

18. Line 147-148. They do not entirely mirror each other - I would expect the background flow into, and *downstream of, the TP to be quite different in the two cases, which could lead to different responses. I think you can use these two experiment sets better than just describing both.*

Response: Thank you very much for this comment. NoTibet and OnlyTibet do not entirely mirror each other. As in our reply to Question 8, this revision focused on the linear part. The nonlinear part is small, and it may be interesting but we decided not to include it in this work. By focusing on the linear responses, this revision is much more improved over the previous version.

Section 3

19. *Line 155-174. This is a strange way of describing the dynamical forcing from orography. The local change in surface pressure and temperature is mostly just describing the height of the orography (as you confirm). The strength of the response is also strongly dependent on the impinging winds and background flow conditions. For example, if the Tibetan plateau was put into zero wind, there would still be a surface pressure and temperature change, but no direct dynamical forcing. I suggest you revisit Hoskins and Karoly 1981 and consider how they describe the impact of orography, and change this section to be more consistent with this.*

Response: Thank you very much for this suggestion. We would like to keep this paragraph here, because we think this is a good way to understand what the topography forcing really is, and a good way to know the forcing *quantitatively*. As you pointed out, no matter whether there is background wind or not, the local changes in temperature and pressure are always there. This is exactly what we want to emphasize. Only when the background wind exists, the local perturbation can affect the remote regions, following the classical quasi-geostrophic dynamics of Hoskins and Karoly (1981).

Knowing exactly what the local forcing is is also good for us to analyze the model results and understand the mechanism. For example, the topographic forcing is more or less similar to the CO_2 forcing, except that the former is local and strong while the latter is global and relatively weak. In many aspects of the global climate responses, they may share the same mechanisms.

Section 4

20. *Line 178-193. Given the experiments are not in equilibrium, why did you stop at 400 years? If it is for computational reasons, do you have some scientific justification that you would not get a*

different answer if you continued the experiments? Please discuss.

Response: Thank you very much for this comment. Recently, we ran these experiments for 1200 years and found the results in the later stage have little difference from that at the end of 400 years. The TOA radiative imbalance becomes very small in the later stage (Fig. R8a). In particular, the changes in AMOC and PMOC reach equilibrium states in 400 years (Fig. R8b). In the revision, this paragraph was rewritten carefully.

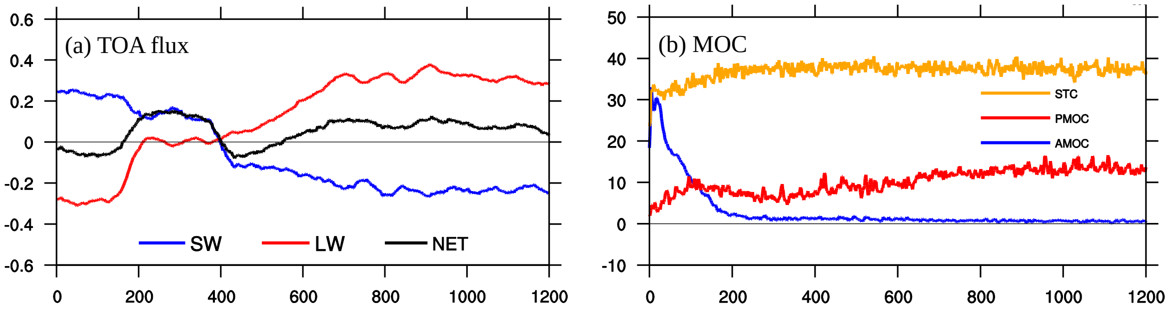

Figure R8 (a) Changes in globally integrated radiation flux at the TOA due to the TP removal. Black is for net radiation flux, blue is for net downward SW and red is for net outgoing (units: PW, 1 PW=10^{15} W, positive for downward anomaly). (b) Changes in AMOC (blue), Pacific meridional overturning circulation (PMOC) and subtropical cell (STC, yellow) due to the TP removal. Units: Sv.

21. Line 184. I think you can bring in a discussion of why the orography increases outgoing LW here, rather than waiting until line 199. Also, while it is from local surface warming, this is a little misleading - it is simply because the surface from which the radiation is coming from is lower down (and therefore warmer). Surface warming implies some circulation or cloud radiative process that has warmed the surface.

Response: Thank you very much for this suggestion. This paragraph has been rewritten. Most changes of the surface warming over the TP region are simply due to the lower topography.

22. Line 204. Why of course? Please explain. Do you know this is a nonlinear response and not just internal variability?

Response: Thank you very much for this comment. We have removed this statement for clarity in the revision, since we now focus on the linear responses. The small nonlinear responses will be presented in our next paper.

23. Line 213. Clouds are also affecting the albedo

Response: Thank you very much for this comment. We rewrote it. Here, the SW reduction is mainly due to the increases of low clouds and albedo.

24. Line 215. You do show sea ice, in figure 10.

Response: Thank you very much for this comment. We revised these two paragraphs significantly. The sea-ice margin is shown in Fig. 11b.

25. Line 233. Most of this temperature change is simply because part of the NH is higher up, which is not worth reporting. Is there a hemispheric mean temperature change excluding regions where surface height has changed?

Response: Thank you very much for this comment. The 4°C temperature change is NOT local change due to the topography, it is the REMOTE change that excludes the TP region. In Fig. 5b, the 4°C change is located nearly 60°N, while the TP region is located around 30°N. In Fig. 5a, the mean SAT cooling averaged over the whole NH is about 0.8°C. Considering the local SAT change over the TP is warming, the mean SAT cooling in the NH when excluding the TP region is nearly 1°C. The mean SST cooling in the NH is about 0.5°C as shown in Fig. 5a (since SST does not include the local TP effect).

26. Line 235. Have you proven significance?

Response: Thank you very much for asking. We have done the student *t*-test for all the linear responses. These responses can easily pass the 99% significant level. These responses are mean state changes due to a strong external forcing (removing or adding the TP), and they usually have strong signals. This is not like "internal variability," whose amplitude can be very small when compared to the mean state and is likely to fail the significance test.

27. Line 238. Do you have an explanation for why it cools and then warms? That would be interesting.

Response: Thank you very much for this question. The NH warms up first and cools later when the TP is removed. This is in fact the most important point of this paper. The key to this reversal is attributed to the ocean, more specifically, the AMOC change. In the early stage after the TP removal, the ocean has not yet responded. Because of the local warming over the TP (simply due to the lower topography), the whole NH warms up (Fig. 2a). (We have discussed that the consequence of the local warming is qualitatively similar to the consequence of the CO_2 forcing, except that the latter is a global warming. See section 3.). However, because of the changes in the atmospheric circulation and moisture (section 4.3 and Fig. 8), there would be more freshwater flux transport from the tropical Pacific to the North Atlantic, freshening the North Atlantic, weakening the deep-water formation there and eventually triggering a slowdown of the AMOC. The AMOC weakening will reduce the northward ocean heat transport, and in turn cause remarkable cooling in the North Atlantic. In the revision, we briefly discussed these processes. More detailed analyses are provided in Yang and Wen (2019). In a word, the slow evolution in the ocean thermohaline circulation leads to the surface temperature reversal in the NH.

28. Line 252. Your SH results only appear in one of your experiment sets. Are these significant? Do you have an explanation for why the Tibetan plateau only affects the SH when no other orography is present? If these results are robust, then this is interesting, and deserves more attention.

Response: Thank you very much for this concern. The SH response is significant (at the 99% level). It can also be seen clearly in Fig. 7a, in which the SAT warming can exceed 2°C near the Rose Sea of the Southern Ocean. The SH response can be seen in both NoTibet and OnlyTibet experiments. This strong remote effect of the TP on the SH needs more investigation with more model experiments. We are working hard right now to reveal these dynamics. Regardless, our experiments suggest that the presence of the TP plays an important role in the see-saw between two hemispheres, resulting in higher mean temperature in the NH than in the SH.

29. Line 253. What is the bipolar see-saw?

Response: This is the see-saw change in SAT between two hemispheres.

30. Line 259. Do you know it is purely ocean dynamics? Have you run a slab-ocean experiment to test this?

Response: Thank you very much for this concern. We have done slab-ocean experiments, and we are very confident the strong cooling in the extratropics is most attributed to the ocean dynamics, that is, due to the weakening of the AMOC. We have done parallel NoTibet experiment using a slab-ocean. Figure R9a shows the evolution of SAT change averaged in the NH, which is about 0.3°C and very stable in 200 years (does not have the reversal shown in Fig. 5a). Figures R9b and c show the patterns of SAT and SST changes averaged over years 150-200. We can see that in the North Atlantic there is weak cooling in SAT and SST, due to the sea-ice melting in the subpolar North Atlantic. They are much weaker than cooling shown in Fig. 7a and Fig. 10a.

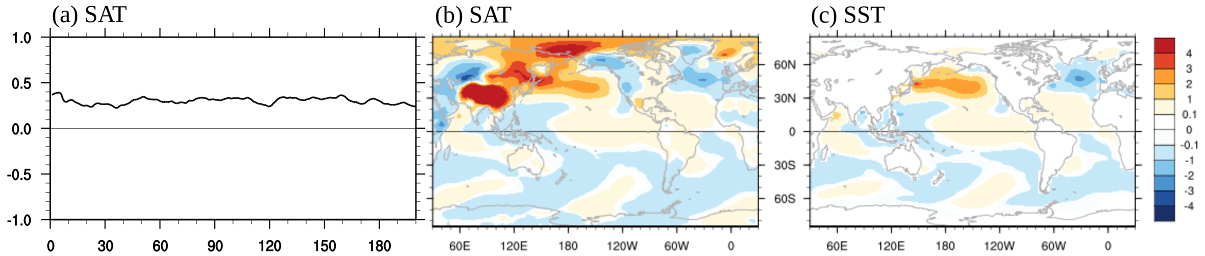

Figure R9 Surface temperature changes in **NoTibet slab-ocean** experiment. (a) Temporal evolution of SAT change averaged over the NH. (b) Pattern of SAT change averaged over years 150-200. (c) Pattern of SST change averaged over years 150-200.

31. Line 265. Again, you have tested the significance? (It almost certainly is, but you can't say that it is significant just because you have an explanation for it)

Response: Thank you very much for this concern. We have tested the significance, and it is significant. In this revision, we have tried our best not to use the word "significance." This statement is revised as "This will lead to remarkable warming in the upper-level atmosphere of the polar region."

32. Line 273. Do you know that the extra water vapor is coming from the SH, and not just increased E-P in the NH?

Response: Thank you very much for this concern. Combining Figs. 6a and 8c, we can say the extra water vapor comes from the SH in the presence of the TP. Figure 8c shows the vertically integrated moisture transport and its divergence. The divergence (convergence) is practically equivalent to the EMP (E minus P). We plot moisture transport instead of EMP in Fig. 8c because the vector field shows where the water vapor is coming from (or going to). EMP usually cannot be locally balanced, which has to be related to moisture transport.

33. Line 283. Figure 8 doesn't give a description

Response: Thank you very much for catching this. This statement is revised as "Figure 7 shows a strong remote effect of the TP on the North Atlantic. This teleconnection is established mainly through changes in the atmosphere circulations and will be discussed in section 4.3 (Fig. 8)."

34. Line 284. Again - do you have slab ocean runs to confirm that it is entirely from the dynamical ocean response?

Response: Thank you very much for this question. We have done slab-ocean experiments. Please see our reply to Q. 30 and Fig. R9.

35. Line 287-291. I don't understand this section. Please rephrase.

Response: Thank you very much for this concern. The local SAT change over the TP in the equilibrium stage is the same as that in the first year, suggesting that this is merely due to the lower topography and dominated by the lapse rate. The atmospheric humidity over the TP, on the contrary, is not determined by the local evaporation and precipitation in the TP, but determined by the moisture transport from other regions, i.e., moisture advection and its divergence (convergence). The atmosphere over the ocean can gain freshwater mostly from the ocean below, while the atmosphere over the land cannot gain too much freshwater from surface land, it has to rely on moisture advection.

36. Line 298. Figure 8 doesn't discuss anything, it's a figure.

Response: Sorry about this. We have rewritten this paragraph.

37. Line 299-308. I appreciate the attempt to put the results in the context of previous work, however, *this section needs more description of what these previous studies did, and how the current one is different. If you just agree with previous studies, how is yours new?*

Response: Thank you very much for this comment. We have rewritten this paragraph as follows.

"Here, we compare briefly our results with those in previous studies. The pattern of SAT change in response to the TP removal (Fig. 7a) is consistent with previous studies (Kitoh et al., 2002; Maffre et al., 2018). Different from previous studies, we emphasize in this work that the SAT change over the North Atlantic is mainly determined by ocean dynamics. It is straightforward that the change of orography will result in change in global precipitation pattern. For the local precipitation change, The implied precipitation change by surface specific humidity (Fig. 7b) around the TP and east Asia is consistent with the conclusions of Boos and Kuang (2013), Fallah et al. (2016) and Maffre et al. (2018). In particular, when the TP is removed, the moisture over the South China decreases, consistent with recent studies of Fallah et al. (2016) and Maffre et al. (2018). The moisture increase in the north rim of TP (Fig. 7b) is also consistent with early studies of Manabe and Broccoli (1990) and Broccoli and Manabe (1992). For the implied remote precipitation change over the North Atlantic, we will show that it is mainly due to the moisture transport from the tropical Pacific along a so-called atmospheric river passage in next section."

38. *Line 299-301 the Southern ocean changes only occur in one of your experiments (Flat-OnlyTibet). Are you suggesting that changes in the Southern Ocean seen by Sinha et al 2012 are from the Tibetan plateau, and not Southern Hemisphere orography? If so, this is a very important result, and you should provide more evidence, particularly given you don't see it in Real-NoTibet. If not, then take out this sentence.*

Response: Thank you very much for this concern. In NoTibet minus Real, SST change in the Southern Ocean is also clear (old Fig. 11a, Fig. R10a). For the SAT change, the local topography change should be the main reason as shown in Fig. 2a, since the lapse rate dominates the local air temperature. For the SST change, we are not sure about relative contributions of the local land height and remote topography. We are running more sensitivity experiments and hope to answer this question in the future. In this study, we only state that the TP can lead to remarkable change in the Southern Ocean. This does not exclude the contribution of other topography. Whether the changes in the Southern Ocean found in Sinha et al. (2012) can be exclusively attributed to the TP needs to be investigated. In general, we found the TP can affect the remote SH but the detailed dynamics is not clear right now. We have revised this paragraph carefully following this suggestion.

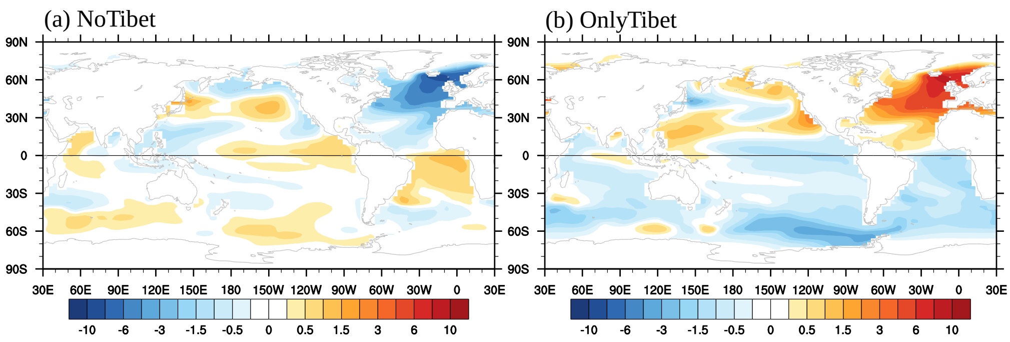

Figure R10 Time mean SST changes in (a) NoTibet and (b) OnlyTibet.

39. Line 314. I see eastward, not northeastward.

Response: Thank you very much for this concern. Figure 8a shows the northeastward propagation. The TP is located around 30°N, while strong responses are seen near 60°N in the North Pacific and subpolar Atlantic. The structure of the wave train in the NH shows a northeastward propagation of the wave energy, i.e., the group velocity, which establishes a robust teleconnection between the perturbation over the TP around 30°N (Fig. 2a) and the atmosphere circulations over North America around 40°-70°N (Fig. 8a).

40. Line 318-325. I don’t understand why this is included - do you use this at all?

Response: Thank you very much for this concern. We would like our readers to understand why the signals propagate northeastward. The dynamics is pretty classic. The propagation direction is roughly determined by the ratio of meridional group velocity and zonal group velocity, which in turn is determined by the ratio of zonal and meridional scales of the perturbation (Pedlosky, 1987). So, $\tan\theta = \frac{c_{gy}}{c_{gx}} \sim \frac{l}{k} \sim \frac{L_x}{L_y}$, where $(c_{gx},\ c_{gy})$, $(k,\ l)$ and $(L_x,\ L_y)$ are zonal and meridional group velocities, wave numbers and horizontal scales of perturbation, respectively. Given comparable L_x and L_y of the TP, the energy propagation is northeastward (Fig. 8a).

41. Line 328-344. This could be described much more succinctly.

Response: Thank you very much for this suggestion. We have tried our best to make this paragraph succinct. In this revision, we removed the description of annular mode, because this needs more investigation.

42. Line 345-352. This paragraph seems out of place, and doesn't provide enough context. What does the paleorecord show? How is this connected to the presence of the TP?

Response: Thank you very much for this suggestion. We would like to say that the teleconnection between the TP and other places is well recognized. Paleo-records also support the teleconnection between the North Atlantic and East Asia including the TP (Liu et al., 2013).

43. Line 349. How do you know the ocean process does not feed back onto the teleconnection through changing the background flow in which the Rossby waves propagate?

Response: Thank you very much for this concern. We conclude that the ocean process does not feedback to the teleconnection too much, because the teleconnection pattern does not change too much with time. This is the key point we want to emphasize in this paper. This feature is very useful for separating the roles of atmosphere and ocean processes in a fully coupled climate model. Figure R11 shows the atmospheric changes in Stage-I and Stage -II, which are found to be nearly identical. This suggests that the ocean process does not feed back to the teleconnection. In other words, the TP affects ocean circulation and buoyancy fields mainly via atmospheric processes. This is discussed further in section 5.

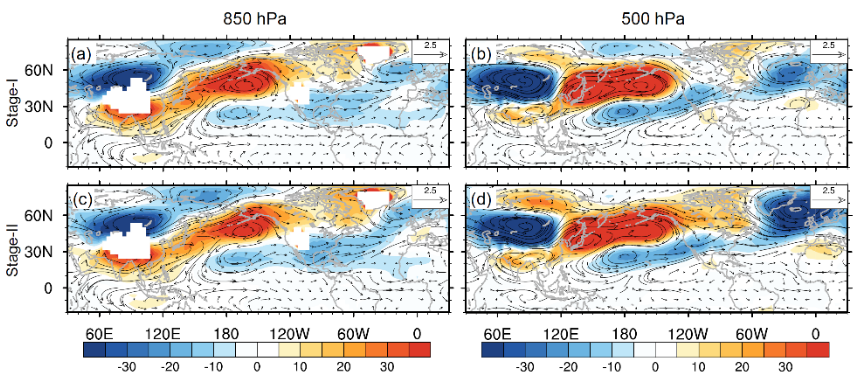

Figure R11 Changes in geopotential height (shading; m) and wind (vector; m/s) at (a, c) 850 hPa and (b, d) 500 hPa in NoTibet with respect to Real. Top panels are for Stage-I, and bottom panels are for Stage-II.

To better see the wave structure, the zonal-mean value of geopotential height has been removed. Stage-I is from model years 10-50 and Stage-II is from model years 300-400. After Yang and Wen (2019).

44. *Line 353. "Can thus be modulated…" this suggests the previous paragraph proved this, or was at least connected. I don't see the connection.*

Response: Thank you very much for this comment. Revised.

45. *Line 366. Where becomes drier? If you say vertical moisture convergence equals E-P over oceans, you are assuming no change in atmospheric moisture content.*

Response: Thank you very much for this comment. The atmosphere over the North Atlantic becomes drier, since atmosphere freshwater (rainfall) goes into the ocean. This leads to freshening of the North Atlantic. Yes, we assume the atmospheric moisture content does not change too much in an equilibrium state. This paragraph has been carefully revised.

46. *Line 370. South Asia becomes drier - I don't see this, where do you mean by South Asia? Again, are these differences statistically significant?*

Response: Thank you very much for this comment. We revised this statement: "the atmosphere over the tropical Indian Ocean (30°-100°E, 20°S-20°N) will become drier ($\nabla \cdot \vec{v}q < 0$), because of a weakened (i.e., southward) cross-equatorial flow (Figs. 8a, c)." These differences are significant at the 99% level, based on the student *t*-test.

47. *Line 361-375. changes in ocean surface humidity most likely because of changes in temperature? Does RH change?*

Response: Thank you very much for this concern. We would like to say that the change in ocean surface humidity is mostly due to atmospheric moisture transport. The temperature change may also play a role. We have not done detailed diagnosis to pinpoint the contribution of temperature change to surface humidity. Since the saturated specific humidity is also reduced over the North Atlantic because of the lower SST, the relative humidity change over there is small.

48. Line 380. “Well recognized…” please give citations

Response: We revised this statement and provide a citation: “Because of its huge heating effect on the upper atmosphere in the boreal summer (Wu et al., 2012a), the presence of the TP can shift the convective center over the Indian-Asian-Pacific region toward the NH.”

49. Line 381. How are you calculating these changes? Line 387-389 suggests you are calculating percentage changes at each point? You should use an established metric for Hadley cell strength and provide a citation.

Response: Thank you very much for this comment. The percentage change is calculated based on changes in maximum streamfunction.

50. Line 399. Shaping (not sharping)

Response: Sorry. Revised.

Section 5

51. Please start with the bigger picture result, and then go into more details. Also, if this is already published, or in press, this section can be significantly shorter!

Response: Thank you very much for this suggestion. We have revised and shortened this section substantially. We have moved section 5.2 to section 5.1, and combined the old Figs. 11 and 12 into the new Fig. 10. The discussion on surface buoyancy flux is moved backward, following the discussion on the ocean SST, SSS and SSD.

Also, following your suggestion, section 5 now starts with a bigger picture: “Removing the TP in the Asia can lead to strong surface cooling and freshening in the remote North Atlantic Ocean. In fact, in our experiments we found as much as 8°C cooling and 4 psu freshening in the North Atlantic. The surface freshening can eventually result in about 3 kg/m^3 decrease in the surface density, strong enough to even shut down the AMOC."

52. Line 409-422. This is just a description of a figure, with no significance testing. Please provide some dynamical or physical explanation for robust changes.

Response: Thank you very much for this comment. We have removed this paragraph.

53. Line 445. This suggests the sea ice changes because of advection of the ice by the ocean, not melting of ice by heat advection in the ocean. Is this what you mean? Can you prove which mechanism is occurring?

Response: Thank you very much for this comment. We think both mechanisms are important. The time sequence of the ocean processes occurred in the North Atlantic is: *weakening of AMOC due to surface freshwater flux + enhanced westerlies → southward movement of subpolar sea ice → melting of sea ice south of GIN seas → further weakening of the AMOC*. The sea-ice melting is not due to heat advection. The in-depth investigation on these processes is in Yang and Wen (2019). In this paper, we do not provide these details. Figure R12 shows the sea-ice change when the TP is removed. Accompanying the southward advection of sea ice, the sea ice melts, providing freshwater to the ocean, which furthers the weakening of the AMOC.

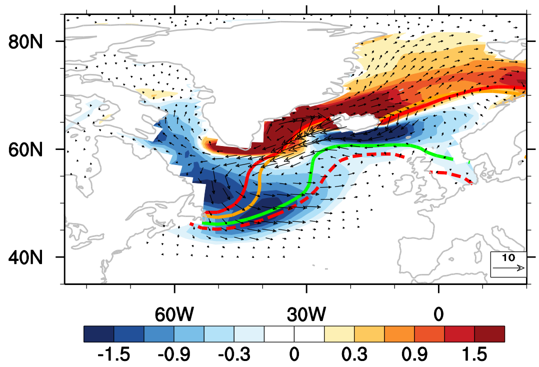

Figure R12 Quasi-equilibrium changes in sea-ice formation (color, psu/year) and sea-ice velocity (vector, cm/s). Positive (**negative**) value means sea-ice formation (**melting**). Solid and dashed red curves represent the sea-ice margin in Real and NoTibet, respectively. Orange and green curves show the sea-ice margin in the 100th year and 200th year, respectively, in NoTibet. The sea-ice margin is defined by the 15% sea-ice fraction in the Atlantic. Adopted from Yang and Wen (2019).

54. Line 451: I don’t think that you have shown that the remote effect is due to the local heating effect, and not a dynamically driven stationary wave?

Response: Thank you very much for this comment. We wrote that *in response to* local heating. The atmospheric processes convey the local forcing to the remote region via stationary waves. We have rewritten this paragraph carefully.

55. Line 481-484. Can you be more quantitative here? The SST pattern is also broadly similar.

Response: Thank you very much for this suggestion. Quantifying the impact is the core target of this work. Since the SST cooling should lead to SSD increase, the SSD decrease here can only be caused by SSS decrease. Therefore, although the SST pattern is broadly similar to the SSD pattern, it is not the reason for the SSD decrease.

56. Line 549: have you shown weakened storm activities? Have you proven this connection?

Response: Thank you very much for this question. To answer this question, we added one plot of AHT change due to eddy activities and mean circulation in Fig. 13. We found the weakened AHT in the NH high latitudes is not due to storm activities, but the mean circulation change. Please see the new Fig. 13b. Thank you very much for pointing out this mistake. This paragraph is rewritten as follows.

“In the NH high latitudes, the northward AHT is also reduced, same sign as the OHT change. This is due to the weakened meridional winds in the mid-high latitudes, which reduces the northward dry-air static energy transport (Solid black curve in Fig. 13c). It has been shown in Fig. 8b that, without the TP, the westerlies in the mid-latitude are enhanced while the meridional winds are weakened. Also, it is noticed that the northward AHT in the mid-high latitudes due to eddy activities are enhanced (dashed curves in Fig. 13c). This can be attributed to the enhanced northward Rossby waves group velocity (Fig. 8a). In general, the weakened OHT and AHT in the mid-latitudes contributes to the 4°C cooling in the extratropical NH (Fig. 5b). And the weakened northward moisture AHT (solid blue curve in Fig. 13c) across the equator contributes to the dry climate in the NH. In other words, the presence of TP would result in a warmer and wetter NH, by enhancing both northward OHT and northward atmospheric moisture transport.”

Section 6

57. *Line 560: 'State-of-the-art' is perhaps not accurate for such a low resolution model these days.*

Response: Agree. This word phrase is removed.

Figures

All figures are replotted! In the previous version, we plotted figures for both NoTibet and OnlyTibet. In this revision, we only plot figures for linear responses (please see section 2). Therefore, total pieces of subplots are reduced by more than 40%.

58. *Figure 2. I'm not sure how useful it is to show SAT and surface pressure - the differences in SAT are largely because of the change in surface pressure (due to altered surface elevation). Could you subtract the lapse rate effect from the SAT fields?*

Response: Thank you very much for this suggestion. The local SAT and SLP changes in the TP region are mostly due to the lower surface elevation; this is also the lapse rate effect. Figure 2 is usually to let us know ***quantitatively*** the magnitude of each forcing.

59. *Figure 4. Which color bars are for which plots? Do figure a and d have the same color bar but different units? As discussed, I would consider putting the majority of the OnlyTibet results into supplementary material, except when they show a significant difference from NoTibet results. You could also multiply the OnlyTibet results by -1.0 to allow a easier comparison with NoTibet.*

Response: Thank you very much for this suggestion. This figure is replotted, showing only the linear responses to the TP removal. Subplots on the left and right share the same colorbar on the right side, but with different units.

60. *Figure 5. Is this Global mean, or NH? The caption says global mean, but your text says NH (line 236)*

Response: Thank you very much for this question. Figure 5a is for NH mean.

61. *Figure 7. I don't like the apparently random placement of the numbers on the plots. In particular the positive values are to over a region where there is little to no warming. Whilst I see that you have done this so they are visible, it's a little confusing to have them there. Please add some significance shading to plots like these, especially since you talk about relatively small changes, such as those in the tropical Pacific.*

Response: Thank you very much for this suggestion. We have replotted this figure and fixed the problem.

62. *Figure 9. Again, the numbers given on the plot are in confusing locations, and do not help the reader to understand the plot.*

Response: Thank you very much for this suggestion. We have replotted this figure and fixed the problem.

63. *Figure 10. The caption does not explain the solid/dashed lines (they are explained in the text, but this should also be in the caption). What is the threshold cut-off for sea ice edge? Is the sea ice for winter in each hemisphere, or annual mean. In either case, there's clearly a strong bias in the southern extent of the sea ice. This should be evaluated, quantified, and, given sea ice plays a role in the mechanisms you discuss, you should consider how this may influence your results.*

Response: Thank you very much for this suggestion. We have replotted this figure and fixed the problem. Figure 10 in the previous version is now Fig. 11 in this version. The sea-ice margin is defined in the figure caption. The sea-ice change is for annual mean. Please see our reply to Q. 2 (Fig. R6b). We discussed the model bias, including the sea-ice bias.

64. *Figure 13. I can't read the numbers on the contours, making it hard to compare with observed values. How does it compare? What are the biases and how might they affect your results. In addition, what are the biases in the regions of deep water formation?*

Response: Thank you very much for this question. Now, it is bigger and more clear in Fig. 12. Please see our reply to Q. 1 (Fig. R1). We discussed the model bias, including the MOCs.

Replies to Reviewer #2:

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

Many previous studies have demonstrated important role of Land surface and atmospheric heat source/sink over the Tibetan Plateau (TP) in driving the local and large-scale circulation and weather systems. However, so far the global climate effect of the Tibetan Plateau still remains unclear. In this manuscript, two groups of experiments by using the full coupled CESM1.0 climate model are conducted to investigate this key point. Overall, most of the conclusions are acceptable due to the reasonable experiment design, and the manuscript is basically well organized. I recommend this manuscript can be accepted after some modifications, and particularly two points need be further clarified. First, to what a degree the results, especially the quantitative parts, are influenced by the ***model bias*** *or* ***model dependent****? For example, can the sea ice content and SST in north and south poles can be well reproduced by the CESM1.0 model? Second, the statistical confidence level in difference fields between pairs of experiments need be shown in the all related figures.*

Response: Thank you very much for your encouraging comments and specific suggestions.

For the first question about model bias, we would like to say that the CESM can well simulate mean climate of the Earth climate system. The model bias of CESM with respect to the observation has been examined comprehensively in previous studies (https://journals.ametsoc.org/topic/ccsm4-cesm1). In our previous studies using CESM, we also examined the bias of CESM carefully.

Please also see our replies to Reviewer #1's Qs. 1-2, where we provided six figures and compared the CESM results with the observations in several aspects: the AMOC and PMOC (Fig. R1), the meridional heat transport (Fig. R2), the global mean SST pattern (Fig. R3), the ENSO pattern in the tropical Pacific (Fig. R4), a historical run from 1850 to 2010 (Fig. R5), and sea ice (Fig. R6).

For the question "to what extent the model bias can affect our results," we think this influence can be neglected. The results we present are differences between the control run and topography-perturbation experiment, and the model biases in these runs are comparable, so most of the model biases should not be in the differences.

We also made a brief comparison between our results and Su's recent work (Su, 2018: Simulation of the climate effect of the Tibetan Plateau uplift using a coupled general circulation model, Ph.D. thesis, Chinese Academic Sciences). Su used the same CESM as we did. Our results are generally consistent with his results.

For the concerns about the sea ice and SST in the North and South poles simulated in CESM1.0, we have checked its sea-ice simulation (Fig. R13a). Generally, the sea-ice coverage is well simulated. In the

North Atlantic, the sea-ice margin (denoted by 0.15 sea-ice fraction in Fig. R13a) is roughly aligned from the south of the Labrador Sea northeastward to the Nordic Seas. This sea-ice coverage is obtained under the preindustrial climate conditions, which is more extensive than that in current observation.

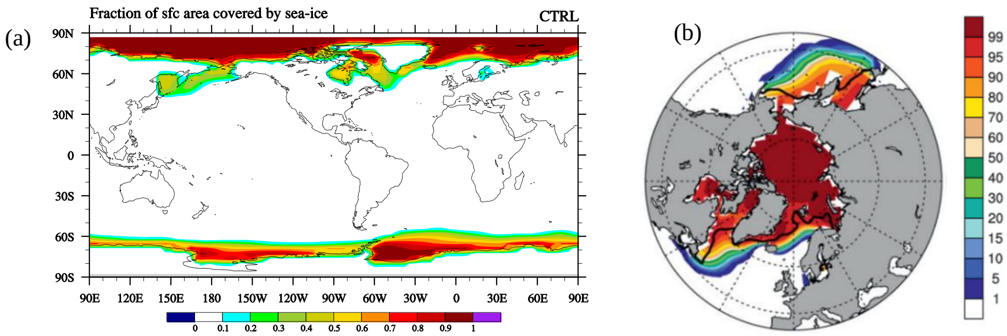

Figure R13 (a) Sea-ice coverage simulated in CESM control run. (b) Sea-ice fraction (%) for historical run from CCSM4 T31 in NH boreal winter. SSM/I observations for 10% sea-ice concentration are shown as a heavy black line for reference.

Shields et al. (2012) compared the sea-ice extent from CCSM low-resolution simulation results with observations from SSM/I (Fig. R13b). They showed that some problems existed for NH Arctic locales where sea-ice extent and thickness were excessive, and that the sea-ice extent in the Arctic improved with higher resolution. The relatively poor performance of the model in representing the NH sea ice compared to the SH sea ice is due to different processes involved. In the NH, it is accomplished by coastal boundary currents, which are neither resolved nor parameterized. This led to a too-small poleward heat transport in the Arctic. With higher resolution, these coastal currents were resolved, which led to a redistribution of heat and a reduced sea-ice bias in the NH (Jochum et al., 2008).

Jochum, M., G. Danabasoglu, M. Holland, Y. O. Kwon, and W. G. Large, 2008: Ocean viscosity and climate. Journal of Geophysical Research Oceans, 113(C6).

*Shields, C.A., D.A. Bailey, G. Danabasoglu, M. Jochum, J.T. Kiehl, S. Levis, and S. Park, 2012: The Low-Resolution CCSM4. J. Climate, **25**, 3993–4014, https://doi.org/10.1175/JCLI-D-11-00260.1*

We suspect the sea-ice bias may affect our results, but we are not sure how big this influence is. This needs further investigation with more model experiments.

For the concern about the results that may be model-dependent, we admit that it is an important issue. Using the model ECHAM5/MPI-OM, Fallah et al. (2016) studied the TP effect on the Asian summer monsoon and AMOC. Using the model ISPL-CM5, Maffre et al. (2018) studied the influence of orography on model ocean circulation. These works all showed that removing the global continental orography (i.e., in a world with a globally flat land) would lead to the shutdown of the AMOC, similar to our CESM results. At this point, different coupled models give a consistent picture of the change in terms of global overturning circulations.

Fallah, B., U. Cubasch, K. Prömmel, and S. Sodoudi, 2016: A numerical *model study on the behaviour of Asian summer monsoon and AMOC due to orographic forcing of Tibetan Plateau. Clim. Dyn., **47**, 1485-1495.*

*Maffre, P., J. B. Ladant, Y. Donnadieu, P. Sepulchre, and Y. Goddéris, 2018: The influence of orography on modern ocean circulation. Clim. Dyn., **50**, 1-13.*

For the second question about the statistical confidence level in difference fields, we have done student *t*-test for all difference fields. All these differences are significant at the 99% level. In this revision, we focused on the linear component of the change (please see our reply to Q.8 of Reviewer #1). The linear response to the TP removal is defined as

$$\text{Linear Change} = [(\text{NoTibet} - \text{Real}) - (\text{OnlyTibet} - \text{Flat})]/2 \quad (3)$$

The linear response is statistically significant. The nonlinear changes are small in most regions, and are not discussed in this version.

Specific comments:

1. *.Introduction. page 3-5. A recent review paper (Wu et al., 2015, NSR) and another work concerning on the TP thermal effect on East Asian summer monsoon (Duan and Wu, 2005, Clim. Dyn.) should be mentioned to explain the vital role of the TP thermal and mechanical forcing on climate.*

Response: Thank you very much for this suggestion. We have added their work in lines 40-48.

2. *Introduction. Lines 85-93, page 5. More literatures concerning on the rapid warming over the Tibetan Plateau need be cited here.*

Response: Thank you very much for this suggestion. Discussion on the rapid warming over the TP has been moved to section 6 in this version. The following recommended literatures are included in lines 556-583.

*Li, L., S. Yang, Z. Wang, X. Zhu, and H. Tang, 2010: Evidence of Warming and Wetting Climate over the Qinghai-Tibet Plateau. Arctic, Antarctic, and Alpine Research, **42**, 449-457, DOI: 10.1657/1938-4246-42.4.449.*

*Xu, Y., A. Knudby, H. C. Ho, Y. Shen, and Y. Liu, 2017: Warming over the Tibetan Plateau in the last 55 years based on area-weighted average temperature. Regional Environmental Change, **17**, 2339-2347.*

Kuang, X., and J. J. Jiao, 2016: Review on climate change on the Tibetan Plateau during the last half century. J. Geophys. Res. Atmos., ***121****, 3979–4007, doi:10.1002/2015JD024728.*

Yin, Y., S. Wu, D. Zhao, D. Zheng, and T. Pan, 2013: Modeled effects of climate change on actual evapotranspiration in different eco-geographical regions in the Tibetan Plateau. J. Geogr. Sci., ***23****(2), 195–207, doi:10.1007/s11442-013-1003-0.*

Zhang, D., J. Huang, X. Guan, B. Chen, and L. Zhang, 2013: Long-term trends of precipitable water and precipitation over the Tibetan Plateau derived from satellite and surface measurements. J. Quant. Spectros. Radiat. Transfer, ***122****, 64–71, doi:10.1016/j.jqsrt.2012.11.028.*

3. *Introduction. Lines 94-110, page 5. This paragraph is more like the summary and conclusions, and should put in the end of text.*

Response: Thank you very much for this concern. This paragraph is a brief summary of the results, and is shortened significantly in this version (new lines 83-91) as follows.

"As the first step to fully recognize the TP's role in the Earth planet, we try to answer a fundamental question in this work: how different would the global climate be with or without the TP? Through sensitivity experiments using a coupled Earth system model, we quantify the impact of the TP on the global climate. By comparing the climate in the realistic world and in a world without the TP, it is found that the presence of TP would result in a 5°C warmer and 10% wetter climate in the Northern Hemisphere (NH). Without the TP, more moisture would be relocated eastward from the tropical Pacific to the North Atlantic, shutting down the Atlantic thermohaline circulation, which can eventually result in more than 15°C cooling and 20% drying in the western hemisphere. The presence of the TP has contributed greatly to the present milder climate in the NH."

4. *Model and experiment. Line 126, page 6. "The model grid employed in this study is T31_gx3v7". What's the meaning of gx3v7?*

Response: Thank you very much for this concern. It is the alias of the ocean model grid used in this CESM. Gx3v7 means the ocean model has a displaced pole and 60 vertical levels, and a uniform 3.6 ° spacing in the zonal direction. In the meridional direction, the grid is non-uniformly spaced. It is 0.6 ° near the equator, gradually increasing to the maximum 3.4 °at 35 °N/S and then decreasing poleward. The explanation of gx3v7 is in lines 111-116.

5. *Model and experiment. Lines 141-151, page 6. The design of the two group experiment need be further clarified. Why in the first experiment there is a global uniform topography that is 50 m above the sea level rather than real globally except the Tibetan Plateau?*

Response: Thank you very much for this suggestion. The experiment with a global uniform topography (Flat) is used as the reference experiment of OnlyTibet, so that the TP effect can be obtained by subtracting the results of Flat from those of OnlyTibet. The experiment with real global topography except the TP is named as NoTibet. By comparing this experiment with the control run (Real), we can also know the TP-alone effect. In this revision, the introduction to the experiment is rewritten carefully. Please see the text in lines 117-139.

6. *.Model and experiment. Lines 145-146, page 6. "The climate changes without the TP are obtained by subtracting the results of Real from those of NoTibet". Usually, climate change is defined as the response to the anthropogenic forcing, difference in climate pattern seems to be more appropriate.*

Response: Thank you very much for this suggestion. This statement is revised as "*The changes in Earth climate due to the TP removal are obtained by subtracting the results of Real from those of NoTibet.*" We use "changes in Earth's climate" to replace "the climate changes." This can avoid misleading expression of "climate change."

7. *Forcing. Lines 163-164, page 6. References are needed here.*

Response: Thank you very much for this suggestion. This sentence is revised as "*Under the background westerlies, both local forcing fields would have strong effects on remote downstream regions, which can be understood by classical large-scale stationary wave dynamics (Hoskins and Karoly, 1981).*"

8. *Change in global atmosphere. Line 185-190, page 9. "There are quick decreases in both SW and LW around year 200." Why there is an abrupt change in rapid southward expansion of sea ice from the Arctic Ocean around year 200?. Also,*

Response: Thank you very much for this question. The rapid decreases in both SW and LW around year 200 are closely related to the sea-ice change in the subpolar Atlantic. We apologize that we did not discuss them in detail in this manuscript. In this revision, we added "*These changes are closely related to ocean dynamics as discussed in Yang and Wen (2019).*"

Figure R14a shows the sea-ice index values in the control run Real (black curve) and in NoTibet (blue curve). We can see that around year 200 the sea-ice area has a sudden jump. This quick increase can be seen in the sea-ice margin in the North Atlantic (Fig. 14b). The green curve in Fig. 14b denotes the sea-ice margin in year 200, which shows a great southward expansion of the sea ice in the subpolar Atlantic. In year 100, the sea-ice margin does not change too much (orange curve in Fig. 14b), when compared to the result of Real, i.e., the initial location (solid red curve in Fig. 14b).

The dynamic processes can be described briefly as follows (detailed analyses were made in Yang and Wen, 2019): Removing the TP➔more freshwater to the North Atlantic➔AMOC weakening➔SST cooling in the North Atlantic➔southward expansion of sea ice + albedo increase➔cooling in the North Atlantic➔positive feedback among sea ice – albedo – SST ➔rapid increase in sea ice (i.e., rapid southward expansion) ➔ large amount of sea-ice melting at the same time ➔ freshwater input ➔ AMOC shutdown.

We can see that the strong positive feedback occurs around year 200 after the TP removal. At that time, the absorbed SW by the ocean is reduced quickly. The outgoing LW is also reduced quickly due to sea ice and colder SST.

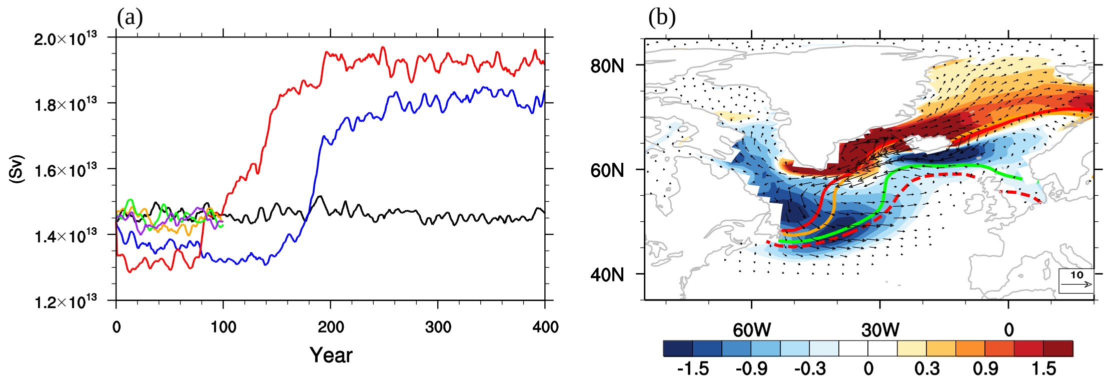

Figure R14 (a) Sea-ice index, defined as the total sea-ice volume (units: km^3) in the Northern Hemisphere, black curve for the control run (Real), blue curve for NoTibet and red curve for Flat. (b) Changes in sea-ice formation (color; psu/year) and sea-ice velocity (vector; cm/s) in Stage-II of NoTibet. Positive (negative) value means sea-ice formation (melting). Solid and dashed red curves represent the sea-ice margin in Real and NoTibet, respectively. Orange and green curves show the sea-ice margin in the 100th year and 200th year, respectively, in NoTibet. The sea-ice margin is defined by the 15% sea-ice fraction in the Atlantic.

9. *Change in global atmosphere. Line 190-191, page 9. The global energy imbalance initiated by the TP remains about 0.2 PW even after 400 years' evolution. Can we conclude that the global energy imbalance will always exist in the model with TP removing?*

Response: Thank you very much for this question. We think the global energy will eventually regain its balance, given long enough time. Recently, we ran the experiments for 1200 years. The net TOA radiative imbalance becomes very small in the later stage (black curve in Fig. R15).

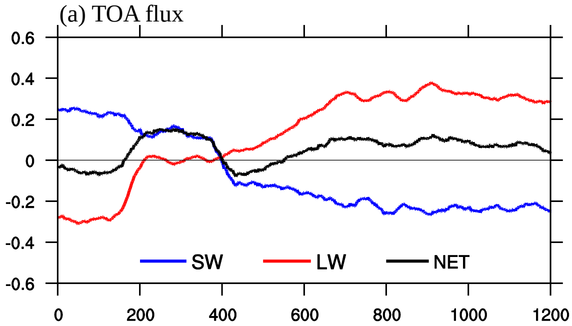

Figure R15 (a) Changes in globally integrated radiation flux at the TOA due to the TP removal. Black is for net radiation flux, blue is for net downward SW and red is for net outgoing (units: PW, 1 PW=10^{15} W, positive for downward anomaly).

10. *Change in global atmosphere. Line 197-199, page 9. In NoTibet, the enhanced SW near 30?N is mainly due to reduced low clouds; in other words, it is due to reduced planetary albedo. The casual relationship between low clouds and albedo need be further explained, and same for LW and high clouds.*

Response: Thank you very much for this comment. We rewrote this statement as follows: “When the TP is removed, the enhanced SW near 30°N (Figs. 3b, 4b) is mainly due to reduced low clouds over the TP region (Fig. 4e), which is consistent with reduced planetary albedo (Fig. 4d). The enhanced outgoing LW near 30°N (Figs. 3b, 4c) is mainly due to local surface warming (Fig. 2a), as well as reduced high clouds (Fig. 4f) over the TP region.”

11. *Global temperature and humility. Lines 243-246, page 11-12. "while the warming (cooling) reversal in the later stage is due to ocean dynamics, particularly the thermohaline dynamics which is closely related to the change in AMOC". A brief explanation need here.*

Response: Thank you very much for this suggestion. In the revision, a brief explanation is given in the later section. “Removing the TP would lead to more freshwater flux transport from the tropical Pacific to the North Atlantic, triggering a slowdown of the AMOC. The AMOC weakening will reduce the northward ocean heat transport and eventually cause remarkable cooling in the North Atlantic. The detailed processes are provided in Yang and Wen (2019). In a word, the slow evolution in the ocean thermohaline circulation lead to the surface temperature reversal in the NH.”

Replies to Reviewer #3:

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

This paper used the CESM model to examine an effect of the TP on the formation of modern climate, which includes surface air temperature and moisture, radiation, atmospheric circulation, and ocean. These results are interesting. Meanwhile, I also think that they should be supported by some proxies. Thus, this paper needs some substantial revisions before acceptance.

Response: Thank you very much for this suggestion. Due to the height and extension of the TP, there are a lot of paleoclimatic studies on the TP at the geological timescale. Most of these studies are based on climate models. Although there are also many proxy data showing the Earth climate evolution since 60 million years ago, there are fewer proxy data that can *reveal the connection* between the uplift of the TP and the global climate evolution. We can better study the connection through numerical model experiments.

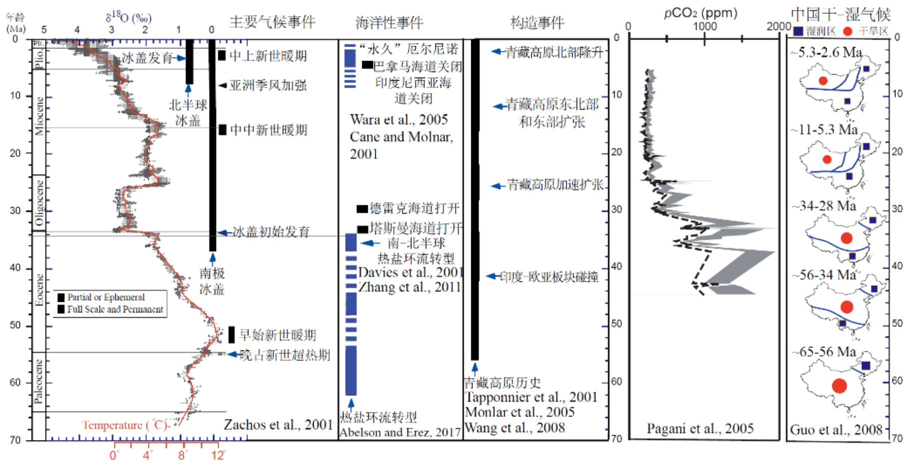

Figure R16 Key environmental changes since the Cenozoic. Adopted from Su (2018).

Su, B., 2018: Simulation of the climate effect of the Tibetan Plateau uplift using a coupled general circulation model, Ph.D. thesis, Chinese Academic Sciences.

The uplift of the TP began about 50 Myr (million years) ago, and was accelerated about 10-8 Myr ago or more recently (Fig. R16) (Harrison et al., 1992; Molnar et al., 1993), which pushed the establishment of the monsoon system in East Asia (Ruddiman and Kutzbach, 1989; Kutzbach et al., 1993; An et al., 2001). Accompanying the rapid orographic change, Asian climate experienced an enhanced aridity in the Asian interior and an onset of Indian and East Asian monsoons about 9-8 Myr ago (An et al., 2001). This intensification of the East Asian summer and winter monsoons had lasted for about 5 Myr, together with increased dust transport to the North Pacific Ocean (An et al., 2001). The latter was attributed to the enhanced westerly jet in winter, which in turn could have resulted in the cooling of the North Pacific (Rea et al., 1998). Although detailed mechanisms of the effect of TP uplift on the Earth climate remain to be explored, we recognize that the TP has played a critical role in shaping the modern climate.

In this study, we have compared our results with the above mentioned paleoclimatic studies in section 6. Our model experiments suggest that there would be an enhanced meridional atmospheric heat and moisture transports across the equator over the Indian Ocean and western Pacific Ocean in the presence of the TP, which is consistent with the result that the rapid uplift of the TP during 10-8 Myr ago might have pushed the establishment of the monsoon system in East Asia (Kutzbach et al., 1993; An et al., 2001). Moreover, in the presence of the TP, there would be more moisture transport from the North Atlantic to the tropical Pacific. This may increase the surface salinity and density in the North Atlantic, leading to strong deep-water formation over there. Since the timing of the rapid TP uplift is consistent with the timing (10 Myr ago) of the strong North Atlantic Deep Water (NADW) formation (Fig. R17) (Ferreira et al., 2018), our modeling results provide a mechanism of how the TP might have affected the formation of the AMOC.

Some studies (e.g., Schmittner et al., 2011) suggested that the Rocky Mountains may be the key to the establishment of the AMOC. However, by 45 Ma, the Rocky Mountains had reached their modern elevation, while there was still no sustained NADW formation (Fig. R17). Proxy data suggest that the timing of the TP's full establishment (Fig. 17) is consistent with the timing of the strong NADW formation. The data cannot tell us the causality, however. In this work, through our TP-perturbation experiments, we demonstrate that removing (adding) the TP would lead to the shutdown (establishment) of the AMOC.

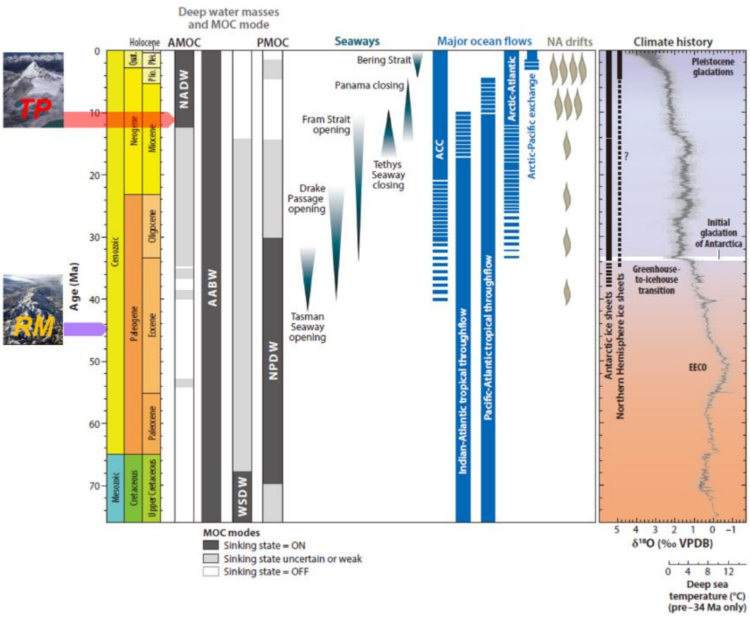

Figure R17 A history of global MOC since the Upper Cretaceous based on geological data and models. Water masses are named after their formation sites and eventual depths, not their water mass properties as done sometimes in the convention. The elongated triangles represent the gradual nature of, and the uncertainty in, the timing of tectonic gateway opening and closure. For the ocean flows, dashed segments signify phases when flows were weak or uncertain. “NA drifts” refers to deep-sea sedimentary drift deposits (i.e., sedimentary piles created by strong bottom-flowing currents) close to locations of NADW formation. The climate history (right panel), which emphasizes the transition from a greenhouse to an icehouse climate, is based on a compilation of deep-sea oxygen stable isotope ($\delta^{18}O$) records (Zachos et al. 2001; Cramer et al. 2009). The white line indicates glacial inception on the Antarctica ~34 Ma. The $\delta^{18}O$ bottom-water temperature scale was calculated for an ice-free ocean and, therefore, applies only to the time before Antarctic glaciation; thereafter, the δ18O variability includes changes in global ice volume. Abbreviations: AABW, Antarctic Bottom Water; ACC, Antarctic Circumpolar Current; AMOC, Atlantic meridional overturning circulation; EECO, Early Eocene Climatic Optimum; MOC, meridional overturning circulation; NA, North Atlantic; NADW, North Atlantic Deep Water; NPDW, North Pacific Deep Water; Ma, million years ago; PMOC, Pacific meridional overturning circulation; VPDB, Vienna Pee Dee Belemnite; WSDW, Warm Saline Deep Water. Adopted from Ferreira et al. (2018).

*Ferreira, D., and coauthers, 2018: Atlantic-Pacific asymmetry in deep water formation. Annu. Rev. Earth Planet. Sci., **46**, 327-352.*

Major comments:

1. *In introduction: Some important references on the global effects of the Tibetan Plateau or Asian land heating should be reviewed, which can help authors to understand the advances of this research field. For example, Zhao et al. (2009, 2011, 2012, 2018) used climate models to investigate the global climatic effects of the TP on global atmospheric circulation, surface temperature, rainfall, and SST.*

Response: Thank you very much for this suggestion. We have added the following references in several places in this version. In section 4.3, we cited their studies as “Different from previous studies focusing on the TP effect on East Asia (Zhao et al., 2009, 2011, 2012), we focus on the TP effect on North America.”

In section 6, we cited their studies as “The melting of ice sheet and degeneration of permafrost have emerged as practically serious problems, and will have tremendous impact on the global environment. How much more will the TP be warmed? How will the future change around the TP feed back onto the global warming? These imperative questions have drawn considerable attention from Chinese scientists (Zhao et al., 2018). An in-depth investigation on these questions will greatly enhanced our ability to cope with the climate change in Asia and the world (Zhao et al., 2018, 2019).”

Zhao et al., 2009: Remotely Modulated Tropical-North Pacific Ocean-Atmosphere Interactions by the South Asian High. Atmospheric Research, 94, 45-60.

Zhao et al., 2019: Global climate effects of summer Tibetan Plateau. Science Bulletin, 64, 1-3, DOI: 10.1016/j.scib.2018.11.019.

Zhao et al., 2018: The Third Atmospheric Scientific Experiment for Understanding the Earth-Atmosphere Coupled System over the Tibetan Plateau and Its Effects. Bulletin of the American Meteorological Society, 99, 757-776.

2. *In model and experiment: The experiments in Fig. 1b and 1d removed or added the entire topography of Asia and should not be called the Tibetan Plateau. The similar experiment was also conducted by Zhao et al. (2011 and 2012). Compared to their results with those of changing the Tibetan Plateau, some differences may be found though the Tibetan Plateau's effect is likely larger. Thus, I suggest that these two experiments in Fig 1b and d may be unsuitable to call the Tibetan Plateau experiments. Additionally, the results from the Fig. 1a and b experiments are generally similar to those from the Fig. 1c and d experiments except for an opposite sign. Thus it is unnecessary to do too much work.*

Response: Thank you very much for this suggestion. We agree that, strictly speaking, removing or adding the entire topography of Asia should not be called the TP experiments. Mongolian Mountains to the north of the TP may also be important to the changes discussed in this work. Some studies (Shi et al., 2015; Sha et al., 2015 and White et al., 2017) also investigated the role of Mongolian Mountains in global climate. Shi et al. (2015) found that the Mongolian Plateau (MP), despite its smaller size, exerts a great influence on the planetary-scale circulation and subtropical westerly jet. Sha et al. (2015) used the outputs from the experiments in Shi et al. (2015) and found that the MP plays a significant role in strengthening the East Asian winter monsoon. White et al. (2018) also found that the MP and nearby mountains have an impact on the upper-level wintertime jet stream that is much stronger than the TP and Himalayas to the south. However, these studies all show the impact of MP on the ***winter*** atmospheric circulation over East Asia, and their conclusions are all obtained based on experiments from ***atmospheric general circulation model (AGCM)***. For the ***summer*** monsoon circulation over the Euro-Asian region, they all stated that the TP plays a much more important role.

Therefore, although topography changes in our experiments include both the TP and MP, considering the area and height of the TP, we think the TP plays a much more important role than Mongolian mountains, in ***global-scale atmosphere and ocean circulations***. Now, we are working intensively on separating the TP effect from the MP effect on the global climate. For the time being, we still call our experiments “TP-perturbation experiments.” We are sorry for the inconvenience.

Thank you very much for these references. We have read these papers and cited their works in this version. Zhao et al. (2011, 2012) focused more on the TP effect on East Asia, and on atmosphere circulation. We focus more on TP effect on the North Atlantic and western hemisphere, and on ocean circulation.

Zhao et al., 2011: Relative Controls of Asian-Pacific Summer Climate by Asian Land and Tropical-North Pacific Sea Surface Temperature. Journal of climate, 24, 4165-4188

Zhao et al., 2012: Asian Origin of Interannual Variations of Summer Climate over the Extratropical North Atlantic Ocean. J Climate, 25, 6594-6609

In this revision, to make the analyses more succinct, we defined the linear and nonlinear changes as follows:

Linear Change = [(NoTibet – Real) – (OnlyTibet – Flat)]/2

Nonlinear Change = [(NoTibet – Real) + (OnlyTibet – Flat)]/2

By doing so, the total subplots have been reduced by 40%. In this revision, we focused on the linear changes due to the TP removal. The nonlinear changes are no long discussed, because we found that the nonlinear changes are very small in most regions.

3. *In forcings: It is not necessary to analyze the first model year results because the model is in the adjustment. The results in this process are insignificant. The authors may remove most of those statements. Additionally, in lines 161-163: I do not understand "The thermal forcing here is analogous to the global warming situation…". Please give more explanations.*

Response: Thank you very much for this comment. Exactly as you said “the model is in the adjustment in the first model year.” The ocean change in the 1st year is negligible, but the atmosphere change in the 1st year is remarkable. We would like to take advantage of this feature, i.e., the short timescale of atmosphere and long timescale of the ocean, to separate the atmosphere response and ocean response in a unified coupled climate model.

We would like to keep this paragraph here, because this is a good way to understand what the topography forcing really is, and a good way to know the topographic forcing *quantitatively*. Knowing exactly what the local forcing is is also good for us to analyze the model results and understand the mechanisms. For example, the topographic forcing is more or less similar to the CO_2 forcing, except that the former is local and strong while the latter is global and relatively weak. In many aspects of the global climate responses, they may share the same mechanisms.

4. *In section 4.1 of Changes of global atmosphere: The authors may greatly cut down the related statements in lines 178-193 because of the previous questions 2 and 3.*

   In section 4.2: The authors may greatly cut down the related statements because of the previous question 2.

Response: Thank you very much for this suggestion. In this revision, we focused on the linear response. Both the text and figures have been refined substantially.

5. *In sections 4.3-4.5 and 5: The stationary wave and its propagation along the westerly jet, atmospheric circulation, the Hadley circulation, SST and related mechanisms forced by changing the Tibetan Plateau topography have been analyzed to a certain extent by Zhao et al. (2009, 2011, 2012). The authors may compare with those results. I suggest that authors may pay more attention to oceanic changes instead of atmosphere.*

Response: Thank you very much for this suggestion. In this revision, we cited their papers, reduced the statement on the atmosphere changes and paid more attention to the ocean changes, as suggested.

6. *In summary and discussion: This work tried to discuss the effect of the Tibetan Plateau uplift on the formation of modern climate and all results came from the simulations. Thus I strongly suggest the authors may do their best to add some comparison with some proxies (that may reflect the climate evolution around the Tibetan uplift) in this section. This comparison may help readers to see the reliability of the simulations.*

Response: Thank you very much for this suggestion. In this work, we have compared our results with the above mentioned paleoclimatic studies in section 6. Our model experiments suggest that there would be an enhanced meridional atmospheric heat and moisture transports across the equator over the Indian Ocean-western Pacific Ocean in the presence of the TP, which is consistent with the result that the rapid uplift of the TP during 10-8 Myr ago might have pushed the establishment of the monsoon system in East Asia (Kutzbach et al., 1993; An et al., 2001). Moreover, in the presence of the TP, there would be more moisture transport from the North Atlantic to the tropical Pacific. This may increase the surface salinity and density in the North Atlantic, leading to strong deep-water formation over there. Since the timing of the rapid TP uplift is consistent with the timing (10 Myr ago) of the strong NADW formation (Fig. R17) (Ferreira et al., 2018), our modeling results provide a mechanism of how the TP might have affected the formation of the AMOC.

Please also see the reply to the general comment in the beginning of these replies.