Replies to Reviewer #1:

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.

"The authors studied the impacts of the Tibetan Plateau (TP) on the Atlantic Meridional Overturning Circulation (AMOC) using NCAR CESM. They found that removing the TP in the model leads to an AMOC strengthening during the first few decades and a subsequent weakening till collapse of the AMOC in their 400-year simulation. They examined the mechanisms on this two-stage AMOC change and argued that the initial AMOC weakening (should be strengthening) is related to the cooling in the North Atlantic as caused by enhanced southward Ekman flow and surface latent and sensible heat losses due to stronger westerlies, whereas the latter AMOC weakening is triggered by the water vapor transport from the tropical Pacific to the North Atlantic. Particularly, the authors highlighted a positive feedback between the AMOC and sea ice that eventually leads to the AMOC shutdown."

"This paper is very interesting and potentially contributes to our understanding of the TP impacts on the AMOC. I thereby strongly support this manuscript be published on Journal of Climate after the authors satisfyingly address the following issues."

Response: Thank you very much for your encouraging comments and specific suggestions. Please see our replies below.

Major comments:

1. a) *North Atlantic Deep Water (NADW) formation region is the key region for the heat/salinity budget analysis in this study. However, it is defined based on surface density change (Fig. 3c, rectangle box). I would like to suggest the authors' starting from the control run. March mean mixed layer depth (MLD) is a nice indicator of NADW formation as it suggests the wintertime deep convection sites in the North Atlantic (e.g., Hu et al. 2008, Liu and Liu, 2013, Liu et al. 2019). As such, the authors may want to first show the March MLD in their control run to define the NADW formation region. This will also echo Fig. 8b and d, letting readers know whether the maximum of MLD change indeed occurs at NADW formation region.*

Response: Thank you very much for these comments. The definition of the NADW region in this work is indeed based on the pattern of the March MLD. Fig. R1 shows the March MLD in Real and the changes in NoTibet (with respect to Real). The MDL is calculated following Large et al. (1997). As the reviewer pointed out, it is true that in Real the maximum mean MLD is located in the GIN seas (Fig. R1a). In NoTibet, the MLD becomes deeper in Stage-I (Fig. R1b) and shallower in Stage-II (Fig. R1c), in association with the strengthened and weakened AMOC, respectively (Fig. 2a in the paper). Following this suggestion, we added Fig. 3 in the revision, showing the mean March MLD in Real and its changes in Stage-I and Stage-II of NoTibet. The NADW region is defined as the region where the MLD becomes shallower, as shown in Fig. R1c, enclosed by the grey zero contour of the MLD change. Accordingly, Figs. 2b, 6 and 8 are all re-plotted based on the new NADW definition.

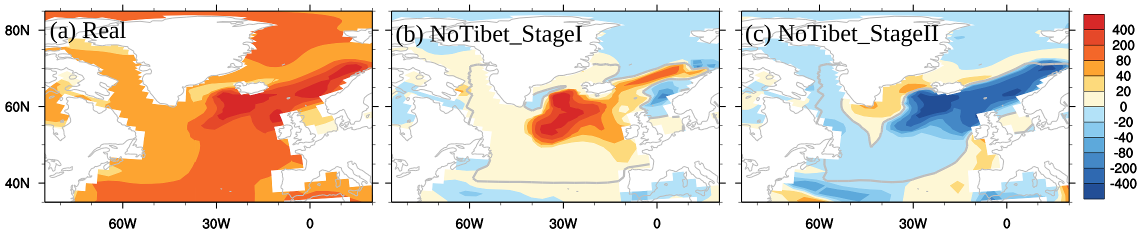

Fig. R1 (a) Mean March mixed layer depth (MLD; m) in the North Atlantic in the control run (Real), and its changes in (b) Stage-I and (c) Stage-II of NoTibet (with respect to Real).

b) (continued from Q.1a) Besides, the authors may want to pay a bit more attention on illustrating the NADW formation region in their plots for three reasons: (1) In current Fig. 3c, the grey solid rectangle box goes across land, which is not correct since the NADW formation region should be entirely within ocean. (2) I suspect that deep convection/NADW formation may also happen in the Greenland, Iceland and Norwegian (GIN) Seas. If it is the case, the authors may want include the GIN Seas area in their budget analyses. (3) The MLD changes are opposite in two areas (to the south/southeast of Greenland and to the south of Iceland), however, both areas are included in current definition of NADW formation region.

Response: Thank you very much for these suggestions. Based on the new definition of the NADW region, the above questions all go away. Please refer to the new Figs. 2b, 3, 6, and 8. The NADW region is entirely within the ocean, and it includes the GIN seas. The conclusions are nearly unchanged.

2. *Budget analyses: Since the authors conduct heat/salt budget analysis over an enclosed area, the advection term is actually the convergence or divergence of the heat/salt transports across the zonal and meridional boundaries of the enclosed area. Take the meridional boundaries as example, how do the heat/salt transports change at the northern and southern boundaries? Which determines the change of meridional advection term? By the northern or southern boundary, or both? I am asking this because I can see complex sign changes in Fig. 6b and e. More importantly, I would like to suggest the authors discussing these heat/salt transport changes in the context of particular currents. For example, are they related to the changes of the Eastern Greenland Current, the Greenland-Iceland overflow, the North Atlantic Current, etc.?*

Response: Thank you very much for these suggestions. In the revision, the heat/salt budget analyses in Figs. 6a and 8a (old Figs. 5a and 7a) are conducted over the new NADW region defined in Fig. 3c. Now, the northern (southern) boundary is not zonal along a certain latitude. From Figs. 7b and e (old Figs. 6b and e), we can see clearly that in Stage-I of NoTibet, the surface cooling in the North Atlantic (30-60°N) is mainly due to the enhanced southward Eastern Greenland Current and Labrador Current (Fig. 7b), which are in turn forced by the enhanced westerlies over the same region. In Stage-II of NoTibet, the surface cooling in the North Atlantic is mainly due to the weakened North Atlantic Current (Fig. 7e), which is one of the manifestations of the weakened AMOC in this stage. In the revision, we have specified the contributions of these currents.

Although the change in horizontal temperature advection contributes to the surface cooling in the North Atlantic all the time, the dominant cooling effect in Stage-I is from enhanced latent and sensible heat losses, and the dominant cooling effect in Stage-II is from reduced SW.

Moreover, we notice that salinity advection term does not play a freshening role in the North Atlantic (Fig. 8a). The surface freshening is mainly caused by the local EMP term in Stage-I and by sea-ice melting in Stage-II (Fig. 8a). Vertical salinity diffusion is also important to the SSS change in the North Atlantic (Fig. 8b). We have added detailed analyses on the mechanism of the SSS change in Section 4.2 of the manuscript.

In general, we find that the changes in temperature and salinity advections are not the dominant factors to the surface buoyance change. Nevertheless, the enhanced southward Eastern Greenland Current and Labrador Current, and the weakened North Atlantic Current do contribute to the surface buoyance change.

3. *I am somewhat confused by the positive feedback between the AMOC and sea ice that eventually leads to the AMOC shutdown. Why will a sea ice expansion lead to more sea ice melting (Fig. 12)? Following this argument, a pulse-like hosing experiment will most likely end up with a collapsed AMOC, which, however, is not the case. Recent studies (Sévellec et al. 2017, Liu et al. 2019, Liu and Fedorov, 2019) have suggested that an abrupt Arctic sea ice decline will cause an AMOC weakening but with a lag of several decades, whereas the weakened AMOC is inclined to recover the Arctic sea ice via a negative feedback elaborated in Zhang (2015). The authors may want to make further explanation of their positive feedback and make discussions in the context of these previous studies. Also, I am wondering about the role of other feedbacks in the AMOC collapse, such as the Stommel's salinity advection feedback (Liu et al. 2014, 2017). Could the authors make some related discussions?*

Response: Thank you very much for these comments. The southward sea-ice expansion leads to more sea ice being transported to the lower latitudes (south of 60°N), where the background SST can be still above the freezing point (Fig. R2), even after the significant cooling between 40 and 60°N (as shown in Fig. 4d). In the sea-ice model of CESM, the freezing point is set at -1.8°C. Fig. R2 shows the mean SST and the contour of freezing point in NoTibet. It shows that the contour of freezing point corresponds well to the dividing line between the sea-ice formation and melting (Fig. 9c). The sea ice south of this line is not formed locally; it is due to the southward advection from the ocean north of this line. Therefore, there will be plenty of sea-ice melting south of this line; that is, the sea-ice expansion will lead to more sea-ice melting.

In Fig. 9c, we can see significant sea-ice formation north of 60°N. Accompanied by the AMOC weakening, the sea ice formed north of 60°N can expand to the south of 50°N (denoted by the sea-ice margin, color curves in Fig. 9c), which provides a great amount freshwater to the North Atlantic, furthering the weakening of the AMOC.

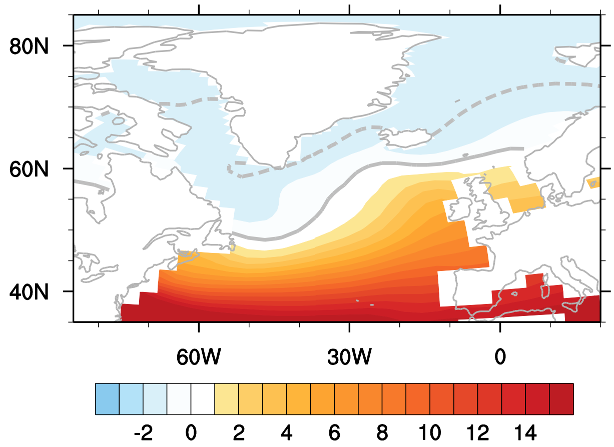

Fig. R2 Mean SST (°C) in the North Atlantic in Stage-II of NoTibet. Solid grey curve denotes the 0°C SST contour, and dashed grey curve denotes the -1.8°C SST contour. Sea ice will form (melt) when SST is lower (higher) than -1.8°C.

Here, we would like to emphasize that the weakening of the AMOC in the initial stage is not due to the sea-ice change; it is triggered by the net surface freshwater flux (e.g., EMP<0). The temporal evolutions of the AMOC (Fig. 2a) and the sea-ice melting in the North Atlantic (orange curve in Fig. 8a) show clearly that the sea-ice change lags the AMOC change by about 100 years. To further understand the positive feedback between the southward sea-ice expansion (i.e., melting) and the AMOC weakening in the later stage (around year 150 to year 250), we plot the temporal evolutions of sea-ice coverage in the North Atlantic in both Real and NoTibet (Fig. R3). This figure is also included as Fig. 9 in the revised manuscript.

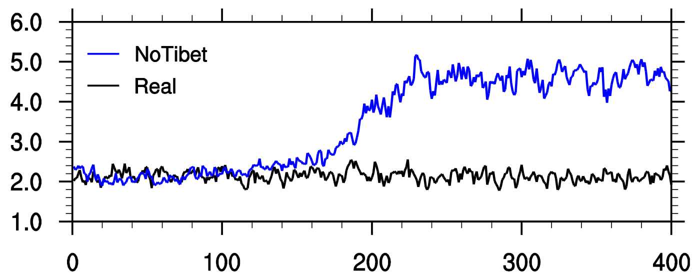

Fig. R3 Temporal evolution of sea-ice coverage (units: 10^6 km^2) in the North Atlantic (60°W-10°E, 40-80°N) in Real (black) and NoTibet (blue).

In the first 100 years after the TP removal, the sea-ice coverage in the North Atlantic remains unchanged (blue curve, Fig. R3), while the AMOC weakens gradually by about 30% (Fig. 2a). The southward sea-ice expansion starts around 150 years after the TP removal, apparently lagging the AMOC change. The positive feedback between the sea-ice expansion and the AMOC operates as follows: the weakening of the AMOC leads to the southward expansion of sea ice. The rapid sea-ice expansion around years 150 to 250 leads to massive sea-ice melting south of 60°N (Fig. 9c). This freshwater provided by the sea-ice melting during years 150 and 250 is the dominant factor to the surface salinity decrease in the NADW region (Fig. 8a), and thus leads to the further weakening of the AMOC (Fig. 2a). Therefore, we conclude that it is this positive feedback that eventually shuts down the AMOC.

Our term balance analyses on the salinity equation suggest that the Stommel's salinity advection feedback does not contribute to the AMOC change in our CESM topography experiments. This does not rule out the possible importance of this advection mechanism in other coupled models. Since we discuss the AMOC change in response to a *strong* and *persistent* external forcing in this study (removing the TP is a remarkable change of the climate system). The sea-ice change and melting in response to the TP change can in turn be treated as a strong external forcing of the ocean circulation. It is thus understandable that the strong external freshwater flux change dominates the AMOC change.

We thank the reviewer very much for bringing our attention to these important studies. Our study here does not support the argument that "*a pulse-like hosing experiment will most likely end up with a collapsed AMOC.*" Instead, our study suggests that only strong and *persistent* perturbation can cause significant change in the AMOC; moreover, it will take hundreds of years.

The mechanism of the AMOC change studied here does not have to contradict to recent studies of Sévellec et al. (2017), Liu et al. (2019), and Liu and Fedorov (2019). One should bear in mind that there are many different kinds of perturbations that can lead to AMOC change. In our work, we perturb topography, which results in more freshwater transported to the North Atlantic. Therefore, the AMOC change is the response to the surface freshwater input. In other words, the starting point of the AMOC change in this study is the surface freshwater input over the North Atlantic. In the studies of Sévellec et al. (2017), Liu et al. (2019), and Liu and Fedorov (2019), the starting point is the abrupt Arctic sea-ice decline, which is equivalent to surface warming and sea-ice melting. This, of course, will lead to the weakening of the NADW formation and thus the AMOC weakening later on. "*The weakening of AMOC is inclined to recover the Arctic sea ice via a negative feedback elaborated in Zhang (2015)*" is actually consistent with the positive feedback proposed in this work, that is, the weakening of the AMOC leads to the southward expansion of sea ice, and more sea-ice melting south of the GIN seas and more sea-ice formation in the eastern Greenland Sea and the Nordic Sea; in other words, the sea-ice recovery in the subpolar Atlantic.

Minor comments:

4. *Lines 96 and 418: result in AMOC collapse -> result in an AMOC collapse.* Revised.

5. *Line 96: The evolution of AMOC -> The evolution of the AMOC Also, add "the" after "of" in Lines 98, 106, 149, 160, 295.* Revised.

6. *Lines 312-314: Fig. 1a -> Fig 2a?* Revised.

7. *Lines 609-612: These two references are the same.* Revised.

8. *Figs. 3 and 4: Is it proper to call "quasi-equilibrium changes" for Stage-I?*

Response: We do not think Stage-I is a quasi-equilibrium stage, since the ocean has not changed too much in this stage.

References:

Hu, A., L. Otto-Bliesner, A. Meehl, W. Han, C. Morrill, C. Brady and P. Briegleb (2008): Response of thermohaline circulation to freshwater forcing under present-day and LGM conditions. J. Climate, 21, 2239-2258.

Liu, W. and Z. Liu (2013): A diagnostic indicator of the stability of the Atlantic Meridional Overturning Circulation in CCSM3. J. Climate, 26, 1926-1938.

Liu, W., Z. Liu and E. C. Brady (2014): Why is the AMOC mono-stable in Coupled General Circulation Models? J. Climate, 27, 2427-2443.

Liu, W., S.-P. Xie, Z. Liu and J. Zhu (2017): Overlooked possibility of a collapsed Atlantic Meridional Overturning Circulation in warming climate. Sci. Adv., 3, e1601666.

Liu, W., A. Fedorov and F. Sévellec (2019): The mechanisms of the Atlantic Meridional Overturning Circulation slow-down induced by Arctic sea ice decline. J. Climate, 32, 977-996.

Liu, W. and A. Fedorov (2019): Global impacts of Arctic sea ice loss mediated by the Atlantic meridional overturning circulation. Geophys. Res. Lett., 46, 944-952.

Sévellec, F., A. V. Fedorov and W. Liu (2017): Arctic sea-ice decline weakens the Atlantic Meridional Overturning Circulation. Nat. Clim. Change, 7, 604-610.

Zhang R. (2015): Mechanisms for low-frequency variability of summer Arctic sea ice extent. Proc. Natl. Acad. Sci., 112, 4570-4575.

Replies to Reviewer #2:

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.

"Both of these papers address a very important first order question in climate science: why is there deep water formation in the Atlantic, but not in the Pacific under current conditions? This paper focusses on the effect of the Tibetan Plateau on deep water formation in the Atlantic. It shows that removing the Tibetan Plateau increases the net precipitation into the North Atlantic, leading to a collapse of deep water formation in the North Atlantic in CESM."

"In general, I am pleased with the structure of the paper, but I am concerned that some of the statements are not supported by enough evidence. My concerns could be addressed in the "Supplementary" section if the authors do not wish to add to the main body of the paper."

Response: Thank you very much for your encouraging comments.

Major points:

1. *Line 231: Can you define what you mean by the NADW region? I eventually figured out that its horizontal extent is shown in figure 3c (this should be clarified in the caption of figure 5 and in the main text). But I still don't know how deep the region is over which you did the temperature and salinity budgets?*

Response: Thank you very much for this suggestion. Reviewer #1 also raises the same question on how we define the NADW region. In the previous version, we did not make it clear enough. In this revision, we define the NADW region based on the March mixed layer depth (MLD). We added Fig. 3 in the revision, which shows the climatology March MLD in Real and its changes in Stage-I and Stage-II of NoTibet (with respect to Real). Please also see Fig. R1 in our reply to Reviewer #1.

The definition of the NADW region in the revision is based on the pattern of March MLD. In Real, the maximum mean MLD is located in the GIN seas (Fig. R1a). In NoTibet, the MLD becomes deeper in Stage-I (Fig. R1b) and shallower in Stage-II (Fig. R1c), in association with the strengthened and weakened AMOC (Fig. 2a in the paper), respectively. The NADW region is defined as the region where the MLD becomes shallower, as shown in Fig. R1c, enclosed by the grey zero contour of the MLD change. Accordingly, Figs. 2b, 6 and 8 are all re-plotted based on the new NADW definition. The depth is 30 m for all the surface terms.

2. *Line 257: "the reduced SW south of the sea-ice region is attributed mainly to increased low clouds." I can see that the patterns of change in SW and low clouds are similar, but it would help to give a quantitative relationship between low cloud and SW and to quantify how much of the change in SW is due to low clouds.*

Response: Thank you very much for this suggestion. We have carefully calculated the contribution of low cloud and sea ice to the SW change. Over the NADW region, the SW is decreased by 21 W/m^2 during Stage-II, in which 6 W/m^2 is due to the increased low cloud south of the sea-ice margin in NoTibet and 15 W/m^2 is due to the increased sea-ice reflection north of the sea-ice margin. The sea-ice margins in Real and NoTibet are denoted by the solid and dashed red curves in Fig. 7d, respectively.

Over the NADW region, the total low cloud is reduced by 2%, which is mainly caused by the low-cloud reduction over the northern sea-ice margin of NoTibet. The low cloud south of the sea-ice margin of NoTibet is increased by 7%, which reflects the downward SW and contributes to the decreased SW of 6 W/m^2.

3. *Line 282: "we focus on vertical diffusion, EMP and sea-ice melting, since these three factors play a deterministic role in …". I am confused by this choice. Figure 7a shows that salinity advection and horizontal diffusion are also important in the salinity budget. Perhaps you mean that vertical diffusion, EMP and sea-ice melting do not change much in response to the oceanic salinity field?*

   More information needs to be provided about what sets the vertical diffusivity in CESM, and ideally the vertical diffusivity would be plotted separately from \partial^2 S/\partial z^2

Response: Thank you very much for this comment. Based on our new definition of the NADW in the revision, we re-plotted the salinity budget in Fig. 8a. In Stage-I, the SSS in the NADW is increased, which is mainly contributed by vertical salinity diffusion. The contributions from horizontal advection and diffusion can be neglected. In Stage-II, the SSS in the NADW is decreased, which is dominated by sea-ice melting. The horizontal advection and diffusion are strong, but they tend to increase the SSS in the NADW region. In other words, the vertical diffusion, EMP and sea-ice melting determine the final sign of the SSS change. That is why we say that they play deterministic roles in different stages of the AMOC evolution after the TP removal. In the revision, we have made this point clearer.

In CESM, the vertical salinity diffusion is given by:

$$\mathcal{D}v(S) = \delta_z(\kappa\delta_z S) = \frac{1}{dz_k}\left(\frac{\kappa_{k-\frac{1}{2}}(S_{k-1}-S_k)}{dz_{k-\frac{1}{2}}} - \frac{\kappa_{k+\frac{1}{2}}(S_k-S_{k+1})}{dz_{k+\frac{1}{2}}}\right) \quad (1)$$

where S_k is salinity at level k; and $k-\frac{1}{2}$ and $k+\frac{1}{2}$ are evaluated on the top and bottom faces, respectively, of the T-cell at level k. Vertical diffusivity κ typically depends on the local state and mixing parameterization, which is calculated in the model based on the KPP mixing scheme (Large, McWilliams and Doney, Reviews of Geophysics, 32, 363, 1994).

The vertical diffusion term ($\kappa\partial^2 S/\partial z^2$) shown in Fig. 8b is calculated online; so, we do not know the exact value of vertical diffusivity. The online-calculated $\kappa\partial^2 S/\partial z^2$ is more accurate (left panel, Fig. R4), which can guarantee the closure of the salinity equation. We also calculate $\partial^2 S/\partial z^2$ offline (right panel, Fig. R4). The pattern difference between $\kappa\partial^2 S/\partial z^2$ and $\partial^2 S/\partial z^2$ (Fig. R4) suggests that vertical diffusivity κ has complex spatial structure. Since we do not know the value of κ, the offline-calculated $\kappa\partial^2S/\partial z^2$ cannot guarantee the closure of the salinity equation.

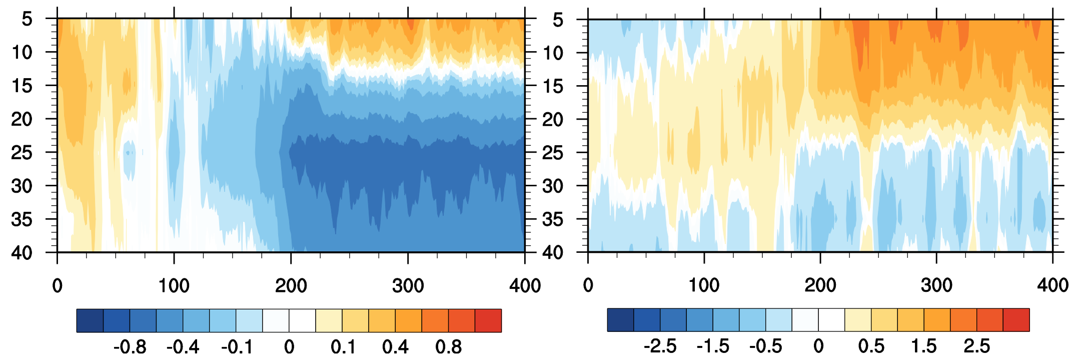

Fig. R4 Changes in (left panel) vertical diffusion term $\kappa\partial^2S/\partial z^2$ (units: psu/ year) and (right panel) vertical salinity gradient $\partial^2S/\partial z^2$ (units: 10^{-8} psu/m^2) in NoTibet (with respect to Real).

4. *The authors should acknowledge other theories for why deep water formation occurs in the Atlantic but not in the Pacific (see e.g. Ferreira et al. 2018). It would be good if the authors could also refer to more idealized climate models (with very thin continents) that show deep water formation occurring in the Atlantic (e.g. Ferreira et al. 2010, Nilsson et al. 2013), even without a Tibetan Plateau.*

Response: Thank you very much for this suggestion. We have added these studies to our reference list. *Particularly in the conclusion and discussion sections, we added a long paragraph about their works*.

"In this work we show that removing the TP can lead to the AMOC shutdown. We have also done experiments that adding the TP in a global flat continent, and found that a sudden uplift of the TP can lead to the AMOC formation (figure not shown). However, the presence of TP does not have to be a necessary condition for the existence of the AMOC. Many idealized coupled model experiments found that the deep meridional overturning circulation exists in the Atlantic because of its smaller width (e.g., Ferreira et al., 2010; Nilsson et al., 2013), regardless of the continental topography. By planting very thin meridional continents in an Aquaplanet, Ferreira et al. (2010) showed that the small and large basins exhibit distinctive Atlantic-like and Pacific-like characteristics, respectively. The small basin is warmer, saltier, and denser at the surface than the large basin, and is the main site of deep-water formation with a deep overturning circulation and strong northward ocean heat transport. Nilsson et al. (2013) further showed that the southward extent of the land barrier can affect the deep-water formation, because the length of meridional barrier controls the wind driven Sverdrup circulation, and thus the interbasin salt transport. These idealized aquaplanet experiments suggest the fundamental roles of the basin geometry in the world ocean circulations. However, our work suggests that under modern basin geometry, the TP uplift may have affected the AMOC formation."

5. *Line 88: the sentence "The latter is probably more important for understanding correctly the mechanisms of topography-induced ocean changes" needs more justification (or you could just cut it)*

Response: Thank you for this suggestion. This sentence is removed.

6. *The change in the overturning of the Southern Ocean should be shown in figure 2. Weaver et al. 1993 is an old reference and the authors need to read more of the modern literature: their description of the AMOC as the sum of a wind-driven circulation in the tropics plus a thermohaline circulation is a bit old-fashioned. Modern oceanographers mostly agree that some fraction of the Meridional Overturning Circulation is driven by winds over the Southern Ocean (see e.g. Nikurashin and Vallis 2012, Gnanadesikan 1999).*

Response: Thank you very much for this suggestion. Actually, the global MOC (GMOC) does not change too much in NoTibet (Fig. R5), because the see-saw changes of the MOC in the Atlantic and Pacific. Therefore, the overturning of the Southern Ocean (SO) is roughly unchanged (right panels, Fig. R5). In this work, we focus on the North Atlantic; and emphasize that the local changes in the North Atlantic lead to the AMOC shutdown, when the TP is removed.

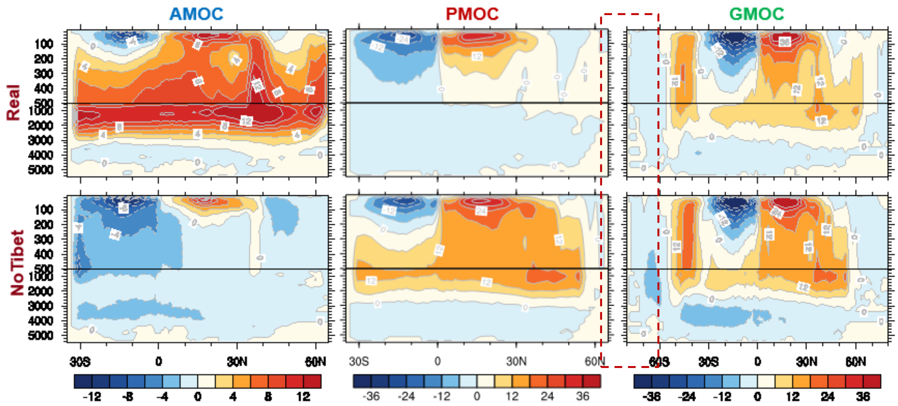

Fig. R5 Upper panels: The AMOC (left), PMOC (middle) and global MOC (GMOC, right) in Real. Lower panels: The AMOC (left), PMOC (middle) and GMOC (right) in NoTibet.

We totally agree that the wind forcing over the SO is very important to the global meridional overturning circulation. It appears that in NoTibet, the wind forcing over the SO does not change much, so its contribution to the AMOC change can be neglected.

In our accompanying paper about TP effect on PMOC, we have a figure (Fig. 3 in that paper) showing GMOC in Real and NoTibet, and its change in NoTibet, along with the global zonal wind change. Please refer to that paper.

We totally recognize the importance of the wind forcing over the SO, because to a great extent the GMOC is maintained by the persistent Ekman pumping in the SO (Toggweiler and Samuels, 1995; Gnanadesikan 1999; Wunsch and Ferrari, 2004; Nikurashin and Vallis, 2012).

In this work, we focus on the *weakening of the AMOC*, so the role of the wind forcing over the SO can be neglected. In our paper about the PMOC, we discussed the wind forcing. We think that In NoTibet, the persistent and stable Ekman pumping in the SO provides a necessary background condition that makes the fully establishment of the PMOC possible.

7. The authors could spend more time looking at how and why their results differ from previous studies. In particular, they do not remove the Andes (which previous studies do) - more discussion of this would be nice!

Response: Thank you very much for these suggestions. In fact, our results are qualitatively consistent with those in previous studies. Schmittner et al. (2011) and Maffre et al. (2018) conducted experiments with global mountains removed. They also showed a see-saw change in the Pacific and Atlantic, i.e., a strong (weak) PMOC (AMOC) in a world with a global flat continent. However, they did not pinpoint the individual roles of different mountains. Fallah et al. (2016) and Su et al. (2018) carried out the same experiments as we did, and drew the same conclusion that removing the TP only can leads to the AMOC weakening. But the mechanisms we propose here are different from theirs.

In this study, we show that in different stages, the dominant process for the AMOC change is different; that is, the wind effect increases the AMOC during stage-I and freshwater effect decreases the AMOC during stage-II. In Su et al. (2018), they tried to explain the transient response of the AMOC; however, they used the physical values from the equilibrium stage, which is not correct in our view. We agree that the detailed processes that lead to the AMOC collapse are different in different studies. So, in the last sentence of our discussion, we added that "*Studies using more coupled models, with more deliberately designed topography experiments, are still extremely valuable*."

In previous studies, the roles of the TP, Rocky Mountains and Andes Mountains are not pinpointed. We did study the individual roles of these mountains. However, due to the limit of paper length, we did not discuss different roles of different mountains in this manuscript, which will be in a separate paper. Fig. R6 shows the global topography, without the Rocky and Andes mountains, and the changes of AMOC and PMOC in these experiments. We have also done the global flat continent experiment as in Schmittner et al. (2011) and Maffre et al. (2018) did. **In short, only the TP removal can cause the significant changes in AMOC and PMOC (Fig. R6c).** Removing other mountains will not cause changes in the global meridional overturning circulations. All experiments are integrated for 400 years.

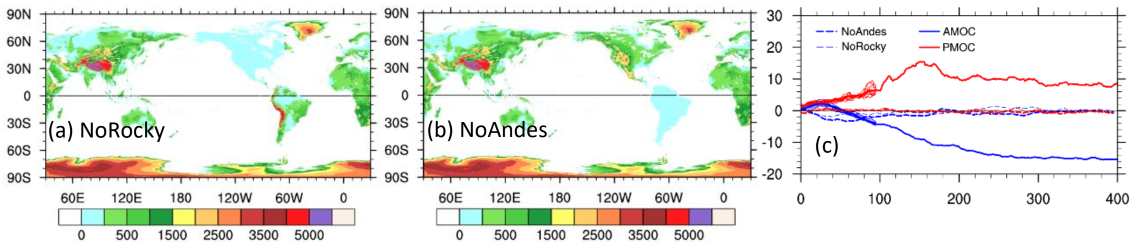

Fig. R6 Topography configuration without (a) Rocky Mountains (NoRocky) and (b) Andes Mountains (NoAndes). (c) Temporal evolutions of the PMOC (red) and AMOC (blue) in NoTibet (solid curves), NoRocky (thin dashed curve) and NoAndes (thick dashed curve). The PMOC (AMOC) index is defined as the maximum streamfunction in the range of 0-10°C between 20° and 70°N in the Pacific (Atlantic). Thin solid curves show the PMOC and AMOC changes in 10 ensemble runs in NoTibet.

We conducted detailed analyses about the TP effect on the AMOC in this study. We prefer not to include the discussion on the Rocky and Andes Mountains, due to the limit of paper length.

Minor points:

8. *The authors might be interested in Cessi 2018, which looks at the effects of local winds in the North Atlantic on the strength of the AMOC.*

Response: Thank you very much for your suggestion. Cessi (2018) showed that the westerly wind stress in the northern part of the Atlantic provides two opposing effects. Mechanically, the return of the Ekman transport in the North Atlantic opposes the sinking in this region, reducing the total overturning; thermodynamically, the subpolar gyre advects salt poleward, promoting Northern-Hemisphere sinking. Depending on which mechanism prevails, increased westerly winds in the Northern Hemisphere can reduce or augment the overturning.

In NoTibet, the enhanced westerlies over the North Atlantic tend to push more cold water southward and thus promote the NADW formation. This process dominates in Stage-I and lead to strengthening of the AMOC (Figs. 2a, 7b and 7c). This is consistent with the finding in Cessi (2018). However, in the later stage, the surface freshwater flux becomes dominant, leading to the weakening of the AMOC.

9. *Line 143: The AMOC index needs to be defined in the main text as well as in the figure captions.*

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

10. *Lines 191-205: It might be good to say something about the equation of state for seawater and to explain that this is why temperature is more important at low latitudes and salinity more important at high latitudes.*

Response: Thank you very much for this suggestion. We added "It is seen again that the salinity change dominates the density change in the high latitudes, while both the temperature and salinity changes contribute to the density change in the tropic, because the thermal expansion become very small in low temperature regime."

11. *Line 455: The citation of Ferreira et al. 2018 is inappropriate here. The authors should find a paper with direct evidence for their statement.*

Response: Thank you very much for this suggestion. We found an appropriate citation here. "Woodruff, F., S. M. Savin, 1989: Miocene deepwater oceanography. Paleoceanography and Paleoclimatology, 4: 87-140."

12. *The legend for figures 2b, 5a and 7a could be improved by writing a better description of each term.*

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

13. *It seems that figures 5a and 7a show the difference in the T/S-tendency between NoTibet and the control run. Can you make it clear in the caption text that what it plotted is the difference from the control run?*

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

14. *Title: I propose the title: "Investigating the Role of the Tibetan Plateau in the Formation of the Atlantic Meridional Overturning Circulation"*

Response: Thank you very much for this suggestion. We revised the title as suggested.

15. *Line 79: should be "...showed that higher mountains reduce water-vapor transport"*

Response: Thanks a lot. Revised.

16. *Line 155: "declining" should be "decline"* Revised.
17. *Line 228: remove the word "effect"* Revised.
18. *Line 295: replace "In accompany with" by "Accompanying"* Revised.
19. *Line 489: the word myth is inappropriate. Perhaps just say that it is still uncertain.* Revised.
20. *Figure 4: Stage-I isn't in quasi-equilibrium* Revised.
21. *Please label the x-axis of figure 5* Revised.
22. *Figure 6: in the caption you need to say that the surface current is shown be arrows and the temperature change due to advection is shown in color* Revised.
23. *Figure 12: Why do you use VT to represent heat transport?* It is changed to HT.

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.

"Impact of each individual large-scale topography such on Tibetan Plateau on ocean circulation is an open question. The authors attempted to reveal the role of the TP in the formation of the AMOC through sensitive experiments from the CESM1.0. The main conclusion, i.e., an initial strengthening followed by a decline of the AMOC in response to the TP removal is different from previous studies."

"However, some crucial points, together with technical issues, need be further clarified. Particularly, some parts in this paper (model, experiments, and text) overlaps with another parallel submitted manuscript named investigating the Role of Tibetan Plateau in the Formation of Pacific Meridional Overturning Circulation. I suggest the authors combine these two manuscripts together."

Response: Thank you very much for these encouraging comments. If we combine these two paper into one, the paper would be too long (total figures would be 25 pieces), which would far exceed the limit of paper length required by *Journal of Climate*. Our paper on the AMOC focuses on thermohaline dynamics, while our paper on the PMOC focuses on both wind-driven and thermohaline dynamics. We want to keep these two papers separately.

"The title of these two manuscripts focuses on the Tibetan Plateau, whereas all the topography in the central and east Asia has been set to 50 m. In other words, the difference fields between the sensitive run and control run are induced by the effect of both Tibetan Plateau and surrounding areas such as Mongolian plateau and loess plateau. This work emphasize that the atmospheric moisture relocation from the tropical Pacific to the North Atlantic is the key to trigger the weakening of the AMOC, and the positive feedback between the southward expansion of sea ice and AMOC leads to the AMOC shutdown, which is different with previous studies. Is there any evidence to illustrate this conclusion is more reasonable due to either CGCM ability or experiment design?"

Response: Thank you very much for these comments. First of all, we would like to say that our results are qualitatively consistent with those in previous studies. Using different CGCMs, Schmittner et al. (2011) and Maffre et al. (2018) conducted experiments with the global mountains removed. They also showed a see-saw change in the Pacific and Atlantic, i.e., a strong (weak) PMOC (AMOC) in a world with a global flat continent. However, they did not pinpoint the individual roles of different mountains. Fallah et al. (2016) and Su et al. (2018) carried out the same experiments as we did, and drew the same conclusion that removing the TP only can lead to the AMOC weakening. However, the mechanisms we propose in this study are different from theirs.

In this study, we show that in different stages, the dominant processes for the AMOC change are different, that is, the wind effect increases the AMOC during stage-I and the freshwater effect decreases the AMOC during stage-II. In Su et al. (2018), they tried to explain the transient response of the AMOC; however, they used the physical values from the equilibrium stage, which is not correct in our view. We agree that the detailed processes that lead to the AMOC collapse are different in different studies. Studies using more coupled models, with more deliberately designed topography experiments, are still needed.

Recently, we also finished an experiment with only TP removed (Fig. R7a), while the Mongolian Plateau is unchanged. This experiment is called No_OnlyTibet. The AMOC change in No_OnlyTibet is almost identical to that in NoTibet (Fig. R7b), suggesting that the TP is important to the AMOC, while the Mongolian Plateau is not. However, for the PMOC, although the TP is the most important, the Mongolian Plateau does play a role. This is qualitatively consistent with the finding of White et al. (2017), which disclosed an important role of the Mongolian Plateau in the Pacific wintertime atmospheric circulation. Since both the wind-driven and thermohaline dynamics are important to the PMOC establishment in our view, the Mongolian Plateau can affect wind-driven part of the PMOC through its role in the Pacific atmospheric circulation. We will study detailed processes of Mongolian Plateau's effect on the PMOC in the near future.

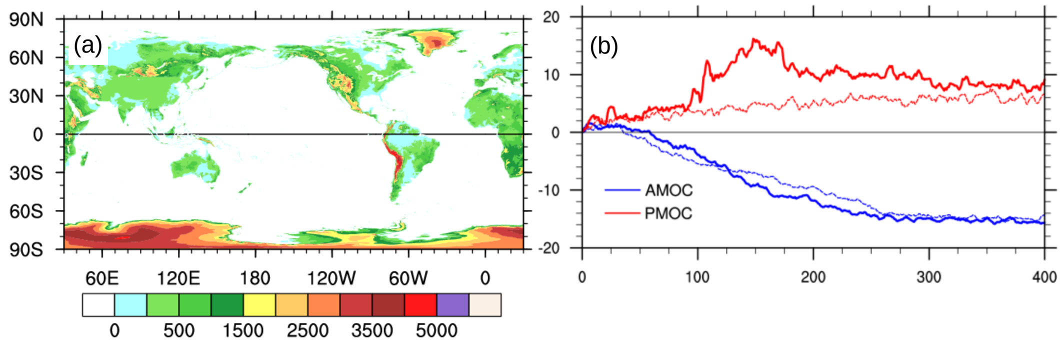

Fig. R7 (a) Topography configuration without Tibetan Plateau (60°-130°E, 20°-45°N) (No_OnlyTibet). (b) Temporal evolutions of the PMOC (red) and AMOC (blue) in NoTibet (solid curves) and No_OnlyTibet (dashed curves).

We are also studying the individual roles of the TP, Rocky Mountains and Andes Mountains. Preliminary results suggest that **only the TP removal can cause significant changes in AMOC and PMOC (please refer to Fig. R6).** Removing other mountains will not cause changes in the global meridional overturning circulations. We will provide more details in our next paper.

Specific comments:

1. Introduction. The last two paragraph looks more like summary rather than review or motivation.

Response: Thanks for this comment. We have carefully revised this paper, including its introduction.

2. Fig. 2. How to define the AMOC index in CESM1.0? It should be explained in text.

Response: Thank you very much for this suggestion. The AMOC index is defined in this revision.

3. Significance test is absent in all figures.

Response: Thank you for your comments. The changes of variables in this study are significant in both stage-I and stage-II. Actually, we did Mann-Kendall test for all figures related to changes. For cleanness of figures in the text, we decided not to show the significance test. Fig. R8 shows the SST, SSS and SSD changes during stage-I with significance levels. Fig. R9 shows the mixed layer depth (MLD) change in the two stages with significance levels. Changes of all these variables exceed the 95% significance level in most regions according to the Mann-Kendall trend test.

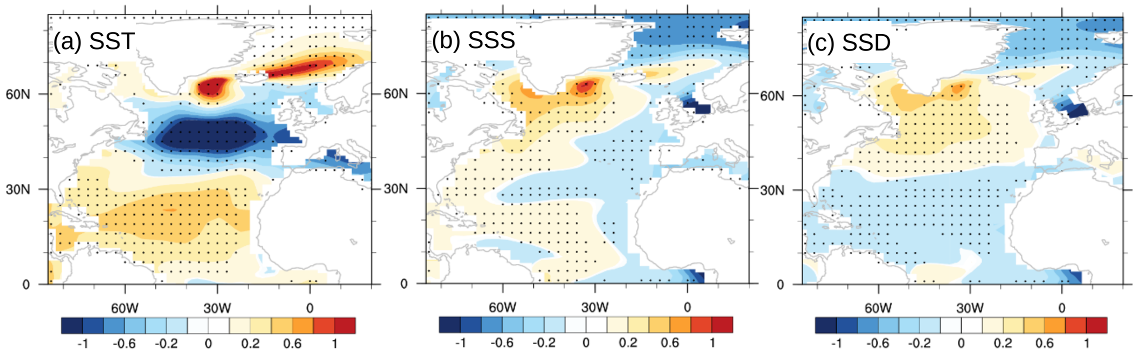

Fig. R8 Changes in (a) SST (°C), (b) SSS (psu) and (c) SSD (kg/m^3) in Stage-I of NoTibet. Stippling indicates changes exceeding the 95% significance level according to the Mann-Kendall trend test.

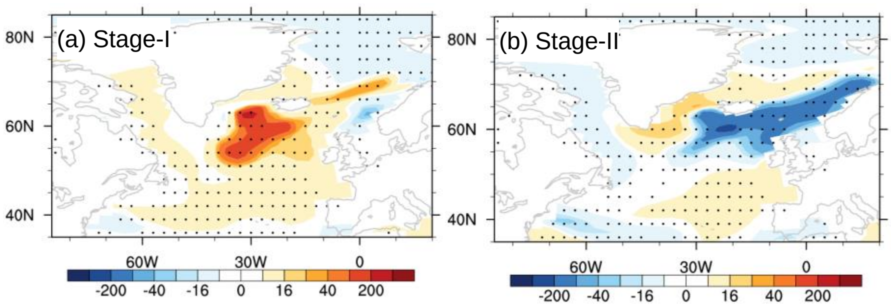

Fig. R9 Changes in annual mean mixed layer depth (MLD; m) during **(a)** stage-I and **(b)** stage-II. Stippling indicates changes exceeding the 95% significance level according to the Mann-Kendall trend test.

4. Line 162-163. "... and the wind-driven circulation in the tropics is nearly unaffected by the removal of the TP?" From where this result can be found?

Response: Thank you for asking. Here, we refer to the shallow wind-driven circulation, that is, the wind-driven subtropical cell (STC). From Figs. 2c and d, we can see the wind-driven STC in the tropical Atlantic is roughly unaffected by the TP removal. We can also see this in Fig. R10. The wind-driven STC is outlined by the red rectangle box. The left panel of Fig. R10 is the same as Figs. 2c and d, in which the color shows the change of the meridional overturning circulation. The right panel of Fig. R10 shows the mean AMOC in Real and in Stage-II of NoTibet. We have revised this paragraph and made the point clearer.

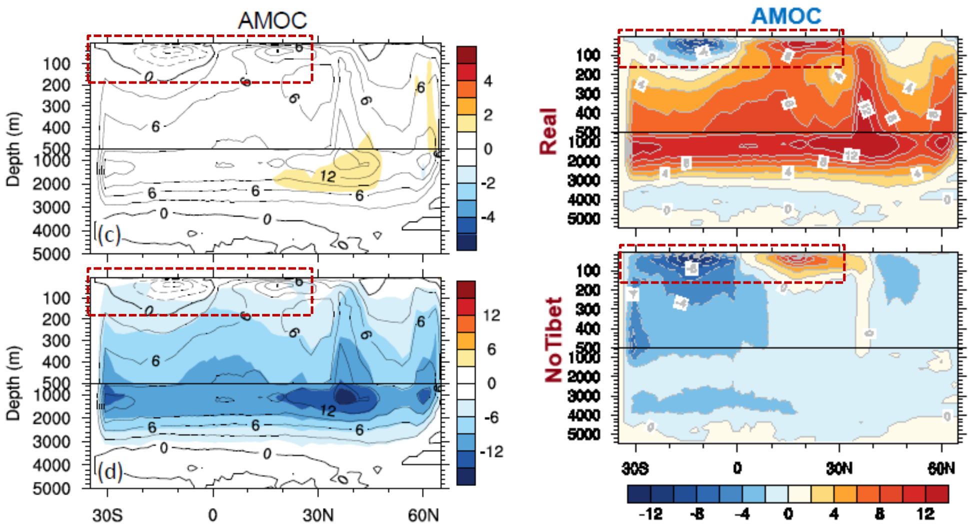

Fig. R10 Left panels: The mean AMOC pattern in Real (black contour) and its changes (color) in (upper) Stage-I and (lower) Stage-II of NoTibet. Right panels: The mean AMOC in (upper) Real and in (lower) Stage-II of NoTibet. The wind-driven STC is outlined by the red rectangle box.

5. *Line 171-172. "The SSD change consists of SST-induced change (dashed red) and sea surface salinity (SSS)-induced change (dashed blue). How to calculate these two parts?*

Response: In the CESM ocean model, the equation of state is given by

$$\rho = \rho_0 + \alpha\Theta + \beta S + \text{higher order term} \quad (1)$$

where ρ is seawater density in g/cm^3, Θ is potential temperature in °C, and S is salinity in psu. $\alpha = -2.5^{-4}$ g/cm^3/°C is thermal expansion coefficient, $\beta = 7.6 \times 10^{-4}$ g/cm^3/°C is saline contraction coefficient. In Real and NoTibet, the equation of state can be written, respevtively, as

$$\rho_1 = \alpha\Theta_1 + \beta S_1 \ , \qquad \rho_2 = \alpha\Theta_2 + \beta S_2 \quad (2)$$

where subscripts 1 and 2 represent Real and NoTibet, respectively. If we fix the temperature as Real, the equations of state in these two simulations are

$$\rho_{1s} = \alpha\Theta_1 + \beta S_1 \ , \qquad \rho_{2s} = \alpha\Theta_1 + \beta S_2 \quad (3)$$

So, the density change caused by salinity can be obtained by

$$\delta\rho_s = \rho_{2s} - \rho_{1s} \quad (4)$$

Similarly, the density change caused by temperature can be obtained by

$$\delta\rho_t = \rho_{2t} - \rho_{1t} \quad (5)$$

Note that in Eqs. (2)-(5), we omit the higher-order terms for simplicity. In our actual calculations, the higher-order terms are considered.

6. Fig.4. Averaged for which longitudes?

Response: Fig. 5 (old Fig. 4) is averaged over the whole Atlantic denoted by the yellow in Fig. R11. The CESM ocean model can output the basin mask data, and "6" represent the Atlantic.

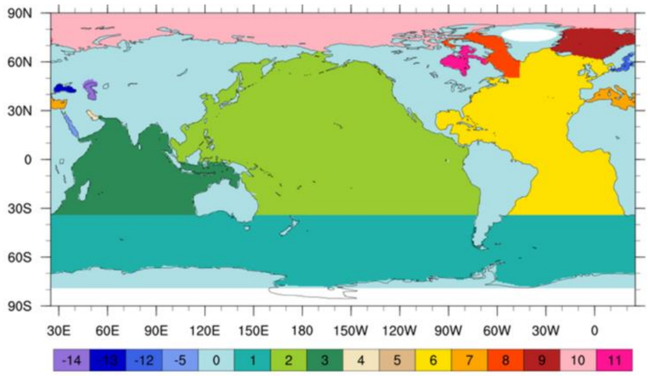

Fig. R11 Basin mask in the ocean model (POP2) of CESM.

7. Equation 1. What are the meaning of AH and k ?

Response: Thank you very much for pointing out our carelessness. In the revision, we added description of these two parameters, namely, A_H is horizontal diffusion coefficient, and κ is vertical mixing coefficient.

8. Fig.6a. The cooled ocean area should be within 40-50 °N , different with the stated in the text.

Response: Thanks for this comment. The cooled ocean is shown in Figs. 4a and 4b. It is within 40-60°N in Stage-I and within the whole North Atlantic in Stage-II. Fig. 7a (old Fig. 6a) shows the temperature tendency caused by LH and SH, which has the cooling effect for the region of 50-70°N.

9. Line 312-313. "The sea-ice melting takes effect later than the AMOC weakening". How can tell?

Response: Thanks for asking. The AMOC weakening starts from year 60 after the TP removal, and persists nearly linearly for 200 years. The significant sea-ice melting starts about 150 years after the TP removal (orange curve, Fig. 8a), caused by remarkable southward sea-ice expansion (Fig. 9a) at the same time. The sea-ice margin can thus approach 50°N in 200 years after the TP removal. This will further the AMOC weakening, by providing a great amount freshwater to the ocean. From the temporal evolutions of the AMOC and sea ice, we can tell the sea-ice melting takes effect later than the AMOC weakening.

10. Fig. 10b and corresponding text. It seems that the eastward wave activity flux originates from the north Atlantic and propagates to the Tibetan Plateau, and then disappears.

Response: Thanks for this comment. Since the source of perturbation is the TP, we say the wave flux propagation from the North Pacific to the North Atlantic. If the source of perturbation is located over the North Atlantic, the wave flux pattern might be similar to that in Fig. 11b (old Fig. 10b); then, we can say it propagates from the North Atlantic. We will study the perturbation of the Rocky Mountains in our future work.

11. Line 409-413. The explanation here is not clear enough.

Response: Thank you very much for this comment. This paragraph is removed in the revision.