Replies to Reviewer #1:

Thank you very much for giving us the second chance to improve this manuscript. We have revised the manuscript carefully based on your constructive suggestions. The following are our point-by-point replies.

This is the second review and I first apologize for taking so long to complete this review.

In my first set of comments, I recommended the manuscript to be returned to the authors with minor revisions. The experimental setup was sound and the results generally robustly demonstrated but the context in which the study is integrated was not very well described and the relevance of the findings to the understanding of the MOC evolution remained unclear.

I do not think the manuscript has been much improved from this perspective.

To be more specific, I still think that the core of the paper, that is, the physical results, is a valuable addition to the literature on the role of mountains in the climate system, and I would like to see these results published. However, the Discussion and, to a lesser extent, the Introduction, are still not up to the job, in particular for publication in a journal like Nature Communications.

The Introduction, which mostly reviews the Cenozoic evolution of the AMOC/PMOC and of the mountain ranges that are investigated later in the manuscript (l. 66-99), has been slightly expanded and reads better, though some errors remain (see below). My issue in this section is mainly with the last paragraph, which needs to be re-written to 1) state more clearly why the authors chose to keep the modern land-sea mask and 2) not elude the fact that your Flat2Real simulation is an attempt of simulating the Cenozoic AMOC/PMOC evolution.

Something along these lines for example: "Here, as a first step, we numerically investigate how the presence or absence of various major mountain ranges affect the GMOC as well as their cumulative impact when they are uplifted sequentially in the model in an initial flat Earth simulation. In order to isolate the topographic effects from plate tectonics and long-term bathymetric and atmospheric changes, we keep the modern bathymetry and continental positions, as well as modern greenhouse gas concentrations, incident solar radiation and orbital parameters. We show that, in our simulations, the TP uplift is the primary driver of the shutdown of a PMOC and initiation of an AMOC, although high Antarctic topography is required to drive a strong, modern-like, AMOC. We further discuss the implications of our results to the long-term Cenozoic history of the AMOC/PMOC."

Responses: Thank you very much for your valuable comments. The last paragraph of the introduction is revised following your suggestion (see below). Please also refer to lines 45-53 of the revised text.

"Here, as a first step, we numerically investigate how the presence or absence of various major mountain ranges affect the GMOC as well as their cumulative impact when they are uplifted sequentially in the model from an initial flat Earth. In order to isolate the topographic effects from plate tectonics and long-term bathymetric and atmospheric changes, we keep the modern bathymetry and continental positions, as well as modern greenhouse gas concentrations, incident solar radiation and orbital parameters. We show that, in our simulations (Methods and Table 1), the TP uplift is the primary driver of the PMOC shutdown and the AMOC initiation, although high Antarctic topography is required to drive a strong, modern-like AMOC. We further discuss the implications of our results to the long-term Cenozoic history of the AMOC/PMOC."

The Discussion is perhaps more problematic, as it currently stands, and I note that it has only barely been revised, in spite of comments from both reviewers. The work presented here may well be "just a beginning" as the authors claim in their rebuttal, yet this must not preclude them to better discuss their results.

Here are a couple of comments to improve it:

1) the 3rd paragraph (l. 289-300) reads quite weird. It starts with "rather than mimicking past climate evolution" but the next sentence ends with "The results […] provide insight into […] the climate development in the Late Cenozoic". A few lines later "this study establishes the direction of MOC change as a function of increasing uplift over time". What is the goal of the study in this case? I do not see it problematic that the authors attempt to interpret their results in light of long-term Cenozoic climate change but it needs to be discussed: what are the implications of a modern land-sea mask? And of modern gateways and/or greenhouse gas?

Responses: Thank you very much for your valuable comments. This paragraph is rewritten based on your suggestion (see below). Please also refer to lines 248-264 of the revised text.

"This research marks the beginning of our investigation into the effects of the presence or absence of various major mountain ranges on the GMOC, as well as their cumulative impact under modern conditions. Our research aims to specifically discern the impact of topography on ocean circulation from other contributing factors, by incorporating modern bathymetry and continental configurations, current greenhouse gas levels, incident solar radiation, and orbital parameters in our experimental setup. The individual mountain uplift experiments in this study allow us to explore the linkages between uplift regions and climate changes in remote areas of the modern world. The sequential mountain uplift experiment Flat2Real provides insight into specific periods of paleoclimate, such as the climate development in the late Cenozoic. Previous studies suggested that the TP uplift played a key role in shaping Cenozoic climate through circulation changes and weathering[13,49,50]: over the past 40 million years, this uplift led to substantial deflection of the atmospheric jet stream, intensified monsoonal circulation, increased rainfall on the front slopes of the Himalayas, and conductive conditions for the formation of deep and intermediate waters in the North Atlantic. By using experiment Flat as a starting point and tracing the changes due to various uplifts, we can quantify the linkages between uplifts and climate changes and understand how the MOC changes as the progressive uplift of continental mountains."

The implications of a modern land-sea mask, ocean gateways and greenhouse gas in our experiments implies that the TP impact on the GMOC could be exaggerated. We have added the discussion in the following paragraph.

2) Just a single line stating that the authors "did not consider the effects of continental drift and oceanic gateway switches" is just simply not enough, in particular as the interpretation of the results as consistent with the general direction of Cenozoic climate change is repeated several times in the Discussion (see comment above and again l. 316).

Responses: Thank you very much for your valuable comments. In this paragraph, more discussions are added (see below). Please also refer to lines 270-282 of the revised text

"Additionally, we did not consider the effects of continental drift and oceanic gateway switches, nor did we treat Greenland and Antarctic glaciation dynamically. *The setup of modern land-sea mask and ocean gateways in our experiments implies that the TP impact on the GMOC could be exaggerated. After all, the Tethys Seaway closing, the Panama Isthmus closing and the Bering Strait opening all occurred after the TP uplift*[8]*, all of which may lead to significant increases in the continental ice sheets in both hemispheres and enhance the NADW formation*[51-53]. Furthermore, the background climate in the experiments uses the preindustrial conditions with constant CO_2, and the effect of chemical erosion in rapidly uplifted areas on atmospheric CO_2 is not accounted for. Typically, atmospheric CO_2 levels cannot remain in a steady state during the time of intense tectonism, which can last for millions of years, due to the temperature-weathering feedback mechanism. *The TP uplift may have resulted in a decrease of atmospheric CO_2 level, which can also enhance the NADW formation due to the growth of large continental ice sheets in the NH*[49]."

3) The list of other studies referenced in the last paragraph should be better integrated. For example, the authors state that "Maroon discovered that the RM can have a significant effect on GMOC through its impact on hydrology", which is opposite to what the authors found in their experiments (l. 242-245). Why?

Responses: Thank you very much for this suggestion. In the revised manuscript, we added several new references and more analyses in several places. Due to the limitation of paper length and references required by the journal, we deleted these lines and three references. However, here we would like to provide an answer to why Maroon's finding is difference from ours.

In Maroon (2016), removing only RM leads to a significant weakening of the AMOC by about 30%, however, removing the orography globally only leads to the AMOC weakened slightly. The latter result is not consistent with results of Schmittner et al. (2011), Sinha et al. (2012) and our study.

Maroon herself stated in her PhD. thesis that "*The AMOC collapses due to the removal of global orography in the models of Schmittner et al. (2011) and Sinha et al. (2012), a result which our simulations do not produce. We performed an additional simulation (not shown) in which we removed orography globally and found that the AMOC weakened slightly …*". "*The AMOC in our model is not as sensitive to orography as in the models used in other studies. One possible explanation for the weak response of the AMOC in CM2Mc is that the region of net freshening (P-E>0) extends too far south in our Control simulation when compared to the freshwater input from observed storm track. This bias likely originates from the low resolution of the atmospheric model (Brayshaw et al., 2009) and is reflected in the SST biases in the North Atlantic*".

Maroon also stated that "*Warren (1983) argued that the North Pacific does not have a PMOC because P - E > 0. Following his argument, the freshwater biases in the North Atlantic of the Control simulation should make the North Atlantic more similar to the North Pacific and prevent an AMOC.* ***And yet, there is an AMOC in the Control simulation, and it is relatively insensitive to the removal of the Rocky Mountains. These results suggest that the Rockies are not the primary reason why there is an AMOC.***"

Specific comments in the Introduction:

1. *l. 68-70. Worth integrating DeepMIP results? Most models do not simulate an AMOC or PMOC in the Early Eocene (Zhang et al. 2022).*

Responses: Thank you very much for this suggestion. The results from DeepMIP project are very interesting and provide us with new insight.

In the revised manuscript, we removed the old reference 5 and cited Zhang et al. (2022) as the reference 8. In lines13-16 of the revised text, we state that "a recent study from DeepMIP project found that neither model results nor proxy data suggest NADW formation during the early Eocene, while the evidence for NPDW formation remains inconclusive[8].”

2. *l. 70-71. “during the Early Oligocene period (about 35-33 Ma)” => “at a later stage”. By the way, the 35-34 Ma interval is still Eocene.* Revised as suggested.
3. *l. 77. “the former has a net evaporation” => “the former is a net evaporative basin”.* Revised
4. *l. 81. “is believed” => “is also believed”* Revised
5. *l. 82. Incorrect reference. Ref. 12 does not discuss the impact of Drake and Tasman gateways on the AMOC. Could cite Hutchinson et al. (2019) instead.*

Responses: Thank you very much for pointing out this mistake. The reference is changed to Hutchinson et al. (2019).

6. *l. 84-85. “can precipitate contrasting shifts in the global MOC” => “holds the potential to trigger large-scale transitions in the global MOC”.* Revised.
7. *l. 87 and throughout. “over the Antarctica” => “over Antarctica” or “over the Antarctic continent”.* Revised.
8. *l. 88-89. “the glaciation of the Antarctica is believed to have occurred during the Eocene-Oligocene (56-23 Ma).” This is very vague. The consensus today is that the first large-scale glaciation of Antarctica occurred at the EO transition (34-33.5 Ma).*

Responses: Thank you very much for this suggestion.

In lines 31-32 of the revised text, this statement is changed to “… the first large-scale glaciation of Antarctica is believed to have occurred during the Eocene-Oligocene transition period (34-33.5 Ma)[20,21]…”.

We added two relevant references as ref. 20, 21 in the revised manuscript:

20. Jamieson, S. S. R., Sugden, D. E., & Hulton, N. R. J. The evolution of the sub-glacial landscape of Antarctica. *Earth and Planetary Science Letters* **293**, 1-27 (2010).

21. Jamieson, S. S. R., Ross, N., Paxman, G. J. G., Clubb, F. J. An ancient river landscape preserved beneath the East Antarctic Ice Sheet. *Nat Commun* **14**, 6507 (2023).

9. *l. 90. Incorrect reference. Ref. 20 is not about the Andes. I suggest that the authors use the Boschmann (2021) paper as their new reference on the uplift of the Andes, and revise their chronology in consequence.*

Responses: Thank you very much for pointing out this mistake.

The reference 20 is changed to Boschmann (2021). In lines 32-33 of the revised text, the statement about AMs is changed to “The uplift of Andes Mountains (AMs) started in the Late Cretaceous (~70 Ma)[22] and matured around 15-10 Ma[23,24].”

10. *l. 93-94. “the rapid and main uplift of the TP was realized between 10 and 8 Ma”. Not sure this is correct. At least two of the references (25-29) are > 30 years old and they should be updated, given the dynamism existing about the TP uplift history. Latest reviews suggest that only small parts of the TP reached their modern elevation after the Early (20 Ma) or Mid (15 Ma) Miocene. See, e.g., Tardif et al. (2023).*

Responses: Thank you very much for this comment. We rewrote the statement on the TP based on your suggestion. Please also refer to lines 35-44 of the revised text.

“The timeline for the formation of the Tibetan Plateau (TP) is a topic of highly debate. Some studies argue that parts of the TP were already in place during the late Eocene (38-33 Ma)[26,27], while other research suggest that the TP's rapid and main uplift occurred between 10 and 8 Ma[28-31]. A more recent study proposes that most of the TP had attained its current elevation before the Mid-Miocene (15 Ma)[32]. This timing coincides with the onset of NADW formation, suggesting a possible link between the TP uplift and the development of NADW. Recent research also indicates that the TP is a critical factor affecting changes in the GMOC[33,34]. Nevertheless, it is important to recognize that the chronology of the uplift of major mountain ranges remains a subject of intense discussion and investigation.”

Here we removed the old reference 26 and added Tardif et al (2023) as reference 32.

In the end, I think that the revisions required are still quite minor, but necessary, and I trust the authors to adequately revise their manuscript.

Responses: Thank you very much for your invaluable comments, which help us to improve this manuscript greatly. All 4 references you provided are cited in the revised manuscript.

References

1. Boschman, L. M. (2021). Andean mountain building since the Late Cretaceous: A paleoelevation reconstruction. Earth-Science Reviews, 220, 103640. https://doi.org/10.1016/j.earscirev.2021.103640
2. Hutchinson, D. K., Coxall, H. K., O′Regan, M., Nilsson, J., Caballero, R., & de Boer, A. M. (2019). Arctic closure as a trigger for Atlantic overturning at the Eocene-Oligocene Transition. Nature Communications, 10(1), 3797. https://doi.org/10.1038/s41467-019-11828-z
3. Tardif, D., Sarr, A. C., Fluteau, F., Licht, A., Kaya, M., Ladant, J. B., ... & Banfield, W. (2023). The role of paleogeography in Asian monsoon evolution: Associated a review and new insights from climate modelling. Earth-Science Reviews, 104464. https://doi.org/10.1016/j.earscirev.2023.104464
4. Zhang, Y., de Boer, A. M., Lunt, D. J., Hutchinson, D. K., Ross, P., van de Flierdt, T., ... & Huber, M. (2022). Early Eocene ocean meridional overturning circulation: The roles of atmospheric forcing and strait geometry. Paleoceanography and Paleoclimatology, 37(3), e2021PA004329. https://doi.org/10.1029/2021PA004329

Replies to Reviewer #2:

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

The manuscript by Haijun Yang et al. offers a comprehensive description of the physical mechanisms influenced by the presence / absence of large landform systems. The article is commendably clear and well-written. The responses to the reviewers' inquiries provide valuable insights that enhance our understanding of the authors' methodology.

The authors appropriately acknowledge the study's limitations in the conclusion, notably highlighting factors such as the paleogeography anchored in the present and the absence of atmospheric CO2 variation, preventing direct comparison with the evolution of Cenozoic climates. I fully concur with this assessment.

However, in the conclusion, certain sentences imply that Tibet played a dominant role in the evolution of deep-water formation. To enhance clarity, it is advisable to ensure that the key points and the summary explicitly convey the nuanced nature of the study's findings and the limitations acknowledged in the conclusion. This adjustment will contribute to a more precise and comprehensive understanding of the research presented.

Responses: Thank you very much for these suggestions.

In Section summary and discussion, we added statement in lines 248-253 of the revised text to explicitly address the goal of this study as “This research marks the beginning of our investigation into the effects of the presence or absence of various major mountain ranges on the GMOC, as well as their cumulative impact under modern conditions. Our research aims to specifically discern the impact of topography on ocean circulation from other contributing factors, by incorporating modern bathymetry and continental configurations, current greenhouse gas levels, incident solar radiation, and orbital parameters in our experimental setup.”

And we also added statements to address the limitation of this study. In lines 272-276 of the text we state that “The setup of modern land-sea mask and ocean gateways in our experiments implies that the TP impact on the GMOC could be exaggerated. After all, the Tethys Seaway closing, the Panama Isthmus closing and the Bering Strait opening all occurred after the TP uplift[8], all of which may lead to significant increases in the continental ice sheets in both hemispheres and enhance the NADW formation[51-53].” Further in lines 280-282 we state that “The TP uplift may have resulted in a decrease of atmospheric CO_2 level, which can also enhance the NADW formation due to the growth of large continental ice sheets in the NH[49].”

1. *Key 1 : The authors contrast their results with previous studies, but the conditions of the Jones and Cecci (2017) study are different, so the comparison seems risky to me.*

Responses: Thank you very much for this comment.

In lines 18-25 of the revised text, we listed three reasons that cause the different overturning modes between the Atlantic and Pacific: the asymmetry of net surface freshwater flux, the basin geometry and the ocean gateways. The key point #1 is also revised.

Jones and Cecci (2017)'s study focused on the effect of ocean basin geometry on the GMOC. They showed that the North Atlantic has higher salinity than the North Pacific because the Atlantic basin is narrower than the Pacific basin. They further explained that, because the southward western boundary current associated with the wind-driven subpolar gyre has higher velocity in the wide basin than in the narrow basin, it overwhelms the northward western boundary current associated with the MOC for wide-basin sinking, so freshwater is brought from the far north of the domain southward and forms a pool on the western boundary in the wide basin. The fresh pool suppresses local convection and spreads eastward, leading to low salinities in the north of the wide basin for wide-basin sinking. While in the narrow basin, the northward MOC western boundary current overcomes the southward western boundary current associated with the wind-driven subpolar gyre, bringing salty water from lower latitudes northward and enabling deep-water mass formation.

Jobes and Cecci (2017) did a perfect work to understand the different overturning modes from the point of view of ocean dynamics, particularly the wind-driven circulation dynamics, through sensitivity experiments using an ocean model (MITgcm). In their work, readers can learn a lot about how the wind-driven circulation affects the thermohaline circulation and their interplay.

Our work focused on the effect of the surface freshwater flux on the GMOC under the condition of current realistic ocean geometry. We just would like to show that, provided with an ocean geometry, the continental orography may significantly affect the global hydrological cycle and thus the surface freshwater fluxed over different oceans, which can cause remarkable changes of overturning modes in different oceans.

Our conclusion does not have to contradict to that of Jones and Cecci (2017), neither we compare our results with that of Jones and Cecci (2017). Our results are obtained from a coupled Earth system model while Jones and Cecci's results were obtained from an ocean GCM.

In lines 18-25 of the revised text, this paragraph is rewritten as follows.

"The asymmetry of net surface freshwater flux is often cited as the cause of different overturning modes between the Atlantic and Pacific. The North Atlantic has higher sea-surface salinity (SSS) than the North Pacific because the former is a net evaporation basin, while the latter is nearly neutral[9]. Additionally, ocean basin geometry plays a role in the different overturning modes. Research indicated that narrow basins are more conducive to deep overturning circulation than wide basins[2,10,11]. Furthermore, ocean gateways also contribute to the different overturning modes. The opening of the Drake Passage/Tasman Seaway in the late Eocene is thought to have promoted the NADW formation and thus the AMOC formation[12]."

2. *L84: add more recent references*

Responses: Thank you very much for this suggestion. We have replaced ref. 12 with a more relevant reference "Hutchinson, D. K., Coxall, H. K., O′Regan, M. *et al.* Arctic closure as a trigger for Atlantic overturning at the Eocene-Oligocene Transition. *Nat Commun* **10**, 3797 (2019)". This is also suggested by Reviewer #1.

3. *L89: The authors are encouraged to update the age of glacial inception in Antarctica, recognizing that the onset of glaciation in Antarctica is typically dated to the Late Eocene. This timing is attributed to a CO2 atmospheric level that was considered too high before the Late Eocene. Please consider incorporating a relevant reference to support this updated information, providing a solid foundation for the revised age of glacial inception in Antarctica.*

Responses: Thank you very much for this comment.

In lines 29-32 of the revised text, this statement is rewritten as follows.

"Although the transantarctic and Gamburtsev Mountains over Antarctica were likely already present at the start of the Cenozoic (65 Ma)[18,19], the first large-scale glaciation of Antarctica is believed to have occurred during the Eocene-Oligocene transition period (34-33.5 Ma)[20,21]"

We added two relevant references as ref. 20, 21 in the revised manuscript:

20. Jamieson, S. S. R., Sugden, D. E., & Hulton, N. R. J. The evolution of the sub-glacial landscape of Antarctica. *Earth and Planetary Science Letters* **293**, 1-27 (2010).

21. Jamieson, S. S. R., Ross, N., Paxman, G. J. G., Clubb, F. J. An ancient river landscape preserved beneath the East Antarctic Ice Sheet. *Nat Commun* **14**, 6507 (2023).

4. *L90: Change the reference.*

Responses: Thank you very much for this suggestion. The old reference 20 is changed to Boschmann (2021) (ref. 22 in the revised manuscript). And the statement about AMs is changed to "The uplift of Andes Mountains (AMs) started in the Late Cretaceous (~70 Ma)[22] and matured around 15-10 Ma[23,24]."

5. *L83-99: The paragraph in question lacks clarity as the authors attempt to draw a comparison between the age of Antarctic glaciation and the age of the Andes uplift. It is recommended that the authors revisit and revise this paragraph to articulate the comparison more explicitly, providing a clearer and more coherent presentation of the relationship between the two geological events.*

Responses: Thank you very much for this comment. This paragraph is revised significantly following suggestions from you and Reviewer#1. The references within are also changed and updated. The revised paragraph reads as follows:

"Geological evidence also suggests that the uplift of large continental mountains has had a significant impact on the climate[13]. The evolution of continental terrain holds the potential to trigger large-scale transitions in the global MOC (GMOC)[14-16]. The Rocky Mountains (RMs) rose from the sea level about 80 Ma[17], and reached its current elevation about 45 Ma[8]. Although the transantarctic and Gamburtsev Mountains over Antarctica were likely already present at the start of the Cenozoic (65 Ma)[18,19], the first large-scale glaciation of Antarctica is believed to have occurred during the Eocene-Oligocene transition period (34-33.5 Ma)[20,21]. The uplift of Andes Mountains (AMs) started in the Late Cretaceous (~70 Ma)[22] and matured around 15-10 Ma[23,24]. The uplift of these mountains predated the onset of the NADW formation. The Greenland (GL) underwent its initial phase of uplift in the late Miocene (11-10 Ma)[25]. The timeline for the formation of the Tibetan Plateau (TP) is a topic of highly debate. Some studies argue that parts of the TP were already in place during the late Eocene (38-33 Ma)[26,27], while other research suggest that the TP's rapid and main uplift occurred between 10 and 8 Ma[28-31]. A more recent study proposes that most of the TP had attained its current elevation before the Mid-Miocene (15 Ma)[32]. This timing coincides with the onset of NADW formation, suggesting a possible link between the TP uplift and the development of NADW. Recent research also indicates that the TP is a critical factor affecting changes in the GMOC[33,34]. Nevertheless, it is important to recognize that the chronology of the uplift of major mountain ranges remains a subject of intense discussion and investigation."

6. *Leier et al. 2013: First uplift Early Miocene, second uplift Late Miocene – Andes Bolivia*

Responses: Thank you very much for this suggestion. We added the newer study of Leier et al. (2013) as the new reference 24.

7. *L142-151: The manuscript does not thoroughly discuss the intensification of the Pacific Meridional Overturning Circulation (PMOC) in relation to the uplift of the Andes*

Responses: Thank you very much for this comment. In this study, we did not provide detailed analyses on the PMOC change in response to the uplift of the Andes mountains (AMs), which, we think, deserve a full-length paper. We are working intensely on this issue through more sensitivity experiments. On page 2 of the revised manuscript, we delete the key point #5.

In lines 188-192 of the revised text, we briefly discussed the mechanism of the PMOC intensification in response to the uplift of the AMs as follows.

> "The presence of the AM reduces the equatorial trade wind, but amplifies the off-equatorial Ekman pumping (Extended Data Fig. 5a), thereby boosting the wind-driven STC in the South Indo-Pacific (Extended Data Fig. 5b) and augmenting the thermohaline component of the PMOC there, leading to a stronger PMOC (Fig. 3e2)."

8. *Figure 4 -Line 8: Does the case Flat2Real correspond to the end of the experiment when all reliefs are uplifted (year 5601-6000)?*

Responses: Thank you very much for this comment. Yes, the equilibrium change in Flat2Real with respect to Flat corresponds to year 5601-6000 of Flat2Real.

9. *L201: During the summer in Antarctica, the flat topography of the continent is likely to promote convection. Additionally, the presence of a high-pressure anomaly can indeed be seasonal.*

Responses: Thank you very much for this comment. We agree that "the flat topography in Antarctica during the summer is likely to promote convection", however, this convection is much weaker than that in the presence of the AT mountains. It is also likely that "the presence of a high-pressure anomaly can be seasonal". We would like to say that on seasonal timescale, the effect of convection or high-pressure anomaly cannot lead to significant change in AABW and AMOC/PMOC, which has to rely on the accumulated annual changes.

10. *Figure 5a1: the figure 5a1 represents the difference Real minus Flat (on the plot) or Flat2Real minus flat (in the caption)*

Responses: Thank you very much for this comment. Fig. 5a1 represents the difference between Flat2Real (year 5601-6000) and Flat. which is practically identical to the difference between Real and Flat. We corrected the small mistake on the plot.

11. L246-262: The manuscript describes the retreat of sea ice in the subpolar Atlantic, but the underlying cause of this retreat is not clearly explained. It is crucial to address this gap by providing a detailed and explicit discussion on the factors driving the retreat of sea ice in the subpolar Atlantic.

Responses: Thank you very much for this comment.

This paragraph is revised and states explicitly what initialize the sea ice change in the subpolar Atlantic (see below). Please also refer to lines 195-221 of the revised text.

"The AMOC gradually increases in the first few hundred years after the TP uplift in both Flat2Real and TP+AT, followed by an acceleration and eventual return to a normal state in Real (Figs. 1, 2b). The latter process is accompanied by a swift sea-ice loss in the subpolar Atlantic. In response to the TP uplift, more atmospheric moisture converges (diverges) over the North Pacific (Atlantic) (Extended Data Figs. 2b2, 2b5, 3a2, 3a3), shutting down the PMOC and triggering a gradual increase of the AMOC at the same time. The latter enhances the northward heat transport in the Atlantic, leading to a gradual retreat of sea ice in the subpolar Atlantic. The sea ice retreat is also helped by the anomalous northward Ekman flow, forced by the anomalous easterlies over the subpolar Atlantic (Extended Data Fig. 3b2).

The evolution of sea ice in Flat2Real is shown in Fig. 6. The northward retreat of sea ice is illustrated by the sea ice velocity in Fig. 6b, which leads to additional freshwater loss in this region (Extended Data Figs. 2b2, 2b5). This freshwater loss in turn increases SSD, the NADW formation and thus the AMOC consequently. In the first few hundred years after adding the TP, the sea-ice in the subpolar Atlantic retreats northward slightly (Fig. 6b), and the March mixed layer depth (MLD) deepens slightly (Fig. 6a), corresponding to a gradual increase of the AMOC. About 500 years after the TP uplift, the collective effects of accumulated saline water in the subpolar Atlantic and Ekman pumping in Southern Ocean accelerate the AMOC, so that the sea-ice margin retreats rapidly northward (Fig. 6b, dashed red curve), resulting in a large amount of freshwater flux loss in the subpolar Atlantic, a rapid deepening of the MLD (Fig. 6a), and a further enhancement of the AMOC (Figs. 1, 2b). The sea-ice margin reaches its quasi-equilibrium roughly 1000 years after the TP uplift (Fig. 6b), accompanied by significant sea-ice melting in the GIN seas. The sea-ice evolution in TP+AT (Extended Data Figs. 7a-b) displays a similar pattern to that in Flat2Real, while changes in sea ice and MLD in TP are minimal (Extended Data Figs. 7c-d). The evolutions of AMOC, the MLD and sea-ice coverage and margin in Flat2Real and TP+AT suggest a positive feedback between the AMOC and sea ice changes, which eventually leads to the establishment of AMOC. This feedback has been shown in many previous studies (e.g., Brady and Otto-Bliesner 2011; Yang and Wen 2020)."

Reference:

47. Brady, E. C., and B. L. Otto-Bliesner, 2011: The role of meltwater induced subsurface ocean warming in regulating the Atlantic meridional overturning in glacial climate simulations. Climate Dyn., 37, 1517–1532, https://doi.org/10.1007/s00382-010-0925-9

12. L289-291: the authors explained clearly on lines 289-291 that this study is based on sensitivity experiments (presence or absence of relief) and does not mimic past climate evolution. I fully agree with that. I regret that the key points and the abstract are less explicit.

Responses: Thank you very much for this comment.

In the revised manuscript, on page 2 the key point #1 is changed to "A series of coupled model sensitivity experiments with/without continental orography suggest that the thermohaline circulation could occur either in the Pacific or in the Atlantic, depending much on how the continental giant mountains affect the pattern of global hydrological cycle.", which explicitly state that all results are based on sensitivity experiments.

In abstract, we explicitly state that "we design a series of coupled model *sensitivity* experiments to investigate …", and tone down the TP's effect by removing the word "*paramount*".

13. Then the authors compare with their results with the climate evolution during the Cenozoic. I disagree with the sentence (L300) suggesting that "this study establishes the direction of MOC change as a function of increasing uplift over time". The position of continents (and relief) remains unchanged, as are the gateways (these limitations are indicated on L307).

Responses: Thank you very much for this comment. In the revised manuscript, this paragraph is revised significantly. Please refer to lines 248-264 of the revised text.

Particularly, the last sentence of this paragraph is changed to "By using experiment Flat as a starting point and tracing the changes due to various uplifts, we can quantify the linkages between uplifts and climate changes and understand how the MOC changes as progressive uplift of continental mountains."

14. The clarification provided in lines 289-291 regarding the study's reliance on sensitivity experiments rather than a direct mimicry of past climate evolution is appreciated. However, *there seems to be a discrepancy in the key points and abstract, where the explicitness might be lacking.*

Responses: Thank you very much for this comment. Please refer to our replies to Q.12-13, the key points, the abstract and the old paragraph in lines 289-300 are rewritten in the revised manuscript, in which both the consistency and explicitness are improved significantly.

15. Concerning the comparison with the climate evolution during the Cenozoic, there is disagreement with the sentence on line 300 suggesting that " this study establishes the direction of MOC change as a function of increasing uplift over time ". The disagreement arises from the understanding that the positions of continents (including relief) and gateways remain unchanged, as stated on line 307, and this should be reflected in the conclusions. A careful review and adjustment of these statements in the key points and abstract will contribute to a more accurate representation of the study's findings and limitations.

Responses: Thank you very much for these suggestions. Please also refer to our replies to Q.12-14, the key points, the abstract and Section "Summary and discussion" are rewritten to better state the findings and limitations of this study.