

## Replies to Reviewer #1:

We appreciate your time to review this manuscript and all your constructive comments. We have incorporated these suggestions into the revised manuscript. The following are our point-by-point replies.

### General Comments:

*“Unclear Research Motivation: It is not clear whether the study aims to investigate the influence of topography—such as continental configuration and mountain ranges—on the PMOC in general, or whether it specifically focuses on the PMOC during a particular paleoclimate period. For example, the Introduction refers to the strengthening of the PMOC during the Last Glacial Maximum and describes the impacts of the opening and closing of the Panama and Indonesian seaways in a rather dismissive manner. The authors also mention studies on how northern hemisphere topography, such as the Tibetan Plateau, affects the GMOC/AMOC, emphasizing that the PMOC has not been sufficiently discussed in previous work. These descriptions make the paper appear to address more general effects of topography and continental configuration rather than the mechanisms behind the intensified PMOC during the late Miocene to Pliocene. Such ambiguity is found not only in the Introduction but also throughout the manuscript. On the other hand, parts of the Introduction, as well as the Abstract and Conclusion, clearly state that the study’s main objective is to examine the impact of the Andean uplift from the late Miocene to Pliocene on the PMOC. After reading the entire manuscript, I found that the fundamental motivation of this study remains insufficiently conveyed.”*

**Response:** Thank you very much for this insightful comment. We agree that the research motivation was not stated with sufficient clarity in the original manuscript. The primary objective of this study is to investigate the impact of Andes topography on the PMOC using topography-sensitivity experiments, rather than to conduct a dedicated paleoclimate simulation for a specific period.

In the original version, our discussion of active PMOC during the late Miocene to Pliocene, as well as references to the opening of tropical seaways and other paleoclimate studies, may have obscured this focus and led to confusion regarding the intended scope of the study. These references were meant to provide broader context, but we acknowledge that they were overemphasized and not sufficiently aligned with our core objective.

To address this issue, we have thoroughly revised and reorganized the manuscript, with particular attention to the Abstract, Introduction, and Conclusion, to clearly state the motivation and scope of

the study. The revised manuscript consistently emphasizes that our goal is to isolate and clarify the role of Andes topography in modulating the PMOC, rather than to explain PMOC during a specific paleoclimate period.

*“Problems in Experimental Design: The comparison between the Flat and OnlyAndes experiments is inadequate for discussing actual paleoclimate conditions. The Flat configuration is unrealistic, and therefore examining the influence of Andean uplift against such an artificial baseline has little meaning. If the study evaluates how much the PMOC intensifies from the Flat state to the OnlyAndes state, the analysis essentially describes a transition from one unrealistic climate to another. Given the strong nonlinearity of the coupled atmosphere–ocean system, the basic climate state itself critically affects the system’s response to external forcing. Thus, the control experiment should reproduce as realistic a paleoclimate condition as possible. In this case, a more appropriate approach would be to use a topography and land–sea configuration representing the late Miocene to Pliocene as the control, and then perform sensitivity experiments by either uplifting or flattening the Andes.”*

**Response:** Thank you for your comments. Given our focus on the impacts of topography on the PMOC, it is reasonable to hold external forcings constant and isolate the effects of the Andes Mountains. To this end, we compared the *Flat* and *OnlyAndes* scenarios. This experimental design is consistent with numerous prior studies, including those by Kitoh (1997), Maffre et al. (2018), Sinha et al. (2011), and Wen and Yang (2020). The primary rationale for integrating experiments from the *Flat* configuration is that it more closely mimics natural geological processes in the real world.

Although our study is not centered on paleogeography or paleoclimate, our results offer valuable insights into the fundamental mechanisms driving the PMOC and its potential role in paleoclimate dynamics. We also examined the *Real* and *NoAndes* experiments, which exhibit minimal differences (see Fig. R1 and Fig. 3 in the revised manuscript). This is attributable to the influence of other topographic features under these conditions, such as the dominant role of the Tibetan Plateau in suppressing the PMOC. We have added these two experiments in the revised manuscript. In our future study, we plan to conduct experiments under Pliocene forcings to further elucidate the Andes Mountains' role from a paleoclimate perspective.

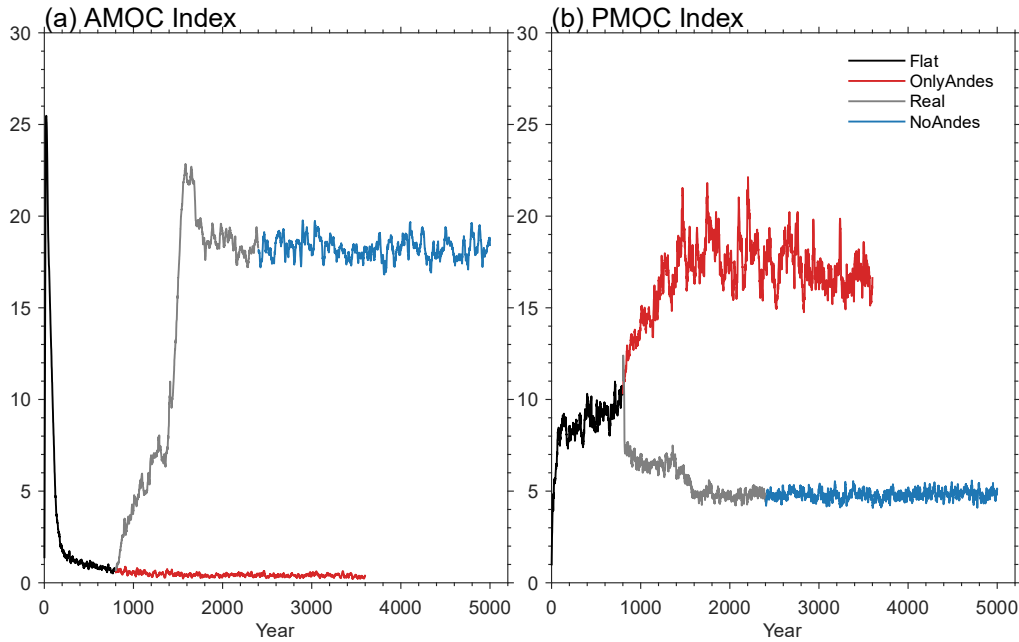


Fig. R1. Time series of the meridional overturning circulation in the *Flat*, *OnlyAndes*, *Real*, and *NoAndes* experiments, where *NoAndes* is constructed by removing the Andes Mountains from the realistic topography of the *Real* experiment.

*“Overly Detailed but Low-Relevance Setup Descriptions: In reality, the Andes did not suddenly uplift and induce an immediate atmospheric and oceanic response. Therefore, the extensive descriptions in the manuscript of the model’s initial wave adjustments and atmospheric setups are largely model-specific artifacts. While such analyses may have some limited value, the current manuscript devotes disproportionate space to them. At least the initial atmospheric responses appear to be of little significance. It would be sufficient to focus on the sequence whereby the Andean uplift modifies equatorial winds, subsequently alters oceanic circulation, and finally affects mid-latitude circulation in the North Pacific.”*

**Response:** Thank you for this insightful comment. We completely agree that the Andes did not uplift instantaneously in reality and that the very rapid atmospheric and oceanic responses following the imposed uplift represent idealized adjustments in the model framework. However, our intention in discussing these adjustments was not to suggest a realistic uplift timescale, but rather to elucidate the physical pathway through which Andean topography influences large-scale circulation.

Specifically, we aim to understand how the uplift modifies atmospheric circulation, how the ocean subsequently responds, and how ocean-atmosphere coupling acts to transmit the influence of South American topography to the subpolar North Pacific. In this context, the adjustment process helps clarify the sequence of dynamical interactions linking equatorial atmospheric changes to basin-

scale ocean circulation and, ultimately, to the strengthening of the PMOC. We consider this pathway analysis to be an important component of the study, particularly because the PMOC enhancement in the quasi-equilibrium state shares similarities with previously studied topography-induced changes in the AMOC.

That said, we acknowledge that parts of the original manuscript placed excessive emphasis on early transient features. In the revised version, we clarify the initial adjustment processes as a necessary step to disentangle the coupled mechanisms that ultimately shape the quasi-equilibrium responses. This restructuring allows the quasi-equilibrium signals—emerging from mutually interacting ocean–atmosphere processes—to be interpreted in a physically coherent framework, and strengthens the linkage between Andean uplift and mid- to high-latitude North Pacific circulation (Lines 385–389 in the revised manuscript).

## *Line-by-line comments*

### *1. Introduction*

*The overall logic and flow of the Introduction are not smooth, and a major reorganization is needed. In particular, the part discussing the LGM disrupts the narrative coherence and obscures the study's main motivation. Moreover, it gives an impression of insufficient respect for previous studies that investigated the influence of the Panama and Indonesian seaways. A more effective structure would be to first summarize when and how the PMOC intensified in each period, then describe what mechanisms have been proposed specifically for the late Miocene to Pliocene, and finally state the remaining gaps or limitations in previous research.*

*Line 74–87: This paragraph merely points out that the Panama and Indonesian seaways did not change during the LGM and therefore cannot explain the strong PMOC at that time. However, it does not rule out the possible influence of those seaways on the PMOC during the late Miocene to Pliocene. Unless the authors explicitly intend to deny such influence, this paragraph seems unnecessary.*

**Response:** We thank this detailed and constructive comment. We agree that the overall logic and narrative flow of the Introduction in the original manuscript were not sufficiently clear and that a major reorganization was necessary.

Following the suggestion, we have substantially restructured the Introduction to improve coherence and to clarify the study's motivation. Regarding the discussion of the Last Glacial

Maximum (LGM), we agree that this paragraph disrupted the narrative coherence and could give the impression of downplaying or dismissing previous studies on oceanic gateways.

As pointed out by the reviewer, the fact that the Panama and Indonesian seaways did not change during the LGM does not preclude their important influence on PMOC evolution during the late Miocene to Pliocene. In the revised manuscript, we removed this paragraph and have therefore revised the LGM-focused paragraph and restricted references to the LGM to brief contextual remarks only, without using it to evaluate or challenge the proposed gateway mechanisms.

We believe that these changes significantly improve the clarity, balance, and focus of the Introduction, and better position the present study within the context of previous work while clearly articulating its novel contribution.

## 2. Model and Experiments

*Line 130–131: Before describing the Flat experiment, the authors should clearly explain its purpose and relevance to the study's objectives. Without such clarification, readers are left unable to judge whether this highly unrealistic and extreme experiment is appropriate, and they finish reading the paper without being given sufficient basis for that judgment.*

**Response:** Thank you for the comment. Our goal is to isolate the impact of individual topographies on ocean circulation. While the Flat scenario is not tied to any specific geological period, using it as a benchmark allows us to assess the specific contribution of the Andes Mountains. However, in the context of all topographies, removing the Andes while keeping the other landforms would fail to isolate the Andes' individual contribution, as it would include the synergistic effects of the remaining topographies. We added the explanation in the revised manuscript (Lines 123-135).

*Line 137–138: Given that other topographic features are not realistically represented, it is questionable what can be learned by isolating the effect of the Andes alone. The impact of the Andes must depend on the basic climate field shaped by other major topographic features.*

**Response:** Thank you for this comment. We agree that the climatic impact of the Andes is modulated by the background climate field, which is itself shaped by other major topographic features. The purpose of isolating the Andes uplift in the present experiment is not to represent a fully realistic topographic configuration, but to diagnose the direct dynamical contribution of the Andes to the ocean-atmosphere system. This idealized approach allows us to identify the physical

pathways through which Andean topography influences atmospheric circulation and, in turn, the PMOC.

To address the reviewer's concern, we conducted additional sensitivity experiments (Andes2TP and TP2Andes) in which both the Andes and the Tibetan Plateau are uplifted. In such configuration, the PMOC weakens and ultimately shuts down, while the AMOC is slightly enhanced, leading to a circulation pattern more consistent with the modern climate (Figs. R2, R3). This result suggests that although the uplift of the Andes alone can promote a stronger PMOC, its effect can be completely suppressed by the subsequent uplift of the Tibetan Plateau. Please see Section 4d in the revised manuscript.

Together, these experiments indicate that even a limited set of major topographic changes is sufficient to illustrate how the PMOC may evolve during realistic uplift histories, in which the early influence of the Andes is later modified by the development of Northern Hemisphere topography. We have added clarification in the manuscript to emphasize this point.

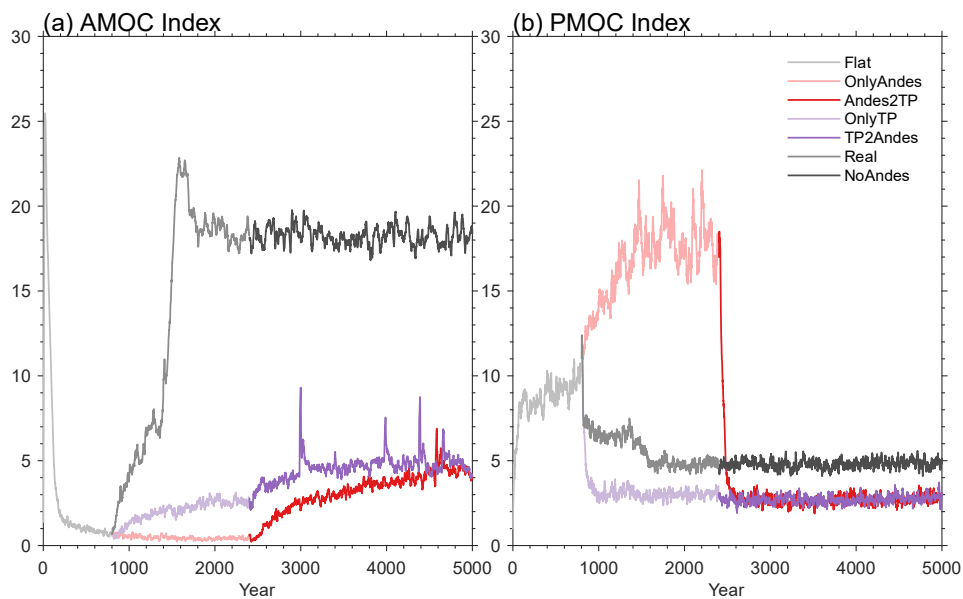


Fig. R2 Time evolution of the AMOC and PMOC indices in all Andes-related topographic experiments, including *Flat* (all major topography removed), *OnlyAndes* (Andes uplift only), *Andes2TP* (further uplift of the Tibetan Plateau from *OnlyAndes*), *OnlyTP* (Tibetan Plateau uplift only), *TP2Andes* (further uplift of the Andes from *OnlyTP*), *Real* (realistic topography), and *NoAndes* (Andes removed from *Real*).

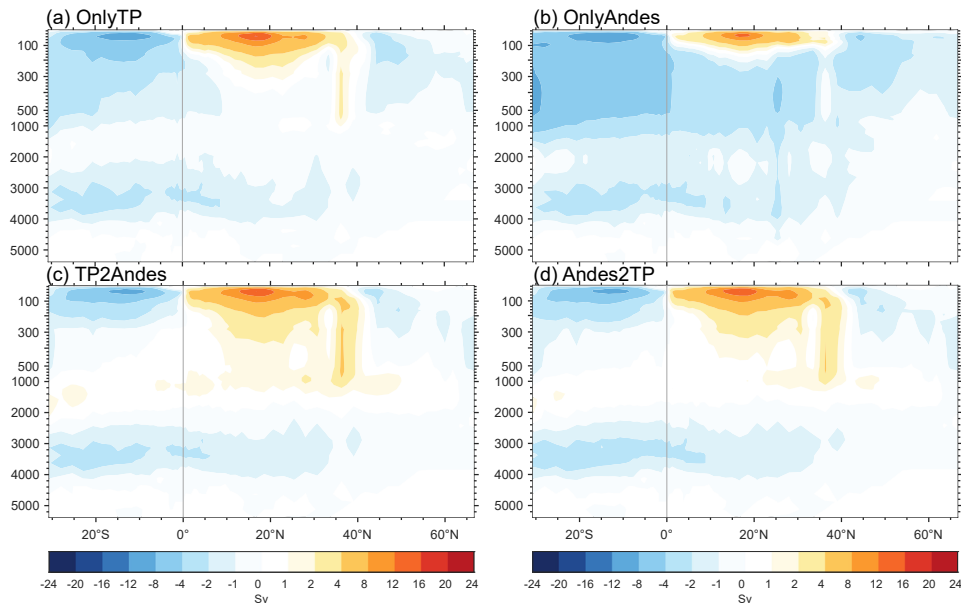


Fig. R3. Zonal-mean latitude profiles of AMOC strength (Sv) for the OnlyTP, OnlyAndes, TP2Andes, and Andes2TP experiments.

*Line 145–146: Under this configuration, essential features of ice sheets—such as low surface temperature and moisture availability—are not represented. Is this not a critical problem? If it is not, the authors should convincingly explain why this limitation does not affect the results.*

**Response:** We thank the reviewer for this important comment. We acknowledge that low surface temperatures and limited moisture availability are key characteristics of real ice sheets, which are not fully represented in our simulations. In CESM1, ice sheets are represented as fixed topography with prescribed albedo, and the dynamic ice sheet component (CISM) is not active in our setup.

Our study focuses specifically on the direct topographic effect of the Andes on atmospheric and oceanic circulation. Under this framework, the main pathway connecting Andean uplift to PMOC involves tropical and subtropical atmospheric circulation and ocean dynamics, which are largely insensitive to the thermal and moisture biases associated with high-latitude ice sheets. Therefore, although the setup does not include fully interactive ice sheets, this simplification does not significantly affect the conclusions regarding the Andes' influence on the PMOC.

### 3. Regional Impacts of AM Uplift

*Line 167–171: Are the results consistent with previous studies? In this study, the influence of latent heat flux associated with evaporation appears small, as suggested by the mismatch between*

*SSS and SST changes. Instead, changes in shortwave radiation due to low-level clouds and Ekman transport seem to play a more significant role.*

**Response:** Thank you for the comment. We intended to convey that the regional cooling is consistent with previous studies; however, the underlying mechanisms are not the same, as our simulations are based on a fully coupled model rather than an atmosphere-only model in Takahashi and Battisti (2007). Both evaporation and latent heat flux increase significantly in our simulations, particularly over the tropical southeast Pacific. However, strong SST cooling occurs without a corresponding increase in SSS, indicating that evaporation alone cannot explain the magnitude of the cooling. The absence of such a signal suggests that latent heat flux largely acts as a response to SST and wind changes, rather than as the leading forcing.

Instead, we agree that the regional SST cooling is primarily driven by reduced shortwave radiation associated with increased low-level cloud cover and enhanced Ekman upwelling. Thus, the increased evaporation and latent heat flux represents a response to the cooling and strong winds (as shown in Fig. 2 of the revised manuscript), rather than the dominant mechanism. We have revised this paragraph with additional explanations to clarify these points (Lines 168-174 in the revised manuscript).

#### **4. Remote Responses and the PMOC**

*The part before subsection a in this section (and subsection a) is too long, including an overly extended preamble. It would be clearer to divide this section into smaller parts and assign subsection numbers to each, including the introductory part.*

**Response:** Thank you for the suggestion. We have restructured this section by introducing a new subsection, “a. PMOC in the equilibrium state”. This reorganization improves clarity.

*Line 192–195: This statement is not accurate, because PMOC (i.e., deep-water formation in the Pacific) is also present in the Flat experiment. The Andean uplift can enhance the PMOC, but it cannot act as its trigger.*

**Response:** Sorry for the misleading wording. We have replaced “triggered” with “intensified”. Please see Lines 199 in the revised manuscript.

*Line 204–208: These cited studies emphasized the importance of mountain ranges as potential triggers for shifts in deep-water formation from the Atlantic to the Pacific. However, drastic experiments such as Flat or those by Maffre et al. (2018) cannot serve as realistic control experiments. Control simulations should be based on realistic topography, because the impacts of external factors (including Andean uplift) depend strongly on the model’s background climate state.*

**Response:** Thank you for the comment. As discussed in our response to the major comment, the primary motivation for including experiments initialized from the Flat configuration is that this approach more closely mimics the progressive development of topography in natural geological processes. Although our study is not explicitly focused on paleogeography or paleoclimate reconstructions, the results provide insight into the fundamental mechanisms governing the PMOC and its potential relevance to paleoclimate dynamics.

We also examined the Real and NoAndes experiments, which show only minor differences (Fig. R1 and Fig. 3 in the revised manuscript). This limited sensitivity arises from the presence of other major topographic features, most notably the Tibetan Plateau, which exerts a dominant control and suppresses the PMOC under these configurations. In our future study, we plan to conduct additional experiments under Pliocene forcings to further investigate the role of the Andes Mountains within a more realistic paleoclimate framework.

*Line 258–261: The mechanism mentioned here has not actually been demonstrated in this paper. How can it be shown from the presented results?*

**Response:** We appreciate the reviewer’s careful assessment of the proposed mechanism linking subpolar Pacific surface warming to enhanced NPDW formation. To clarify this point, we have revised the relative paragraph (Lines 281-285 in the revised manuscript).

In our simulations, enhanced evaporation produces a positive surface salinity anomaly in the subpolar Pacific, which would act to increase surface density. However, this occurs concurrently with significant surface warming, which has an opposing effect by reducing density through thermal expansion. The competing effects of salinity increase and temperature increase suggest that local surface buoyancy forcing alone is unlikely to be sufficient to fully account for the intensification of NPDW formation.

We therefore revised the manuscript to clarify that increased evaporation and salinity contribute to—but do not solely trigger—the enhanced NPDW. The strengthened PMOC is instead primarily

associated with large-scale circulation adjustments and dynamical processes, with surface buoyancy changes playing a secondary role.

*Line 263–266: The Kuroshio Extension change is observed not only during the first 100 years but also several times afterward, when wave propagation from the equator is likely re-initiated. Can the first event be distinguished from the subsequent ones?*

**Response:** We agree that changes in the Kuroshio Extension (KE) are not confined to the first 100 years but recur several times afterward, likely associated with the re-initiated equatorial wave propagation. However, the first KE event can be distinguished from subsequent ones in a dynamical sense.

The initial KE response occurs during the early adjustment phase following Andean uplift, when the coupled ocean-atmosphere system is still responding to the imposed topographic forcing. This first event is characterized by a more coherent, sustained adjustment that coincides with the rapid strengthening of the subtropical gyre and the basin-scale reorganization of wind stress and thermocline structure.

In contrast, later KE fluctuations emerge after the system has reached a quasi-equilibrium state. These subsequent events are more intermittent and reflect internal variability or the re-excitation of equatorial-to-midlatitude wave dynamics under an already adjusted background circulation. While these later signals demonstrate that equatorial wave propagation remains active, they do not represent the primary forced response to Andean uplift. We have clarified this distinction in the revised manuscript (Lines 256-267).

*Line 312–313: Is this warming not the same as the quasi-steady state shown in Fig. 2? If not, what is its significance for the establishment of the equilibrium climate state?*

**Response:** Thank you for the comment. We apologize that we did not explain the relationship between the initial warming and quasi-steady warming. Actually, the warming shown in Fig. 5a represents the early adjustment phase and gradually evolves into the same warming pattern seen in the quasi-steady state (Fig. 2) in approximately 100 years. We analyze this initial warming because it captures the transient adjustment process through which the equilibrium climate is established. Examining this early phase helps clarify the mechanisms responsible for setting the quasi-steady warming pattern. We have added corresponding explanation in the revised manuscript (Lines 357-363).

*Line 323–325: What is the relationship between the PMOC and atmospheric dynamics? This connection is not clearly discussed.*

**Response:** Thanks for your comment. We agree that the connection between the PMOC and atmospheric dynamics requires clarification. During the early adjustment phase, transient atmospheric responses—including shifts in the Hadley and Ferrel circulation—leading to a weakening of the Pacific trade winds. This reduction in trade wind strength directly modifies surface wind stress, Ekman transport, and air-sea heat and freshwater fluxes over the Pacific. These atmospheric changes, in turn, precondition the upper ocean by promoting surface salinization and mixed-layer deepening, thereby favoring the initial strengthening and reorganization of the PMOC. Thus, atmospheric dynamics do not merely respond passively to oceanic changes; rather, they play an active role in initiating and shaping PMOC-related oceanic processes. We have added a sentence in the manuscript to clarify this link (Lines 380-384).

*Line 344–346: What is the cause of the high salinity? The enhancement of the subtropical gyre seems to be an important mechanism.*

**Response:** Thanks for your comment. We agree that the enhanced subtropical gyre plays an important role in the development of the PMOC; however, we suggest that its influence on salinity is indirect. During the adjustment process, we do not observe a clear propagating salinity signal (Fig. 6c), indicating that large-scale horizontal salinity advection is unlikely to be the dominant mechanism. Instead, the increased salinity is more likely associated with intensified evaporation (Fig. 2c and 4c), resulting from the enhanced transport of warm water by the subtropical gyre.

*Line 351–353: In the present climate, subduction alone cannot sustain deep downwelling. A basin-scale change in stratification is required. The authors should describe the stratification structure in the North Pacific and how it is modified. Overall, the downwelling mechanism described here appears common to both the Flat and OnlyAndes experiments.*

**Response:** Thanks for this comment. In the OnlyAndes experiment, North Pacific stratification is substantially weakened relative to Flat, as evidenced by persistent positive anomalies in surface salinity ( $\Delta SSS$  up to +0.6–0.8 psu), deepened mixed-layer depth ( $\Delta MLD > 150$ –200 m), and enhanced net evaporation minus precipitation ( $\Delta EmP \sim 0.1$ –0.2 mm/day; Fig. 4 in revised manuscript). These changes reduce the upper-ocean density gradient (implying lower buoyancy

frequency), allowing subduction to evolve into sustained basin-scale downwelling and convection. In contrast, the strong ambient stratification in Flat limits deep ventilation. The weakening is preconditioned by Andes-induced atmospheric adjustments that favor salinification over the subpolar North Pacific, with salinity effects dominating over concurrent warming ( $\Delta\text{SST} \sim 1\text{--}1.5^\circ\text{C}$ ) to destabilize the water column.

## 5. Conclusion and Discussion

*Line 386–388: The statement seems overstated. While the results may suggest a potential for the Andean uplift to strengthen the PMOC, they do not demonstrate that it created the PMOC itself.*

**Response:** We thank the reviewer for this comment. We agree that our results do not imply that the Andean uplift created the PMOC itself. Rather, our simulations indicate that topographic forcing alone may be sufficient to enhance or strengthen an existing PMOC. We have revised Section 5 substantially to avoid overstating the results.

## Reference list

- Kitoh, A., 1997: Mountain uplift and surface temperature changes. *J. Climate*, **10**, 185–188, <https://doi.org/10.1029/96GL03953>.
- Maffre, P., J.-B. Ladant, Y. Donnadieu, P. Sepulchre, and Y. Godd  ris, 2018: The influence of orography on modern ocean circulation. *Climate Dyn.*, **50**(3–4), 1277–1289, <https://doi.org/10.1007/s00382-017-3683-0>.
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- Wen, Q., and H. Yang, 2020: Investigating the role of the Tibetan Plateau in the formation of Pacific meridional overturning circulation. *J. Climate*, **33**(9), 3603–3617, <https://doi.org/10.1175/JCLI-D-19-0206.1>.
- Takahashi, K., and D. S. Battisti, 2007: Processes controlling the mean tropical Pacific precipitation pattern. Part I: The Andes and the eastern Pacific ITCZ. *J. Climate*, **20**(14), 3434–3451, <https://doi.org/10.1175/JCLI4198.1>.

#### Replies to Reviewer #4:

We appreciate your time to review this manuscript and all your constructive comments. We have incorporated these suggestions into the revised manuscript. The following are our point-by-point replies.

##### General Comments:

*Tong et al. present two idealized climate simulations using a coupled atmosphere-ocean climate model: one with flat global topography everywhere and one with only the Andes. They find that while both simulations have no AMOC and some deep-water formation in the North Pacific, the PMOC is significantly stronger when orography is prescribed over the Andes. They carefully investigate the mechanisms for the enhanced PMOC in the Andes only simulations.*

*While the paper is written clearly, and the analysis seems generally sound, I have serious concerns about the differences between their findings and prior work and the suitability of the experimental setup. My major concern is that, as the authors show in their own simulations and cite in the existing literature, flat orography promotes an active PMOC and inactive AMOC. With such a baseline climate, is the difference between these two simulations scientifically meaningful in the context of our understanding of climate dynamics and paleoclimate? I include major and minor comments below.*

**Response:** We thank the reviewer for this thoughtful and important comment. We agree that flat-orography configurations are known to favor an active PMOC and a weak or absent AMOC, as demonstrated in previous studies and also evident in our simulation. We acknowledge that such a baseline climate state does not represent a realistic paleoclimate configuration.

Although the baseline climate already supports an active PMOC, the introduction of Andes orography leads to a systematic and robust intensification of the PMOC through a physically coherent sequence involving changes in equatorial winds, ocean circulation adjustments, and delayed extratropical responses. The scientific significance of our results therefore lies in identifying this dynamical pathway and clarifying how large-scale topography can modulate PMOC, rather than in the absolute strength of the PMOC itself.

We have revised the manuscript to better clarify the scope and limitations of this idealized experimental setup, and to emphasize that our conclusions are intended to inform mechanistic understanding of PMOC sensitivity not in a paleoclimate perspective.

*Major Comments:*

*1. The Andes orography is added onto a baseline simulation where the topography is everywhere flat. I would imagine that differencing "flatAndes" and "real" simulations would provide much more connection to the real world. However, as the "real" simulation has no PMOC, and the uplift of the Andes is shown to increase SSS of the north Pacific, the "flatAndes" simulation would almost certainly not have a PMOC either. The uplift of the Andes with realistic orography everywhere else would presumably promote net evaporation and higher SSS, but not sufficiently to erode the North Pacific halocline. So the conclusion is that the uplift of the Andes encourages a PMOC, but not enough to produce one. One might ask, are these simulations too detached from the real world to be scientifically meaningful?*

**Response:** Thanks for this insightful comment and we agree with the reasoning presented. As shown in our supplementary simulations, removing Andes only (i.e., NoAndes) cannot change the AMOC nor PMOC strength (see Fig. R4 and Figs. 3, 9 in the revised manuscript). Although the North Pacific is also stratified and does not support a strong PMOC in the real world. It means that the uplift of the Andes increases sea surface salinity in the North Pacific, this effect alone is indeed insufficient to erode the halocline and generate a PMOC under realistic boundary conditions.

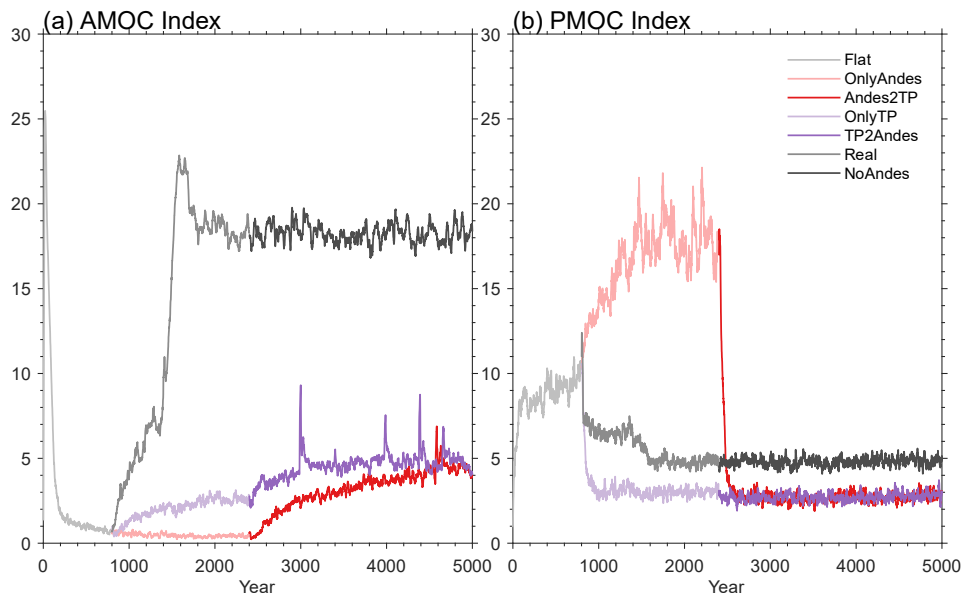


Fig. R4 Time evolution of the AMOC and PMOC indices in all Andes-related topographic experiments, including *Flat* (all major topography removed), *OnlyAndes* (Andes uplift only), *Andes2TP* (further uplift of the Tibetan Plateau from *OnlyAndes*), *OnlyTP* (Tibetan Plateau uplift only), *TP2Andes* (further uplift of the Andes from *OnlyTP*), *Real* (realistic topography), and *NoAndes* (Andes removed from *Real*).

*2. Ln 172: The East Pacific warming appears to be the opposite of previous studies? While this is a very crucial part of their results, I am skeptical because Feng and Poulsen 2014 found equatorial east Pacific cooling in their simulations in response to uplift. Xu and Lee 2021 also found cooling, as did Maroon et al. 2015 (J. Cli). Several of these studies use CESM1. Could the authors clarify on why in their experiments uplift warms, not cools the east Pacific when consensus seems to learn towards cooling? Does it have to do with the different model setup discussed in point 1 (most prior studies prescribed flattening the Andes rather than raising them from a flat baseline).*

**Response:** We appreciate the reviewer's comment. Our results do not indicate uniform warming over the eastern Pacific. Instead, the dominant SST response is pronounced cooling over the Southeast tropical Pacific (Fig. 2a in the revised manuscript), consistent with previous studies, accompanied by a weak, localized, and secondary warming confined to the near- and north-equatorial region (0°–10°N).

This asymmetric SST response reflects the complex atmosphere–ocean adjustment to Andean topographic forcing. The weak equatorial warming is dynamically linked to a reduction in trade wind strength. Andean uplift induces a Southern Hemisphere cyclonic anomaly and associated Rossby wave response, weakening the equatorial easterlies and disrupting Ekman-driven westward transport. This allows warmer waters from the western Pacific warm pool to extend eastward, producing a modest SST increase north of the equator, analogous to an El Niño–like dynamical response.

We acknowledge that several previous studies reported equatorial eastern Pacific cooling following Andes uplift (Feng and Poulsen. 2014; Maroon et al. 2015; Xu and Lee. 2021). A key difference lies in the experimental design: most prior studies prescribed Andes flattening within a realistic topographic background, whereas our experiments raise the Andes from a flat baseline. This difference modifies the background circulation and leads to a different balance between wind-driven upwelling and horizontal heat transport. We therefore interpret the weak equatorial warming as a setup-dependent response superimposed on a robust and well-established Southeast Pacific cooling signal. This clarification has been added in Lines 176–178 of the revised manuscript.

#### *Minor Comments:*

*1. Fig.1b Why is there no net evaporation signal seen in the region of increased SSS?*

**Response:** Thank you for the comment. There is indeed a net evaporation signal in the subpolar North Pacific (Fig. 2c), but it is weaker compared to the tropical regions.

*2. Fig. 1, why not include latent heat flux as a panel on this figure?*

**Response:** Thanks for your suggestion. Now added latent heat flux in this figure.

3. Ln 87: "may not be"

**Response:** The original sentence with spelling and grammatical errors has been deleted and replaced with a revised version in the manuscript.

4. Ln 86: *On balance, there is not a consensus on whether PMOC was active during the Pliocene, see Novak et al. 2024 for instance.*

**Response:** Thanks for your suggestion. We have reorganized the Introduction and addressed the PMOC consensus argument in Lines 58-59 to maintain balance.

5. "subtropic gyre" -> "subtropical gyre"      Revised.

6. Ln 69: "despite studies"      Deleted.

7. Ln 129: "in detail"      Deleted.

8. Ln 131: "their original"      Revised.

9. Ln. 173: remove "the"      Removed.

10. Ln. 114: "respond"      Deleted.

11. Ln. 193: *I would probably not describe a 400 ppm climate as "extreme"*

**Response:** Apologies for the confusion. We have now removed the term "extreme."

## Reference

Feng, R., and C. J. Poulsen, 2014: Andean elevation control on tropical Pacific climate and ENSO. *Paleoceanography*, **29**(9), 795–809, <https://doi.org/10.1002/2014PA002640>.

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Xu, W., and J.-E. Lee, 2021: The Andes and the southeast Pacific cold tongue simulation. *J. Climate*, **34**(1), 415–425, <https://doi.org/10.1175/JCLI-D-19-0901.1>.