

Response to Editor and Reviewer

Dear distinguished Atmospheric Chemistry and Physics (ACP) editor and reviewer,

First, we sincerely thank you for taking your valuable time to provide such rich, valuable, detailed, and highly constructive comments on our study (**Manuscript title:** Forest and bioenergy expansion amplifies climate warming by accelerating regional cloud loss; **Manuscript ID:** egusphere-2026-367). We must admit that some issues were not considered very comprehensively by us at the beginning; however, we are very fortunate to have this valuable opportunity to discuss and exchange with top-level expert in this field to improve those issues. Undoubtedly, these precious discussion comments have improved the fluency of the language expression and the scientific rigor of the manuscript to a great extent. We once again maintain our most sincere gratitude to the respected editor and reviewer for the time and energy they have devoted, and we feel honored by such in-depth exchange. We are full of interest in how large-scale land-use and cover change (LUCC) affects cloud cover and its radiative effects. This valuable discussion opportunity has completely stimulated my research interest in this topic (including the new work we are currently carrying out). We have been reflecting on the potential climate impacts of currently widely popular afforestation programs, as reported by recent research (Dror and Feingold, Science: Amazon forest loss: An all-sky biophysical top-of-atmosphere cooling feedback, although it is Amazon deforestation): if the role of cloud cover is not considered, the biophysical responses of LUCC may be underestimated. Finally, we once again extend our most sincere thanks to the distinguished ACP editor and reviewer. We are very grateful to you for the time and energy you have devoted in the midst of your busy schedule to help us gradually improve this manuscript. We are honored to have this very precious opportunity to discuss with top-level expert. I must admit that the serious and cautious spirit with which the editor and reviewer treated the manuscript has moved me, and I will certainly carry this research spirit forward so as to contribute my own modest effort in advancing scientific research. Finally, we wish the respected ACP editor and reviewer a happy life and smooth work.

Now we provide a comprehensive, point-by-point response to the specific comments raised by the reviewer as follows.

1. I thank the authors for their detailed and thoughtful responses to my comments. The revised manuscript is well-written, the methodology is clearly articulated, and my previous concerns have been addressed. I am pleased to support the publication of this work. Please note a small grammatical error in the abstract (line 18): replace "cloud trends response to" with "cloud trends respond to".

First, we would like to express our sincere gratitude for the valuable time and effort you dedicated to helping us improve our manuscript; your detailed and insightful comments have significantly enhanced the original submission. It has been a privilege to engage in such a profound exchange with you through ACP, and I am grateful that your valuable feedback has further stimulated my interest in pursuing future research on large-scale land-use change and its effects on clouds and radiation. Regarding the phrase "cloud trends response to" in line 18 of the Abstract, please accept our apologies for the oversight; we truly appreciate your meticulous review. In the revised version, we have adopted your suggestion and changed the wording to "cloud trends respond to." Finally, we once again thank you sincerely for your time and effort, and we wish you all the best in your life and work.

Finally, we have standardized the reference formatting in our manuscript following the style used in a recently published article in Atmospheric Chemistry and Physics (ACP) (<https://acp.copernicus.org/articles/26/12591/2026/>). The reference format is as follows:

Alkama, R., Taylor, P. C., Garcia-San Martin, L., Douville, H., Duveiller, G., Forzieri, G., Swingedouw, D., and Cescatti, A.: Clouds damp the radiative impacts of polar sea ice loss, *The Cryosphere*, 14, 2673–2686, <https://doi.org/10.5194/tc-14-2673-2020>, 2020.

Andersen, H., Cermak, J., Zipfel, L., and Myers, T. A.: Attribution of observed recent

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Now we provide a comprehensive, point-by-point response to the specific comments raised by the reviewer as follows.

The revised manuscript has improved substantially, and the authors have addressed many of my previous concerns; however, several issues regarding the interpretation of local versus non-local effects and the mechanisms underlying the high-cloud response still require clarification. I have a few remaining points below.

Specific comments

1. The response letter states that no suitable method is available to disentangle local and non-local cloud responses to land-cover change. I do not think this statement is sufficiently justified. Zhao et al. (2026, Earth's Future, doi:10.1029/2025EF007704) recently provided an intercomparison of several approaches for separating local and non-local biophysical effects of land-cover change. Although their quantitative evaluation focuses primarily on land surface temperature, some approaches, such as checkerboard experiments, are applicable to both land and atmospheric variables without relying on variable-specific physical assumptions. In addition, Chen et al. (2022, Journal of Climate, doi:10.1175/JCLI-D-22-0144.1) applied a checkerboard framework to separate the local and non-local responses of low-, middle-, and high-level cloud fractions (their Fig. 11). I therefore suggest that the authors reconsider the statement that no suitable method exists. More importantly, the current temperature-advection and circulation diagnostics provide evidence for non-local atmospheric responses, but they do not quantitatively separate the local and non-local components of the cloud-cover response itself. Therefore, the conclusion that cloud-cover reduction is “dominated by non-local atmospheric circulation” appears stronger than what the current analysis can support, unless a dedicated local/non-local separation analysis of cloud cover is provided.

We greatly appreciate and agree with this valuable suggestion, and sincerely thank you for this important methodological clarification. After carefully examining Zhao et

al. (2026) and Chen et al. (2022), we recognize that our previous statement that no suitable method is available to disentangle local and non-local cloud responses was overly absolute. Zhao et al. (2026) systematically intercompared multiple approaches for separating local and non-local biophysical effects of land-cover change and explicitly noted that the checkerboard method is applicable to different land and atmospheric variables without relying on variable-specific physical assumptions, this implies that a variable such as cloud cover can also be decomposed into local and non-local effects using this method. However, the study by Zhao et al. (2026) also notes at the end of the paragraph that careful consideration of the regions is required when applying the checkerboard method in practice, which may indicate limitations associated with the method's use. In the checkerboard framework, land-cover-change (LCC) grid cells contain the combined local and non-local response, whereas adjacent non-LCC grid cells sample the non-local response; the latter can be spatially interpolated and subtracted from the total response to isolate the local component. Importantly for our study, Chen et al. (2022) demonstrated this approach directly for clouds using regional WRF checkerboard afforestation experiments, separating the local and non-local responses of low-, middle-, and high-level cloud fractions. We have therefore removed the statement that no suitable method exists and have added both studies to the revised manuscript and added citations (Z. Zhao, Y. Li, R. Xu, R. Meier, L. Liu, L. Chen, L. Zhao, Y. Liu, S. Zi, D. Zhao, Intercomparison of Methods for Disentangling Local and Nonlocal Biophysical Impacts of Land Cover Changes. *Earth's Future*. 14 (5), e2025EF007704 (2026); C. Chen, J. Ge, W. Guo, Y. Cao, Y. Liu, X. Luo, L. Yang, The Biophysical Impacts of Idealized Afforestation on Surface Temperature in China: Local and Nonlocal Effects. *J. Clim.* 35 (23), 7833–7852 (2022)).



et al., 2018; Schultz et al., 2016), may affect the validity of the assumption that subgrid PFTs are independent and differ only in biophysical properties.

The nonlocal and total effects in the Checkerboard method were significantly weaker than those of other methods, while the local effects remained comparable. This is because the imposed partial deforestation in the checkerboard scenario was only half that of the complete global deforestation in other methods. Since nonlocal effects are more sensitive to the spatial extent of land cover changes than local effects, the sparse and extensive LCC and their grid size influence the strength of nonlocal effects due to the scale-dependency (Winckler et al., 2017a). The reconstruction of local and nonlocal effects could yield a stronger local and total signal due to interpolation artifacts (De Hertog et al., 2023; Jäger et al., 2025). Therefore, the interpretation of the relative strength of local and nonlocal effects should account for these issues. Moreover, the interpolated local and nonlocal effects are influenced by different checkerboard layouts and subject to high uncertainty near the edges of LCC due to a lack of reference grids (e.g., the Sahara in our case). This means that the spatial distribution of land cover in the real world, especially when it is scattered and unevenly distributed, may affect the effectiveness of this method. It is crucial to create a proper checkerboard design that balances the intended LCC pattern while maintaining sufficient and representative samples to ensure the accuracy of spatial interpolation. An advantage of the Checkerboard method is that it does not rely on variable-specific physical assumptions, making it extendable to other climate variables (such as precipitation (Winckler et al., 2017a), cloud cover (Hua et al., 2023)). Similar extensions are also possible for other methods, like the Window Regression, Spatial Comparison, and Spectral Separation. Even without a dedicated checkerboard LCC design, there have been attempts to directly apply the spatial interpolation strategy, the essence of the Checkerboard method, to historical LCC experiments (Hua et al., 2023). However, the accuracy of this interpolation approach is compromised by the spatial sampling and representativeness of the historical LCC pattern, and careful selection of these regions is required in practical applications.

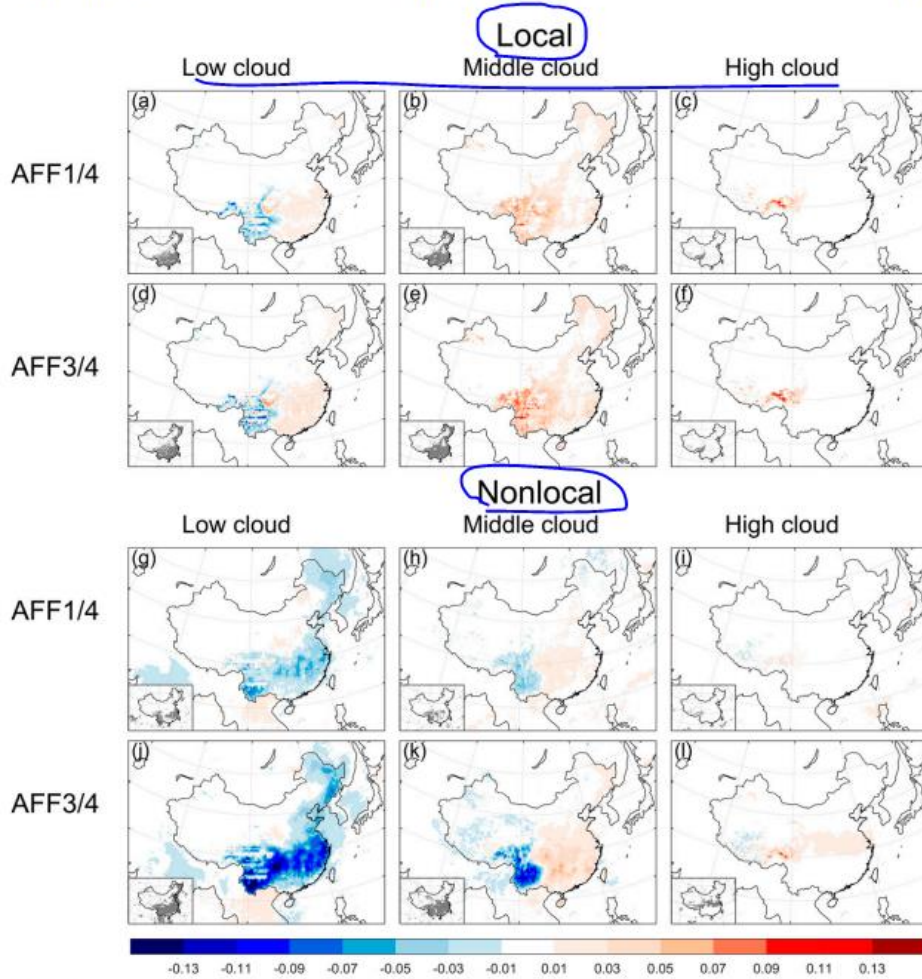


FIG. 11. Annual mean values of the (a)–(f) local and (g)–(l) nonlocal effects of afforestation on cloud cover fractions (unitless) in AFF1/4 in (a)–(c) and (g)–(i) and AFF3/4 in (b)–(d) and (j)–(l). Changes in (left) low (300–2000 m above the ground), (center) middle (2000–6000 m above the ground), and (right) high (>6000 m above the ground) cloud cover fractions are shown. For (a)–(f), only the grid cells that have valid values [e.g., obtained by Eq. (3) or the spatial interpolation] in both AFF1/4 and AFF3/4 are shown.

We also agree with you that our present analysis does not quantitatively partition the cloud cover response into local and non-local components. The AF50, BE50, and REAL experiments were designed as transient land-use forcing experiments rather than as dedicated checkerboard separation experiments, and therefore they do not contain the regularly alternating LCC and non-LCC grid structure required for the checkerboard interpolation used by Chen et al., 2022 and evaluated by Zhao et al., 2026. Zhao et al., 2026 also emphasize that checkerboard separation requires customized experiments and that the inferred responses can depend on the checkerboard design, the spatial extent of the land cover perturbation, and interpolation artifacts. Although Zhao et al., 2026 discuss additional separation approaches, we do not apply a dedicated local/non-local cloud-separation method in the present study. Accordingly, the 850 hPa

temperature-advection and 500 hPa wave-activity-flux diagnostics are now used only to diagnose the existence and pathways of non-local atmospheric dynamical adjustment; they are not interpreted as a quantitative attribution of the cloud-cover response to local versus non-local processes.

To avoid overinterpretation, we have revised the relevant causal language throughout the manuscript. Statements implying that cloud cover loss is “*dominated by*” non-local atmospheric circulation have been replaced by more appropriately constrained wording such as “*modulated by* non-local atmospheric dynamics” or “consistent with a contribution from remote dynamical adjustment”. We now explicitly state that the relative magnitudes of the local and non-local cloud responses are not quantified in the present study, and we identify dedicated checkerboard experiments or other validated separation frameworks as an important direction for future work.

The specific improvements are as follows:

(1) Added methodological clarification in the Discussion section: “Several methods have been developed to disentangle local and non-local biophysical responses to LUCC (Zhao et al., 2026). In particular, checkerboard experiments can be applied to both land and atmospheric variables without variable-specific physical assumptions, and Chen et al. (2022) demonstrated their application to the separation of cloud-fraction responses. Because the transient AF50, BE50, and REAL experiments in the present study were not designed for checkerboard separation and we do not apply another dedicated cloud-separation framework here, we do not quantitatively partition cloud-cover changes into local and non-local components. The temperature-advection and wave-activity-flux diagnostics are therefore interpreted as evidence for remote dynamical modulation and teleconnection pathways rather than as a quantitative local or non-local decomposition of the cloud response.”

(2) Revised interpretation of the non-local diagnostics: “The wave-activity-flux patterns are consistent with a remote dynamical pathway through which atmospheric energy propagation can modify subsidence, static stability, and humidity in regions

beyond the land-cover perturbation, thereby modulating cloud cover. These diagnostics demonstrate the presence of non-local atmospheric adjustment but do not determine its contribution relative to local processes. Thus, the substantial mid- and low-level cloud loss under AF50 is interpreted as being influenced by local thermodynamic processes together with non-local dynamical adjustment; because the present experiments do not explicitly separate these components of the cloud response, we do not assign dominance to either pathway.”

(3) Added limitation and future-work statement: “Future studies could employ dedicated checkerboard experiments, or other separation frameworks evaluated by Zhao et al. (2026), to quantify the local and non-local components of LUCC-induced cloud changes explicitly.

2. The revised manuscript still contains internally inconsistent interpretations of the high-cloud response. At present, three different explanations appear to coexist:

- Moisture-driven explanation. In Lines 791–794, the manuscript states that “the increase in water vapor induced by large-scale idealized forest expansion leads to an increase in CLDHGH,” associated with “cooling and increased humidity in the upper troposphere”.

- Indirect/non-local dynamical explanation. In the response letter (p. 13), the authors state that the simulated increase in high-level clouds should be viewed as “an indirect response mediated by non-local dynamical adjustment, rather than a direct result of local surface forcing.”

- Direct surface-flux influence on deep convection versus weak surface coupling. In the response letter (p. 36), the authors acknowledge that CLDHGH includes both thin cirrus and the upper parts/anvils of deep convective clouds and state that, “although isolated cirrus clouds may not directly respond to surface fluxes, deep convection undoubtedly does.” They further invoke enhanced surface buoyancy, CAPE, deep convective updrafts, and moisture transport to the upper troposphere as the pathway linking afforestation to CLDHGH. However, the

revised manuscript states in Lines 492–494 that “high clouds and deep convection are generally weakly coupled to local surface perturbations, responding primarily to large-scale upper-tropospheric dynamical and thermodynamic conditions”. These statements are not necessarily mutually exclusive, but the manuscript does not currently explain how they fit together. I therefore suggest that the authors provide a more internally consistent interpretation of the CLDHGH response. In particular, because CLDHGH is a pressure-based diagnostic that includes different cloud types, the present analysis does not uniquely identify whether the simulated increase is primarily associated with cirrus, convective anvils, direct surface-flux influences on deep convection, or indirect large-scale dynamical adjustment. The manuscript should clearly distinguish these pathways and avoid presenting any one of them as the established dominant mechanism unless it is demonstrated by the diagnostics.

We sincerely thank you for identifying this inconsistency. We agree that our previous manuscript and response letter did not clearly distinguish among several plausible pathways affecting high-level cloud (CLDHGH) and, in some places, presented these pathways too categorically. In particular, our previous statements concerning moisture-driven changes, non-local dynamical adjustment, and the influence of surface fluxes on deep convection were intended to describe potentially complementary processes, but the wording could incorrectly imply that one of them had been uniquely established as the dominant mechanism.

We have therefore substantially revised our interpretation of the high-cloud response. First, we now explicitly clarify in the Methods that CLDHGH in CESM is defined according to cloud-top pressure (>400 hPa) and is therefore an aggregate diagnostic that may include physically distinct high-cloud types, including cirrus and the upper portions or anvils of deep convective clouds. Consequently, CLDHGH alone cannot identify which cloud type is responsible for the simulated increase or distinguish between surface-forced convective influences and indirect large-scale dynamical or thermodynamic adjustment. *Second*, we have reorganized the mechanistic

interpretation into a hierarchical and non-exclusive framework. Changes in surface albedo and turbulent heat fluxes represent the upstream land-surface perturbation and may alter boundary-layer buoyancy, deep convection, and vertical moisture transport. At the same time, our simulations show upper-tropospheric temperature and humidity changes, while the circulation diagnostics demonstrate that large-scale atmospheric adjustment also occurs. These processes can operate simultaneously rather than representing mutually exclusive explanations. We therefore interpret the increase in CLDHGH as an integrated response to the altered upper-tropospheric thermodynamic and dynamical environment. *Third*, we have removed or softened statements that exceeded what the present diagnostics can demonstrate. Specifically, we removed the statement that the CLDHGH response is “dominated by large-scale thermodynamic forcing,” revised the causal statement that increased water vapor “leads to” increased CLDHGH to an association between CLDHGH and upper-tropospheric thermodynamic changes, and removed the categorical statement that high clouds and deep convection are generally weakly coupled to local surface perturbations. We also clarify that the observed increase in TOA longwave cloud radiative effect demonstrates the radiative consequence of the high-level cloud increase but does not identify its formation pathway. *Finally*, because our diagnostics do not distinguish cirrus from convective anvils and do not quantitatively isolate the contributions of surface-forced convection and large-scale dynamical adjustment, we no longer assign dominance to any one pathway. We believe that these revisions provide a more internally consistent and appropriately constrained interpretation of the CLDHGH response.

3. The interpretation of the SHAP-based contribution percentages should be clarified. In the abstract (Lines 33–36), the manuscript states that “changes in relative humidity account for 20.4%, 20.4%, and 15.3% of the low-level cloud change under the AF50, BE50, and REAL scenarios, respectively.” However, these percentages appear to represent normalized SHAP feature importance in the machine-learning model rather than the fraction of the physical cloud-cover change attributable to relative humidity. SHAP values quantify the contribution

of predictors to model predictions and should not be interpreted directly as a causal or process-based decomposition. I therefore suggest that the authors revise the wording throughout the manuscript.

We appreciate your reminder and we agree that our previous wording could incorrectly imply that the normalized SHAP percentages represent fractions of the physical cloud-cover response attributable to individual variables. This was not our intention. The reported percentages are measures of relative feature importance within the trained machine-learning models and should not be interpreted as a causal or process-based decomposition of the simulated cloud-cover changes. We have therefore revised the SHAP terminology throughout the manuscript. First, the Methods now explicitly distinguish SHAP-based model interpretation from physical attribution. We clarify that SHAP values quantify how individual predictors shift model predictions relative to the model baseline, and that the reported percentages represent normalized SHAP feature importance rather than percentages of the physical cloud-cover change. *Second*, in the Abstract and Results, expressions such as “relative humidity accounts for 20.4% of the low-level cloud change” have been replaced by statements that relative humidity is the highest-ranked predictor, with normalized SHAP importance values of 20.4%, 20.4%, and 15.3% under AF50, BE50, and REAL, respectively. Similar revisions have been made for the SHAP percentages reported for total, middle-, and high-level cloud analyses. *Third*, we systematically replaced causal SHAP terminology such as “driver,” “contribution to cloud change,” “directly dictates,” and “mechanistic modeling” with more appropriate expressions such as “predictor,” “normalized SHAP feature importance,” “model-learned association,” and “contribution to the model prediction.” Figure captions have also been revised accordingly. *Finally*, we now distinguish the role of SHAP from that of the independent physical diagnostics. SHAP is used to rank influential predictors and characterize their relationships with model predictions, whereas the physical interpretation of the cloud response is based on the combined evidence from CESM-simulated surface energy fluxes, boundary-layer structure, atmospheric temperature and humidity, circulation diagnostics, and

established physical understanding. We believe these revisions avoid overinterpreting SHAP as a causal decomposition while retaining its value as an interpretable machine-learning diagnostic.

Technical corrections

4. The abstract is relatively long and could be shortened.

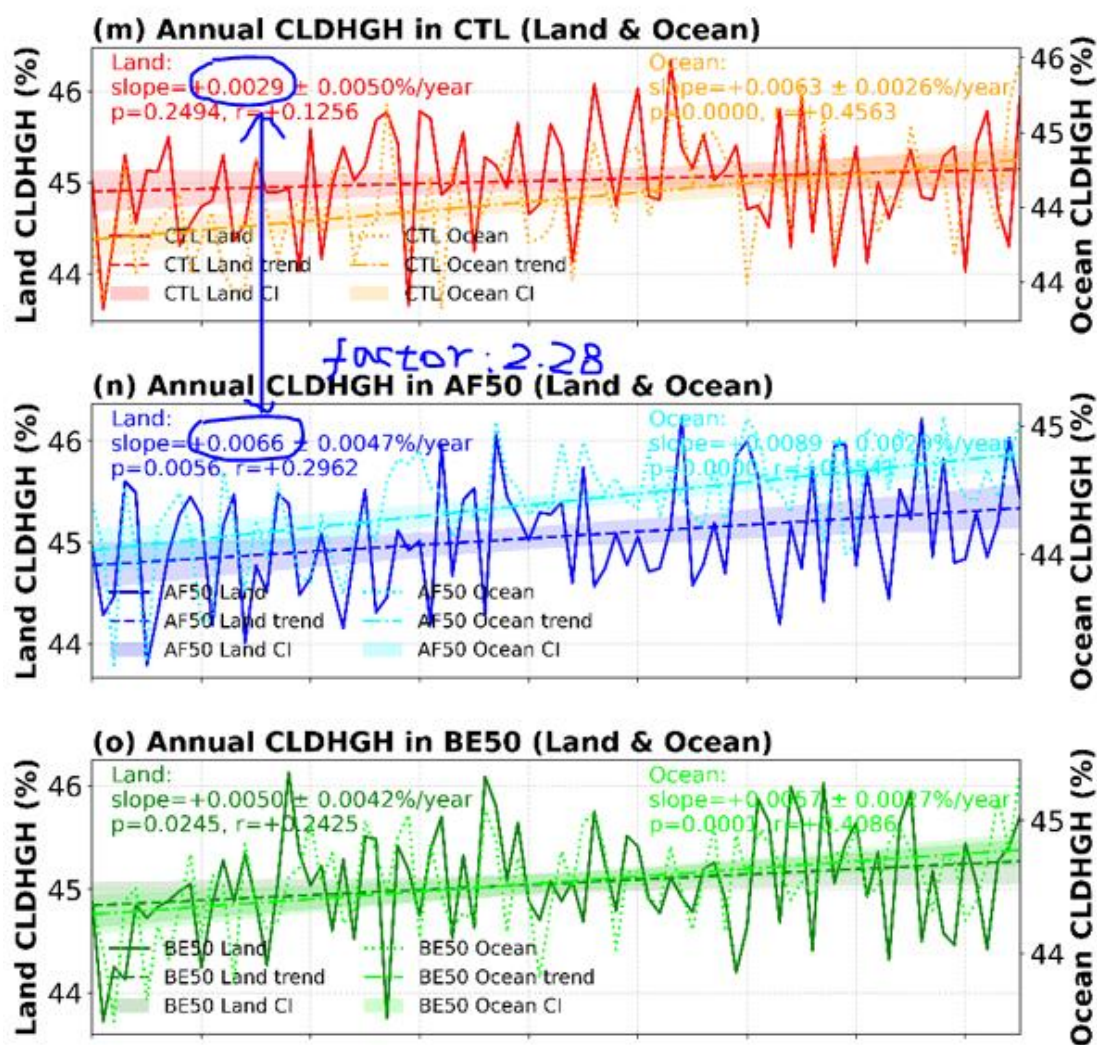
We greatly appreciate your valuable suggestion. Indeed, our previous version contained excessive methodological and quantitative detail. We have substantially shortened and streamlined the Abstract by removing redundant background information, secondary numerical results, and overly detailed mechanistic descriptions, while retaining the experimental design, principal findings, key physical interpretation, and broader climatic implications of the study.

5. Line 18: “response” should be changed to “respond”.

We greatly appreciate your careful review; please accept our apologies for the oversight regarding this word. We have corrected “response” to “respond” in the Abstract.

6. Line 423: “R = -6363” appears to be a typo; please check the value.

We greatly appreciate your careful review, which helped us eliminate this typographical error. We rechecked the value against the corresponding result in Fig. 1c. The correct value is $R = -0.6363$, and the manuscript has been corrected accordingly.



8. Line 1064 and Line 1079: The same sentence is repeated; please remove the duplication.

We thank you very much for identifying this duplication. We have removed the repeated sentence and retained only its first occurrence. The surrounding paragraph has also been checked to ensure a smooth transition after deletion.

Finally, we have standardized the reference formatting in our manuscript following the style used in a recently published article in Atmospheric Chemistry and Physics (ACP) (<https://acp.copernicus.org/articles/26/12591/2026/>). The reference format is as follows:

Alkama, R., Taylor, P. C., Garcia-San Martin, L., Douville, H., Duveiller, G., Forzieri, G., Swingedouw, D., and Cescatti, A.: Clouds damp the radiative impacts of polar sea ice loss, *The Cryosphere*, 14, 2673–2686, <https://doi.org/10.5194/tc-14-2673-2020>, 2020.

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