Reply to the reviewers' comments on:

"A Robust Aerosol Impact on Clouds Along the Subtropical to Tropical Transition"

We would like to thank the reviewers for their constructive and thoughtful reviews that helped us improve this paper.

Below please find a point-by-point reply to all of the reviewers' comments (in blue). Citations from the paper are in italic with new additions in bold.

Reviewer #1:

The manuscript explores aerosol effects along the stratocumulus to cumulus (Sc–Cu) transition and further downwind into deep cumulus convection in the tropics. The authors combine satellite observations, reanalysis data, Lagrangian trajectories, and large-eddy simulations. The results concerning the Sc–Cu transition are generally consistent with earlier work. The main contribution of this study is its extension toward the transition to deep convection and the suggestion that aerosols may affect thermodynamic properties along this evolution.

The extension of Lagrangian analyses into the deep convective regime is a potentially valuable direction, and the manuscript addresses an important question in this context. However, several aspects of the methodology raise concerns that may affect the strength of the conclusions regarding aerosol-driven thermodynamic feedbacks, particularly in distinguishing aerosol effects from aerosol–meteorology co-variability. Overall, the results are suggestive of a possible aerosol influence on thermodynamics, but stronger evidence would be needed to robustly support this interpretation.

In addition, the manuscript would benefit from improved writing, particularly in the organization and clarity of the methodology section. Below I provide detailed comments.

Reply: We would like to thank the reviewer again for providing these constructive comments. Below we have addressed all of the reviewer's comments.

Major concerns

1. Simulations

The authors note that aerosol–meteorology co-variability may precondition the Sc–Cu–DC evolution, which is clearly evident, for example, in Figure 3. Several recent studies have emphasized that polluted and clean air masses often exhibit systematically different thermodynamic and meteorological properties, which complicates causal interpretation. Relevant literature on aerosol–meteorology co-variability should be more thoroughly discussed and cited (e.g., https://acp.copernicus.org/articles/25/3413/2025/ and https://doi.org/10.5194/acp-24-7331-2024).

Reply: Thank you. The fact that polluted and clean air masses often exhibit systematically different thermodynamic and meteorological properties, which complicates causal interpretation, is mentioned a couple of times in the manuscript. For example, we state that: *"Yet these differences cannot be attributed solely to aerosol impacts, since the two groups also differ in their underlying thermodynamic environments (Fig. 3; Figs. S2-S9, SI), reflecting potential confounding factors, i.e., co-variability with meteorological state* ***(****Gryspeerdt et al., 2019****; Mülmenstädt et al., 2024; Goren et al., 2025)****".*

And:

"The interpretation of ACI from observations is complicated due to the co-variability between aerosol concentration and meteorological conditions. Air masses differ not only in aerosol loading but also in their thermodynamic environments (e.g., temperature, stability, humidity), making it difficult to determine whether observed cloud differences arise from aerosols or from pre-existing environmental variability (Gryspeerdt et al., 2016, 2019; McCoy et al., 2020; Christensen et al., 2021; Fons et al., 2023). Consequently, it is often assumed that the apparent cloud adjustments are strongly shaped, or even dominated, by background meteorological variability rather than the correlated aerosol influence itself (Nishant and Sherwood, 2017; Christensen et al., 2021; Wall et al., 2022; Gulistan et al., 2024)."

We also mention it in the concluding section:

"Second, meteorological co-variability remains a fundamental challenge within the dataset. Polluted and clean trajectories differ not only in AOD but also in their thermodynamic environments (e.g., SST, LTS, and humidity). Despite our efforts, a complete separation of aerosol and meteorological influences could not be achieved with the observational data, ***potentially due to inherent co-variability of aerosol loading*** *with large-scale meteorological conditions**.** This separation was only possible in the numerical simulations."*

In addition, the entire modeling section is designed to address this issue, as the model framework allows us to isolate the aerosol microphysical impact from its co-variability with meteorological conditions.

This is explained in: "*To better isolate and understand the direct influence of aerosols on cloud and radiative properties, we turn to model simulations where, by construction, the aerosol influence is decoupled from the confounding meteorological factors present in the satellite data. Unlike the observations, where aerosol and meteorological effects are intertwined, the model simulations isolate the impact of aerosols by holding environmental conditions the same between polluted and clean runs. This allows the simulated cloud adjustments to be more directly attributed to aerosol perturbations."*

Following the reviewer's suggestion, we have added the recommended references (Mülmenstädt et al., 2024; Goren et al., 2025), as shown here in the first example.

To isolate aerosol effects while keeping meteorology fixed, the authors employ LES simulations. Figure 6 shows differences between polluted and clean simulations, but the description of the simulation methodology is unclear and raises concerns regarding the interpretation of these results. It is not clear whether the LES are intended to closely mimic the observed meteorological evolution along the trajectories or whether they represent semi-idealized simulations. While the authors state that simulations follow the trajectories, the description suggests a more idealized setup.

This ambiguity is reinforced by the statement in the conclusions that the simulations rely on idealized setups with prescribed CCN perturbations.

Reply: Thank you for this helpful comment, which allowed us to clarify this point. The simulations follow the *mean* trajectory from each initiation point rather than individual trajectories. As such, they are not intended to represent specific conditions or a particular trajectory but are rather more idealized. To clarify this, we have revised the following section in the manuscript as follows: *“To address the impact of aerosols on tropical cloud transition, simulations are conducted using a**n idealized** Lagrangian framework (Sandu et al., 2010; McGibbon and Bretherton, 2017; Goren et al., 2019; Erfani et al., 2025), based on the mean trajectory derived from each initiation point in the observational data. In doing so, we do not aim to exactly reproduce the observed mean evolution, acknowledging the nonlinear relationship between individual trajectory behavior and their ensemble-mean response**, but rather, to represent an observationally-based idealized evolution.**”*

Several key methodological details are insufficiently described:

Are the simulations nudged, and if so, how?

Reply: The simulations are not nudged. This was explicitly mentioned in the manuscript: *"No nudging is applied to the dynamic or thermodynamic variables to allow them to evolve based on the local conditions (including the aerosol conditions)."*

How do atmospheric profiles evolve during the simulations?

Reply: Thank you. Following this comment, we have added a figure to the Supporting Information demonstrating the evolution in time of the temperature and humidity profiles in each simulation. The additions to the main text: ***"The evolution of atmospheric temperature and humidity profiles along the clean simulations is shown in Fig. S63, SI, for reference."***

The additions to the SI:

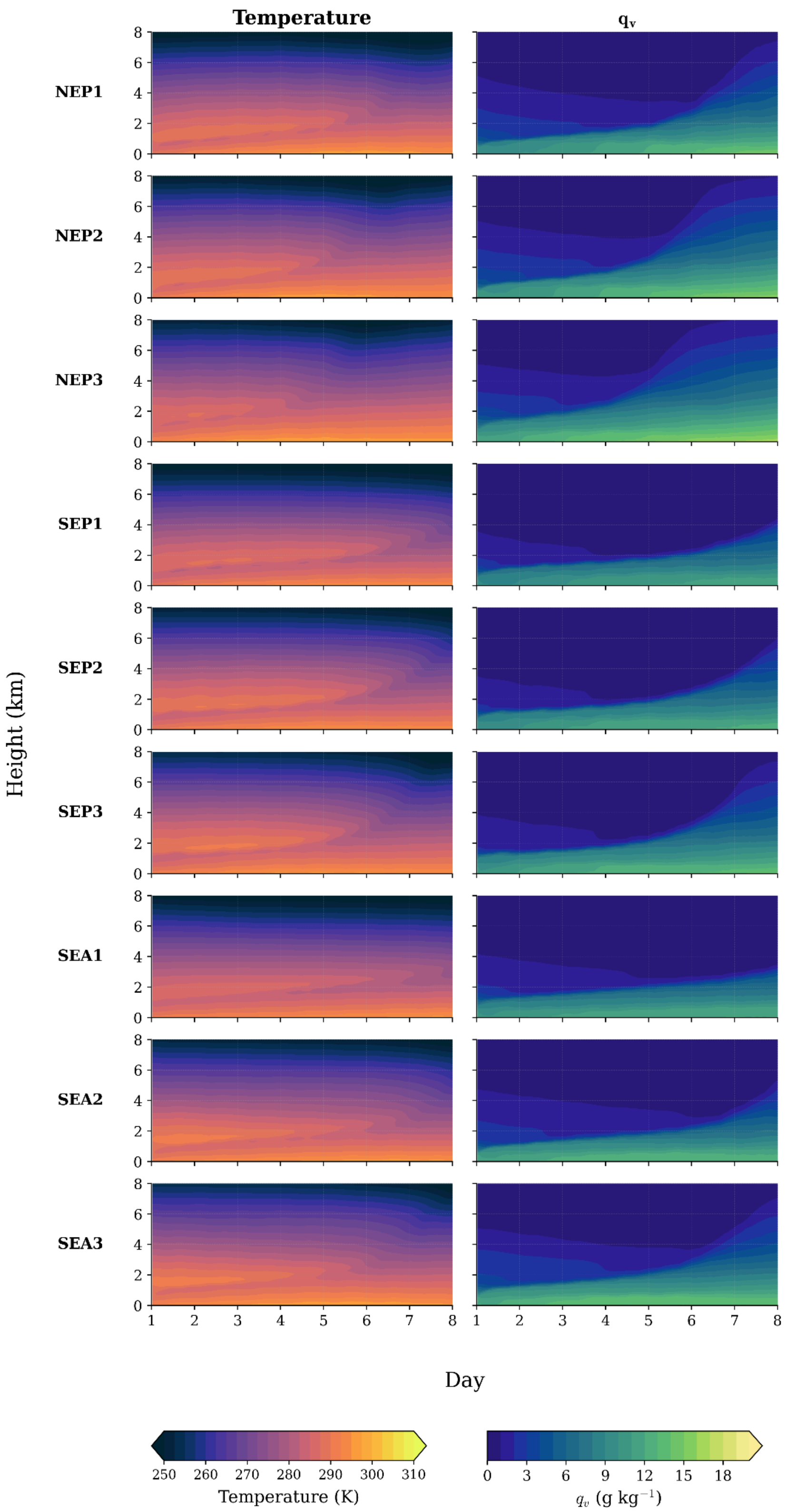

Figure S63: *Hovmöller diagram of temperature (left column) and specific humidity (q_v; right column) along the different clean model simulations for all nine initiation locations (NEP1–3, SEP1–3, SEA1–3). Color scales are shown under each column.*

How are differences in temporal resolution between the LES and reanalysis data handled?

Reply: The observational data (reanalysis and satellite) were regridded to a 1° × 1° resolution (approximately 100 km × 100 km). The LES domain was set to 57.6 km × 57.6 km. The initial conditions and large-scale forcing were applied homogeneously across the model domain. We therefore assume that the 1° × 1° observed meteorological conditions are representative of, and apply uniformly to, the entire 57.6 km × 57.6 km LES domain, which appears to be a reasonable approximation. Following this comment, we have added the following to the revised manuscript: ***"The initial conditions and large-scale forcing were applied homogeneously across the model domain. We therefore assume that the 1° × 1° observed meteorological conditions are representative of, and apply uniformly to, the entire 57.6 km × 57.6 km LES domain."***

In addition, we added a new Supplementary figure (Fig. S62) comparing the original 57.6 km × 57.6 km simulation with a larger, 102.4 km × 102.4 km, simulation, which is comparable to the 1° × 1° observational grid scale. The results show a very similar temporal evolution of the main cloud and radiative properties, supporting the robustness of the simulated bulk response to the chosen domain size.

Aerosols are not prognostic in the simulations, which is a critical limitation given that the study focuses on aerosol–cloud interactions. This point requires deeper discussion regarding its implications.

Reply: Thank you. Following this comment, we have added the following discussion to the revised manuscript conclusion section: *"In addition, our model simulations rely on idealized setups with prescribed CCN perturbations and a limited domain size* ***… Thus, future work should use prognostic aerosols, rather than prescribed CCN, and hence better represent the full spectrum of ACI and its impact on the thermodynamic conditions (Xue et al., 2010 , Leung et al., 2023 , Arieli et al., 2025)."***

The horizontal domain size (57.6 × 57.6 km) is marginal for resolving mesoscale cloud structures during the later stages of the Sc–Cu transition, and probably not sufficient to resolve deep convection. This concern is acknowledged by the authors themselves in the conclusions, where they state that the domain is not large enough to capture convective organization. This limitation weakens the interpretation of the simulated deep convective response and raises concerns regarding the conclusions.

Reply: Thank you for this comment. The chosen domain size reflects a compromise between the need to adequately resolve Sc, which requires high spatial and temporal resolution (200 m horizontal grid spacing, 147 vertical levels, and a 2 s time step in this study), and the desire to capture mesoscale cloud structures and organization (on the order of tens of kilometers (Seifert and Heus, 2013; Jansson et al., 2023)).

While this trade-off is common in numerical modeling studies, it is particularly pronounced here, as this is, to our knowledge, the first study to simulate the full Sc-Cu-DC evolution. The simultaneous requirement to resolve both Sc and deep convection (DC) across multiple simulations necessitates compromises in certain aspects of the model setup. This is explained in the manuscript as follows:

"This configuration balances the need for a sufficiently large spatial domain to resolve mesoscale cloud structures and dynamics while still resolving small-scale cloud processes at LES resolution, enabling us to capture the full tropical cloud transition (Seifert and Heus, 2013; Jansson et al., 2023)".

However, following the reviewer’s suggestion, we have now tested the sensitivity of our simulations to domain size by performing an additional simulation with a larger domain (102.4 km × 102.4 km). The comparison (Figure S62, SI) shows that the temporal evolution of key cloud and radiative properties for case NEP1, as an example, is very similar between the two domain sizes. This indicates that, despite the limited domain size, the bulk thermodynamic evolution and cloud properties remain almost the same.

At the same time, we emphasize that the domain remains insufficient to fully capture mesoscale convective organization in the deep convective regime (Muller and Held, 2012), and therefore our conclusions regarding deep convection should be interpreted with this limitation in mind. This point is now further clarified in the revised manuscript.

We have now added in the manuscript: ***"However, sensitivity tests with a larger domain (Fig. S62, SI) show very similar evolution of the bulk cloud and radiative properties, suggesting that the main conclusions of this study are not strongly sensitive to domain size, while mesoscale organization remains unresolved."***

And in the Methods section: ***"To assess the sensitivity of the results to domain size, we performed an additional simulation with a larger domain (102.4 km × 102.4 km) at the same horizontal resolution (200 m × 200 m), as shown in Figure S62, SI. The results are qualitatively consistent with the baseline configuration, indicating that the chosen domain size does not substantially affect the main conclusions. This larger domain is comparable to the 1° × 1° grid box of the observational data."***

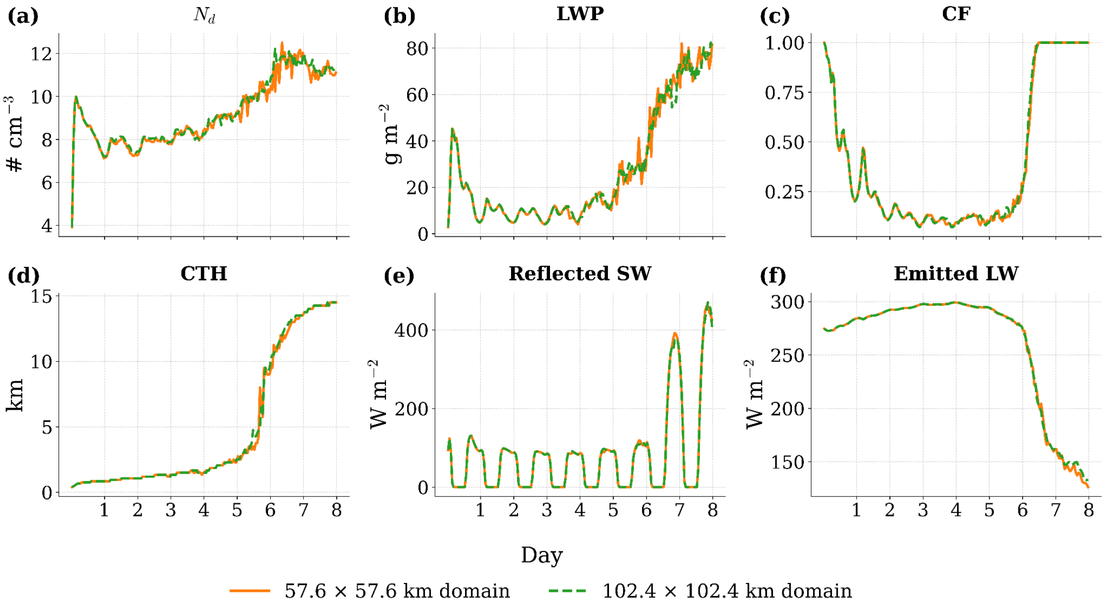

Figure S62: *Sensitivity of simulated cloud and radiative properties to domain size for the NEP1 case. Time evolution of (a) droplet number concentration (N_d), (b) liquid water path (LWP), (c) cloud fraction (CF), (d) cloud-top height (CTH), (e) reflected shortwave (SW) flux, and (f) emitted longwave (LW) flux. Results are shown for simulations with domain sizes of 57.6 km× 57.6 km (288 × 288 grid points; orange) and 102.4 km× 102.4 km (512 × 512 grid points; green dashed).*

In addition, as the review mentioned, this limitation is acknowledged in the manuscript. However, following this comment, we have added to the conclusion section a sentence motivating future examination of the sensitivity of the results to the domain size: *"For example, our domain size is not sufficiently large to capture convective organization in the deep convective regime (Muller and Held, 2012). Thus, future work should examine the sensitivity of the results to the domain size.* ***In particular, larger domains may be expected to promote earlier transitions due to the higher probability of localized precipitation events (e.g., Yamaguchi et al., 2017)."***

2. Observations

Several issues arise in the satellite-based analysis. For example, Figure 4 shows droplet effective radius along the 8-day evolution. During days 6–8, clouds appear substantially deeper and likely include glaciated cloud tops. In such cases, MODIS liquid cloud retrievals such as re and LWP may not represent the deep convective clouds.

Reply: Thank you. Following this comment, we have added a clarification to the revised manuscript: "***We note that during the final stage of the trajectories (days 6–8), clouds become substantially deeper, as indicated by increasing CTH, and may include mixed-phase or glaciated cloud tops. In this regime, MODIS liquid-phase retrievals such as r_e and LWP may not fully represent the cloud column and should therefore be interpreted with caution. Importantly, this limitation applies only to MODIS liquid cloud retrievals, while variables such as precipitation are not affected by the liquid-cloud sampling.***"

The authors choose AOD as the primary aerosol proxy, while acknowledging its limitations later in the manuscript. It is not clear why cloud droplet number concentration (Nd), which may be more directly related to CCN, was not considered more centrally. AOD is not available under cloudy conditions, especially over stratocumulus regions where cloud fraction is high, which adds further uncertainty. To address this, the authors use reanalysis AOD, which may introduce additional uncertainty beyond that of the AOD retrieval.

Reply: We agree that N_d is more directly linked to CCN and aerosol-cloud interactions. However, no aerosol proxy is without shortcomings; satellite retrievals of cloud droplet number concentration have their own limitations. We haven't used MODIS N_d as the primary grouping variable due to its limited coverage and larger uncertainties. N_d retrievals are restricted to liquid clouds and are particularly sparse or missing in convective regions, with substantial relative uncertainties (~30% in stratocumulus and up to 60–80% elsewhere; Bennartz and Rausch, 2017). In addition, N_d is not directly observed but derived from optical properties under assumptions (e.g., adiabatic structure) that often break down, especially along trajectories transitioning from stratocumulus to deep convection. (Bennartz and Rausch, 2017, Passer et al., 2025, Gryspeerdt et al., 2022)

Furthermore, N_d retrievals are only available under cloudy conditions and are strongly reduced after applying standard quality filters, leading to inconsistent sampling along multi-day trajectories. In contrast, MERRA-2 AOD provides continuous coverage along the full trajectory, including under cloudy conditions.

As mentioned in our response to the MODIS liquid-phase retrievals comment, N_d retrievals are restricted to liquid clouds and are therefore subject to the same limitations discussed above.

Thus, here we use two widely adopted proxies, AOD and lower-tropospheric SO_4 concentration. While AOD retrievals are known to be problematic in the vicinity of clouds, as shown in previous studies**,** the use of SO_4 provides a complementary proxy that is not subject to these limitations. Both yield consistent results and are in agreement with the simulation outcomes.

We have also added this in the revised manuscript *"Satellite retrieval errors can affect AOD values above and below clouds**, and retrievals are particularly uncertain in the vicinity of clouds (Koren et al., 2007).** AOD is not always consistently correlated with*

CCN due to variations in aerosol composition, size distribution, and vertical placement relative to cloud layers.”

Furthermore, AOD is averaged along the entire trajectory. Why was aerosol loading at the initial day not used, as in previous studies? This relates directly to the unexplained increase in AOD after day 5 in Figure 2, which requires clarification.

Reply: Thank you for this comment. Our approach differs from previous studies that focused primarily on the aerosol conditions on the initial day because our trajectories extend 8 days. In this case, using only the initial-day AOD would not adequately represent the aerosol environment influencing the cloud evolution throughout the entire evolution. Over such a timescale, air masses can experience substantial changes in aerosol loading due to transport, mixing, and removal processes. Therefore, classifying cases based solely on initial-day AOD would risk misrepresenting the aerosol conditions that actually affect the later stages of the cloud transition.

Instead, we group trajectories according to their aerosol characteristics along the trajectory rather than relying on a simple initial-day value. Importantly, this is not a simple arithmetic averaging of AOD over time. The grouping is designed to reflect the overall aerosol regime experienced by the air mass during its evolution, thereby providing a more physically consistent basis for comparison across clean and polluted cases.

Regarding the increase in AOD after day 5 in Fig. 2, this behavior reflects the temporal evolution of aerosol conditions along a specific trajectory presented as an example. In this specific case, this increase is mostly driven by sea salt aerosols, probably related to an increased wind speed, and is probably not influenced by the continent (as seen below in figures R1,2).

We have clarified this point in the revised manuscript to better explain both the rationale for the trajectory-based aerosol grouping and the late-stage AOD behavior in the specific example presented.

The clarified section: ***"We note that this trajectory-based classification differs from approaches that use only initial-day AOD (Christensen et al., 2020). Because our trajectories span 8 days, aerosol conditions can evolve substantially due to transport, mixing, and removal processes, making initial-day AOD potentially unrepresentative of later cloud development. The signed deviation metric, therefore, captures the relative aerosol exposure along the trajectory while accounting for its temporal evolution. We note that the increase in AOD after day 5 in the polluted example presented in Fig. 2 reflects changes in aerosol conditions along this specific trajectory driven by sea salt aerosols, probably related to an increased wind speed."***

Examination of AOD composition along the individual example trajectory, as seen here in Fig. R1, reveals that the transient increase in total AOD at day 5 is dominated by sea salt, likely associated with stronger surface winds (Fig. R2).

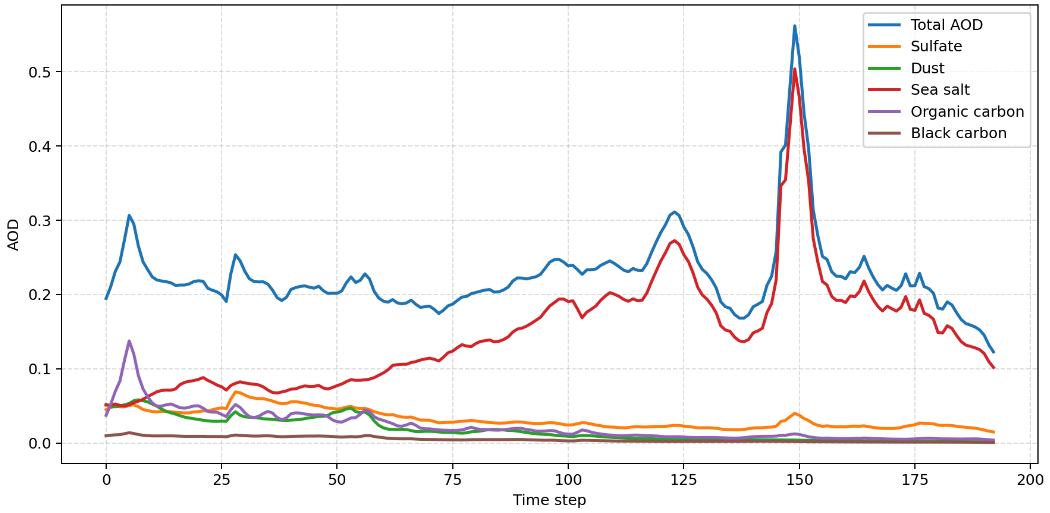

Figure R1: *Time evolution of aerosol optical depth (AOD) along a represented Lagrangian trajectory (trajectory 90, serves as an example, presented as the polluted example in Fig. 2 of the manuscript). The total AOD is shown in blue, along with contributions from individual aerosol species: sulfate (orange), dust (green), sea salt (red), organic carbon (purple), and black carbon (brown).*

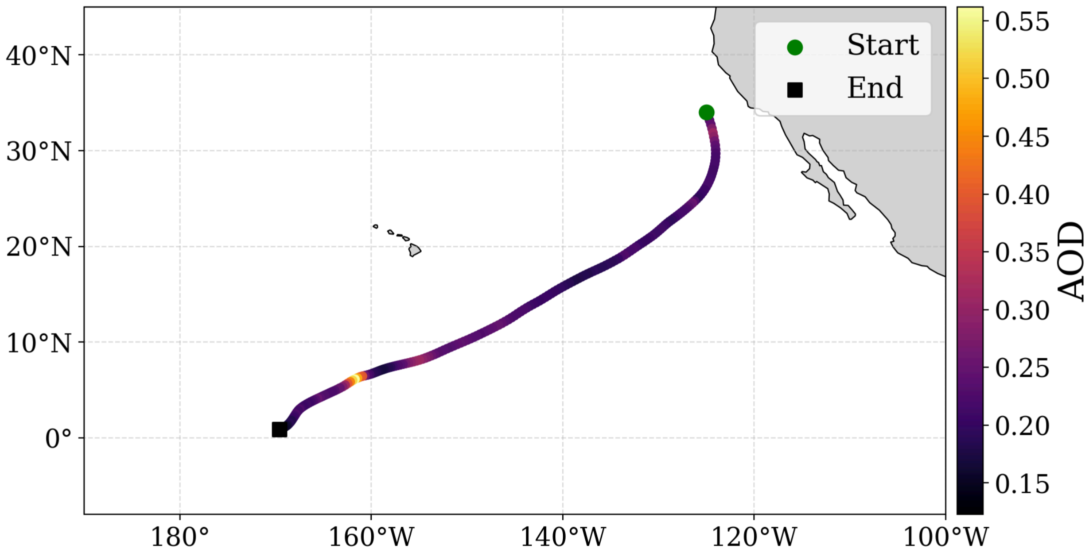

Figure R2: *Spatial evolution of trajectory 90, as an example, colored by aerosol optical depth (AOD). The start and end points are marked by green and black symbols, respectively.*

Regarding MODIS observations, it should be clarified whether there are time periods without observations due to satellite overpass limitations and how this affects the daily averaging.

Reply: Thank you for this comment. MODIS observations are not continuously available along the trajectories due to satellite overpass limitations and filtering by viewing and illumination geometry. In our data, among the points that pass masking filters, the vast majority contain valid MODIS retrievals: about 96-98% for liquid water path, 100% for cloud fraction, and 99.8% for cloud-top height. Daily averages are therefore computed from all available valid retrievals, and reduced sampling is reflected in the confidence intervals.

Following this comments, we have now clarified in the manuscript that MODIS observations are not continuous along the trajectories. Daily means are therefore computed using only valid, non-missing retrievals, which are available at the vast majority of data points. The clarified statement in the manuscript: *“**Due to satellite overpass limitations and the applied viewing-geometry filters, MODIS sampling can***

be sparse. However, among the points that satisfy the viewing-geometry constraints, the vast majority contain valid retrievals (typically above 96%); thus, missing data do not have a significant impact on the results."

3. Trajectories

The choice of three initial trajectory points that are geographically very close requires justification. Given the scale of spatial meteorological variability, one would not expect substantial differences between such closely spaced points. The noisy results in Figure 5 across these points may introduce additional uncertainty that should be discussed.

Reply: Thank you for this thoughtful comment. We agree that the proximity of the three initiation points within each region warrants clarification.

Our intent was not to sample fundamentally different meteorological regimes at very small spatial scales, but rather to examine robustness. Specifically, we selected three distinct ocean basins to span a broad range of large-scale environmental conditions. Within each basin, the use of three nearby initiation points serves two purposes: (1) to increase the effective sample size, and (2) to assess the sensitivity of the results to small variations in the initial conditions and local meteorological states.

Importantly, while the initiation points are geographically close, the trajectories quickly diverge and sample different environmental conditions along their evolution. This is illustrated by the SST evolution along the mean trajectory from each initiation point (Fig. R3), which shows that even within the same basin, trajectories can experience distinct SST conditions. This indicates that geographical proximity at initialization does not necessarily imply similar meteorological conditions along the Lagrangian path.

Although the points are geographically close, subtle differences in thermodynamic profiles, large-scale forcing, and trajectory evolution can still lead to variability in the observed/simulated response. We have now clarified this rationale in the manuscript:

“These three initiation points within each basin are not intended to represent distinct meteorological regimes, but rather to increase sampling and assess the robustness of the results to small perturbations in the initial conditions. Although the points are geographically close, the resulting trajectories diverge (Fig. 1) and sample different environmental conditions along their Lagrangian evolution (Figs. 3, S2 - S9, SI).”

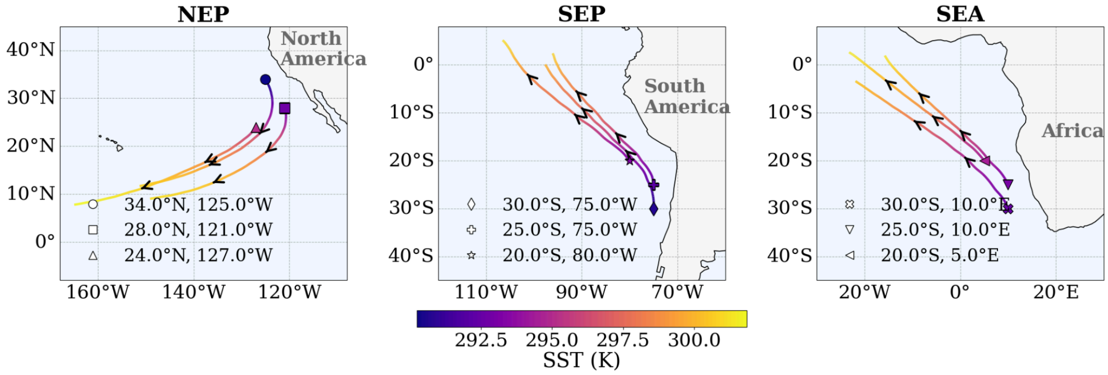

Figure R3. *Mean trajectory paths across the three ocean basins: (a) Northeast Pacific (NEP), (b) Southeast Pacific (SEP), and (c) Southeast Atlantic (SEA). Each line represents the average trajectory path initiated from a distinct starting point, marked by a unique symbol, with arrows indicating the direction of propagation with the trade winds. The trajectories are colored by sea surface temperature (SST) along their evolution.*

4. Interpretation of the thermodynamic effects

One of the most interesting aspects of the manuscript is the suggestion that aerosols may actively modify thermodynamic profiles along the cloud transition, as discussed in Section 3.3 and illustrated in Figure 9. This interpretation is intriguing and consistent with mechanisms proposed in previous modeling studies. However, given the co-variability between aerosols and large-scale meteorological and thermodynamic conditions (see major comment 1), the current analysis does not yet unambiguously isolate an aerosol-driven thermodynamic effect.

While the consistency between observations and LES results is encouraging, the observational evidence alone cannot fully rule out the influence of pre-existing environmental differences, and the idealized nature of the simulations limits the strength of causal attribution. As a result, the conclusions regarding aerosol-induced thermodynamic feedbacks may benefit from a more cautious framing or from additional sensitivity analyses that further constrain the role of co-variability.

Reply: We agree that "the observational evidence alone cannot fully rule out the influence of pre-existing environmental differences" and mention it many times in the manuscript (see our reply to the first point above). Following this comment, we have made sure that the conclusions regarding aerosol-induced thermodynamic feedbacks are framed cautiously. For example: *“Aerosols, therefore,* ***might*** *act as an internal driver contributing to cloud and moisture adjustments alongside the external modulation imposed by the large-scale environment.”*

"The general agreement between observations and model simulations … ***is consistent with*** *the interpretation that aerosols may contribute to shaping thermodynamic structure through* ***cloud–environment*** *interactions,* ***although we cannot fully rule out the influence of co-variability with large-scale meteorology in the observational analysis."***

*As well as this clarification: “****Importantly, we suggest this as a possible interpretation. We do not wish to imply that this effect is conclusively demonstrated by our analysis.****”*

Minor comments

The manuscript provides very detailed figure-by-figure descriptions. In many cases, the figures can speak for themselves, and the text could focus more on interpreting the key results.

Reply: Thank you. We revised some parts of the manuscript accordingly by reducing detailed figure-by-figure descriptions.

Line 120: The rationale for limiting SST variability is unclear and should be better explained.

Reply: Thank you. From visual inspection of the trajectories, we noticed that some meander strongly and therefore do not represent well the classical Sc-Cu-DC transition. Limiting the SST variability helps exclude these outlier (noisy) trajectories and ensures a more consistent representation of the transition regime. This rationale is now clarified in the revised manuscript: *"To reduce noise and avoid highly variable environmental changes* ***(for example, due to strong spatial meandering of the trajectory)****, we limit SST variability by selecting only trajectories with a standard deviation in SST below 4.0 K."*

Line 124: Is a precipitation threshold defined?

Reply: No precipitation threshold was defined. However, applying the rest of the conditions ensures that precipitation forms at some point along the trajectory. This is explained in the manuscript: *"Under this framework, all remaining trajectories exhibit upward vertical velocity and precipitate at some point along their evolution, thus representing deep convection formation at a certain time in the trajectory."*

We initially tested the inclusion of an explicit precipitation threshold, but found that it did not materially change the selected trajectories. This indicates that the existing filtering criteria already capture the relevant physical conditions associated with precipitation onset. Therefore, an additional precipitation threshold was not applied.

Line 203: Why is this procedure applied only to the polluted group?

Reply: The purpose of this procedure is to isolate the impact of aerosol on the humidity profile from the impact of the differences in SST between clean and polluted conditions. Hence, it should be applied just for one of the cases, either polluted or clean. In this case we decided to normalize the polluted conditions by the clean conditions. Following this comment, we have added the following clarification: *"For the polluted group, we include a uniform offset, ΔT, based on the location's SST difference, to represent the observed warmer temperature background.* ***This procedure is applied only to the polluted group to normalize it by the ΔT and compare it with the clean group."***

Line 218: Clarify what is meant by "Differences will be mentioned."

Reply: Thank you. This sentence was changed in the manuscript to: ***"In case differences arise, they will be explicitly mentioned."***

In figures such as Figure 3, consider using δ notation in parentheses for clarity.

Reply: Thank you. Δ notation was added in parentheses in Figures 3, 4, and 7

Line 261: Missing citation of relevant recent studies (https://acp.copernicus.org/articles/25/3413/2025/ and https://doi.org/10.5194/acp-24-7331-2024).

Reply: Added to the manuscript. *"Yet these differences cannot be attributed solely to aerosol impacts, since the two groups also differ in their underlying thermodynamic environments (Fig. 3; Figs. S2-S9, SI), reflecting potential confounding factors, i.e., co-variability with meteorological state (Gryspeerdt et al., 2019****; Mülmenstädt et al., 2024; Goren et al., 2025)****."*

Figures 5 and 8 are visually noisy. Consider improved visualization.

Reply: Thank you. We have revised Figures 5 and 8 to improve visual clarity by increasing subplot spacing and reorganizing the layout.

Figure 7: Differences between polluted and clean simulations are difficult to discern. Improved visualization would help.

Reply: Thank you. To improve visualization, we have revised Figure 7 by adding shading between the two curves to highlight the differences explicitly.

Line 387: Clarify what is meant by “after accounting for SST differences.”

Reply: This was clarified in the following sentence in the manuscript: *"Thus, they represent the humidity change, which is decoupled from the SST differences (Sect. 2.3)."*

Section 2.3 elaborates on the methodology.

Lines 421–423: This statement is not fully supported by the presented results.

Reply: True. This sentence refers to the cited paper at the end of this sentence (Dagan et. al. 2023) and serves to connect the current paper with previous literature regarding large-scale tropical circulation adjustments to ACI.

We have now clarified that: *“Instead, **previous studies have suggested that** aerosols may contribute to the transport of moisture and energy within the large-scale overturning circulation, and potentially feed back on the circulation itself (Dagan et. al. 2023).”*

An AI assistance statement appears after line 210 and seems misplaced.

Reply: Thank you. This sentence was moved to the acknowledgements section.

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