

We thank the reviewers for their thoughtful comments and suggestions. We considered them carefully and were addressed in the revised manuscript. In the original manuscript, we used year-round ERA5 SLP and 975 hPa wind data of 11 years to identify SOM patterns, which was justified by the use of deseasonalized and normalized meteorological fields so the seasonal dependency is removed. However, both reviewers had questions about this approach since our analysis focused only on the warmer months. To avoid confusion, we have now generated SOM-based synoptic patterns using exclusive April to September 11 years of meteorological data, and not surprisingly resulting patterns are consistent with those from the full-year dataset (shown in Figure below). We also updated our analysis to use only April–September, corresponding to the Southern California spring (April–June) and summer (July–September), with October removed from the original manuscript version. Following reviewers’ feedback, we have also reordered the synoptic patterns to reflect the progression of cyclonic development, and presented the results in that order. To reflect these changes we revised our manuscript title as “Spring-summer atmospheric circulation and boundary layer processes modulating aerosol and cloud characteristics over the coastal Northeast Pacific during EPCAPE”. We are including herewith a revised version of Track-change-enabled manuscript with implementing afore mentioned changes and addressing the other comments from reviewers. Below we offer response to reviewers per comment.

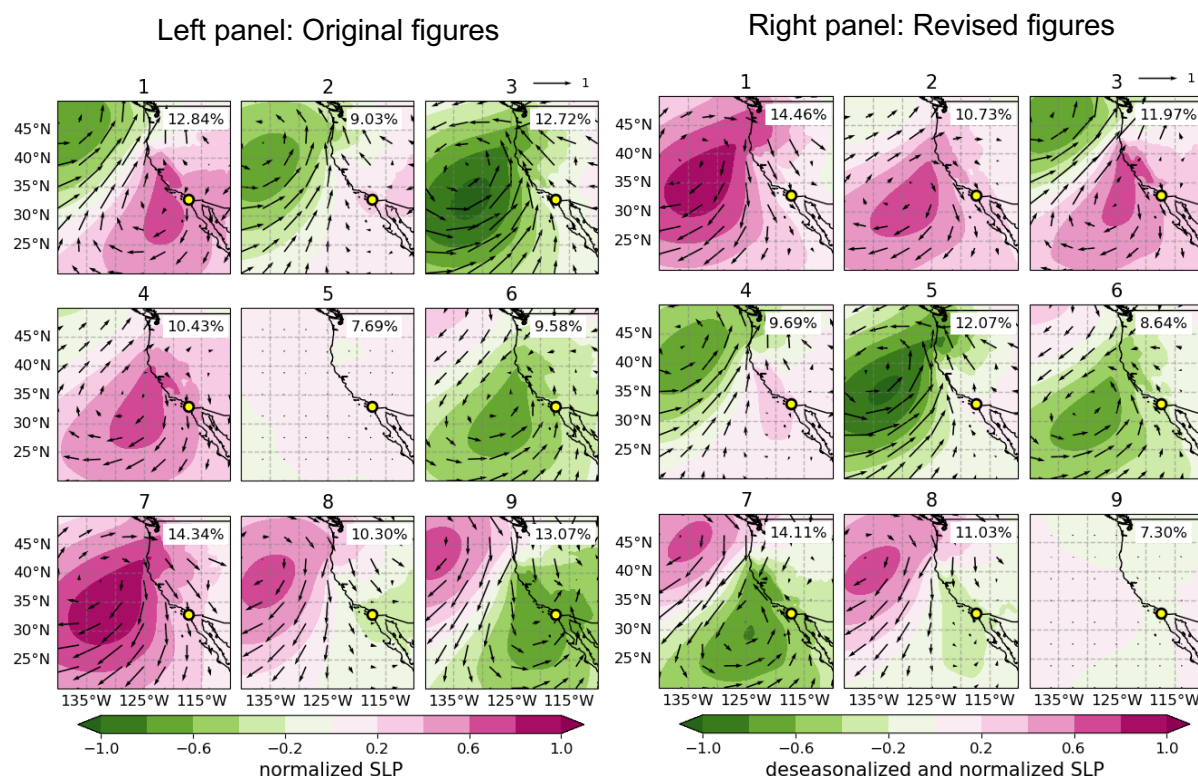


Figure: **Left panel:** Nine synoptic regimes derived from 11 years of year-round daily composite maps of deseasonalized and normalized SLP and 975-hPa winds using SOM. **Right panel:** SOM

characterized nine regimes based on 11 years of April–September daily composites of deseasonalized and normalized SLP and 975-hPa winds, and are reordered to follow cyclonic

## Reply to RC1

### Summary

This study focuses on the aerosol, cloud, and boundary layer properties during the ARM EPCAPE field campaign by contextualizing the analysis in the different synoptic patterns, classified using self-organizing maps. Overall, this paper presents an interesting perspective by linking the in situ and remote sensing data of aerosol and clouds, and thermodynamics data collected during the field campaign with the background synoptic pattern, which is key in determining the cloud types and the aerosol transport. However, I believe further clarification is needed regarding the analysis. I recommend accepting this paper with the revisions suggested below.

### Major comments

#### 1. The writing and structure.

- The results section is quite long. Please consider adding a brief introduction at the start of Section 3 to show how each subsection is connected.

**Response:** We included a brief description in the Result section to introduce the various analysis. Refer line#304-320 in the revised manuscript.

- The SOM classified 9 clusters, which are hard to keep track of their numbers throughout the manuscript. I suggest the authors may consider naming each cluster based on its pattern, so it would help the physical interpretation and make the paper easier to follow.

**Response:** We have rearranged the synoptic patterns on the order of anticyclone/cyclone evolution. Throughout the manuscript, we have also arranged the rest of the figures and interpretation accordingly.

#### 2. Equations for deriving Nd and LWP based on satellite data

- Please add references for the equations used for calculation, and evaluation results for how these retrievals work.

- These two equations use different underlying assumptions, with the formula for  $N_d$  assuming an adiabatic cloud with LWC increasing linearly with height, while the equation for LWP assumes a vertically homogeneous cloud with constant LWC. Given the focus on low clouds in this study, the LWP assumption might not be proper, as the observed LWC profiles typically increase with height. Please explain the assumptions here and how this would affect the results.
- Please explain how the constants for  $N_d$  are derived here, as it will depend on the ambient temperature.

**Response:** GOES-18 LWP is computed from the retrieved cloud optical thickness ( $\tau$ ) and droplet effective radius ( $r_e$ , e.g. Nakajima and King, 1990). The algorithm follows the bispectral approach and simultaneously retrieves  $\tau$  and  $r_e$  using the 0.64 $\mu$ m and 3.9 $\mu$ m reflectances under the assumption of a 1-D, plane-parallel, homogeneous cloud layer. Because passive satellite sensors are most sensitive to the upper few optical depths of the cloud, the retrieved  $r_e$  corresponds to values near cloud top rather than the vertically averaged droplet radius. To account for this bias, we apply a vertically stratification correction following Wood and Hartmann (2006), which reduces the LWP by approximately 17%, consistent with the expected vertical increase of liquid water content in marine stratiform clouds. In this study, we used adiabatic LWP.

Cloud droplet number concentration ( $N_d$ ) is derived using the adiabatic formula of Painemal and Zuidema (2011). This approach assumes that liquid water content increases linearly with height and incorporates an adiabatic factor of 1.0.  $N_d$  computed from MODIS  $\tau$  and  $r_e$  using PZ2011 had the best agreement with the in-situ  $N_d$  for the subtropical marine stratiform regime. In our study, we utilized  $1.4067 \times 10^{-6} [\text{cm}^{-1/2}]$  constant from PZ2011 and paired with  $\tau$  and  $r_e$  of GOES-18 SatCORPS retrieval to compute  $N_d$ . Furthermore, passive satellite retrieved  $r_e$  shows an overestimate of 15-20% in compared to in-situ measurements (e.g. Minnis et al., 2021; Painemal et al., 2023; Painemal et al., 2025). Hence, we applied a 0.8 correction factor to GOES-18  $r_e$  before computing  $N_d$ .

These details including the references are incorporated now in the revised version of the manuscript. Refer line#241-264 in the revised manuscript.

#### Minor Comments

Line 20. It seems confusing to have “9 regimes”, but 4 points listed.

**Response:** We understand the sentence specifying regimes is confusing in the abstract. We have listed the four dominant features observed in the overall SOM patterns, rather

than the individual regimes. We modified the sentences to make it easier for the readers to understand, in the revised abstract in manuscript.

Line 30. Please be more specific about what “smaller-sized particles” mean.

**Response:** For clarity, “smaller-sized particles” is now explicitly addressed as “particles with diameter  $\leq 0.1 \mu\text{m}$ ” in the abstract and other part of the manuscript.

Line 99-100. I suggest that the authors note the dominant cloud type in the abstract as well.

**Response:** Done.

Line 137. To avoid confusion with the minus sign before the numbers for frequency, I suggest using parentheses here instead of hyphens.

**Response:** Done.

Line 143. Please clarify how these rain flags are derived. Does this mean that the LWP results presented later are for non-precipitating clouds only? If so, this should be noted in the figure captions and main text.

Line 149-151. If LWP data have been filtered to exclude raining time periods, does the  $N_d$  derived here only apply to non-precipitating clouds?

**Response:** The microwave radiometer is sensitive to the total liquid water in the column, and its brightness-temperature measurements cannot distinguish between cloud water and rainwater. When precipitation is present, the retrieval of LWP becomes unreliable. The rain can wet the radiometer, which can further contaminate the signal. To avoid these artifacts, LWP retrievals in presence of rain are excluded from the analysis, which approximately removed ~1% of the dataset. This filtering effectively eliminates periods of substantial surface precipitation while retaining cases of light drizzle aloft that evaporate before reaching the ground (this behavior is frequently observed in subtropical marine stratiform clouds) and remain part of the dataset. As a result, the LWP values analyzed in this study reflect non-precipitating cloudy conditions, and this clarification has been explicitly noted in the main text and figure captions. Consistent with this filtering, the cloud droplet number concentrations ( $N_d$ ) derived here also apply only to non-precipitating cloud conditions. Refer line#149-154 of revised manuscript.

Line 207. Are the times here in UTC or local time?

**Response:** Local time. We have now clarified that in the text as well.

Line 287-290. Do the SOM maps based on data for all the months, or only for data in April-Oct of these 11 year period? If so, please clarify in the text and also in the caption of Figure 1.

**Response:** In the original manuscript, we used year-round ERA5 SLP and 975 hPa wind data of 11 years to identify SOM patterns, which was justified by the use of deseasonalized and normalized meteorological fields so the seasonal dependency is removed. However, both reviewers concerned about this approach since our analysis focused only on the warmer months. To avoid confusion, we have now generated SOM-based synoptic patterns using exclusively 11 years of meteorological data for the April-to-September period. Unsurprisingly, the results are consistent with those from the full-year dataset.

We now reorganized the nine regimes based on the progression of anticyclonic and cyclonic circulation patterns, and all interpretations are presented following this updated ordering.

Line 306. Please explain briefly what the silhouette score is and how one should interpret the values.

**Response:** We included the description of silhouette score as follows in the revised manuscript 'Line#345-350': "First, we used the silhouette score to evaluate the uniqueness of patterns across different node selections. This metric informs about how close each point in one cluster is to the points in the neighboring cluster, with score ranging between -1 and 1. A higher score ( $> 0.5$ ) suggests the clusters are well-separated, whereas a negative score suggests wrong cluster assignment. In our case, the scores ranged from 0.065 for six clusters to 0.021 for nine clusters, degrading further with higher node counts, which rendered this method unsuitable."

Line 411. Please double-check the temperatures given in the text, which are inconsistent with Figure 4a.

**Response:** The numbers given are lower tropospheric stability (LTS) estimate as the potential temperature difference between 850 hPa and surface. We now clarified this in the revised manuscript, line#446-447 of revised manuscript.

Line 502. Are the low clouds below 2km?

**Response:** Yes, the low clouds are defined as clouds with tops within 2km in SatCORPS retrievals. Also, in the ARM ECAPE ground based data, we filtered out clouds with KAZR CTH>2km and Ceilometer CBH>2km. This is stated in the text in both the satellite data description section, as well as, in ARM observations section.

Line 586, Figure 9. MWR3 data has been filtered based on rain flags. Are there similar filters for the satellite data?

**Response:** Yes! Line#232-241 describes the filters used in satellite data, copy pasted below:

Given the large uncertainties and limitations of optical retrievals during nighttime (optical depth and droplet effective radius) we only retain daytime observations with solar zenith angle (SZA) less than 70°. To minimize retrieval artifacts from thin or broken clouds, we apply additional screening criteria in our analysis:  $CF > 10\%$ ,  $\tau > 2$  (Grosvenor et al., 2018). Also, we filter-out grid boxes with cloud top temperature (CTT) lower than 273.15 K and specifically select for this analysis averaged retrievals corresponding to the low height category (< 2 km) with LWP range of 5 – 250 g m<sup>-2</sup> to limit the analysis to stratus and stratocumulus clouds and minimize retrieval uncertainties. On average these filters reduce the data availability by approximately 60 %, however, provides a cleaner and more physically consistent sample for evaluating regime-dependent aerosol–cloud relationships.

Line 596. If the aerosol data is a merged product based on both SMPS and APS, should the larger sizes also include coarse mode?

**Response:** For the accumulation mode particles, we only included particles with diameter ranging 0.1 to 1 micron. This information is included now in the revised manuscript text, line#641. Thank you.

Line 606. Please clarify why only these two levels are chosen for analysis, given that ECAPE has CCN data measured at 6 supersaturation levels.

**Response:** We clarified the choice of SS in the description in line#187-189 in the revised manuscript text as “This analysis focuses exclusively on CCN measurements at supersaturations (SS) of 0.1% and 0.2% because they are more representative of the environmental conditions at the Scripps Pier (Dendrick et al., 2026).”

Line 624. Perhaps just revise the order of Figure 11 and Figure 12, given that Figure 12 is presented first.

**Response:** Thank you for the suggestion. The figure11 and Figure 12 in original manuscript are reordered accordingly, to go with the description.

Line 624-630. The results in Figure 12 are quite interesting, but some more explanation is needed. Line 626 states, “enhanced turbulence promotes aerosol growth into accumulation mode aerosols”. Is there a decrease in Aitken mode aerosol concentration with increasing TKE to suggest that the enhancement in the aerosols larger than 100nm is due to enhanced aerosol growth? Or is that just because stronger TKE is associated with enhanced aerosol production, such as sea spray, or aerosol transport from the coast?

**Response:** We now reworded the description as in line#670-680 in the revised manuscript.

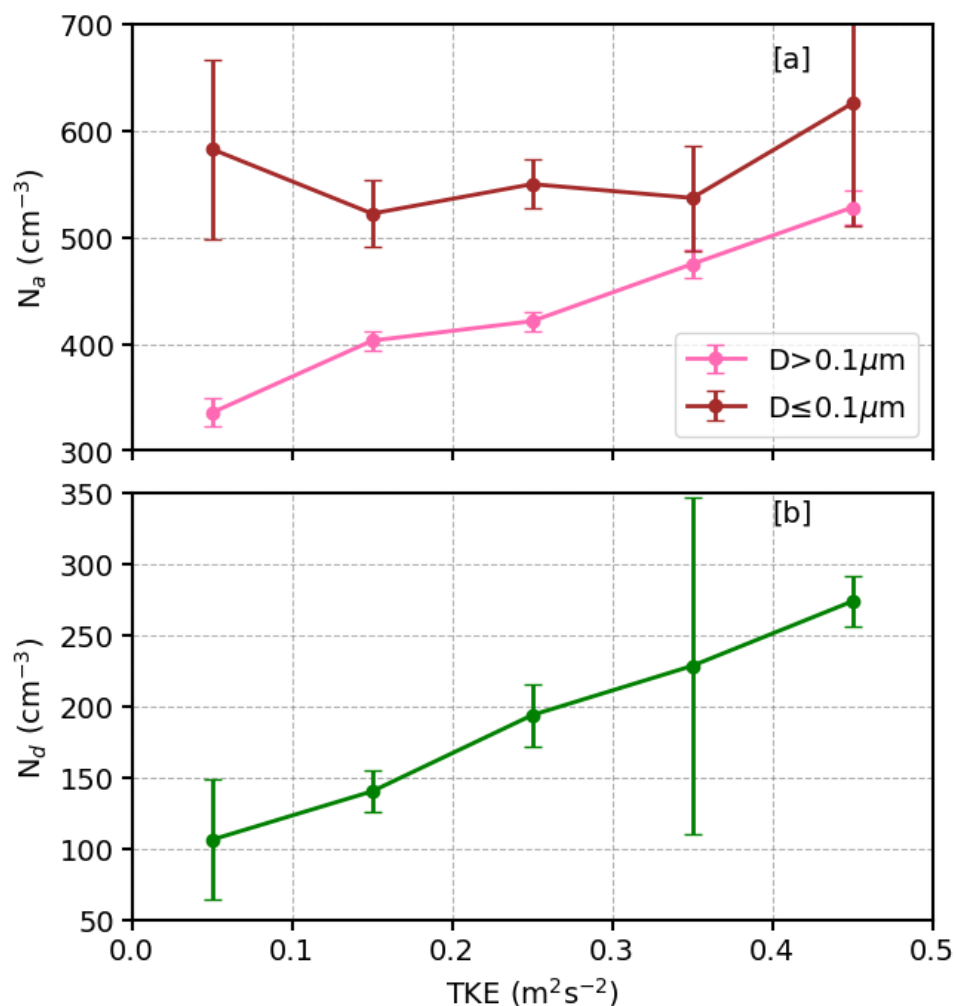


Figure: Variation in  $N_a$  and  $N_d$  with respect to TKE

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## Reply to RC2

This study investigates aerosol, cloud, and boundary-layer properties during the warm season (April–October 2023) of the ARM EPCAPE campaign by linking in situ observations, satellite retrievals, and ERA5 reanalysis with synoptic regimes identified using Self-Organizing Maps. Overall, the manuscript presents an interesting and valuable framework for examining how large-scale circulation and boundary-layer dynamics modulate cloud structure and aerosol properties along the coastal Northeast Pacific. However, several aspects of the methodology and interpretation require further clarification. I therefore recommend publication after these have been adequately addressed.

Major comments:

I am somewhat confused about the temporal setup of the SOM analysis. The SOM training was performed using daily ERA5 data from 2012–2022, whereas the subsequent analysis focuses specifically on the April–October 2023 EPCAPE period. Could the authors clarify whether the SOM training included all months or only April–October periods? If all months were included, please discuss the rationale for this choice and whether the inclusion of winter circulation regimes may influence the identified clusters relevant to the stratocumulus season.

In case the full year data has been used, I also suggest examining the monthly occurrence frequency of the identified regimes. Although the SOM input fields were normalized to reduce the influence of seasonality, presenting when the different regimes occur most frequently could provide additional climatological context and help better interpret the physical mechanisms associated with the observed cloud, aerosol, and boundary-layer characteristics.

The manuscript identifies a coherent clockwise evolution of the synoptic regimes resembling the progression of weather disturbances (4, 1, 2, 3, 6, 9, 8, and 7). Given this physically meaningful sequence, could the authors clarify why the clusters were not presented in this order throughout the figures and discussion? Presenting the regimes following their identified evolution may improve the readability and physical interpretation of the results. In addition, assigning physically descriptive names to the regimes, rather than repeatedly referring to cluster numbers, could make the manuscript substantially easier to follow.

The study domain includes significant orographic influences, while the SOM is trained using 975 hPa wind fields. Could this affect the resulting SOM classification?

Could the authors further justify the use of daily data for the SOM clustering analysis? Some of the processes discussed throughout the manuscript, such as the land-sea breeze circulation and boundary-layer coupling/decoupling, are inherently subdaily processes. It would therefore be helpful to discuss whether using higher temporal resolution data (e.g., 6-hourly fields) could potentially better capture physically distinct circulation regimes.

**Response:** In the original manuscript, we used year-round ERA5 SLP and 975 hPa wind data of 11 years to identify SOM patterns, which was justified by the use of deseasonalized and normalized meteorological fields so the seasonal dependency is removed. However, both reviewers concerned about this approach since our analysis focused only on the warmer months. To avoid confusion, we have now generated SOM-based synoptic patterns using exclusive April to September 11 years of meteorological data, and not surprisingly resulting patterns are consistent with those from the full-year dataset.

We now reorganized the nine regimes based on the progression of anticyclonic and cyclonic circulation patterns, and all interpretations are presented following this updated ordering. The monthly occurrence frequencies of all nine regimes are provided in Table S2 as the Supplementary Material for readers interested in the detailed SOM distribution.

We use 975 hPa winds primarily to capture the coastal jet, which plays a key role in modulating transport processes along coastal California. We have tested the SOM patterns with inputting 850 hPa winds, the resulted SOM patterns were similar.

The reviewer is correct that to understand land-sea interactions and diurnal boundary-layer features, the data needs to be used at higher temporal resolution. Since we focus on synoptic-scale variability, meteorological inputs at daily scale is able to discern meaningful pattern.

The manuscript applies several filtering criteria, including the removal of precipitating cases and profiles with  $LWP < 5 \text{ g m}^{-2}$ . Could the authors clarify how much of the dataset was excluded by these filters and whether the amount of removed data differs across the identified regimes? In particular, it would be useful to know whether certain regimes are characterized by a substantially larger fraction of precipitating clouds, as this could potentially influence the reported inter-regime differences in cloud and aerosol properties.

**Response:** We remove all microwave-radiometer LWP retrievals when rain contaminated, which represents less than 1% of the total. Among the regimes, precipitation occurrence is greatest in regimes 6 and 7 (0.45% and 0.95%, respectively). Regimes 2, 3, 5, and 9 exhibited respectively (0.18%, 0.09%, 0.11%, and 0.12%), while regimes 1, 4, and 8 show only about (0.007%, 0.017%, and 0.01%) precipitation frequencies. Since satellite cloud retrievals are uncertain, we uniformly excluded nighttime periods from the analysis. To ensure comparability across instruments with different sampling intervals, all cloud and aerosol datasets for the Scripps Pier are aggregated onto a common 1-hour time grid.

Additional quality filters are applied to retain only robust low-cloud scenes: thin clouds are excluded using thresholds of cloud optical depth  $<2$  and LWP  $<5 \text{ g m}^{-2}$ ; cases with very low cloud fraction (GOES-18 LCF  $<10\%$ ) or low cloud-frequency occurrence (ceilometer-based hourly cloud frequency fraction CFF  $<20\%$ ) are removed; cloud-top height (KAZR) and cloud-base height (ceilometer) must both be below 2 km; and only warm clouds are retained by requiring GOES-18 cloud-top temperature  $>273.15 \text{ K}$ . These filters minimize observational uncertainty and ensure that the analysis focuses on frequent, well-defined low-level cloud conditions within the domain.

Applying these filtering reduces data availability in each regime to approximately: 61%, 58%, 78%, 63%, 68%, 55%, 50%, 48%, and 50% of the data are excluded for regimes 1 through 9, respectively. Despite this, the resulting dataset provides a cleaner and more physically consistent sample for evaluating regime-dependent aerosol–cloud relationships.

In addition, the rationale for excluding precipitating cases could be further clarified. Since coalescence scavenging and wet deposition are important components of aerosol–cloud–precipitation interactions, removing precipitating periods may also remove physically meaningful aerosol and cloud processes relevant to the interpretation of the observed regime differences. A brief discussion of the implications and potential biases introduced by these filtering choices would strengthen the manuscript.

**Response:** Our precipitation filter is intended to remove periods of substantial rainfall that impact the quality of measurements by wetting the ground based instruments, and contaminating microwave radiometer LWP retrievals, satellite cloud property retrievals, etc. Moreover, events with low-cloud occurrence and significant precipitation are infrequent. These stratiform clouds frequently produce light drizzle aloft that evaporates before reaching the surface, such cases are included in the analysis. Because Ka band Zenith Radar reflectivity provides drizzle rate estimates,

these data will be further examined in a forthcoming work that will explore in detail aerosol-cloud-precip interaction over ECAPE.

Minor comments:

Abstract: The manuscript refers to the identification of nine synoptic regimes; however, only four regime numbers are explicitly listed!

**Response:** We understand the sentence specifying regimes is confusing in the abstract. We have listed the four dominant features observed in the overall SOM patterns, rather than the individual regimes. We modified the sentences to make it easier for the readers to understand, in the revised abstract in manuscript. Refer Line#21-29 of the revised manuscript.

L354: I suggest considering the Estimated Inversion Strength (EIS) in addition to LTS, as EIS is often a more physically representative stability metric for marine stratocumulus environments (Wood & Bretherton, 2006) and may help better explain the observed differences across the regimes.

**Response:** In the Northeast Pacific and other subtropical stratocumulus regimes, EIS and LTS structures are very similar and their correlation with cloud fraction is quite comparable (e.g. Wood and Bretherton, 2006). EIS is better than LTS primarily in the extratropics. We also included the spatial patterns of LTS vs EIS as below:

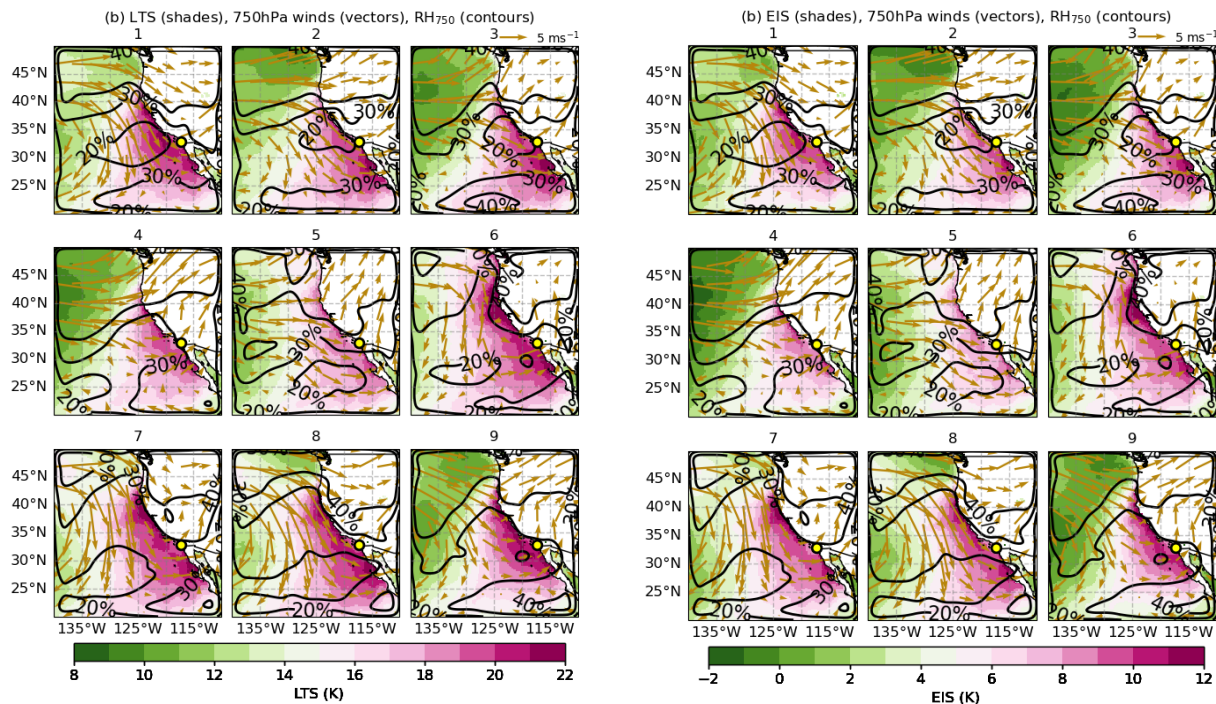


Figure: (left) Figure 2b in the revised manuscript, and (right) same but replacing LTS with EIS

L287-290: The manuscript states that the SOM input fields were deseasonalized using a 30-day moving mean and standard deviation in order to reduce the influence of seasonal variability and emphasize synoptic anomalies. While this approach is reasonable, it would be helpful if the authors could further justify this preprocessing choice and discuss how sensitive the identified regimes are to the deseasonalization procedure.

**Response:** We have now generated SOM-based synoptic patterns only using April to September for 11 years of meteorological data. This should resolve the reviewer's concern.

Figure 4. It is unclear whether the differences between the profiles (e.g., Fig. 4) are statistically significant or simply visual. Please clarify whether any statistical tests were performed to support these differences.

**Response:** Thank you. We now evaluated the width of the 95% confidence intervals (CIs) of medians of these profiles at various heights to understand if the profiles are significantly different.

Zonal winds: CIs in the lowest 250 m generally range from 0.2 to 0.4 m s<sup>-1</sup>. Between 250 m and 1 km, most regimes show CIs of 0.5–1 m s<sup>-1</sup>, with dynamically active regimes such as 3, 6, 7, and 8 reaching values closer to 0.7–1 m s<sup>-1</sup>. Above 1 km, zonal wind CIs are around 0.6–1.2 m s<sup>-1</sup> depending on regime.

Meridional winds: CIs in the lowest 250 m range from 0.2 to 0.8 m s<sup>-1</sup>, increasing to 0.5–1.0 m s<sup>-1</sup> between 250 m and 1 km. Dynamically active regimes exhibit larger variability, with CIs reaching up to ~1.5 m s<sup>-1</sup>. Above 1 km, meridional wind CIs can similarly extend to 1.5 m s<sup>-1</sup>.

Relative humidity: In the lowest 250 m, CIs range from 2–4%, increasing to 2–15% from 250 m to 1 km. Dynamically active regimes reach even higher variability, with CIs up to ~20%. Above 1 km, most regimes exhibit CIs of 4–8%, although regime 3 again shows much larger variability approaching 20%.

Potential temperature: CIs in the lowest 250 m are typically around 1.5 K, except for regime 6, which reaches ~4 K. Between 250 m and 1 km, most regimes show variability of 1–5 K, while dynamically active regime 6 exhibits large CIs up to ~25 K. Above 1 km, CIs generally remain between 2–5 K, with regime 6 again showing ~25 K.

Overall, the profiles shown in Figure 4 of the revised manuscript illustrate that most regimes (reg. 6 potential temperature  $C_{is}$  is an exception) exhibit significantly different dynamical and thermodynamic structures both within and above the boundary layer.

Figure 11: You could strengthen the back-trajectory analysis by also considering the vertical structure of the trajectories. Vertical back trajectories could provide additional insight into the role of free-tropospheric transport, entrainment processes, and the vertical separation between marine and continental air masses across the different regimes. This may help better interpret the observed differences in cloud properties and aerosol characteristics.

In addition, I suggest combining the trajectory analysis with the aerosol composition measurements (mass concentrations of organics, sulfate, nitrate, etc.). Linking transport pathways with aerosol chemical composition could help better constrain the likely source regions and provide a more physically based discussion of the dominant aerosol sources influencing each regime.

**Response:** We have done a backtrajectory height analysis, which showed that most 48-hr trajectories remained below 1 km, while the land-origin trajectories were originated from slightly higher altitude. In addition, we calculated backtrajectory distance for each regime to assess potential source region and their relevance to the observed aerosol composition. However, these analysis did not provide further insight beyond what is already presented and therefore are not include here.

L766: The statement that “Nd drivers can be more explicitly observed in Figure 12” should be revised, as the relationships shown in the figure represent correlations/associations between variables and do not necessarily demonstrate physical causation or “drivers.” A more cautious wording distinguishing association from causality would strengthen the interpretation.

**Response:** We now revised the sentence as follows in line#813-817 of the revised manuscript: “In a broader sense, the  $N_d$  variability shown in Fig. 11, reflects the combined influence of accumulation mode aerosol concentration and boundary layer turbulence, with the highest  $N_d$  occurring under conditions of both elevated accumulation mode concentrations and enhanced TKE, which are two primary factors governing droplet formation.”

Finally, the manuscript discusses relationships between several variables (e.g., BLH, TKE, CCN,  $N_d$ , etc.). A correlation analysis across all data, independent of the weather regimes, would provide a clearer and more robust assessment of these relationships.

**Response:** The correlation across all data is poor (mostly, <0.1), in general, as there are different physical and dynamical controls involved in.

Instead, we looked at regime specific correlations on aerosol–cloud–turbulence patterns. Most regimes show negative (except reg. 9) correlations between cloud-top height and aerosol loading (CCN and accumulation-mode Na), reflecting conditions in which enhanced aerosol concentrations coincide with shallower boundary layers. Nd shows a moderate to weak positive (except reg. 7) correlation with Na. Across all regimes, Na–CCN correlations remain consistently strong.

