We are grateful to the reviewers for their insightful comments and constructive suggestions, which have greatly helped us clarify and improve the manuscript. The main changes are summarized as follows:

1. *Clarification of the methodology and coupling hypothesis*. Section 2.3 now states explicitly that the method treats the boundary layer as a single advecting system, in which ship-emitted aerosol is mixed to cloud base and sampled together with the cloud it influenced. A new diagnostic confirms coupling during the CCN peak, and the eddy velocity scale gives a transport time far shorter than the 4 h transit from the source.
2. *Local confounding influences*. A new paragraph in Section 3.1 rules out local aerosol sources, island heating, and topography using the SMPS size distribution, the sensible heat flux, and new GOES-16 reflectance panels along the trajectory.
3. *Additional discussion, figures, and wording*. The results are now placed in the context of prior ENA studies, the figures have been revised as suggested, and the contradictory statements have been removed.

Below, we provide detailed responses to the reviewers' comments, with the comments in black and our responses in blue. The revised text in the manuscript is indicated in *italics*.

Reviewer 1:

The manuscript by Zhang et al. proposes a methodology for detecting clouds affected by ship emissions using ground-based observations. The authors first identify local maxima in CCN number concentration (NCCN) from ARM measurements. They then calculate 24-hour backward trajectories originating from the ARM site at the times corresponding to these NCCN maxima to determine whether they intersect the path of a ship. If an intersection is found, the observed NCCN enhancement is attributed to ship emissions. If clouds are simultaneously observed, the properties of these potentially ship-polluted clouds are subsequently analyzed. Overall, the manuscript is well written, easy to follow, and the proposed methodology is scientifically sound. However, there are several challenges that should be acknowledged and discussed before the manuscript can be considered for publication.

We thank the reviewer for the thoughtful comments and constructive suggestions, which helped us clarify and improve our manuscript.

One important question concerns whether the clouds sampled during the CCN peaks have actually interacted with the transported ship-emitted aerosols. It is possible that the clouds simply advected into the field of view of the ground-based sensors from elsewhere, while the enhanced CCN remained confined below the cloud layer. In the example case study, the backward trajectories are initialized at altitudes of only 100, 200, and 300 m above the surface, whereas the cloud base is located at approximately 1 km altitude (Figs. 6 and 7). Furthermore, the reported updraft velocities appear relatively weak, raising questions about whether the transported aerosols could realistically reach cloud base within the required timescale. The authors should discuss this potential limitation and provide justification for assuming that the observed clouds were indeed influenced by the detected ship-emitted aerosols.

Thanks for raising this important issue. We'd like to first clarify the conceptual basis of our methodology more explicitly. This method treats the marine boundary layer as one single advecting system rather than treating aerosol and cloud as separate quantities that happen to be observed at the same time. In a boundary layer coupled to the surface, aerosols emitted at the sea surface by a ship are brought upward by turbulence into the overlying cloud layer. The perturbed aerosol and the cloud influenced then advect together with the mean winds, and are sampled together when that air mass reaches the ENA site. In this regard, the CCN peak and the cloud deck observed at the same hour are not two coincident observations, but two measurements of the same air mass, which enable us to attribute the cloud response to the traced ship emission.

The reviewer's concern is the failure case of this picture. If the boundary layer were decoupled at the time of emission, the ship-emitted aerosol would remain confined below the cloud layer, and the clouds observed during the CCN peak could have advected to ENA independently. Coupling is therefore a necessary condition for the method, and we have now made the coupling assumption explicit in Section 2.3 (Lines 129-132): "*This assumes that the cloud deck is coupled to the surface. In a coupled boundary layer, ship-emitted aerosols can be transported from the surface to cloud base by turbulence, and the aerosol and the cloud influenced then advect together as a single system, so that both are sampled when the air mass passes over the ENA site.*" We also acknowledged the general limitation in Section 4 (lines 303-305): "*For example, an important assumption of this methodology is that the cloud deck is coupled to the surface; otherwise, the observed clouds may not have been influenced by the detected ship-emitted aerosols.*"

We have examined the coupling state in the example case. While the coupling state at the moment of ship emission cannot be determined directly, it can be diagnosed when the air mass is observed at ENA. Following Wang et al. (2016), we computed the difference in potential temperature and water vapor mixing ratio between the lower and upper parts of the sub-cloud layer (Figure R1 or Figure A2). Both fall below the coupling thresholds, indicating that the stratocumulus deck is well coupled to the surface during the CCN peak. We have clarified the coupling state in Section 3.1 (Lines 168-169): "*remains coupled with the surface at the time of the N_{CCN} peak (Figure A2).*"

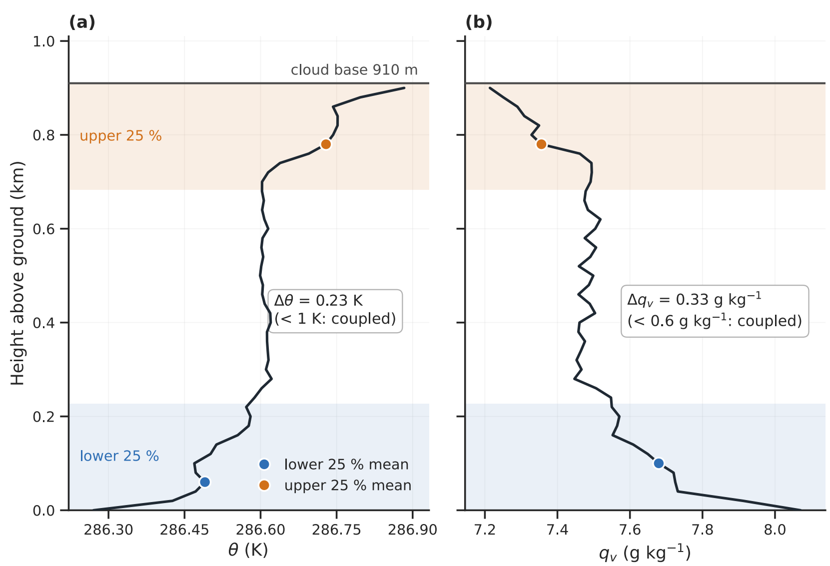

Figure R1. Hourly mean profiles of (a) potential temperature (θ) and (b) water vapor mixing ratio (q_v) at the ENA site for 1800 UTC on December 21, 2023, from the interpolated Sonde product (Fairless et al., 2021). Profiles are shown from the surface up to the ceilometer-derived cloud base (grey line). Shading marks the lower (blue) and upper (orange) 25 % of the sub-cloud layer, and the colored dots denote the layer-mean values of each. The differences between these two layer means, $\Delta\theta$ and Δq_v, are annotated in each panel. Following Wang et al. (2016), the cloud layer is considered coupled to the surface if $\Delta\theta < 1\ K\ or\ \Delta q_v < 0.6\ g\ kg^{-1}$.

Regarding the reviewer's concern about the transport timescale, the real velocity scale is that of the turbulent eddies rather than the mean vertical motion. It can be estimated from the square root of the vertical velocity variance in Figure 7b: The eddy velocity scale near 1800 UTC is around 0.5 m s^{-1}, yielding a transport time of ~0.5 h to the cloud base of ~1 km. This is much shorter than the 4 h transit of the air mass from the ship emission. We note that Figure 7a shows the median vertical velocity, which does not indicate the turbulent eddy velocity scale and may be misleading in this context and we have removed it from the revised manuscript.

In addition, how the authors would use the resulting dataset could be added to the discussion. Given the continuous dispersion and dilution of ship emissions, it is challenging to unambiguously distinguish clouds that are affected by ship emissions from those that are not. Consequently, the conventional approach of comparing polluted and unpolluted cloud populations to quantify cloud susceptibility, and ultimately estimate the effective radiative forcing due to aerosol–cloud interactions (ERFaci), does not appear to be directly applicable using this source-tracing methodology. The authors should comment on the intended applications of the dataset and discuss its limitations in the context of aerosol–cloud interaction studies.
Our approach is similar to the conventional one. Ship emissions indeed disperse continuously along the Lagrangian trajectory, so the surrounding cloud field in the comparison hours may not be entirely free of ship influence. But what we can rely on is the gradient in aerosol loading and the corresponding gradient in cloud properties, rather than an unambiguous separation of polluted and unpolluted populations.

Per your suggestion, we have added the following discussion in Section 4 (Lines 292-297): "*For example, with the dataset, we could quantify cloud susceptibility that provides an additional observational constraint on ACI. For the conventional approach of ship tracks, this is done by comparing the polluted and surrounding unpolluted cloud fields. For our dataset, we could compare the cloud deck during the CCN peak with the surrounding hours, and the gradient in aerosol loading and the corresponding gradient in cloud properties would help quantify cloud susceptibility. Meanwhile, we also need to ensure that the large-scale circulation does not change notably throughout these hours.*"

Finally, Section 3.2 appears incomplete, as it lacks a discussion comparable to that presented in Section 3.1. I recommend renaming the section to "Challenges in obtaining ship-emission-affected cases during MAGIC", which would better reflect its current content.
Good suggestion. We renamed this section to "*Challenges in obtaining ship-emission-affected cases during MAGIC*". Also, we moved Figures 8 and 9 to the supporting information since we did not delve into a detailed case discussion.

Other minor comments:

1. Lines 25-26: I would particularly recommend refraining from relying on scientific findings from the distant past, as these estimates were based on older models that have since undergone

substantial modifications and improvements. For example: “A 4% increase in stratocumulus cloud coverage can offset the warming induced by a doubling of atmospheric carbon dioxide concentrations (Randall et al., 1984).”

Per your suggestion, we replaced it with more recent studies related to the important role of low clouds in radiation balance and climate projection (Lines 26-27): “*Their feedback on global warming remains one of the largest sources of uncertainty in climate sensitivity (Myers et al., 2021; Zelinka et al., 2020).*”

2. Lines 57-59: Satellite retrievals of Nd and LWP are also subject to sampling biases arising from retrieval failures when the liquid cloud effective radius is larger than 30 µm, which may introduce artifacts into the derived correlations and potentially lead to misleading physical interpretations (Cho et al., 2015; Choudhury and Goren, 2025).

Thank you for bringing these studies to our attention. We have added this sentence and references in the main text (Lines 57-60).

References:

Choudhury, G., & Goren, T. (2025). Sampling bias from satellite retrieval failures of cloud properties and its implications for aerosol-cloud interactions. Geophysical Research Letters, 52, e2025GL115429. https://doi.org/10.1029/2025GL115429

Cho, H.-M., Zhang, Z., Meyer, K., Lebsock, M., Platnick, S., Ackerman, A. S., et al. (2015). Frequency and causes of failed modis cloud property retrievals for liquid phase clouds over global oceans. Journal of Geophysical Research: Atmospheres, 120(9), 4132–4154. https://doi.org/10.1002/2015JD023161

3. Lines 119-127: Could the authors please clarify in the manuscript why and how the “Virtual Ship Track” dataset was used in the study, when already a high resolution ship emission dataset is available? Virtual ship track data is tied to MODIS overpass time, and may therefore lead to much less number of intersections with the ENA site.

The virtual ship track was used mainly because the access to high-resolution ship emission dataset is challenging and not available near California for us at the moment. Instead, we used the starting and ending locations of virtual ship track to mimic the emission data. However, as you mentioned, a limitation of the virtual ship track data is its very limited sample size, which makes it difficult to identify suitable cases during the MAGIC campaign. The clarification about why and how to use virtual ship track can be found in the second paragraph of Section 2.2.

4. Figure 2: I do not fully understand why the ship emission locations marked in the two panels of Fig. 2 are so different. If the emission data is an instantaneous snapshot, for every hour, it should just be one pixel for every ship location. What do these lines represent and why is panel (a) not an extension of panel (b), since (b) is 4 hours behind (a)?

Sorry for the confusion. As the ship emission is the hourly dataset, the shot lines in the figure represent the ship movement within one hour. We examined the panel-a like figure hour by hour (see below), and found that panel (a) is indeed the extension of panel (b).

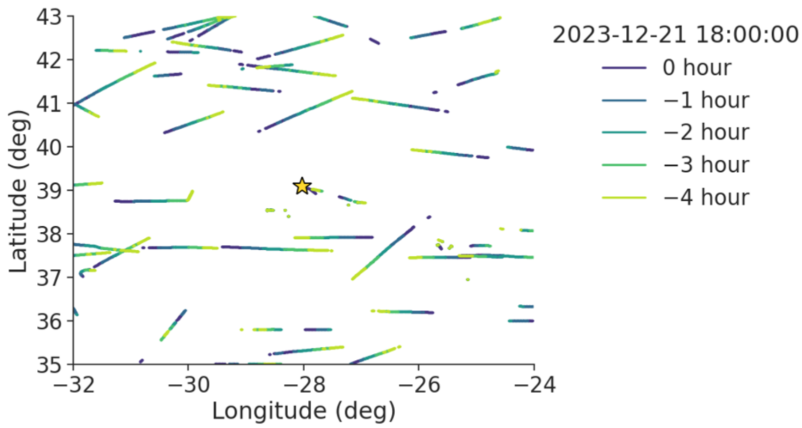

Figure R2. Ship emission locations at different hours denoted by gradient colors. The yellow star marks the ENA site.

5. Line 201: Could the increase in low cloud cover and liquid water path (LWP) be due to the movement of clouds with high LWP from other regions above the ARM site? Can the authors comment on whether the clouds were stagnant or being transported from elsewhere during the peak in CCN concentrations.

The clouds were being transported rather than stagnant. As clarified in our response to the first major comment, this transport is the basis of our methodology rather than a confounding factor. Because the cloud deck is coupled to the surface at the time of observation, the cloud arriving with the CCN peak is the cloud that was exposed to the plume, not a separate high-LWP deck that happened to advect here from elsewhere.

To examine this, we have added GOES-16 imagery along the trajectory (Figures R3f-j). The 0.64 μm reflectance is shown at hourly intervals from 0 to −4 h over the same domain, with the air mass position marked at each hour. There is no distinct high-reflectance cloud deck approaching the site. The changes in cloud properties relative to the nearby hours are therefore less likely to result from the advection of an unrelated cloud layer into the field of view.

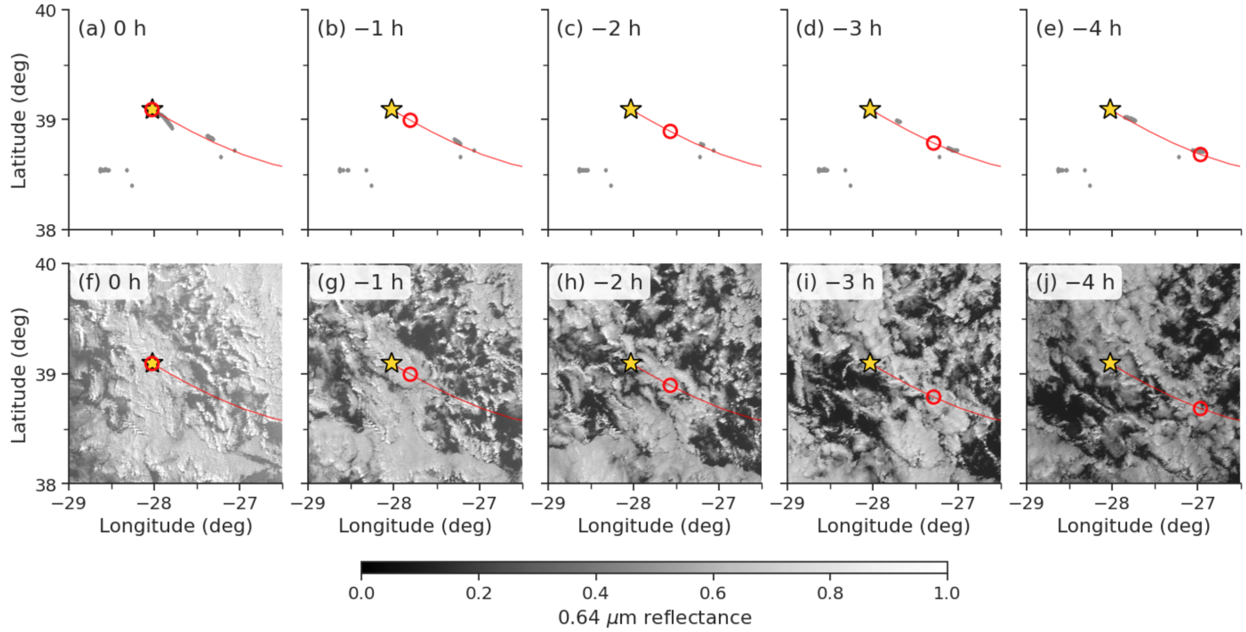

Figure R3. Lagrangian back-trajectory arriving at the ENA site (gold star) on December, 21 2023 at 1800 UTC, shown at hourly intervals from 0 to −4 h. (a–e) Ship SO_x emission locations (gray dash), with the trajectory path (red line) and the parcel position at each hour (red circle). (f–j) GOES-16 ABI 0.64 μm reflectance over the same domain, with the same overlays.

6. Figure 6: I recommend removing the fill color in the pink bar for indicating the Nccn peak. It is interfering with the color keys beneath, confusing its interpretation. I would recommend only highlighting the boundaries of the pink bar.
Following your suggestion, we use the boundaries of black dashed lines to highlight the CCN peak. Please see the new Figure 5.

7. Line 237: On what basis do the authors state that "the boundary layer is topped by stratocumulus"? Please clarify.
The new Figures 2f-j show that cloud decks are blanket-like stratocumulus clouds, and the radar image (new Figure 5) can demonstrate that the cloud tops are immediately below the PBL height that is indicated by the triangle marks.

8. Line 239: I suggest using "cloud radar reflectivity" instead of cloud reflectivity.
We revised it with "radar reflectivity".

9. Line 288: The term "ship-track" could also mean bright linear cloud features observed in satellite images. I would rather use "ship ground track".
Revised as suggested.

Reviewer 2:
Review of the article titled "Investigating aerosol-cloud interactions by fusing DOE ARM observations with ship tracks: A methodology" by Zhang and coauthors for publication in the Atmospheric Chemistry and Physics.

The authors have extended their ship-track impact on stratocumulus work based on satellite data to data collected at the ARM sites. The authors have focused on two ARM sites and have presented a method to identify ship-track signatures in the ground-based data, and then tried to understand the impact of high CCN on cloud properties. The article is straightforward to understand. However, the methods employed by the authors are not rigorous, thereby yielding flawed results/conclusions.

Broadly I find 1) the method to identify ship-track influence from the surface aerosol data, and 2) method to understand impact of changes in CCN on clouds/rain to be deeply flawed. In addition, the authors have ignored a lot of aerosol research already done at the site. The article also has several contradictory statements. Below I am listing the major and minor issues.

We thank the reviewer for the thoughtful comments and constructive suggestions, which helped us clarify our manuscript and reinforce our conclusions.

Major Comments

The authors have cited the Gallo et al. (2020) and Ghate et al. (2023) who have reported the aerosol measurements to be affected by the local sources, especially during southerly wind conditions. Despite this, the chosen case to highlight the ship-track impact on aerosol is of southerly wind conditions. It is possible that this case is an exception and the increased CCN are simply due to ships. To understand this maybe the authors can use other aerosol chemical, and size distribution measurements made at the site. I assume the ship emissions are largely at sub-100nm sizes. As of now, it seems that the local sources that are only ~100 meters away from the site are affecting the measurements rather than a ship that is ~100 km away.

It has been shown by Jensen et al. (2021) and Ghate et al. (2021) that the cloud and precipitation during southerly wind conditions is affected by island heating and topography. So how can the authors be sure that the CCN effects on cloud/rain (Figure 6) are due to aerosol and not due to the topography. This issue needs to be explored further. Especially as the rain shaft during the CCN peak is very similar to the other rain shafts per the radar image (Figure 6). The surface rain gages are well-known to miss light rain, and hence the couple of rain measurements shown in Figure 6 bottom panel cannot truly be trusted.
Thank you for pointing out these issues. We acknowledge that a case under southerly wind conditions may be subject to the influences noted. We selected it because among the 79 cases it is the only stratocumulus case that persisted from emission through to observation at the ENA site. It is also single-layer and well coupled to the surface, which makes it the best suited case for our analysis. We clarified it in Section 3.1 (Lines 166-169): "*This case is best suited for examining cloud adjustments because the cloud deck is single-layer (Figure A1) and horizontally homogeneous (Figure 2), persists from the ship emission encounter to observation at the ENA site (Figure 2), and remains coupled with the surface at the time of the* N_{CCN} *peak (Figure A2).*"

However, we could examine whether each of these influences is significant: local pollution sources, island heating, and topography.

1. Regarding local pollution sources, we examined the aerosol size distribution measured by the SMPS (Figure R4). As the reviewer notes, ship emissions are largely at sub-100 nm sizes (Seppälä et al., 2021). The size distribution at the peak is bimodal, with a mode near 20–30 nm that is absent in the adjacent hours and an accumulation mode near 90 nm. Since plume particle size increases with distance travelled from the source (Seppälä et al., 2021), we attribute the small mode to the ship passing close to the site (Figure R5a or Figure 2a) and the larger mode to the plume emitted 4 h earlier (Figure R5e or Figure 2e), indicating that the peak is dominated by ship emissions rather than local sources.

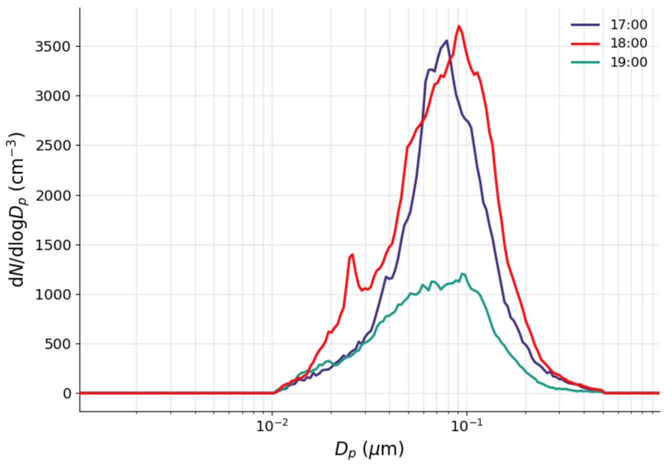

Figure R4. Hourly mean aerosol number size distributions (dN/dlogD_p) measured by the Scanning Mobility Particle Sizer (SMPS) at the ENA site on December 21, 2023, for 1700, 1800 and 1900 UTC.

2. Following Ghate et al. (2021), island heating effects occur predominantly under clear-sky conditions. But the selected case occurs at 1800 UTC, when the sensible heat flux is already weak and even negative (new Figure 4f), so island heating induced changes in surface fluxes are not expected to be significant.

3. The air masses advect from the southeast and have passed near Terceira Island, thus likely subject to the topography effects. Christensen et al. (2023) showed that a significant topography effect would produce a clear gravity-wave signature in the lee side of the mountain (see their Movie S1). We examined GOES-16 0.64 µm reflectance from 0 to −4 h along the trajectory and found no wave pattern near the ENA site (Figures R5f-j, now new Figure 2), indicating that the wake effect is not significant for this case.

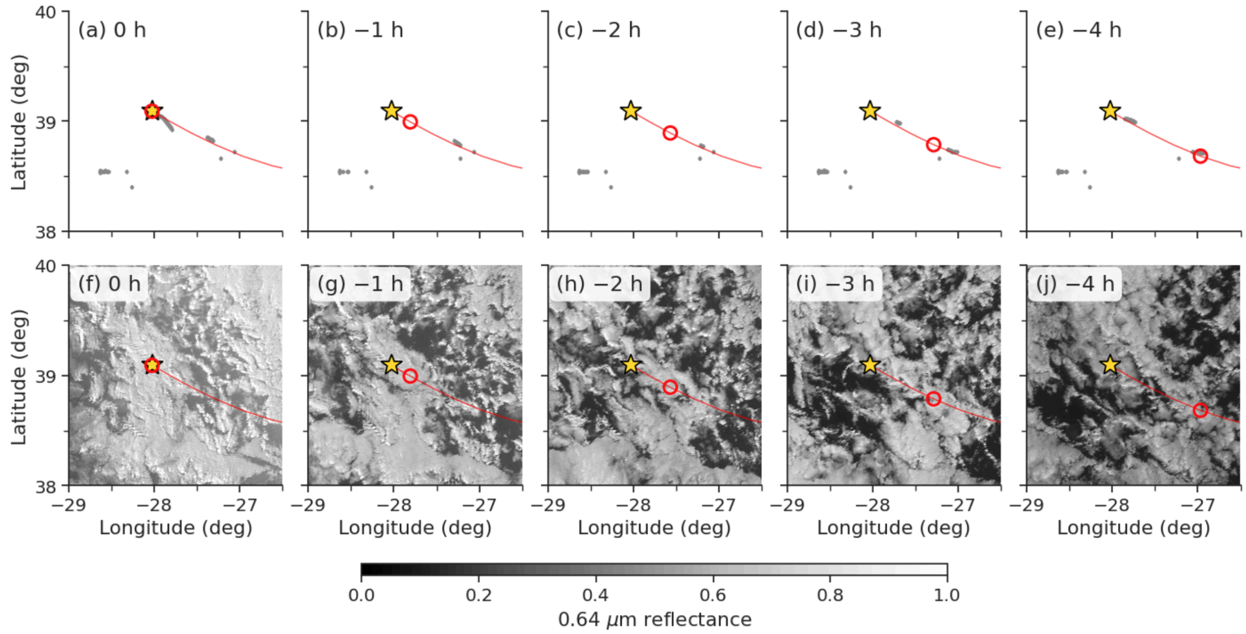

Figure R5. Lagrangian back-trajectory arriving at the ENA site (gold star) on December, 21 2023 at 1800 UTC, shown at hourly intervals from 0 to −4 h. (a–e) Ship SO_x emission locations (gray dash), with the trajectory path (red line) and the parcel position at each hour (red circle). (f–j) GOES-16 ABI 0.64 μm reflectance over the same domain, with the same overlays.

We have clarified these issues in the second paragraph of Section 3.1 (Lines 177-187): "*However, this case occurs under southerly wind conditions, suggesting that it may be influenced by local aerosol sources on Graciosa (Gallo et al., 2020; Ghate et al., 2023), along with island heating and topographic effects on cloud and precipitation (Ghate et al., 2021; Jensen et al., 2021). We therefore examine whether these local influences could account for the observations. The aerosol size distribution at the peak is bimodal, with a mode near 20–30 nm absent in the adjacent hours and an accumulation mode near 90 nm (Figure A4). Since plume particle size increases with distance travelled from the source (Seppälä et al., 2021), these correspond to the ship passing close to the site (0 h) and to the plume emitted 4 h earlier, respectively. Island heating is unlikely to contribute because the peak occurs at 1800 UTC, when the sensible heat flux is already weak and negative (Figure 4f). Such effects are reported predominantly under clear-sky conditions (Ghate et al., 2021). Christensen et al. (2023) showed that a topographic influence would leave a gravity-wave signature in the lee of the island, but no such pattern is evident in the GOES 0.64 μm reflectance along the trajectory (Figures 2f-j).*"

To enhance confidence in the light precipitation signal, we now show both the optical rain gauge and the present weather detector in the revised Figure 5. We further note that the shift of the size distribution toward smaller diameters after the CCN peak (Figure R4) is consistent with the removal of larger particles by collision-coalescence and precipitation.

Since you already have data from geostationary satellite (Figure A2). Maybe for this ship-track case you can show the enhanced cloud reflectivity as it gets advected over the island. This might not be possible as this is a nighttime case, but a similar image for another case might drive home the point.

Thank you for this suggestion. However, several factors make such an image impractical for the present study. First, after the cloud-type screening, we have very few cases with a persistent and single-layer stratocumulus cloud deck for the analysis. Also, a visible reflectivity enhancement is not expected in most ship-influenced clouds. The detectable tracks account for only about 5% of the aerosol indirect forcing from ship emissions (Yuan et al., 2025b). The absence of a visible signature therefore does not indicate the absence of an aerosol effect. Moreover, the geostationary imagery is considerably coarser than the ground-based measurements and resolving the cloud response is precisely what the ENA site provides. This complementarity is the motivation for our approach: satellite imagery identifies the aerosol source region, while the ARM observations resolve the cloud response.

Since you have identified 79 cases of ship-tracks (Line 163) from the ENA data. I suggest you do a scatter plot between the delta-NCCN, and other microphysical properties. The correlations within these plots will tell us if the changes in cloud/rain properties are caused by ship emitted aerosol. Thank you.
We agree that this is a valuable analysis, but a meaningful statistical relationship would first require screening the cases further for a single-layer cloud deck, the same cloud type, and a similar synoptic regime, after which too few cases remain to support such an analysis. The current work focuses on a case study demonstrating the methodology, and we will address the statistical relationship in our follow-up work.

A lot of research has been done to understand the impact of aerosol on clouds using the ARM ENA data. Please see papers by Robert Wood, Jian Wang, Fan Yang, Zheen Zhu etc. Also, some other papers by Xue Zheng, Matt Christiensen, and Xiaojian Zheng. It would be unwise not to put your results in the context of results of these papers. Thank you.
Thank you for your suggestions. We have added discussion placing our results in the context of these studies, which greatly strengthens the interpretation of our case:

"*Christensen et al. (2023) showed that a topographic influence would leave a gravity-wave signature in the lee of the island, but no such pattern is evident in the GOES 0.64 µm reflectance along the trajectory (Figure 2).*" (Lines 185-187)

"*Before 1200 UTC, when* N_{CCN} *remains steady, the precipitation appears modulated by LWP, most evidently near 0700 UTC, where the LWP maximum coincides with notable drizzle indicated by the radar reflectivity (Figure 5). This behavior is consistent with the overcast stratocumulus case observed at Graciosa, in which LWP is the primary modulator of the cloud-base precipitation rate under nearly steady CCN concentrations (Wood et al., 2015).*" (Lines 237-241)

"*For a similar regime of weak-to-moderate drizzle evaporating before reaching the surface, however, Zhu et al. (2023) found precipitation at ENA to be reduced rather than enhanced under stronger turbulence, as entrainment of dry air evaporates cloud water. The net effect of turbulence on drizzle therefore remains uncertain and cannot be resolved from one single case.*" (Lines 250-252)

"*A large library would further allow the cases to be stratified by meteorological regime, which is important because the LWP adjustment to aerosol at ENA differs systematically among synoptic* *regimes (Zheng et al., 2025). The case presented here falls in the ridge regime under a persistent surface high (Figure A3), and its response may not generalize in other regimes.*" (Lines 297-300)

There are several instances of contradictory statements in the text. These need to be redone before the article can be published. For example, Line 180: "indicating an anomalously high enhancement for the site and suggesting that local sources are unlikely to dominate in this case." Followed by "assessment of local aircraft contributions is challenging here due to the lack of flight time schedule data". The local aircraft times are available from the site operators.
We have removed these sentences, since the local pollution's influence can be indicated by the particle size as discussed in the major comment 1.

Line 239: "The precipitation appears to be delayed by approximately 1h, as evidenced by stronger reflectivity and a pronounced surface precipitation rate immediately after the NCCN peak. That said, the delayed precipitation may also be coincidental and not necessarily related to aerosol perturbations, and its significance should be evaluated with a larger sample of cases in the future." So unclear if you do believe that this is delayed precipitation or not, and why?
To avoid the confusion, we removed the sentence "that said, the delayed precipitation may … in the future."

Minor Comments:

Figure 6 and 7: Please clean the image for noise. I am sure there are no lidar returns above the cloud base. Also Figure 7b is not discussed in the text.
Done as suggested. Figure 7b is now shown as Figure 6, and the discussion is also updated. (Lines 245-254)

Since you don't have any results from the MAGIC campaign, I'd remove that figure and related text.
Thanks for your suggestion. We changed the title of this section to "*Challenges in obtaining ship-emission-affected cases during MAGIC*" to emphasize the challenge. And the figures are moved to the supporting information.

Since all four panels of Figure 3 look exactly the same, I'd remove it and put it in supporting information.
Done as suggested. It is now shown as Figure A3.