Summary

We appreciate the valuable reviews and constructive feedback provided by the reviewers. We agree with the reviewers' suggestions and carefully revise the manuscript. Below are our point-to-point responses to the reviewers' comments and suggestions, with the reviewers' comments (RC) in black, our responses in red, and *the revised manuscript content in italicized blue font*.

Minor revision-egusphere-2025-1463-referee-report-1

Second Review of Qiao et al. (2025)

I thank the authors for their thorough responses to my and the other two reviewers' comments and the major revisions made to the text. I believe the paper has been much improved based on these suggestions and the incorporated changes. However, I still have a number of largely editorial and some technical comments and suggestions that I feel will strengthen the paper and overall improve its clarity before it is suitable for publication.

General Comments:

RC1.1: There are several places where the authors use the word "discrepancy" when they mean "differences." "Discrepancy" suggests compatibility while "difference" is a distinction between things. Please make sure that the right word is being used. I believe most times "discrepancy" is used in this paper it should be replaced with "difference", but please verify.

Response: Thank you for your meticulous suggestion. Following your suggestion, we have revised the entire manuscript, replacing "discrepancy" with "difference" where appropriate.

A representative revision example is provided below:

Section 2.3.1, Line 181: "We used above aerosol component data to discuss differences in aerosol distribution over the SCS."

RC1.2: The authors generally took my previous suggestion to remove the article "the" when it was superfluous, but in some places it was incorrected deleted, e.g. Section 3.3, Line 394: "Among them, [the] degree of impact …" The article is necessary here. Please verify that these are revised properly.

Response: Thank you for your meticulous comment regarding the appropriate use of the definite article "the". We have systematically reviewed the entire manuscript to verify all instances where "the" was previously adjusted, including the specific case noted (Section 3.3, Line 496: "Among them, [the] degree of impact ⋯"). The missing "the" has been restored as it is grammatically necessary to modify the specific noun

phrase "degree of impact". All other cases of incorrect deletion of "the" have been corrected to ensure grammatical accuracy and consistency throughout the text. We greatly appreciate your attention to this detail, which enhances the rigor of the manuscript.

A representative revision example is provided below:

Among them, the degree of impact on marine transport and background aerosols caused by continental transport and marine production differs greatly due to differences in the degree of continental transport and marine biological activities at different distances from the coast.

RC1.3: I, again, encourage the authors to make a concerted effort to carefully re-read the paper in its entirety with revisions to ensure its clarity. There are several passages with missing words and punctuation errors that make the text a bit difficult to follow in some place.

Response: We sincerely appreciate the reviewer's valuable suggestion. We have carefully re-read the entire revised manuscript in a concerted effort to enhance its clarity. Specifically, we have thoroughly checked and corrected all identified missing words, punctuation errors, and other grammatical inconsistencies that may have hindered readability. We trust that these revisions have made the manuscript more coherent and accessible, and we apologize for any inconvenience caused by the initial oversights.

Response to Author Responses:

RC1.4: I thank the authors for clarifying in their response why two different reanalysis datasets were used, however I believe this should also be briefly detailed in the main text; ERA5 for dynamical properties (because of better in situ agreement), MERRA2 for thermodynamics and composition.

Response: Thank you for your valuable comment. We agree with your comment and have detailed the rationale for using two different reanalysis datasets in the main text: ERA5 was selected for dynamical properties (10-m wind speed, direction, and friction velocity) due to better in situ agreement, while MERRA-2 was chosen for thermodynamic variables (2-m temperature, sea surface temperature) and related variables (SST-T2m) given its excellent consistency with observational data in the South China Sea.

The ERA5 hourly dataset used in this study was provided by the European Centre for Medium-Range Weather Forecasts (ECMWF) (Hersbach et al., 2023); ERA5 was selected for these dynamical properties due to better in situ agreement (Li et al., 2025).

The Modern-Era Retrospective Analysis for Research and Applications, Version 2 (MERRA-2) provides reanalyzed SST and T2m data that show excellent agreement with observational data in the SCS ($r > 0.9$) (Jiang et al., 2021). We selected SST and T2m data from the MERRA-2 meteorological dataset in this context (Gelaro et al., 2017).

RC1.5: Please specify the range of summed NC in the Flores 2020 study for inclusion in the main text.

Response: Thank you for your valuable comment. We have revised the main text to explicitly specify the range of summed NC in Flores et al. (2020). We appreciate your attention to this detail, which improves the rigor of the manuscript.

The average summed NC observed in this study (54 cm^{-3}, 0.5 μm ≤ Dp ≤ 1.98 μm) was slightly lower than the NC reported for the Western Pacific (83 cm^{-3}, 0.1 μm ≤ Dp ≤ 1.98 μm) by Flores et al. (2020).

RC1.6: These are not discrepancies they are differences.

Response: Thank you for your meticulous comment. As noted in our response to RC1.1, we have changed "discrepancies" to "differences".

Editorial Suggests and Technical Comments on the Revised Manuscript:

RC1.7: Introduction, Line 58: " …between different areas." Replace "areas" with "regions." The same should be done for line 74.

Response: We have replaced "areas" with "regions".

RC1.8: Introduction, Line 62: " …observed that the average micrometer aerosols …" I believe the phrasing here should not be "micrometer aerosols." Do the authors mean, "accumulation- to coarse-mode aerosols"? <1μm aerosol are not "micrometer aerosols" nor is this standard terminology.

Response: Thanks very much for your insightful suggestion. We confirm that we intended to refer to "accumulation- to coarse-mode aerosols" here. To enhance terminological precision and conciseness, we have revised "micrometer aerosols" to "accumulation- to coarse-mode aerosols" in the revised manuscript, with the measured particle size range (500 nm ≤ Dp ≤ 10000 nm).

In the Indian Ocean, Pant et al. (2009) observed that the average accumulation- to coarse-mode aerosols (500 nm ≤ Dp ≤ 10000 nm) mass concentrations were 8.89 μg m^{-3}.

RC1.9: Introduction, Line 75: " …most available marine aerosol data …" Replace "data" with "measurements."

Response: We have replaced "data" with "measurements".

RC1.10: Section 2.2.1, Line 162: "the" (and "diameter") should be included in the sentence, " …which has 52 size channels in [the] 0.5 to 20 μm [diameter] range."

Response: We have added "the" and "diameter" to this sentence as suggested.

RC1.11: Section 2.2.1, Line 170: Replace "Thereby" with "Therefore"

Response: We have replaced "Thereby" with "Therefore".

RC1.12: Section 2.3.1, Line 220: "For atmospheric aerosol [component data]…" The phrase "component data" is vague. This should be replaced with "composition."
Response: Thanks very much for the suggestion. We have changed "component data" to "composition".

RC1.13: Section 2.3.1, Line 226: Here, and wherever appropriate, "discrepancy" should be replaced with "differences".
Response: Thank you for your constructive comments. As noted in our response to RC1.1, we have changed "discrepancies" to "differences".

RC1.14: Section 2.4, Line 264: Can the authors be a bit more specific when they say, "sharp decrease and increase in NCs"? Is this a visual inspection of NC changes or was a statistical method applied to identify these rapid changes in NC?
Response: Thank you for your thoughtful comment. To clarify, the "sharp decrease and increase in NCs" refers to statistically defined rapid changes (not visual inspection). We have added specific descriptions in the revised manuscript.
Therefore, to further screen out the possible influence of ship emissions, we excluded data points where NCs exhibited a sharp short-term fluctuation (i.e. one order of magnitude higher or lower than the average NCs at that time) in the absence of changes in meteorological parameters and influences of continental transport.

RC1.15: Section 3.1, Lines 291-306: It is important for the authors to acknowledge that a potential leading cause of the differences in aerosol NC reported in this study as opposed to those compared to in literature (Table 2) are the differences in size range measured. This is not mentioned anywhere in these passages.
Response: Thank you for your insightful comment. We fully acknowledge that differences in the measured particle size ranges are a potential leading cause of the differences in aerosol NCs between our study and the literature studies compared in Table 2. As reflected in the revised manuscript, we have explicitly added this point with specific details: we clarified that our study focused on particles with a size range of 0.5-1.98 μm, while Flores et al. (2020) included smaller particles (≥0.1 μm), and explicitly linked this size range difference to the observed NC variations. This revision enhances the completeness and rigor of our NC comparison discussion.
The average summed NC observed in this study (54 cm^{-3}, 0.5 μm ≤ Dp ≤ 1.98 μm) was slightly lower than the NC reported for the Western Pacific (83 cm^{-3}, 0.1 μm ≤ Dp ≤ 1.98 μm) by Flores et al. (2020). Notably, this study focused on particles from 0.5 to 1.98 μm, while Flores et al. (2020) included smaller particles (Dp ≥ 0.1 μm). Differences in the measured particle size ranges are a potential leading cause of the differences in marine aerosol NCs.
Although the differences in observation seasons and particle size ranges might influence the average NC observations,

RC1.16: Section 3.1, Line 343: Please revise, "The previous study proposed …" to "Previous work has proposed …"

Response: Thank you for your comment. We have revised this sentence as suggested.

RC1.17: Table 3: I still don't believe it is appropriate to use the word "total" when discussing accumulation + coarse-mode aerosol. I recommend that the authors use "sum" here and elsewhere when the two modes are summed.

Response: Thanks for the reviewer's constructive comments. It is inappropriate to define the aerosol number concentration obtained by integrating the APS data as "total marine aerosols" or even "total aerosols". To avoid misunderstanding, we have used "sum" here and elsewhere when the two modes are summed.

Table 1 (c.f. Table 3 in the manuscript)

Distributions of NCs for different aerosol particle modes in different ocean regions. Mean and SD, respectively, represent the mean values and standard deviations of the related meteorological parameters.

Observation Area	South China Sea			
Route Location	Offshore Region		Pelagic Region	
Marine Aerosol Parameters	Mean	SD	Mean	SD
Accumulation Mode (cm^{-3})	105.57	25.52	47.65	31.63
Coarse Mode (cm^{-3})	2.68	0.38	1.57	0.80
Sum (cm^{-3})	108.25	25.43	49.22	31.97
Accumulation Mode / Sum (%)	97.52	-	96.81	-
Coarse Mode / Sum (%)	2.48	-	3.19	-
Meteorological Parameters				
WS ($m\ s^{-1}$)	10.74	1.95	8.64	3.70
RH (%)	91.20	1.72	82.41	3.40
T_{OBS} (°C)	28.19	0.57	29.18	0.87
SST (°C)	27.71	0.37	29.78	0.33

RC1.18: Section 3.2, Lines 384-385: "These aerosols underwent atmospheric …" The authors should state that the aerosol "likely underwent dry deposition, wet deposition, and aging processes" associated with the "long-range transport." No evidence has been shown that each of these processes definitively happened. This is the same for the subsequent two sentences as well, which are worded a bit definitively without evidence.

Response: Thank you for your thoughtful suggestion. We agree with your suggestion and have revised the manuscript to adopt a more tentative tone. Specifically, we added "likely" to indicate the inferential nature of the processes (dry deposition, wet deposition, aging, and transformation) for which direct evidence was not provided, aligning with the scientific rigor of the discussion.

These aerosols likely underwent dry deposition, wet deposition, and aging processes associated with long-range transport. Such processes could have led to the removal of

continental aerosols or their gradual dilution and mixing with natural aerosols (Hodshire et al., 2019; Ohata et al., 2016; Xu et al., 2021). Over time, the continental and anthropogenic aerosols may have transformed into or been integrated with the background aerosols.

RC1.19: Section 3.3, Line 495: Here and elsewhere, please use "composition" instead of "components."
Response: Thanks very much for the suggestion. We have changed "components" to "composition".

RC1.20: Section 3.3, Line 510 (and elsewhere in this section): The authors have not revised the word "total" to "sum" as discussed in their response. Please make this revision. Figures 7-11 should also not say "total."
Response: Thank you for your constructive suggestion. To avoid misunderstanding, we have revised all such expressions to "summed NC".

RC1.21: Section 3.3.1, Line 570: How do the authors know that the "marine aerosols had a relatively short lifetime"? Where was this discussed or shown?
Response: Thank you for your insightful comment. To address your concern, we have supplemented relevant literature to provide evidence for this statement. The revised manuscript clarifies that the inference that marine aerosols have a relatively short lifetime is based on existing atmospheric science studies, enhancing the rigor of the discussion.
In the pelagic region, the NCs were strongly influenced by the local production and marine aerosols had a relatively short lifetime compared with continental aerosols (Liu et al., 2005; Qureshi et al., 2009).

RC1.22: Section 3.3.1, Lines 570-577: The language in these passages are still more declarative than what can be supported by the measurements. An example of where this can be improved is: "Under the influence of sea surface wind, ocean wave fluctuations and sea surface friction [MAY] increase with intensified wind stress." Word like "can", "may", "might suggest" would improve these passages. Things like "bubble rupture" and "wave fluctuations" were not reported in this study and are speculative based on the assumed relationship to the correlations, therefore should not be discussed definitively.
Response: Thank you for this constructive comment. Your comment has helped us recognize that our previous phrasing was overly definitive. In response, we have revised the relevant passages by using speculative terms (e.g., "may", "might", "could") as suggested. The revised text is provided below:
Under the influence of sea surface wind, ocean wave fluctuations and sea surface friction may increase with intensified wind stresses. Air bubbles generated and present on the sea surface might rupture to form numerous water droplets, which could eventually produce primary marine aerosol after evaporation and crystallization processes (Blanchard et al., 1980; Saliba et al., 2019). Therefore,

increased WS may both intensify bubble rupture by enhancing sea surface friction and promote air-sea gas transfer (Jaeglé et al., 2011; Mårtensson et al., 2003). These processes might elevate the production of marine aerosols and natural marine precursors, ultimately raising the NCs in the pelagic region.

RC1.23: Section 3.3.2, Line 594: The "accumulation mode was likely more sensitive to SST" . Is this sensitivity implied because of the larger slope? Please clarify and state in the main text what is meant.

Response: We thank the reviewer for this insightful comment.

Yes, the statement that the accumulation mode is "more sensitive" to SST is indeed based on the comparison of the regression slopes presented in Fig. S4. In this context, a steeper (more negative) slope for the accumulation mode indicates that its number concentration changes more drastically per unit change in SST compared to the other modes (e.g., the coarse mode). This greater rate of change might be interpreted as higher sensitivity. We have revised the text in Section 3.3.2 to explicitly clarify this point, as shown below.

The steeper negative slope observed for the accumulation mode, compared to the coarse mode, pointed to a greater responsiveness of its NC to changes in SST, suggesting that accumulation mode particles might be more sensitive to these variations.

RC1.24: Section 3.3.2, Line 594-597: "This observed trend was inconsistent…" The authors state that their trend is inconsistent with prior laboratory studies but "consistent with the previous studies." Were these "previous studies" ambient measurements or also laboratory studies but using a different experimental set up? Please clarify and state the difference/similarity in the main text. The same is necessary for the following sentence which states, "a recent study." This description is vague and should state whether laboratory or ambient measurements.

Response: Thank you very much for your valuable comment. We agree with the reviewer that this distinction is crucial for clarity. We have revised the passages to explicitly state the nature (laboratory or field measurements) of all cited studies. The "previous studies" (Salter et al., 2014; Zábori et al., 2012b) and the "recent study" (Christiansen et al., 2019) referenced in the original submission were indeed laboratory studies. To avoid ambiguity, we have now grouped them together as "other laboratory studies" and clarified that their inconsistency with some laboratory findings (Keene et al., 2017; Forestieri et al., 2018) may stem from differences in experimental setups (e.g. plunging jet, water jet, or diffuser systems) or the water types used (e.g. natural, artificial, or synthetic seawater). Furthermore, we have specified that the study by Lehahn et al. (2014) was a field study based on shipborne measurements. These modifications are reflected in the updated manuscript.

This observed negative correlation between SST and NCs was inconsistent with some laboratory studies (Keene et al., 2017; Forestieri et al., 2018) but consistent with other laboratory studies (Christiansen et al., 2019; Salter et al., 2014; Zábori et al., 2012b). These laboratory studies have shown disparate results, which may stem from

differences in experimental setups (e.g. plunging jet, water jet, or diffuser systems) or the water types used (e.g. natural, artificial, or synthetic seawater). A recent field study also reported decreasing NCs with rising SST based on shipborne measurements in the North Atlantic (Lehahn et al., 2014).

RC1.25: Section 3.3.2, Lines 604-613: This passage is still written confusingly and too conclusively. First, I don't understand why "meanwhile" is used at the beginning of the passage. If in relation to the previous sentence/discussion, do the authors mean to use a phrase like "accordingly"? I interpret the structure of this passage as: Near-surface air entrainment volumes and plunging jets were changed (prior sentence). [Accordingly], the bubble rupture changed (following sentence). This should be followed (as a new sentence) with "smaller daughter bubbles" were LIKELY produced BECAUSE larger drops ruptured; then, these smaller drops CAN produce submicron aerosol.

Response: Thank you for your constructive suggestion, which has helped improve the clarity and rigor of our discussion. We sincerely apologize for the confusing structure and inappropriate use of "meanwhile" in the original passage, as well as the overly conclusive tone. Following your insightful guidance:

1. We replaced "meanwhile" with "accordingly" to clarify the causal link between changed near-surface air entrainment/plunging jets and altered bubble rupture.
2. We revised the tone to be more tentative by adding "likely" (as you recommended) to reflect the inferential nature of the relationship.
3. We revised the entire paragraph following your suggestion to address all of your concerns.

The revised passage in the manuscript reads as follows:

Near-surface air entrainment volumes and plunging jets were changed. Accordingly, the bubble rupture changed. Small daughter bubbles (secondary bubbles with smaller diameters, generated at the edges of central bubbles) were likely produced because larger central bubbles (the primary bubbles rising to the sea surface) ruptured; then, these smaller bubbles could produce submicron aerosol. These daughter bubbles are critical for the formation of submicron marine aerosols (Miguet et al., 2021; Sellegri et al., 2023).

RC1.26: Section 3.3.2, Line 608-610: Please revise (in brackets): "The [generation] of daughter bubbles [decreases] with an increasing ratio of seawater density to viscosity and a decreasing ratio of seawater viscosity to surface tension. [Therefore], under increasing SST, the ratio …"

Response: Thank you for your constructive suggestion. The sentences have been carefully modified as suggested.

RC1.27: Section 3.3.2, Line 614: Delete "meanwhile."

Response: Thank you for your meticulous comment. We have deleted "meanwhile".

RC1.28: Section 3.3.2, Line 635: replace "meanwhile, they might" with "they might

also"
Response: Thank you for your suggestion. The sentence has been carefully modified as suggested.

RC1.29: Section 3.3.2, Line 636-638: The sentences in these lines can be combined for clarity, "The difference in the SST-T2m might … during the experiment and should be considered further in subsequent targeted research."
Response: We appreciate the reviewer's suggestion. The sentence has been carefully modified as suggested.

RC1.30: Conclusions, Lines 660-661: The authors should include the size ranges for their defined accumulation and coarse modes here.
Response: Thank you for your meticulous comment. We have added the size ranges for accumulation and coarse modes in the revised manuscript.

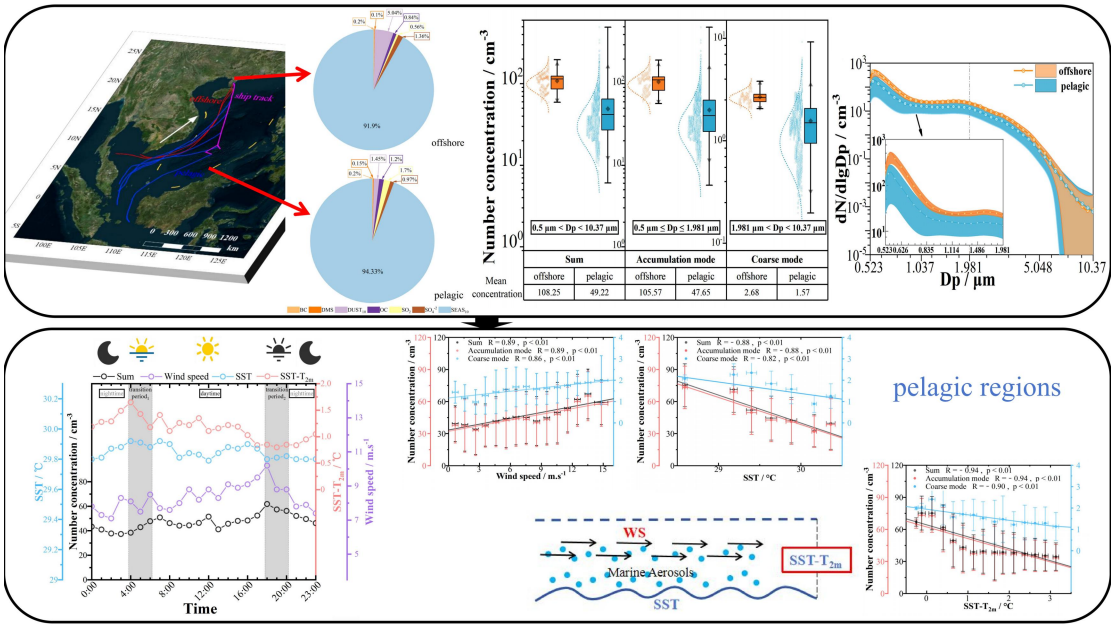
RC1.31: Conclusions, Lines 670, 672 (elsewhere in this section): Please use "aerosol composition" instead of "aerosol components."
Response: Thanks very much for the suggestion. We have changed "aerosol components" to "aerosol composition".

RC1.32: Conclusions, Line 674: " …diminishing continental aerosol [contributions]." Do the authors mean concentrations? Please clarify and revise.
Response: Thank you for your meticulous comment. Indeed, in the context of describing the decreasing trend of aerosol number concentrations (NCs) with distance from the coast, the term "concentrations" is more accurate than "contributions". We have changed "contributions" to "concentrations" as suggested.
Furthermore, NCs exhibited a negative correlation with distance from the coast, and this trend was consistent with diminishing continental aerosol concentrations.

Minor revision-egusphere-2025-1463-referee-report-2

RC2.1. Graphical abstract (a): The authors said the right y-axis shows NC (Same as left y-axis). Ok sounds good. But what about the size distributions shown? Do they have any axis representation? Any diameter? Or is it more graphical only?
Response: We thank the reviewer for raising this point. Indeed, the original box plot in the graphical abstract did not explicitly present the aerosol size distributions. Following the reviewer's suggestion, we have now clearly labeled the specific particle diameter ranges on the box plot. Additionally, in the revised graphical abstract, we have added the average aerosol number size distributions to better illustrate the differences between the offshore and pelagic regions. We believe these improvements make the graphical abstract more effective in conveying key findings of our study in a visually engaging manner.

Graphical Abstract



RC2.2. Can you do a scatter plot between SST-T2m and Nc? And show the correlations?

Response: We thank the reviewer for this suggestion. As requested, we have provided the scatter plot between SST-T_{2m} and summed NCs (Fig. 1) and shown their correlations.

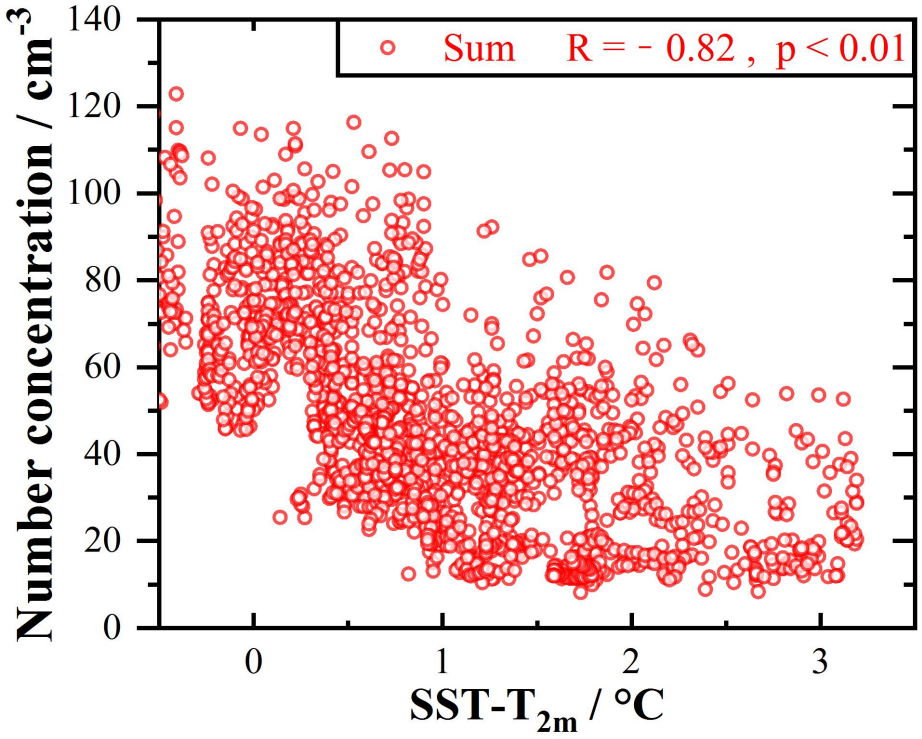


Fig. 1 The scatter plots of summed NCs and SST-T_{2m}.

RC2.3. Whats the uncertainty in measuring SST and T2m? is the difference greater than uncertainty? Try the same with SST-T10m may be?

Response: Thank you for the insightful comment. According to previous studies and product documentation (Bosilovich et al., 2015; Gelaro et al., 2017; Molod et al., 2015), the measurement uncertainty is approximately ±0.2 °C for SST and ±1.0 °C for T2m. The mean difference (1.4 °C) in our analysis is greater than these uncertainties. For SST-T10m, we note that the uncertainty for T10m is not explicitly documented in relevant literature or dataset descriptions. Therefore, we are unable to conduct a reliable uncertainty assessment for T10m and SST-T10m.

Reference:

Bosilovich, M. G., Akella, S., Coy, L., Cullather, R., Draper, C., Gelaro, R., Kovach, R., Liu, Q., Molod, A., Norris, P., Wargan, K., Chao, W., Reichle, R., Takacs, L., Vikhliaev, Y., Bloom, S., Collow, A., Firth, S., Labow, G., Partyka, G., Pawson, S., Reale, O., Schubert, S. D., and Suarez, M.: MERRA-2: Initial Evaluation of the Climate, Technical Report Series on Global Modeling and Data Assimilation, 43, doi:NASA/TM–2015-104606/Vol. 43, 2015.

Gelaro, R., McCarty, W., Suarez, M. J., Todling, R., Molod, A., Takacs, L., Randles, C. A., Darmenov, A., Bosilovich, M. G., Reichle, R., Wargan, K., Coy, L., Cullather, R., Draper, C., Akella, S., Buchard, V., Conaty, A., da Silva, A. M., Gu, W., Kim, G. K., Koster, R., Lucchesi, R., Merkova, D., Nielsen, J. E., Partyka, G., Pawson, S., Putman, W., Rienecker, M., Schubert, S. D., Sienkiewicz, M., and Zhao, B.: The Modern-Era Retrospective Analysis for Research and Applications, Version 2 (MERRA-2), J. Climate, 30, 5419–5454, https://doi.org/10.1175/jcli-d-16-0758.1, 2017.

Molod, A., Takacs, L., Suarez, M., and Bacmeister, J.: Development of the GEOS-5 atmospheric general circulation model: evolution from MERRA to MERRA2, Geosci. Model Dev., 8, 1339–1356, https://doi.org/10.5194/gmd-8-1339-2015, 2015.

RC2.4. If the instrument was at 10m height, then I think in the back-trajectory model, the altitude of 50m should be replaced with 10m. This is because you are measuring PNSDs at 10m height, if you want to make comments about these aerosols, then BJTs has to come down to this altitude.

Response: Thank you for this important correction. We agree with the reviewer's point. The backward trajectories have been recalculated at an altitude of 10 meters above ground level to better represent the air masses at the instrument height. The manuscript text and the corresponding figure (Fig. 2) have been updated accordingly.

The trajectories were calculated at an altitude of 10 m above ground level to match the instrument sampling height

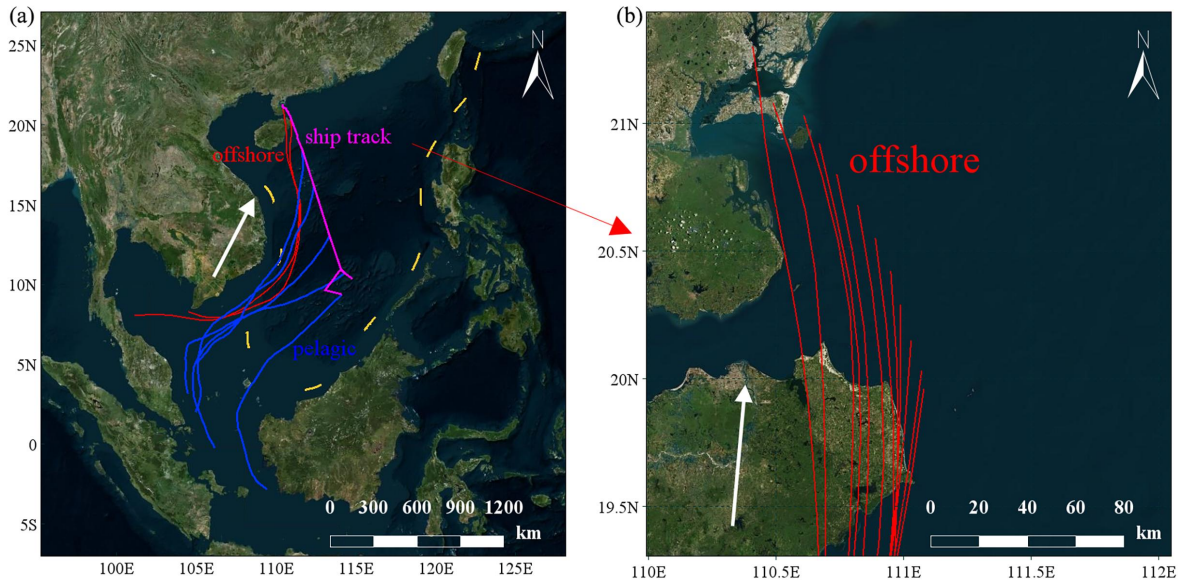


Fig. 2 (c.f. Fig. 6 in the manuscript) (a) The 72-h backward trajectory air mass source traces in the offshore (red solid lines) and pelagic (blue solid lines) regions. The light purple solid lines represented the ship track. (b) Detailed map of the backward trajectory air mass source traces passing through the mainland areas (© Google Earth). The white arrows represented the direction of air mass transport.

RC2.5. Section 3.1 line 229: "for these three aerosol modes" ! well total aerosol, accumulation, coarse aren't modes. I agree that accumulation and coarse are modes but total isn't a mode. So don't use the word modes here

Response: Thank you for your comment. We apologize for the inaccurate terminology. We have revised the sentence structure in Line 229 to avoid referring to the total aerosol as a "mode". The revised passage in the manuscript reads as follows:

During the shipboard observation period, the summed (Dp < 10.37 μm) NC varied from 18.46 to 89.38 cm^{-3}, NC of accumulation mode varied from 17.39 to 87.31 cm^{-3}, and NC of coarse mode varied from 0.83 to 2.49 cm^{-3}. The NCs exhibited substantial temporal fluctuations. The average values for summed NC, accumulation mode NC, and coarse mode NC were 54.01 cm^{-3}, 52.35 cm^{-3}, and 1.66 cm^{-3}, respectively.

RC2.6. Can you show the mode fittings of accumulation and coarse mode? The 1.981 is still not convincing to be classified as accumulation mode. You see peaks at both 0.542 and 1.981um. So these two can be Dg of the modes. IF 1.981 is Dg of the coarse mode then there has to be some range of dia below 1.981 which falls in the coarse mode as well. I would like to see the mode parameters.

Response: Thanks very much for the valuable and constructive comments. As requested, we have supplemented the fitting parameters (Table 2) and specific size distributions (Fig. 3) of the accumulation and coarse modes.

As you mentioned, the measured distribution exhibits distinct peaks around 0.542 μm and 1.981 μm.

However, Fig. 3 shows that the directly observed bimodal peaks are actually formed

by the superposition of three submodes. These three log-normal modes accurately reflect the marine aerosol particle size distribution in the SCS. The specific fitted peak diameters (Dg) and geometric standard deviations (GSD) are listed in Table 2:

Table 2
Summary of the fitting parameters for the accumulation and coarse modes.

Mode	Dg (μm)	GSD
Accumulation Mode 1	0.567	1.076
Accumulation Mode 2	0.440	1.873
Coarse Mode	2.309	1.843

The intersection point between the fitted accumulation mode 2 curve and coarse mode curve is located near 1.78 μm, indicating that the traditional 1 μm boundary is not applicable in our case. The fitted number concentrations provide further justification for our classification: at 1.981 μm, the concentrations for accumulation mode 2 and coarse mode are 3.708 cm^{-3} and 11.22 cm^{-3}, respectively. The contribution of accumulation mode 2 remains non-negligible (> 25%). In contrast, at 2.129 μm, the concentrations of these two modes are 1.898 cm^{-3} and 11.35 cm^{-3}, respectively. Here, the coarse mode is nearly an order of magnitude higher than the accumulation mode 2, indicating its overwhelming dominance.

The classification of particles at 1.981 μm is based on a practical assessment of modal contributions to avoid analytical bias. Although the coarse mode might influence the size distribution below 1.981 μm (Dp ≤ 1.981 μm; dn/dlgDp: 33.65 cm^{-3}), classifying particles from 1.78 to 1.981 μm as part of the accumulation mode (Dp ≤ 1.981 μm; dn/dlgDp: 840.38 cm^{-3}) has a negligible impact, because relative to the total NC of the accumulation mode aerosols with diameters ≤ 1.981 μm, the contribution from coarse mode particles within this size range is negligible. Conversely, classifying particles from 1.78 to 1.981 μm as part of the coarse mode (Dp ≥ 1.981 μm; dn/dlgDp: 53.40 cm^{-3}) could introduce a non-negligible impact, since the contribution from accumulation mode particles (Dp ≥ 1.981 μm; dn/dlgDp: 6.74 cm^{-3}) in this range would be misattributed to the coarse mode.

To better elucidate the distribution characteristics and controlling factors of aerosol number concentrations (NCs) over the South China Sea (SCS), particles with 0.5 μm ≤ Dp ≤ 1.98 μm are attributed to accumulation mode, and particles with Dp ≥ 2.129 μm (i.e. > 1.981 μm) as the coarse mode. It does not compromise the validity of our core conclusions (e.g., the negative correlation between aerosol NCs and the sea-air temperature difference).

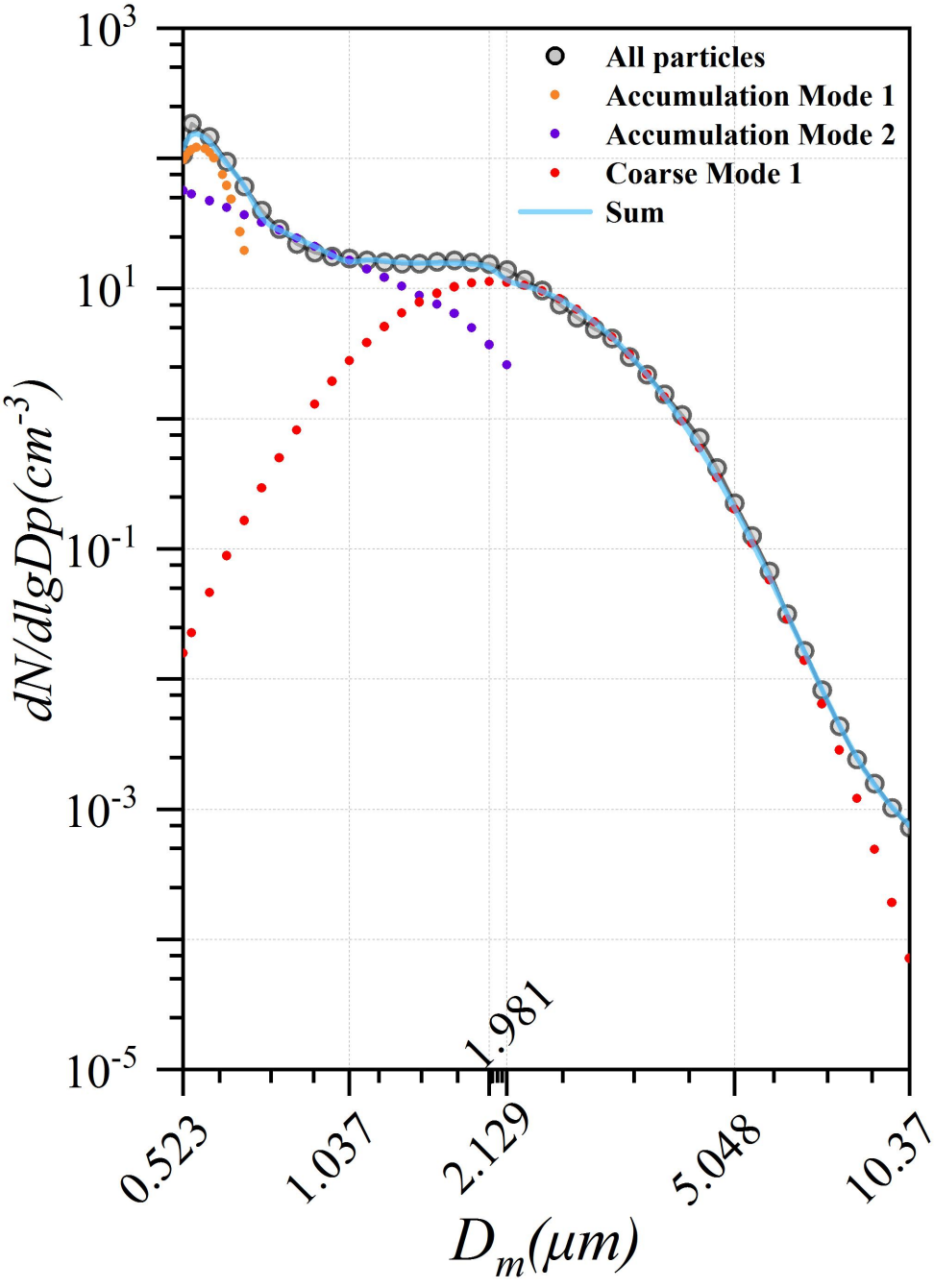


Fig. 3 Number size distribution of marine aerosols with multi-mode log-normal fitting. The measured data are shown as gray circles. The orange, blue, and red curves denote the fitted log-normal distributions for Accumulation Mode 1, Accumulation Mode 2, and Coarse Mode, respectively. The light blue solid line indicates the overall fitted distribution (sum of all modes).

RC2.7. Line 240: "The total NC observed in this study (54 cm-3) was slightly lower than NC in the Western Pacific (83 cm-3) by Flores et al. (2020)" may be add average? Like average NC observed!

Response: Thanks very much for the suggestion. We have added "average" to this

sentence as suggested.
The average summed NC observed in this study (54 cm^{-3}, 0.5 μm ≤ Dp ≤ 1.98 μm) was slightly lower than the NC reported for the Western Pacific (83 cm^{-3}, 0.1 μm ≤ Dp ≤ 1.98 μm) by Flores et al. (2020).

RC2.8. What explains the decrease in dn/dlogdp (Figure 3a) between 5/26 and 5/31?
Response: We thank the reviewer for raising this point. The decrease in dn/dlogdp between 5/26 and 5/31 might be attributed to three concurrent factors. First, the apparent decrease is primarily attributable to the contrast between the exceptionally high values recorded during the heavy rainfall event on 5/26 and the subsequent lower baseline levels. As clearly shown in Figure 3a, the heavy precipitation on 5/26 led to a significant enhancement of dn/dlogdp. Second, ship-borne measurements revealed a substantial decline in aerosol NCs from May 27 to 29 (i.e. 27th: 50.52 cm^{-3}, 28th: 23.04 cm^{-3}, and 29th: 27.14 cm^{-3}), which directly led to a general reduction in dn/dlogdp. Finally, the observed decrease in relative humidity (i.e. 27th: 84.4%, 28th: 79.0%, and 29th: 78.9%) likely weakened aerosol hygroscopic growth, further suppressing the dn/dlogdp values, particularly at larger sizes. The confluence of these factors might explain the decreasing dn/dlogdp values between 5/26 and 5/31.

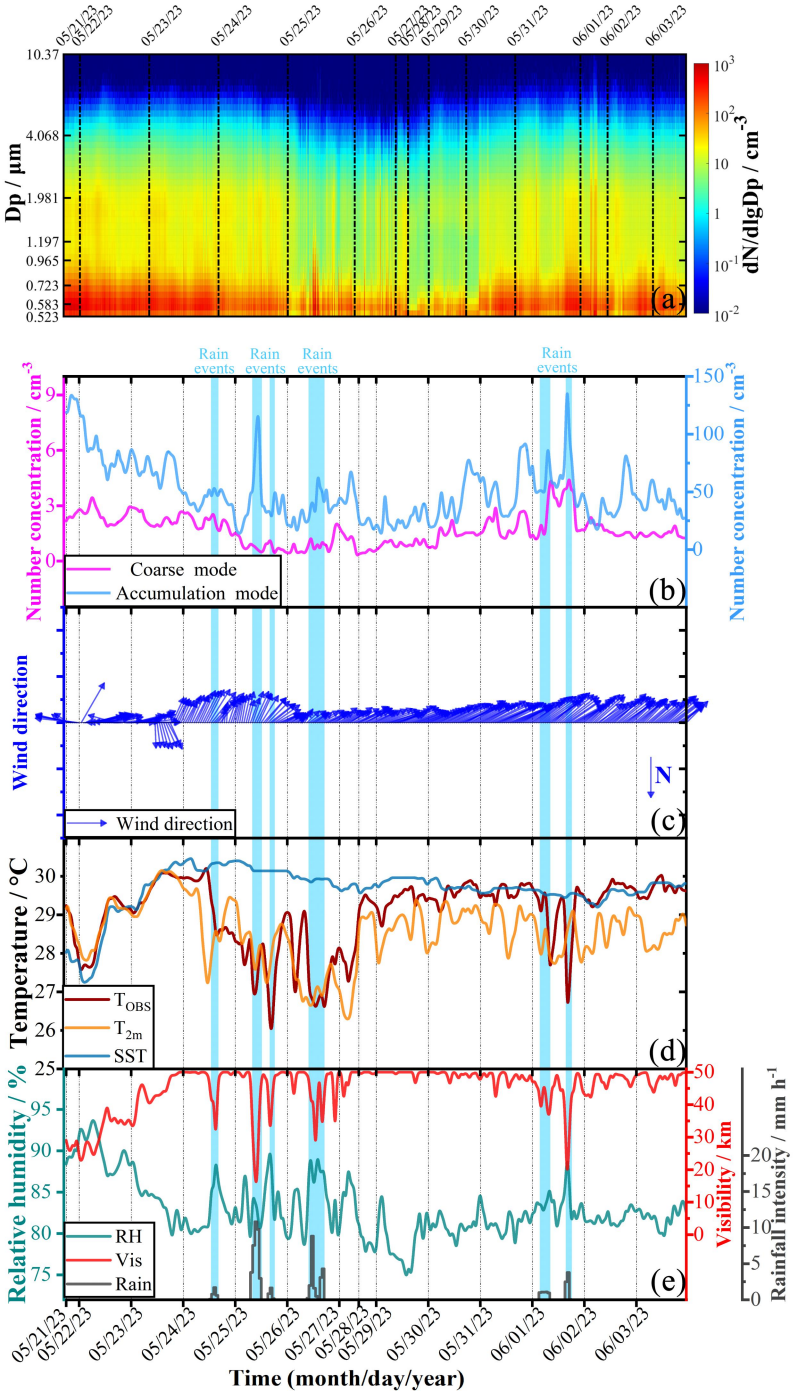


Fig. 4 (c.f. Fig. 3 in the manuscript) The time series of the shipboard observations in the SCS from 21 May to 3 June 2023. The blue-shaded regions represented periods affected by rain events. (a) Trend of the aerosol size distributions. (b) Trends of NCs of the two aerosol particle modes (black solid line represented the NC of the coarse mode, and red solid line represented the NC of the accumulation mode). (c) Trend of the WD. (d) Trends of the TOBS (dark orange solid line), T2m (light orange solid line), and SST (blue solid line). (e) Trends in the RH (gray solid line), the VIS (red solid line), and the rainfall intensity (dark blue solid line).

RC2.9. Line 274: "For example, marine aerosol NCs generated by the bubble bursting process at low WS showed little variation, and the low WS was insufficient to activate spume droplet production." Are you citing literature? Because you didn't 'measure' bubble bursting to talk about it

Response: Thank you for your suggestion. The relevant citations (Pietsch et al., 2018; Russell et al., 2023) have been added to support the statement, as suggested. The manuscript has been revised accordingly.

For example, marine aerosol NCs generated by the bubble bursting process at low WS showed little variation, and the low WS was insufficient to activate spume droplet production (Pietsch et al., 2018; Russell et al., 2023).

Reference:

Pietsch, R. B., Grothe, H., Hanlon, R., Powers, C. W., Jung, S., Ross, S. D., and Schmale Iii, D. G.: Wind-driven spume droplet production and the transport of Pseudomonas syringae from aquatic environments, PeerJ, 6, e5663, https://doi.org/10.7717/peerj.5663, 2018.

Russell, L. M., Moore, R. H., Burrows, S. M., and Quinn, P. K.: Ocean flux of salt, sulfate, and organic components to atmospheric aerosol, Earth-Sci. Rev., 239, 104364, https://doi.org/10.1016/J.EARSCIREV.2023.104364, 2023.

RC2.10. Line 338: ", the number size distributions from offshore and pelagic areas exhibited close agreement, demonstrating consistent correlation patterns that remained robust against instrumentation limitations." Correlation patterns of what exactly?

Response: We thank the reviewer for raising this point regarding the lack of clarity. The "consistent correlation patterns" refer to those of the average number size distributions between offshore and pelagic regions. We agree that this phrase was ambiguous and could be misinterpreted.

Our intended meaning was simply that the average size distributions in offshore and pelagic regions showed a close agreement, and this agreement was robust despite instrumental uncertainties. To address this, we have revised the sentence by removing the ambiguous phrase, as follows:

However, in the 5.0-10 μm particle size range, the number size distributions from offshore and pelagic regions were largely consistent, and this consistency remained robust against instrumentation limitations.

RC2.11. Add your ship track in Figure 6a

Response: We appreciate your constructive suggestions. We have added the ship track in Fig. 6a.

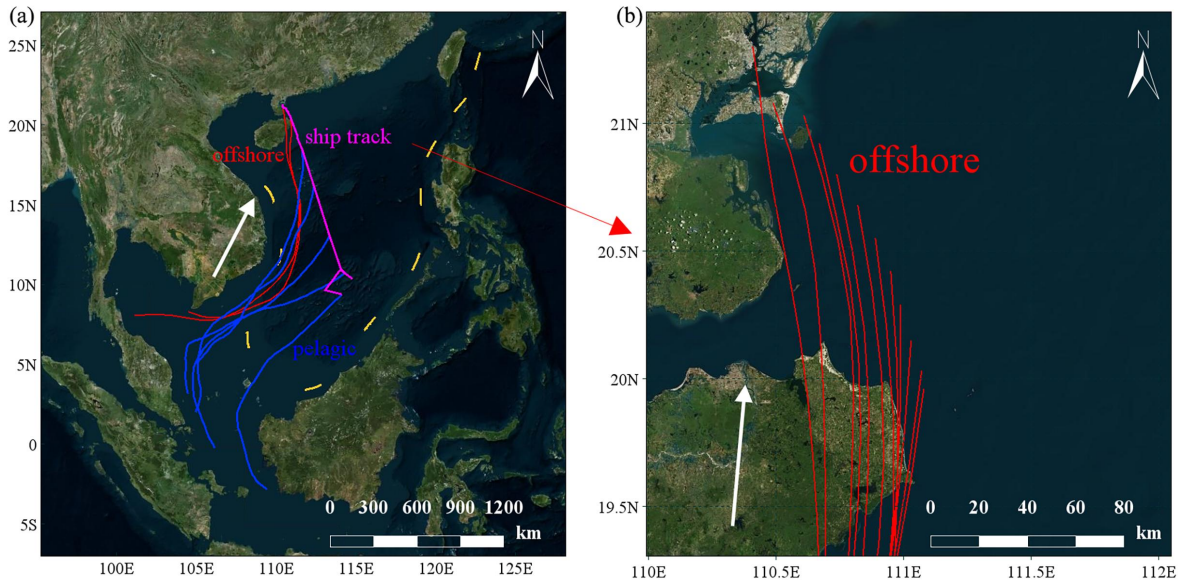


Fig. 5 (c.f. Fig. 6 in the manuscript) (a) The 72-h backward trajectory air mass source traces in the offshore (red solid lines) and pelagic (blue solid lines) regions. The light purple solid lines represented the ship track. (b) Detailed map of the backward trajectory air mass source traces passing through the mainland areas (© Google Earth). The white arrows represented the direction of air mass transport.

RC2.12. Line 368: "cellular metabolism and lysis" whats lysis?

Response: Thank you for your comment. "Lysis" here refers to cellular lysis, i.e., the breakdown of phytoplankton cell structures to release intracellular contents. To improve clarity, we have revised the sentence to explicitly state that it means "cellular lysis".

For instance, phytoplankton releases DMS through cellular metabolism and cellular lysis...

RC2.13. Line 356: ".Higher WS can enhance marine aerosol production." You observed higher WS at offshore (~10m/s) and lower at pelagic (~8m/s). You correctly concluded there's more production of aerosols in offshore regions. But later you say there's more production in pelagic regions because of more sea salt! Contradicting. If you mean more aerosol production in offshore, which type of aerosols are getting produced offshore?

Response: Thanks very much for pointing out this critical contradiction, which has helped refine the precision of our scientific expression. We have revised the erroneous wording in the original manuscript as follows.

The proportion of sea salt aerosol ($SEAS_{10}$; Dp ≤ 10 μm) in the pelagic regions (94.33 %) was higher than that in the offshore regions (91.9%), indicating a significant contribution of $SEAS_{10}$ to the total marine aerosols in the pelagic environments.

Regarding the type of aerosols produced in offshore regions (driven by higher wind speed), $SEAS_{10}$ (Dp ≤ 10 μm) is still the primary wind-generated aerosol. Additionally,

$DUST_{10}$ (Dp ≤ 10 μm) in offshore regions may be generated by wind-driven resuspension of coastal sediments (e.g., sandy shorelines, intertidal deposits).
We apologize again for this misleading wording and have revised the text to clarify the distinction. The revised manuscript has resolved the contradiction in the conclusions, enhanced logical coherence, and improved the overall scientific quality of this study.

RC2.14. Can you make a supplemental figure showing backward trajectory at pelagic locations throughout the year? Just to understand if these regions never receive continental influence.
Response: We thank the reviewer for this suggestion. As requested, we have generated the backward trajectories for the full year of 2023 in the pelagic regions. Fig. 6a presents the full-year backward trajectories at a location about 51 km from the coast (pelagic region), and Fig. 6b shows the corresponding cluster analysis results, including the percentage contribution of each air mass pathway. Fig. 7a shows the full-year backward trajectories at a location about 400 km from the coast (pelagic region), and Fig. 7b shows the corresponding cluster analysis results, including the percentage contribution of each air mass pathway. The results indicate that these regions may still receive continental influence.

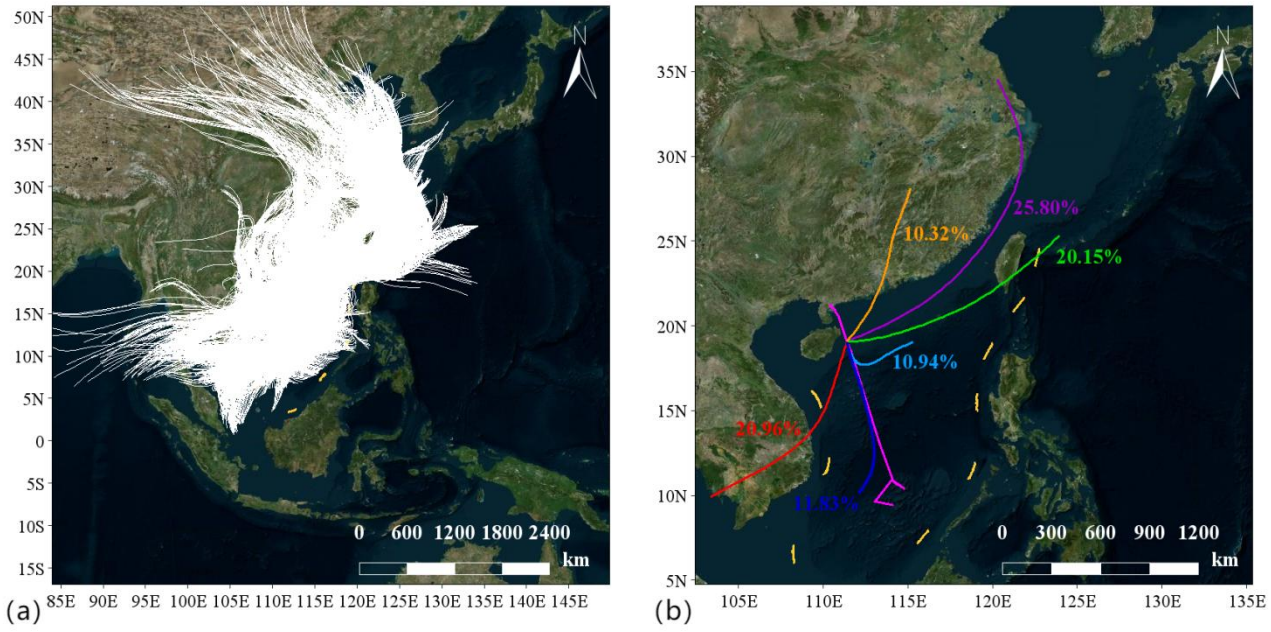


Fig. 6 (a) The 72-h backward trajectory air mass source traces (the white solid lines) for 2023, at a location about 51 km from the coast. (b) Detailed cluster analysis for backward trajectories of air masses (© Google Earth).

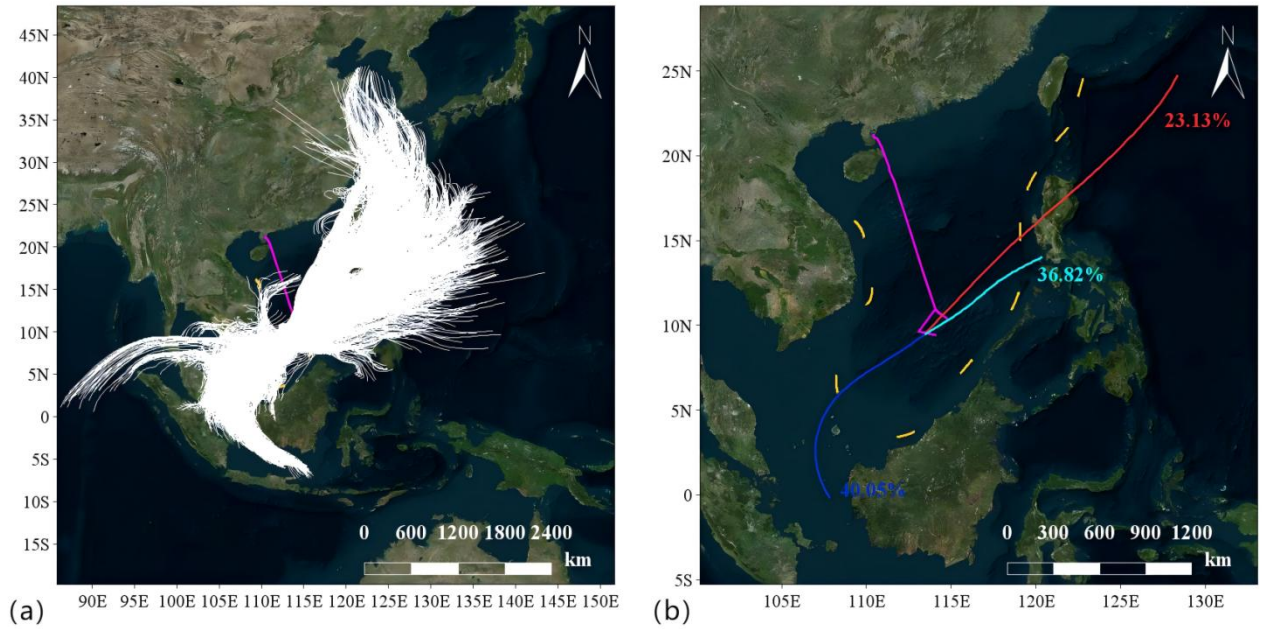


Fig. 7 (a) The 72-h backward trajectory air mass source traces (the white solid lines) for 2023, at a location about 400 km from the coast. (b) Detailed cluster analysis for backward trajectories of air masses (© Google Earth).