

This study presents continuous ship-based observations of marine aerosols and CCN across multiple oceanic regions, providing a comprehensive comparison of aerosol and CCN characteristics using a consistent analytical framework.

I appreciate that this study provides observations and direct comparisons across multiple regions, conducted across diverse sea areas using a consistent framework. This is a clear strength of the manuscript. Overall, the paper is also very well written and grammatically sound. The key conclusions of this manuscript are likely valid; however, I recommend that the authors provide stronger justification and quantitative support to demonstrate the robustness of their results. Several aspects of the analysis currently lack sufficient methodological detail and uncertainty assessment, which makes it difficult to fully evaluate the strength of the conclusions. My specific comments are provided below.

Importantly, the modal fitting presented in the manuscript requires substantial revision. The current fits do not adequately represent the measured size distributions and undermine the interpretation of modal contributions. This issue should be addressed prior to publication.

**Paragraph 1 (Lines 34-48).**

The opening paragraph frames the study in terms of aerosol radiative forcing and its role in the Earth's energy balance, including detailed discussion of IPCC AR6 forcing estimates and comparison to CO<sub>2</sub>. This initially suggests that the manuscript will focus on aerosol radiative impacts. However, the study is instead centered on CCN activity and process-level aerosol-cloud interactions. This creates a disconnect between the introduction and the actual scope of the work. I recommend revising the opening paragraph to better align with the study objectives by reducing the emphasis on radiative forcing and instead focusing more directly on CCN-relevant processes and uncertainties.

**Paragraph 2 (Lines 49-64).**

The introduction provides a limited and somewhat unbalanced overview of marine aerosol sources, and is not well aligned with the scope of the study. For example, while sulfate formation from DMS is discussed, the role of DMS-derived secondary organic aerosol is not considered, and marine organics are only attributed to primary sea spray emissions. More broadly, as no aerosol composition measurements are presented, the source-focused discussion reads more like that of a source apportionment study than one centered on CCN activity and  $\kappa$ . I recommend revising this section to focus more directly on factors controlling hygroscopicity and CCN activation (e.g., particle size, mixing state, and general compositional influences), rather than detailed source attribution that cannot be constrained by the measurements in this work.

**Lines 79-109**

A summary table of the previous studies discussed in the introduction would be helpful. Consider including columns for location,  $N_{\text{CN}}$ ,  $N_{\text{CCN}}$ , and  $N_{\text{CCN}}/N_{\text{CN}}$  ratio to facilitate comparison with the results presented in this study.

**Lines 198-200.**

Please quantify how much data (%) was excluded based on the relative wind direction filtering used to avoid ship exhaust contamination. This will help assess the representativeness of the remaining dataset.

**Line 212-213. “The corrected data were then validated against PM1 data measured by the 213 Aerosol Mass Spectrometer (AMS)”**

I find it problematic that AMS mass is used to “validate” SMPS/APS-derived mass, as the typical approach is the reverse. SMPS measurements are used to constrain AMS mass via collection efficiency and subsequent scaling. As presented, there is insufficient methodological detail to evaluate this comparison. For example, was size-dependent transmission efficiency accounted for? Additionally, how were refractory components treated in the mass comparison, given that the AMS measures only non-refractory species?

**Line 276.**

I initially assumed that the Aitken and accumulation modes were derived from fitted SMPS size distributions; however, this was only clarified upon reading the Figure 5 caption. The size ranges used to define the nucleation, Aitken, and accumulation modes should be explicitly stated in the main text, preferably within the Methods section.

**Line 291.**

Change “were similar to those at the other supersaturations.” to “were similar to  $N_{CCN}$  at the other supersaturations.”

**Line 299-300. “However, the distribution of the geometric mean diameter was larger when exiting the strait.”**

Is this conclusion based on the range of measured values? Please quantify the difference to make this statement clear.

**Line 300-301: “This is because the ratio of Aitken mode to accumulation mode was lower upon exiting the strait.”**

Be explicit about mode number concentrations in text.

**Lines 305-311.**

Please quantify the difference between  $N_{CN}$  and  $N_{CCN}$  during non-precipitating periods over the Indian Ocean. Additionally, indicate what fraction of the dataset (%) is affected. Consider excluding precipitation events in the comparisons to other regions.

**Lines 410-418.**

The values reported for the South China Sea and the Strait of Malacca appear quite comparable. For example, the difference in  $N_{CN}$  (1226 vs. 1446  $cm^{-3}$ ) is likely within measurement uncertainty, and a similar overlap is evident for  $N_{CCN}$  at both supersaturations. With error propagation, the  $N_{CCN}/N_{CN}$  ratios are even more likely to fall within measurement

uncertainty. I recommend including uncertainty estimates in your analysis to assess whether these differences are robust enough to support the conclusions drawn.

### **Section 3.2.2.**

The modal fits shown here are not convincing and do not adequately reproduce the measured size distributions. In particular, the accumulation mode peak height is consistently underestimated (e.g., in the East China Sea, South China Sea, and Strait of Malacca), while the peak width appears overestimated to compensate, resulting in poor representation of the observed distributions. This suggests that the fits are not well constrained and may not reflect a physically meaningful decomposition. Some residual analysis should be presented.

In addition, modal fitting was not described in the Methods section prior to this discussion, making it difficult to assess how these fits were performed or constrained.

Since the SMPS data were merged with APS measurements, the modal fitting should be performed on the merged size distributions to properly capture the full accumulation mode, particularly the coarse-mode shoulder. Fitting only a subset of the size range likely contributes to the systematic underestimation of the accumulation mode peak and misrepresentation of its width.

Finally, these distributions should be presented using a lognormal y-axis scaling (e.g., consistent with  $dN/d\log D_p$  representation) to properly evaluate the quality of the fits and the relative contributions of each mode.

### **Line 409. “dominance of hygroscopic sulfate aerosol”**

The use of “dominance” is unclear in this context. It is not evident whether this refers to sulfate representing the largest mass fraction of the aerosol or to sulfate disproportionately controlling hygroscopicity. Additionally, as this interpretation appears to be based on  $SO_2$  concentrations rather than direct composition measurements, the statement is too definitive. I suggest revising the wording to indicate that sulfate is likely an important contributor, rather than asserting dominance, unless further evidence can be provided.

### **Line 481. “Note that at lower supersaturations, the uncertainties in CCN measurements increase.”**

Please quantify the measurement uncertainties (%) at each supersaturation in a Table (in the supplement should suffice).

### **Line 585-586: “Moreover, the substantial fraction of larger sulfate aerosols in Cluster 2 led to...”**

This statement is too definitive given that no direct compositional measurements are presented. If this interpretation is based on  $SO_2$  or other indirect proxies, the language should be softened. I recommend revising to indicate that sulfate is *likely* an important contributor, rather than asserting a substantial fraction, unless additional supporting evidence is provided.

**Line 634.**

Change “clustering analysis was combined to enable a..” to “clustering analysis enabled...”

**Line 643.** Change “were close to those at higher supersaturations.” to “were comparable to  $N_{CCN}$  at higher supersaturations.”

**Figure 1.**

This can be in the supplement.

**Figure 2.**

Consider labeling the regions along the x-axis, similar to Figure 4, to clearly indicate when the ship is in the East China Sea, South China Sea, Strait of Malacca, and Indian Ocean.

**Figure 5.**

$CN_{15}$  and  $D_g$  should be defined in the main text. Please ensure that all abbreviations are clearly introduced in the text itself, rather than only in the figure captions.

**Figures 8 and 9.**

Please clarify whether the box plots represent interquartile ranges, and explicitly state this in the figure captions.

**Figures 9c and 9d.**

Consider adding more y-axis ticks (e.g., at intervals of 0.2) to improve readability and allow for easier comparison across regions.

**SI Line 110.**

To convert between vacuum diameter and mobility diameter, a typical effective density is between 1-1.6 g/cm<sup>3</sup>. Why was 1.7 g/cm<sup>3</sup> assumed?