**RESPONSE TO REVIEWER COMMENTS**

**Title: Aircraft-derived particle fluxes distinguish entrainment zone and decoupled layer nucleation in marine boundary layers**

**Journal: Atmospheric Chemistry and Physics**

**Ref: egusphere-2026-61**

Referee Comments in 12-point italicized font

Authors' Response in indented, 12-point normal font.

Changes to the manuscript in quotes, 12-point blue font.

## Response to Referee 1's Comments

Specific Comments

**#1.** *Starting an abstract with a size range may not be the most elegant stylistic choice: Aerosol particles in the size range of....*

> **Response:** We respectfully retain the original phrasing, as the size-range definition provides essential context for the SPE terminology introduced in the same sentence. However, we have revised the text to improve its clarity and flow.

**Revised manuscript text: Abstract:** " The vertical distribution of freshly nucleated aerosol particles in the marine boundary layer remains poorly constrained, limiting our ability to represent new particle formation in climate models. Here we characterize 3–10 nm particle events - termed small particle events (SPEs) - by deriving their vertical turbulent fluxes from aircraft measurements during the Aerosol and Cloud Experiments in the Eastern North Atlantic (ACE-ENA) campaign."

**#2.** *Line 168: Please provide numbers instead of vague statements like "…with signal-to-noise characteristics suitable for detecting both remote continental and clean marine aerosol concentrations.." Furthermore, this should be a question of absolute particle numbers due to Poisson statistics – right?*

> **Response:** The reviewer is right to point out that the signal-to-noise ratio should be a question of absolute particle numbers due to Poisson statistics. It is given to be $\sqrt{C}$. A clarifying statement has been added to the revised manuscript

**Revised manuscript text:** "The FIMS provides size distribution measurements at 1-second temporal resolution with signal-to-noise characteristics, given by $\sqrt{C}$ where C is the number of particle counts detected in the corresponding size bin (Kulkarni and Wang, 2006a), suitable for detecting both remote continental and clean marine aerosol concentrations, as demonstrated in aircraft-based deployments (Kulkarni and Wang, 2006a, b; Olfert et al., 2008)"

**#3.** *Line 180: What do you mean with "normal operating conditions"? – Please specifiy! Furthermore, I don't understand this argument concerning possible system malfunctions: the CPC measures in the size range between 10 nm up to about one micron whereas the FIMS is capturing particles between 10 and 600 nm. So I assume the CPC should always measure higher concentration compared to the FIMS – right? So why do you expect a malfunction when the CPC measures about 10% less compared to the FIMS? Sounds a little bit arbitrary…*

> **Response:** "normal operating condition" meant using the instruments on the ground or in a laboratory setting where all the variables are controlled. The reviewer is correct to point out that CPC should always measure a higher concentration than FIMS; that is why if the CPC concentration falls below 10% of the FIMS concentration, the measurement should be flagged and removed. The reviewer is also right to point out that the 10% threshold is an arbitrary value. The manuscript has been revised.

**Revised manuscript text:** "Since both instruments share an overlapping detection size range (10–600 nm for FIMS; >10 nm for CPC 3772), their total number concentrations should be broadly comparable (with CPC showing a higher total number concentration than FIMS) under a laboratory setting where all the variables are controlled. But the CPC concentration was found to be suspect at times, likely because of issues with the working fluid, or a change in flow rate that is controlled using a critical orifice. Hence, an arbitrary yet conservative threshold was set with CPC concentrations falling below 10% of the simultaneously measured FIMS concentration, indicating a physically implausible discrepancy inconsistent with real atmospheric variability, and were therefore excluded from analysis as likely instrument malfunctions."

*I still have some reservations about the fact that Section 2.3.2 contains a discussion that relies entirely on figures in the appendix. In my view, while it makes sense to move lengthy explanations into a separate chapter in the appendix, simply moving the figures there just to keep the manuscript shorter isn't ideal—but that's for the publisher to decide.*

**#4.** *Line 460: maybe a display issue but for me it reads: Where and are fitted parameters. => parameters are missing in the text. Within the following lines it seems there is a "copy-paste" issue or so. The definition of your LoD seems to be duplicated – right?*

> **Response:** The reviewer is right; there seems to be copy-paste issues. We apologize for the oversight and thank the reviewer for pointing it out. The manuscript has been revised, and the duplicate line was removed.

**Revised manuscript text:** "Where ν and k are fitted parameters. The line is fitted till the first zero crossing of the cross covariance. "

**#5.** *The table header line 649 should be separated from the text body —please check whether this issue occurs only in the version I am referring to here*

> **Response:** The table header was separated.

**#6.** *In Fig 3d the parameter names at the y-axis are missing, furthermore, why do you display the LWC on a log-scale? There is no need because the PDF of LWC is not logarithmic.*

> **Response:** The y-axis label was omitted and only the units were shown to save space, the y-axis label is specified in the Figure caption. LWC was displayed on a log scale to accurately display the more than 3 orders of change and simultaneously show measurements that fall below the LWC threshold.

**#7.** *Line 1072ff: I don't understand the reasoning that the strength of the turbulent particle flow is supposed to determine the distance between the aircraft and the source of nucleation—or have I misinterpreted that? Please explain it a little more clearly.*

> **Response:** The reasoning behind the statement was that for two flux events that happened within a span of ~1–2 hours. Change in flux magnitude is likely due to a difference in distance between the flux receptor and the source as the strength of turbulence should remain relatively constant. The manuscript has been revised to clarify the reasoning.

**Revised manuscript text:** "The large difference in flux magnitudes between the two entrainment zone events (−41,092 vs. −2,975 $cm^{-2}$ $s^{-1}$) provides information that flux sign alone cannot supply: it reflects spatial heterogeneity in source strength and the proximity of the aircraft to the nucleation zone during each transect. As turbulent intensity is unlikely to have changed substantially over the ~1–2 hour interval separating the two transects, the order-of-magnitude difference in flux magnitude more plausibly reflects variation in the horizontal distance between the aircraft and the nucleation zone, or spatial heterogeneity in source strength, rather than a change in the turbulent transport efficiency itself."

**#8.** *Line 1080 – 1083: This sentence is difficult to understand and somewhat speculative in parts. Please consider rephrasing it.*

> **Response:** We understand that the sentence does sound complex. We have rephrased the sentence.

**Revised manuscript text:** "Unlike entrainment zone nucleation, which occurs at the boundary layer top, decoupled layer nucleation operates within the interior of the boundary layer at the interface between the well-mixed surface layer and the overlying stratified layer. Both mechanisms share key preconditions: convergence of air masses with contrasting thermodynamic properties, gradients in potential temperature and water vapor mixing ratio, and aerosol dilution that suppresses the condensation sink. However, the decoupled layer mode is distinguished by stratified vertical mixing that confines turbulent exchange to a narrower altitude range. It is also characterized by a substantially larger horizontal extent (>50 km) compared to entrainment zone events (<10 km), suggesting either more persistent favorable conditions or a fundamentally different source mechanism operating at regional scales."

**#9.** *References. I was curious what Etling and Brown, 1993 contributed to this topic but I didn't find an entry in the reference list – please carefully check all references again.*

**Response:** Added the reference to the reference list.