

Peer Review Report After First Revisions

Manuscript title: Frequent new particle formation events in the Indo-Gangetic Plain occur under reduced condensation sink but are obscured by air mass heterogeneity

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General Impressions

Overall, I am very pleased with the revisions made by the authors. They have clearly considered all feedback, provided comprehensive responses, and made significant, thoughtful changes to the manuscript. The lack of clarity regarding data coverage, a concern raised by both initial referees, has been effectively addressed by adding a detailed paragraph in the main text and referencing a data coverage figure in the Supplementary Information. The authors also took great care in refining the manuscript's writing, resulting in a much more readable text.

In short, these changes have significantly enhanced the manuscript's clarity, rigor, and flow. However, there are two minor points I would still like to see addressed before publication:

Minor Comments

- While I commend the authors for clearly stating the NAIS and SMPS data coverage and acknowledging the limited simultaneous measurements, the consequences of these limitations are not yet fully discussed in the text. As Referee 1 astutely noted: “My view is that being upfront about those limitations would make this a stronger paper, not a weaker one. I’d encourage the authors to lean into that rather than away from it.” While the issue of pollution contamination has been well addressed, the implications of the data coverage still need a bit more explicit contextualization. In their rebuttal to Referee 1, the authors state: “Therefore, the CS results are presented as limited concurrent data analysis, not as a seasonal or campaign-wide average.” Similarly, in response to Referee 2, they note about J : “Only three event days satisfied all these requirements, and these are therefore presented as case-study estimates rather than seasonal or campaign-wide averages.” These important framing sentences do not currently appear in the revised manuscript. Integrating these statements (or similar phrasing) directly into the main text would greatly help readers appropriately contextualize this portion of the data analysis.
- I noticed that the statement regarding the statistical significance of the CS reduction on event days was removed in the revision (specifically, the sentence noting a Mann-Whitney U test, $p < 10^{-17}$). I find this regrettable; with the proper framing (i.e., specifying this is calculated based on 24 days between August and November), this is a rigorous metric demonstrating the differences between event and non-event days. However, the previously reported value of $p < 10^{-17}$ raises some eyebrows. For a sample size of 24 days (9 event days and 15 non-event days), there are exactly $\binom{24}{9} = 1,307,504$ possible permutations. Even in the case of perfect rank separation between events and non-events, the absolute minimum possible p-value is approximately $p \approx 7.6 \times 10^{-7}$. An extremely low p-value on the order of 10^{-17} makes me wonder if the test might have been run on the 5-minute windows rather than the 24 independent daily medians. If so, this would skew the test by violating the assumption of independence, as neighboring 5-minute windows are highly correlated. I invite the authors to clarify how this statistic was calculated and encourage them to reinstate the significance metric, provided the test is executed on the independent daily medians.