

I appreciate the authors' substantial efforts in revising the manuscript and addressing my previous concerns. The revised version is clearly improved. In particular, the use of a community-standard Nd product, the correction of the ACI Nd definition, the additional discussion of aerosol-proxy uncertainties, and the more cautious interpretation of the monsoon framework have strengthened the manuscript.

I am generally satisfied with the revision and recommend minor revision before acceptance. However, I still have a few remaining comments that should be addressed to further clarify the interpretation and limitations of the study.

1. The authors now clarify that the monsoon classification represents a coupled large-scale environmental background rather than an attempt to isolate individual causal factors. This clarification is useful. However, the manuscript should state more explicitly that the monsoon-period differences in ACI cannot be uniquely attributed to the monsoon regime itself, because LTS, humidity, cloud regime, aerosol loading, and aerosol type covary systematically. I suggest that the authors further soften causal language throughout the manuscript and consistently frame the monsoon classification as an organizing framework rather than an independent physical driver.

2. The concern regarding AI as a CCN proxy has been partly addressed. Nevertheless, in a marine region such as the South China Sea, the possible role of coarse-mode sea salt and giant CCN remains important. If a full aerosol-type separation is beyond the scope of this paper, the authors should at least add a clearer limitation statement in the Results/Discussion and Conclusions, emphasizing that AI mainly represents fine-mode aerosol variability and may not fully capture sea-salt-related CCN effects. If possible, a simple sensitivity test using AE or MERRA-2 aerosol components would be helpful, but I do not consider it essential for acceptance.

3. The treatment of precipitation has improved, but the possibility of undetected light drizzle at high LWP should be more clearly acknowledged. The authors should clarify whether the main ACI conclusions are sensitive to the selected LWP range. A brief sensitivity check using a lower LWP upper bound, for example 50–150 or 50–180 g m<sup>-2</sup>, would further support the robustness of the conclusions. If this analysis is not added, the limitation associated with hidden precipitation should be stated more explicitly.

4. The revised manuscript still contains some strong interpretive statements, for example implying that stronger LTS enhances ACI through aerosol accumulation and enhanced CCN activation. This mechanism is plausible, but the present satellite/reanalysis analysis cannot fully prove it. I recommend revising such statements to more cautious language, such as “may contribute to,” “is consistent with,” or “suggests a possible role of.”

Overall, the manuscript has been substantially improved and most of my major concerns have been addressed at least qualitatively. The remaining issues mainly concern clarity, interpretation, and appropriate qualification of the conclusions.