Review of egusphere-2025-5935 – 2nd round

I thank the authors for the substantial effort invested in this revision. The majority of my concerns have been considered by the authors, and the new analysis built upon a more confident dataset brings useful food for thought. The manuscript now reads with clearer logic and improved self-consistency. A small number of remaining items, which I classify as minor, warrant attention before potential publication. None requires new analysis, most concern internal consistency and the calibration of interpretive language.

The following line numbers are based on the tracked-change version.

Lines 103–104. The study period stated in the Introduction, "July 2002 to February 2023," is inconsistent with Table 1 and the captions of Figs. 3, 4, 5, and 6, which all read "Jul 2002–Feb 2020." Please correct the Introduction to match the operative end date (Feb 2020) used throughout the figures and Table 1.

Lines 174–184, 858–861. The adopted Nd product (Nd_G18_37) is an adiabatic retrieval subject to quality control (single-layer, sub-pixel homogeneity, optically thin screening) that is most reliable for stratocumulus-like overcast columns and less reliable for the deeper trade cumulus and congestus that the revised analysis now headlines (CTP 650–800 hPa). The adiabatic assumption is most strongly violated in this regime, and the pixel-level screening will preferentially discard broken cumulus scenes, potentially biasing the retained 650–800 hPa sample toward the most overcast, near-adiabatic cases. The current acknowledgment is limited to the fixed sub-adiabatic factor. Please add an explicit caveat in the text that the Nd retrieval validity, and the representativeness of the retained sample, is weakest in precisely the deeper-cumulus regime that carries the central result.

Lines 19–20, 582–583, 820–821. The abstract and conclusions assert a "progressive increase from SWMW to NEMW and NEMD," whereas the body (lines 582–583) correctly concedes that for ACI_r "no consistent ordering is observed between the NEMW and SWMW periods." The reported ACI_{Nd} values reinforce this: SWMW (0.250 ± 0.027) and NEMW (0.286 ± 0.016) overlap within their confidence intervals, so the statistically defensible statement is that NEMD is distinctly highest while SWMW and NEMW are not cleanly separated. Please bring the abstract and conclusions into line with the body. I would also note that the 95% intervals derive from a Student's t test on daily 1° regressions with strong spatial and temporal autocorrelation, so the effective degrees of freedom, and hence the true intervals, are likely overstated for the adjacent pair.

Lines 587–593. The argument that the agreement between ACI_r and ACI_{Nd} demonstrates the signal is "governed by systematic changes in the underlying meteorological environment rather than by the choice of ACI metric" overstates an agreement that is largely mechanical. For the adiabatic Nd, $Nd \propto \tau^{(1/2)} \cdot r_e^{(-5/2)}$, so $d \ln Nd / d \ln AI = (1/2)(d \ln \tau / d \ln AI) + (5/2) \cdot ACI_r$, and the two metrics share the same r_e response by construction, diverging only through the weak τ–AI term. The reported deeper-cumulus values (ACI_r of roughly 0.10–0.14 against ACI_{Nd} of roughly 0.37–0.43) are consistent with this built-in scaling. The two metrics are therefore not independent, and their agreement is a regression consistency check rather than corroboration of the physical signal. Please adjust the tone of this discussion accordingly.

Lines 620–624, 627–628. The claim that "all RH bins consistently exhibit the same enhancement pattern" holds only for the two moister bins. In the dry bin (RH < 45 %), SWMW (-0.126 ± 0.291) and NEMW (-0.055 ± 0.194) are both statistically indistinguishable from zero and from each other if considering the error, and only NEMD (0.127 ± 0.102) is marginally positive, restricting the inference to the RH 45–80 % and 80–100 % bins. Please also specify which relative humidity product is used for the binning, e.g., if it corresponds to the aerosol-layer ambient RH that governs the MERRA-2 swelling effect rather than surface or cloud-top RH. A clarification in the text is sufficient; no further experiment is required.

Lines 757–763. The proposed LTS mechanism, that higher stability promotes "aerosol accumulation and coagulation, leading to an increase in aerosol particle size" and thereby more efficient CCN and stronger ACI_{Nd}, is weakly supported and arguably works against itself, since coagulation reduces number while increasing size, and elevated aerosol loading does not by itself raise the susceptibility slope, which can instead shift toward the updraft-limited regime. The moisture-based mechanism in Section 3.5.1 is more coherent. I suggest softening this passage.

Lines 631. "LST" should be "LTS"?

Line 473. "CER is mostly smaller 15μm" should be "smaller than 15 μm."