The authors presented a revised version of their manuscript. The many changes improve the flow and quality of the presented research. There are two major and few minor concerns. Once resolved, I recommend the manuscript for publication.

We thank the reviewer for the comments. Specific point-by-point responses to each comment are contained below, with the reviewers' comments provided in **blue** and our responses in **black**. Changes to the manuscript made in response to the reviewer are provided in ***red italics***.

We have also made unsolicited wording changes to the manuscript for readability, as captured in a tracked changed document.

Major concerns

Effect of entrainment on LWP and Nd – the authors now briefly mention Nd reduction through entrainment of cleaner free-tropospheric air (ll. 245-247), which I'm glad to see. However, I was also expecting to see updates to Fig. 5a, particularly the direction of the arrow for 5.1, to reflect this process fingerprint, which is typically expected over the ACTIVATE domain. Would considering dual entrainment effects alter the interpretation?

We have updated Fig. 5a accordingly in the revised manuscript. We now use a single arrow to indicate the general effects of entrainment mixing, which can reduce both cloud liquid water and droplet number concentration. This change also avoids invoking the distinction between homogeneous and inhomogeneous mixing, which is increasingly being recognized as a less useful way to describe the mixing process.

In caption of Fig. 5, we now state *"A diagram identifying how individual processes drive the system in LWP-N_d space (1. activation-growth, 2. condensational-growth, 3. collision-coalescence, 4. precipitation, 5. entrainment (mixing and/or dilution)."*

Ice water path uncertainty – The authors now show inter-trajectory variability in Fig. S3 (which I'm glad to see, too), but I wonder if the retrieval uncertainty even permits robust conclusions. While the authors argue that the slopes (rather than absolute values) are important, I'm worried that retrieval assumptions introduce systematic biases with fetch. For example, looking at Chellappan et al. (2024), the case 20200301 (classified as sudden LWP loss increasing IWP) shows evidence of snow upwind of rimed graupel – could such variations in ice crystal morphology affect IWP retrievals? Maybe it would also be good to explain the IWP retrieval assumptions in a bit more detail.

We appreciate the comment. We fully agree with the reviewer that there are uncertainties in the IWP retrievals, for which we have added details about the assumptions made in the retrievals. We believe that by examining 1°-mean (domain-mean) cloud evolutions, rather than detailed spatial structures of the cloud field, the satellite retrievals can still provide useful information on characteristic evolution of the cloud system, which is supported by independent, in-situ observations from ACTIVATE flights (Fig. 7). We have now added details on IWP retrieval assumptions and mentioned caveats on the interpretation of the results in the revised manuscript.

“L126: For IWP retrievals, a cloud reflectance and emittance model based on adding-doubling radiative transfer is used to compute reflectance lookup tables for ice crystals with effective diameters varying from 6 to 135 μm \citep{Minnis98,Minnis11,Minnis21}. This model assumes randomly distributed hexagonal ice columns with roughened surfaces having the normalized roughness parameter set equal to 1.0 and asymmetry factors g between 0.77 and 0.81 at 0.65 μm. As a result, the satellite-retrieved IWP is not able to resolve different morphologies of frozen hydrometeors.”

“L326: In addition, uncertainties in IWP retrievals arising from the assumed ice crystal shapes and related radiative transfer parameters prevent us from resolving small-scale structures and variations in frozen hydrometeor morphology, which may also exhibit distinct signatures across these MCAO events \citep[e.g.,][]{Chellappan24}. In-situ aircraft measurements are clearly the best tool for that. Nevertheless, we do maintain that the domain-mean perspective (i.e., the 1$^\text{o}$ areal mean) still allows us to extract distinct characteristics of system evolution from the satellite view. We do concede, however, that our interpretations are contingent on the realism of the IWP retrievals.”

Minor concerns

ll. 81-83 The authors now explain that cloud water path is retrieved. Would it be useful to clarify this at the beginning of Section 3 as well and even relabel to CWP throughout?

Good suggestion. Done! “L233: We note that, in this study, LWP denotes cloud liquid water path.”

ll. 200-201 The authors could note that 11 January 2022 also exhibits increasing subsidence over time, which would be expected to extend the overcast cloud deck.

Done! “L203: Rather persistent patterns of cloud evolution throughout sunlit hours are observed, except for the 11 January 2022 case where an extended overcast cloud deck is evident later in the day (Fig. 3), consistent with the increasing subsidence over time (Fig. 2).”

Fig. 6 What explains the presence of frozen hydrometeors (i.e., IWP > 0 g m^-2) for cloud-top temperatures well above 0 °C?

Because our analysis is based on domain (1°x1°) averages, and because the cloud field during the broken, open-cellular stage of MCAO tends to exhibit substantial variability in cloud top height and temperature (e.g., Chellappan et al., 2024), a domain with a mean CTT > 0 °C may still contain frozen hydrometeors.