Review of "*Warm-phase Microphysical Evolution in Large Eddy Simulations of Tropical Cumulus Congestus: Constraining Drop Size Distribution Evolution using Polarimetery Retrievals and a Thermal-Based Framework*" by Stanford et al. 2024.

The article presents cloud top microphysical properties (droplet effective radius and number concentration) from polarimetry retrievals from the 25 Sep 2019 CAMP2Ex case (cumulus congestus cloud field) and their comparison with in-situ and LES with bulk and bin microphysics schemes. A LES simulation setup was developed based on observed aerosol properties and thermodynamics and dynamics forcing based on NU-WRF mesoscale simulations. The study shows a good agreement between the retrieved droplet number concentration profile and the LES results (with both bulk and bin schemes). However, the effective radius profiles differ quite a bit. The authors attributed it to a narrower cloud droplet distribution mode in simulations compared to observations. They also discussed the importance of droplet collision-coalescence and vertical variability of aerosol distribution in shaping the droplet number concentration profile.

In my opinion, this article presents useful information on the comparison between polarimetry retrievals and LES and the limitations of the bulk and bin schemes in simulating the cumulus congestus cases. The article is written clearly and would be a good contribution to ACP. However, the authors need to address the following points before considering the manuscript for publication:

- "Benchmark" (L2, L593, L669, and several other places): The authors refer to their LES as a "benchmark" LES case. Considering the difficulty in correctly simulating effective radius and overall DSDs, I'm not sure if the current simulations are truly a "benchmark" LES. It would be misleading.
- L80: By "*which implicitly includes nucleation and condensational growth",* are you referring to aerosol nucleation and growth? But that's a transient phenomenon, and you used a constant vertical profile. So, I'm not sure if that's "implicitly" accounted when you used a constant profile. What about aerosol scavenging and processing?
- L94-L100: There's also a precipitating congestus cloud intercomparison case (based on CAMP2Ex) as a part of the International Cloud Modeling Workshop 2024.
- L130-L132: Can the hygroscopicity parameter be separately determined for the three modes, instead of using a mean value? I think it's a big simplification. This assumption might be critical since the droplet number concentration is predicted and compared here.
- L132-L136: That means the (NH4)2 SO4 and organics are assumed to be internally mixed. Do you think that's a reasonable assumption based on the observation?
- Fig 1a: Please also add the total concentration profile for reference.
- Eq. 4: Shouldn't an approximate sign not "=" be here since it neglects the skewness of DSDs?

- L191: "*subsequent study"* Missing references?

- L252: Here, the authors mentioned that "*aerosol core mass in liquid drops"* is tracked. How did they do it without using a two-dimensional bin approach? Please explicitly state the assumptions made in solute mass growth through coalescence and reproduction of processed aerosols. The processing and regeneration of large aerosols could be important for a long simulation (12 hours in the current case).

- L270: "*Aerosol activation follows from Ackerman et al. (1995)."* Please explicitly state the assumption with the activation scheme. Since you can't track the hydrated mass growth, do you use the equilibrium assumption (neglects the kinetic limitations)? How did you determine the size of the activated droplet (based on the critical radius of activation or just added to the first cloud droplet bin)? Assumptions related to the treatment of the aerosol activation process and wet growth could be critical for subsequent droplet growth.

- L265: This is the opposite of what's expected due to the numerical broadening discussed in Morrison 2018, Chandrakar 2022, and others. Chandrakar et al. 2022 showed significantly broader DSDs that cause early and more intense rain with a doble-moment bin scheme than a reference run with a Lagrangian microphysics scheme in LES of a cumulus congestus case. Does that mean the current simulations setup or the bin scheme has some issues, compensating errors, or some key elements are missing in the setup?

- L274: Did you use the turbulence enhancement table or directly the theoretical Kernel involving radial distribution function and relative velocity parameterizations (along with collision efficiency enhancement) as a function of Stokes number, settling number, radius ratio, dissipation rate, etc. (including collision efficiency enhancement by turbulence)?

- L321: *"cloud top while in situ measurements are ideally in and near the cloud core."* - This can't be the reason for the significant difference seen here. Typically, cloud core contains a large number concentration due to less dilution, and cloud top values may not be expected to be that higher (~1.5 times).

- L366-368: Please discuss in detail how the activation is treated in the bin and bulk schemes (maybe in Section 3.2). Please also see my earlier related comments.

- L387: It seems like even for the cases where droplet number concentration is lower (e.g., KK, 2X_AC) the effective radius is lower than RSP and in-situ observations. It potentially indicates some numerical issues, for example, spurious evaporation and secondary activation from numerical diffusion in physical space in Eulerian microphysics schemes (see Chandrakar et al. 2022).

- L426: Maybe "analyzed" instead of "evaluated"?

- Fig 7: Please use a lighter color for simulation median lines (it is hard to distinguish it from RSP). Also, I recommend adding a median line for in-situ data using some finite vertical binning.

- Fig. 8: Please also add a median line for the in-situ data.

- L444-445: To me the cloud mode appeared to be smaller for CNTL compared to obs at and above 17.45 C.

- L446-448: A recent study by Chandrakar et al. 2024 shows a significantly improved representation of the drizzle range embryo drops and an overall great match with in-situ CAMP2Ex observations when a turbulent collision kernel is used in a Lagrangian scheme in LES of one of the CAMP2Ex case. It also significantly accelerates the rain development.

- L453: "*To further constrain the dynamical conditions."* - Did you also use the same LWC threshold (0.1 g/m3) for obtaining average DSDs from simulations?

- L461-463 and L659-L661: Do the simulations have "a slightly narrow cloud mode" or a shifted mode towards smaller sizes? Can you quantify the difference in spectral width by comparing D_std? A smaller cloud mode might also be causing a smaller r_eff here.

- Fig. 11: Why does the integration of in-situ data start at ~3.5 um? It would be better if you use a consistent minimum size threshold.

- L481-483: Could it be from spurious evaporation and associated secondary activation from numerical issues in Eulerian schemes?

- L655: I do not completely agree with the statement that the cloud mode is captured correctly (also pointed out in my earlier comment). I can see a significant deviation in the cloud mode, especially at 17.45 C and all colder levels above.

References:

Morrison, H., Witte, M., Bryan, G. H., Harrington, J. Y., & Lebo, Z. J. (2018). Broadening of modeled cloud droplet spectra using bin microphysics in an Eulerian spatial domain. *Journal of the Atmospheric Sciences*, *75*(11), 4005-4030.

Chandrakar, K. K., Morrison, H., Grabowski, W. W., & Bryan, G. H. (2022). Comparison of Lagrangian superdroplet and Eulerian double-moment spectral microphysics schemes in large-eddy simulations of an isolated cumulus congestus cloud. *Journal of the Atmospheric Sciences*, *79*(7), 1887-1910.

Chandrakar, K. K., Morrison, H., Grabowski, W. W., & Lawson, R. P. (2024). Are turbulence effects on droplet collision–coalescence a key to understanding observed rain formation in clouds?. Proceedings of the National Academy of Sciences, 121(27), e2319664121.