Review report of manuscript “Aerosol-stratocumulus interactions: Towards a better process understanding using closures between observations and large eddy simulations” by Calderon et al.,

General comment

The authors conducted a closure study to evaluate the UCLALES-SALSA with in situ station measurements during Puijo 2020 campaign. The authors show two cases with different aerosol loadings and meteorology (diurnal v.s. nocturnal) with the boundary layer profiles, cloud macrophysics, and microphysics properties. Sensitivity studies for the second case show that ice processes lead to better agreement between simulated radar velocity and the observed one. The authors highlighted the importance of more observations involving ice particles to reduce the gap in knowledge about the ice-nucleating ability of aerosol particles of both, natural and anthropogenic origin. Overall, I think this is a great manuscript in the closure study of aerosol-cloud interactions. I suggest publishing after the authors address both my key and minor comments below.

Key comments:

1. I think section 3.3.4 is a very important part of this manuscript, which provides information on the closure of the cloud base velocity. Comparing Figure 11b, Figure 15f, and Figure 16f leads to the conclusion that both reducing the total aerosol loadings and turning on the ice processes leads to better agreements between the model and observations, but the hypothesis cannot be tested due to the lack of observations of ice/mixed phase hydrometeor. Please correct me if my understanding is not what the authors would like to convey. Please confirm Figure 11b is used to compare with Figure 15f and Figure 16f. If I am right, my question is whether the role of ice phase particles is only significant with low aerosol loading? What will the result look like if not reducing the aerosol loading? Also in the text, I don’t find any reason to explain the choice of how much reduction in aerosol loading. How do you come up with the number 40% in line 582? Is there any support for that from either the observation side or previous studies?
2. In lines 146-149, the authors said, “the effect of local topography on observed cloud properties is limited to certain high wind conditions”. What is the definition of a high wind condition? Do the two selected cases associate with high wind or low wind? The second case has a lower boundary horizontal wind than the first case. What are the roles of the topography in the clouds examined in these two cases?
3. Figure 1, case 1 is initialized with the boundary layer temperature close to measurements, but the initial temperature above 200m is lower than the observation. Why not use the observed profile to initialize the model?
4. In both cases, simulated activation efficiency has a steeper slope than the DMPS observed. The authors stated in lines 452-456, that it is due to the aerosol mixing state. Can you elaborate on that? My understanding is external mixing leads to a higher

insoluble fraction of aerosols so that the activation of particles is suppressed. But why large particles are suppressed more than smaller particles? Is external mixing on large particles expected more frequent than on smaller particles? I think a more detailed explanation is needed and literature is needed to explain this, although there is no observation to provide the mixing state of aerosols.

5. In Figure 8, the Rotating holographic imager shows a higher concentration on the large particle, and the fog monitor shows a smaller concentration between about 6-25 microns. These differences are not changed at both 3hrs and 5hrs. As a closure study, what is the reason for the differences? Model uncertainties or observation uncertainties?

Minor comments:

1. Figure 4 and Figure 10, change the color of the dark blue bar, otherwise, it is hard to tell the differences between UCLALES-SALSA updrafts with Halo Doppler lidar. Figure 5 and Figure 11 are good examples.
2. Line 436, change mum to μm.
3. Line 472, correct the format of the citation.