Major Comments

The manuscript titled "*Organized Variations in MBL Cloud Microphysical Properties Observed by Aircraft and Satellite and Simulated by Model*" discusses microphysical properties of drizzling and non-drizzling marine boundary layer (MBL) clouds observed during ACE-ENA airborne campaign in the Eastern North Atlantic. The authors argue distinct cloud properties are caused by varying updraft velocities rather than distinct MBL aerosol signatures below each cloud regime. Lower aerosol concentration observed below drizzling cloud is instead linked to precipitation scavenging.

Linking below-cloud aerosol observations to precipitation scavenging and thereby promoting limitations to aerosol indirect effects (AIE) is a hot topic and certainly a valuable contribution to the field but cannot be the only criterion for publication. A prepared manuscript should also meet accepted scientific publication standards.

A manuscript should contain a meaningful introduction highlighting previous research conducted in the field as well as a brief explanation of concepts and terms discussed in the manuscript.

Specifically, this manuscript completely lacks an informative summary of AIE publications related to MBL clouds. Simply listing various cloud properties and broadly concluding AIEs alter these properties is not enough. Which AIE are you referring to? How does it change the listed cloud property? Why is this relevant to your manuscript? For instance, liquid water path (LWP) is mentioned in this list but nowhere else in the manuscript is this variable discussed nor are LWP observations shown.

The authors introduce the term mesoscale convective cells (MCCs) in the abstract but merely one sentence mentions it in the introduction and this sentence does not even describe it. What is it? How does it relate to your research? How do cloud properties change in MCCs? Perhaps coupled/decoupled MBL could be introduced and discussed in relation to cloud properties?

The use of a WRF model is indicated in the abstract but no mention of it can be found in the introduction. Summarize WRF setup for MBL clouds and discuss previous use of the model to understand Stratocumulus (Sc) clouds.

A prepared manuscript **should also contain readable figures** that clearly highlight scientific findings and support information.

Specifically, different data sets should be labeled as such in each figure. It is not sufficient to declare blue and red colored data sets as "along-wind" and "cross-wind" in a couple of figures and have the reader guess for the rest of the manuscript whether this is still the case in the current figure. Color use in Figure 5 shows such an approach is flawed. The use of a legend has emerged as helpful tool.

Figures 10 and 11 are a curiosity. Why would the authors not split this plot into subplots so the y-axis is readable? This is absolutely not an acceptable way to present the most important data set in the manuscript!

Figure 8 redefines the variable r_c, used in the manuscript to describe droplet radius (via CDP data) up to this point, as droplet effective radius. These are two completely different cloud properties. This error alone makes the manuscript hard to read.

The latitude-longitude plots of cloud optical depth (COD) should be a proper map projection.

Most importantly, **the scientific discussion should be comprehensible, supported by data and concepts should be explained correctly.**

Specifically, it appears the authors use “MBL clouds” as synonym for Sc clouds in the introduction which is not appropriate. The incorrect use leads the reader to believe that all MBL clouds are driven by cloud top longwave radiative cooling. Cumulus clouds are MBL clouds and cloud top radiative cooling is not their main driver.

Figure 3 does not show cloud bands. A quick look at MODIS data from that day appears to show a closed Sc deck (https://worldview.earthdata.nasa.gov/). Perhaps, visible satellite imagery could be provided alongside?

The selected data separation into “along-wind” and “cross-wind” is misleading. Why choose these labels just so you have to remind the reader every other page that those are arbitrarily selected terms and that there is no link between wind or flight direction and observed cloud properties. Labels like “MCC” region vs. open cells/non-drizzling regime could be thought of. Additionally, Figure 10 shows quiet some variation in PCASP number concentration within the defined “along-wind” and “cross-wind” sections. Why not sort the data based on drizzle rate, drizzle concentration or PCASP number concentration instead?

Declaring the drizzling cloud or “along-wind” portion of the flight track as “MCC” is not supported by data. Why do the authors not show a radar plot similar to what is presented by Zhang et al. (2020) who also discuss ACE-ENA cloud microphysics (see their Fig.1d). A radar plot might also help elucidating the observed PCASP number concentration variation within a defined section.

Figures 8 and 9 do not add anything to the discussion in the form presented in the manuscript. The authors simply reiterate the same points already made about Figs. 6 and 7 with regard to droplet radius, droplet concentration and cloud liquid water content in the two cloud regimes.

Figure 12 is the heart of the paper as it argues for precipitation driving of MBL aerosol concentration. However, the log-log format makes it hard to estimate whether the number concentration loss in the accumulation mode (around 0.2 μm) is balanced by coarse mode number concentration. Ignoring sulfate mass production in cloud, about 1000 particles with a diameter of 0.2 μm would be needed to form one 2 μm particle. It looks like the plot may just show that but why not provide a simple calculation that compares submicrometer volume to supermicrometer volume? Also, this section is good example of how the authors incorrectly use previously defined terms. The coarse mode (above 1 μm) is not the “large accumulation mode”. It is simply the coarse mode.

Figure 12 is left largely undiscussed and it is up to the reader to know whether previous research has concluded the same or found contrasting information. If it is true that the increase in coarse mode aerosol is related to drizzle evaporation, then most of the coarse mode aerosol mass should be sulfate since this is what accumulation mode aerosol is mainly composed of in the clean MBL. This is not what previous research has found. A sensible discussion on Figure 12 should consider sea spray aerosol as potential coarse mode aerosol source. Discuss differences between July 18 and January 25 cases!

Nonetheless, the authors may just have found observations that show drizzle evaporation as source of coarse mode aerosol but that requires more support information. Was the MBL actually clean as claimed? Did you observe entrainment-mixing? The ACE-ENA report (https://www.osti.gov/biblio/1526025) shows a Single Particle Soot Photometer as well as CO instrumentation was aboard the aircraft. Use these data and show vertical profiles as well as any horizontal gradients. I am puzzled by the authors "concerns about processing time delay" between external and internal instrumentation aboard a research aircraft. What are you concerned about and did you actually corrected CCN data presented for that time delay?

The report also lists PILS (particle-into-liquid sampling) and TRAC sampling (particle onto substrates) systems which may help show differences in coarse mode aerosol composition between drizzling and non-drizzling MBL. How about temperature changes? Can you link PCASP number concentration to drizzle evaporation-induced cold pools under drizzling cloud?

Also, clean the data and remove droplet shatter. Figures 10 and 11 clearly show spikes in PCASP number concentration and even CCN concentration that are obviously droplet shatter and will skew the data averages presented.

Lastly, what is the point of the WRF simulation? I don't see much similarity between satellite data and WRF run. The authors never discuss the initial conditions used to set up the model. What do we learn from the model run? It appears the authors simply use it to conclude drizzling cloud is associated to increased updraft velocity but what does it add to the discussion? Where is the WRF run for the second case study?

Needless to say, I recommend rejection as the manuscript requires more than just major revisions. The authors presented potentially valuable observations but a complete rewrite is warranted. It is the obligation of the coauthors, not the journal reviewers, to carefully read the manuscript and correct such astonishing mistakes.

Zhang, Z., Song, Q., Mechem, D., Larson, V., Wang, J., Liu, Y., Witte, M., Dong, X., and Wu, P.: Vertical Dependence of Horizontal Variation of Cloud Microphysics: Observations from the ACE-ENA field campaign and implications for warm rain simulation in climate models, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-788, in review, 2020.