

Atmos. Chem. Phys. Discuss., referee comment RC2
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Comment on acp-2021-391

Anonymous Referee #2

Referee comment on "Environmental effects on aerosol–cloud interaction in non-precipitating marine boundary layer (MBL) clouds over the eastern North Atlantic" by Xiaojian Zheng et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-391-RC2>, 2021

Review of Zheng et al., "Environmental Effects on Aerosol-Cloud-Interaction in non-precipitating MBL Clouds over the Eastern North Atlantic," acp-2021-391

As the title suggests, this paper describes an aggregated analysis of aerosol-cloud interaction (ACI) in non-precipitating marine boundary layer clouds at the Eastern North Atlantic ARM remote sensing supersite. A relatively narrow view of ACI is taken in which the bivariate relationship between aerosol and cloud drop number concentration and the ACI index were calculated numerous times, compositing by various column-mean or column-integral quantities (e.g., water vapor path, cloud adiabaticity, lower tropospheric, turbulence). My main concern with the study is that each of these purported controlling factors is analyzed in isolation, which implicitly assumes no covariability among them. This assumption is not valid and no attempt to address this issue was given. As such, I find it difficult to accept many of the mechanistic arguments made by the authors. They cannot demonstrate cause and effect, and there are clearly confounding variables that limit their ability to draw stronger conclusions (for example, lines 243–244: "the coincidence of high NCCN and PWV does not necessarily imply a physical relationship"). I therefore recommend the manuscript be rejected and the authors encouraged to resubmit after broadening their analysis. The premise of evaluating ACI with the authors' retrieval product is promising, but to understand the role of the controlling factors, they must be analyzed in a multi-dimensional framework (principal component analysis, k-means clustering, etc.) that allows the authors to identify and, more importantly, *interpret* co-variability among environmental factors. As it currently stands, the conclusions of this study point vaguely toward correlations with large-scale variables but give no clear guidance.

I have a number of other concerns the authors may also wish to consider:

- How good of a proxy is PWV for PBL relative humidity? Are there cases when non-drizzling stratocumulus occur with a relatively moister free troposphere? Perhaps you could estimate the fraction of PWV in the PBL using the interpolated sonde product or Raman lidar (note: Raman will only get you subcloud vapor)?
- Not enough information is given about how the vertical velocity variance TK_{Ew} is calculated. Is it a PBL average? A Doppler lidar column-deep average? Column max. value? And what Doppler lidar product are you using to get variance? The standard 10-minute integration? The median value seems low for surface-coupled stratocumulus cases. Are you evaluating any decoupled cases? There is also a diurnal and season cycle of turbulence at this site (at least, when sampling an undisturbed marine airmass; see more below), which may also be affecting your statistics.
- Have you controlled for wind direction in your analysis? It has been shown that there is an island effect when the surface wind is from the island (e.g., Zheng, Rosenfeld and Li 2016). Overland flow affects boundary layer turbulence and may also impact surface fluxes, PBL depth and CCN composition.
- How much does LTS tell us at a site like ENA, and what physical motivation do you have for including it as a sorting variable? I always envision LTS as having the most meaning in the subtropical eastern boundary current (EBC) areas, i.e., northeast/southeast Pacific and southeast Atlantic. The Azores are more of a mixed subtropical/midlatitude site that has much warmer SST than in the traditional EBC areas where MBL clouds are studied, and much of the cloud cover at ENA occurs in transient postfrontal subsidence vs. longer-lasting large-scale subsidence where the spatial gradient (of both subsidence and SST) matters more in defining cloud type transitions.
- For arguments you make about the relationship between entrainment, collision-coalescence and number concentration, it is problematic that your retrieval assumes constant N_c throughout the cloud layer. When entrainment-induced evaporation and/or collision-coalescence are active, this assumption is broken. In general, I don't understand your argument that entrainment is a sink of N_c .
- High CCN events at ENA are not only from North America. They have also been traced to North Africa and Europe.