Review of Zheng et al., "Distinctive aerosol-cloud-precipitation interactions in marine boundary layer clouds from the ACE-ENA and SOCRATES aircraft field campaigns"

SUMMARY

The authors present a comparative study of aircraft in situ aerosol and cloud microphysical measurements of liquid phase boundary layer (BL) clouds from two different regions: over the Eastern North Atlantic (ENA) region near the Azores and the Southern Ocean (SO) in an area spanning south of Tasmania. The ENA measurements are from the summer and winter seasons, while the SO measurements are from summertime only. The overall conclusions are that:

1. Clouds across different regions and seasons have differing collections of microphysical properties
2. These differing microphysical regimes exhibit different susceptibilities to aerosol perturbations
3. Drizzle has a big impact on the BL CCN budget
4. Turbulence plays a leading role in enhanced precipitation seen in the ENA winter regime

The primary data analyzed in the study are aerosol and cloud microphysical properties averaged over all full cloud soundings of warm BL clouds during each campaign. These properties include number concentration (total, modal, fully size-resolved, etc.), measures of drop size distribution (DSD) width, effective radius/mean diameter, liquid water content, sedimentation rate, etc. Many (if not most) of these observations have been analyzed in other recent studies, and indeed many of the conclusions reached by the authors are "consistent with" (or similar language) the results of these other papers. The frequency with which conclusions are followed by such qualifiers gives the impression that there is not much new added by the manuscript. It would be more effective to give a condensed overview of in situ work on the ACE-ENA and SOCRATES campaigns in which you lay out what has already been done. Then in the results section, devote a subsection (or a couple paragraphs, whatever) to explain how your work fits with what's already been published. This would be easier to digest than the piecemeal and repetitive referencing in the manuscript's current state. I do see novelty in the specific focus on interactions among aerosol, clouds and precipitation (abbreviated ACI or ACPI, depending on the context), which to my knowledge have not been addressed in the literature for either field campaign (ACE-ENA and SOCRATES) analyzed.

I have serious concerns about the lack of justification for combining analysis of the field campaigns used here as well as the scientific reasoning leading to the point that turbulence-enhanced collision-coalescence explains more intense drizzle during one campaign (ACE-ENA winter) versus the other two. In fact, I think the differences in precipitation attributed to turbulence can be explained much more simply without appealing to turbulence-microphysics interactions *at all* (see 3rd major comment below). While there is clearly a body of legitimate analysis presented in the study, I recommend the manuscript either undergo VERY major revisions or that it be rejected so the authors have sufficient time to rewrite the manuscript before resubmitting it.

GENERAL COMMENTS

1. **The unifying thread tying together measurements from the two regions is not clear to me, and I ask that the authors further emphasize/clarify the scientific motivation for combining the campaigns**. This would give a stronger basis for communicating the significance of your results. As it stands, there are no previously unexplored commonalities across the 3 campaigns. Yes, cloud effective radius increases with height for all the campaigns; yes, drizzle mean diameter increases from cloud top to base – these results (among others) are expected, and simply demonstrate that atmospheric physics as we know it isn't completely "broken." But beyond that, what purpose does this comparison serve?

   The aerosol and meteorology driving the clouds in each regime are quite different, so it's somewhat of a trivial conclusion that the microphysical properties differ as well. The argument that "SOCRATES and ACE-ENA both took place in the midlatitudes, so they're directly comparable" is insufficient. The SO region sampled by SOCRATES is more consistently impacted by midlatitude cyclone systems than ENA during summer, which is more often dominated by the nearby Azores high (i.e., ENA is borderline subtropical during summer). In addition, I do not buy that these aircraft campaigns can be taken as "representative" samples of their respective latitude bands/ocean basins; Mechem et al. (2018) show significant interannual variability in the synoptic conditions experienced at the ENA site, and the summer ACE-ENA IOP was characterized by anomalously low BL heights *and* substantial BL decoupling.

2. The authors go to some length to justify their assertion that turbulence-enhanced collision-coalescence is the reason for stronger precipitation during winter at ENA, but the evidence given does not prove their hypothesis. **The discussion of TKE is illustrative but quantitatively insufficient.** For one, very few details are given on how the velocity perturbations are calculated; for example, what is the integral length scale obtained with a 10 s moving window (i.e., are you capturing the inertial subrange)? Is it the same for ACE-ENA and SOCRATES? (no) Is any window function applied or is this a simple "boxcar" moving average? Is 1 Hz data used or did you analyze high-rate data? Is "high-rate" the same for ACE-ENA and SOCRATES? (no) In addition, TKE is not the relevant quantity for evaluating turbulent enhancement of collisional growth; rather, it is the TKE dissipation rate ε that is used in parameterizations (e.g., Grabowski and Wang 2013 as referenced in the manuscript). For another, $\epsilon \sim O(10^{-4}$ m^2/s^2) in stratiform BL clouds while in shallow cumulus it is about an order of magnitude higher. Based on the sampling goals of both ACE-ENA and SOCRATES, mostly stratiform clouds were sampled, suggesting generally low turbulence intensities. A past modeling study on the feasibility of turbulence to overcome the "warm rain bottleneck" and accelerate drizzle formation via collision-coalescence enhancement in subtropical marine stratocumulus (Sc) showed a minor impact (Witte et al. 2019, doi: 10.1175/MWR-D-18-0242.1) – why is a different answer expected in the same cloud dynamical regime? Finally, cold pools are a dynamical forcing mechanism more prevalent during ACE-ENA winter than either of the other 2

campaigns; this is not mentioned at all.

For you to continue pushing the line of reasoning that turbulence directly *causes* stronger precipitation, at very minimum you need to demonstrate that ε is substantially stronger for the ACE-ENA winter campaign than both typical marine Sc *and* SOCRATES/ACE-ENA summer (i.e., much greater than the 30-40% difference in mean cloud-top TKE shown). If you are unable to demonstrate this, I cannot support the heavy reliance on turbulence-enhanced collision-coalescence peppered throughout the manuscript. If you move forward with quantifying ε, please explicitly detail your approach in the methodology section – I recommend Siebert et al. (2010, doi: 10.1175/2009JAS3200.1) or Waclawczyk et al. (2017, doi: 10.5194/amt-10-4573-2017) as starting points for developing your own analysis.

I will note that your point that the *activation fraction* of available CCN to cloud droplets is highly correlated with turbulence intensity (as stated in the abstract, albeit differently worded) is valid, but this is not quantitatively demonstrated either; you could adopt a framework as in Hu et al. (2021, doi: 10.1029/2021JD035180) to explore this point further.

3. Beyond the lack of quantitative evidence supporting the hypothesis that turbulence is the cause of increased drizzle production during ACE-ENA winter, **there is a simpler, more parsimonious explanation that I believe is given short shrift in the manuscript: that a combination of a deep cloud layer along with relatively clean aerosol conditions during ACE-ENA winter results in robust drizzle generation**. You do discuss the relationship between cloud depth and precipitation susceptibility very briefly in section 4.2, but it essentially reads as a footnote versus a primary point. Given the strong dependence of cloud base rain rate on cloud depth in the empirical relation discussed in this same section (4.2), it was a major oversight that you didn't explore this aspect further.

4. The discussion of the role of thermodynamic decoupling is incomplete, but decoupling ostensibly plays a significant role in the low values of f_{ad} encountered during all three campaigns. It would be well worth taking the next step and directly quantifying at least one decoupling metric from the observed profiles as defined by Jones et al. (2011).

5. I am not familiar with "retrospecting" as discussed in section 4.3 and shown in Fig. 8. What is the procedure for performing this analysis? Please explain in the manuscript, as this appears to be simultaneously one of the most tentative aspects of the paper as well as something the authors are rather excited about.

SPECIFIC COMMENTS

Each comment refers to a specific line/passage, figure, table or caption. Specific line(s) are denoted by LXX (or LXX-YY for longer passages).

L24-26: the lack of sensitivity of precipitation to aerosol during SOCRATES suggests that it inhabits a different microphysical regime than ACE-ENA. In other words, there are sufficient CCN during SOCRATES that the ACPI are effectively "saturated" with respect to increasing aerosol loading. There are numerous references discussing such buffering effects (a good starting point is Stevens and Feingold 2009, doi: 10.1038/nature08281) that I recommend the authors consult to reframe this discussion.

L154-157: with respect to what are the "changes" in θ_L and q_t evaluated? The mean of the cloud layer or surface layer? Based on Fig. S1, I assume cloud layer. I am confused by this definition because "mixed layer" typically implies surface mixed layer, while you are using it to describe an elevated mixed layer. In general, I found your analysis of decoupling to be incomplete.

L191-195: When you discuss "airmass origin," at what vertical level(s) are back trajectories taken? I believe you in terms of PBL airmass, but is this also true above the PBL?

L192 and elsewhere: Zhang et al. (2023) reference is missing in bibliography – I assume the correct reference is from (most of) the same authors in Atmosphere (doi: 10.3390/atmos14081246)?

L216-217: please add a location for the further discussion, i.e., "and will be further discussed in Section X.X" or "further discussed later in this section"

L259: are there any measurements that support your assertion that it's both coalescence-enlargement and sea salt contributing to heightened concentration at Dp>1 um?

L268-269: I don't think you've improved understanding of the first indirect effect. Rather, you're adding another data point that supports what we already understand about it. So it's more of a "confirmation" than anything novel.

L296: Verb tense disagreement. Recommend you make everything present tense: "…air **is** entrained into the boundary layer…"

L300-302: Does this difference in r_c profiles tell you anything about mixing regime (i.e. homogeneous vs. inhomogeneous)?

L304: I can't imagine the water vapor source from entrainment evaporation is a leading order term in the BL q_t budget, and I'm having some difficulty understanding the relevance of raising this point. As you state, the net impact of entrainment on BL q_t should be negative (i.e., entrainment mixes in drier air, so BL-mean q_t should decrease), which would imply this evaporative source is a relatively minor offset to the entrainment drying sink. Eyeballing it from Fig. 3c, it looks like there's maybe 0.2 g/m3 of vapor that is liberated from the clouds (extrapolating the midcloud q_l lapse rate to z_i=1) – but I can't assess this any further since you don't show any mean q_t or q_v profiles. Both the G1 and the GV have open path hygrometers from which q_v can be accurately measured in cloud – if you want to get into a discussion of the vapor budget, it would be helpful to explicitly show some of these measurements.

L305-310: What evidence do you have for re-condensation beyond the inferences made from bulk profiles? And shouldn't there be *more rapid* growth on smaller drops since condensation rate is inversely proportional to surface area? You have the full DSDs to demonstrate the validity of the generalizations you're drawing. Please evidence for these assertions.

L313: What is gained by quantifying Δr_c? Is this not just a different way of expressing the subadiabatic q_l lapse rate via the relation $r_e = kr_v$ where $r_v \propto (q_l/N)^{1/3}$?

L337-340: Please define what terms are being used to calculate the "reduction of LWC_c" – it's not clear to me what you're doing here.

L368-369: Water vapor competition matters in a water-vapor limited regime (which seems quite obvious when stated that way…), but it seems to me that ACE-ENA winter is more of an aerosol limited regime. Appealing to water vapor competition is not a "one size fits all" conclusion that can be universally applied.

L370: do you quantify skewness or is this a qualitative description?

L371-372: can you say with certainty whether coarse mode aerosols are drizzle residuals vs. "primary production" of sea salt from the surface? Seems like a difficult "chicken and egg" problem to assess from in situ data without either aerosol composition information or some modeling work to back up the statement.

L436: I assume you mean liquid water *content*, but this is not stated

L490-491: What are the uncertainties in S_0? The correlations do not look very strong in Fig. 7a.

L514: Double check the equation, it looks like something is not formatted correctly or there are some extra characters.

L536: Should there be a minus sign in the 2nd parenthetical?

L538-539: This sentence needs to be restructured, it is currently a fragment.

L542-543: you could expand upon this point more, it's a bit too concisely expressed to be easily understood.

L569: it is rather counterintuitive that the "pristine" environment has the strongest aerosol loading. This is paradoxical because we often use the terms "pristine" vs "polluted" to imply low vs. high aerosol loading, respectively. You clearly mean it in the sense that the SO region is minimally impacted by anthropogenic emissions. So a little word-smithing is needed to resolve this incongruity.

L594: fully agreed that the assumption of constant f_{ad} used in satellite N retrievals is problematic, but how do the campaign-average profiles presented here improve this situation? On a profile-by-profile (or, from the satellite perspective, pixel-by-pixel) basis, is there anything from the measurements that suggest potential predictors of f_{ad}? Or do you view your contribution as simply another data point showing that an assumed value of 0.8 is unrealistic?

Figure 2: This figure could be compressed in the horizontal, which would accentuate the shape of the distribution in a manner pleasing to the eye. As it currently stands, this looks “stretched out” and there aren’t many interesting detail/wiggles in any of the curves that merit such a long aspect ratio.

Figure 3: What do the shaded regions denote? Interquartile range? Standard deviation? 5^{th}-95^{th} percentiles? No info in caption.

Figure S2: put the two panels on the same plot so they can be directly compared.

Figure S3: why does this size range needs to be separated from Fig. 2 in the main manuscript?

Figure S4: please add uncertainty shading as you did in Fig. 3 of the manuscript, it would be helpful to see the variability of subadiabaticity within campaigns

Table S1: Please include f_ad in this table.

Table S2: it looks like this table is cutoff. Are there more variables not shown?