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Interactive comment on "Organized Variations in MBL Cloud Microphysical Properties Observed by Aircraft and Satellite and Simulated by Model" *by* Dale M. Ward et al.

Anonymous Referee #2

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Review of submitted manuscript "Organized Variations in MBL Cloud Microphysical Properties Observed by Aircraft and Satellite and Simulated by Model"

This manuscript investigates aerosol-cloud interactions in marine boundary layer clouds using two case studies from the ACE-ENA field campaign. Each case is split into two regions. Through analysis of aircraft observations, and the inclusion of a WRF simulation modeling one of the cases, the authors conclude that 1) aerosol differences between regions is due to drizzle coalescence scavenging, and not variation in background aerosol, and 2) updraft velocity is the key to explaining differences in cloud microphysical processes. While this is very much a relevant area of research, and within the scope of this journal, there are a number of significant issues with the manuscript that must be addressed before publication can be recommended. Given the expected revision required, line-by-line comments will be minimal, and this review will focus on the major changes needed to improve this manuscript.

1) Introduction/Literature review: Broadly speaking, the introduction lacks focus; the authors touch on various important areas of marine boundary layer research, but much of the cited literature seems less relevant to the message of this particular research than other papers would be. The two key conclusions of this paper relate to cloud-aerosol interactions (specifically the role of drizzle in affecting aerosol properties), and the influence of boundary layer and cloud dynamics on cloud micro- and macrophysical properties. Yet these two topics are reviewed only in the most superficial sense, with an insufficiently detailed discussion of the current understanding. An overview of prior observation and modelling results focusing on marine Sc (e.g. Wang and Feingold, 2009; Bretherton and Blossey, 2017; Xue et al, 2008, to name a few), and their specific findings, would help orient the reader to the topics that will be discussed in the rest of the paper. The paragraph discussing the 'too few, too bright' issue can be removed without any loss of relevant information. 2) Compositing strategy: The data in this work is broken down into an along-wind and a cross-wind sample. Based on the clouds sampled in each, these are taken to represent two 'regimes', one of drizzling, cleaner aerosol conditions, and high COD, and the other less drizzling, with a higher aerosol concentration, and lower COD. While I do not have a fundamental problem with this splitting of the data, the authors need to better justify what exactly the rationale for this split is, other than convenience. The discussion of MCC's is muddled – if the point is to separate the data based on MCC (i.e. convective-core vs edge/clearing), then based on Figs 3 and 4, the along-wind vs cross-wind does not do a great job; the July case shows high COD in both legs, and the January case along-wind leg shows that it samples over a strong gradient in COD. Clearer specification of what the reason and criteria of this split was, and then subsequent demarcation of sample regions within the map plots, would help strengthen the compositing strategy. If the goal is to split the data

in such a way that 1) both sets have more or less the same large-scale forcings and background conditions and 2) One set represents the rainier/thicker cloud/MCC cores, why not composite the observations by e.g. LWP? 3) The term “MCC” occurs 9 times in the abstract, and 5 times in the entire remainder of the paper. This seems very odd; the abstract makes many claims relating to clouds within MCC’s and clouds between MCCs, but it’s just not clear that the way the data is split justifies those conclusions. An image showing e.g. cloud visible imagery from MODIS for the two cases, or a plot like Figure 10/11 showing e.g. COT/LWP along the relevant flight legs, would justify the equivalence drawn between the along/cross-wind and MCC terminology. 4) Figure 6/7: These figures should have some estimate of variability at each level, ideally std deviation or some percentiles to assess significance of differences. 5) It is unclear what value Figures 8 and 9 have to the broader message of this manuscript. Given the importance of the precipitation estimates in the next section, it would help with the focus of the manuscript if this section were removed in favor of a more in-depth explanation of those precipitation estimates. 6) On that note, the precipitation estimates, as well as the corresponding aerosol removal rate estimates, seem to be an important finding, and more detail should be given to them. Given that input values to the precip and loss rates have some uncertainty about them (the rain rate is an average, presumably), some range or uncertainty should be provided, not just mean values. 7) Line 297: “the observed aerosol is 24% lower” Sure, but as in Figure 10, there are some noticeable aerosol gradients along the sampled path, and the text does not really explain them. Near Graciosa, the observed aerosol concentration is the same in both legs. Both this figure and Figure 10 are difficult to read with the shared axes. 8) The WRF simulation results: The implication, as drawn in the abstract, is that the fact that the regions of COT and vertical velocity co-occur is evidence of the velocity controlling cloud properties. While I do not disagree with the analysis presented in the WRF simulation section, none of it is novel. The extent to which the WRF results support the message of the paper, to me, are just that similar-sized variations in COT are produced without any aerosol field heterogeneity. This should be compared to prior literature for completeness. Given that the WRF simulation was performed with interacting cloud and aerosol, why is there no discussion of the actual WRF simulated aerosol? Presumably, some of the claims made based on observations, that the aerosol differences are due to precipitation removal, would be more directly verifiable using these simulations. Without any additional analysis, the inclusion of the WRF simulation seems very thin.

Minor comments: Line 28: remove comma before “such that” for better understanding of the sentence Line 54: no “the” before MBL Aerosol discussion: Mention Figure 10 earlier, around line 265. Line 220: “probably due TO impacts…” Lines 224-229: This is a mix of general knowledge and information better presented in the introduction and feels out of place. Notational consistency: please pick one of cloud optical depth or thickness and use it throughout, including figures. Figure 8: caption is incorrect for panels c/d. Clarify (also throughout paper) whether you are using the droplet effective radius (i.e. area-weighted) or mean radius. Line 312: Unless I am missing something, the shift to larger aerosols on January 25 is in no way at all obvious. Perhaps “extremely subtle” would be a better choice. Line 313: Nice though it would have been to have a below-cloud base leg on January 25th, this sentence should be rewritten in a more suitable tone, perhaps just “There was no below-cloud horizontal leg on January 25th”. If there was an upward or downward profile, the authors could consider including that.

Citations: Bretherton, C. S., & Blossey, P. N. (2017). Understanding Mesoscale Aggregation of Shallow Cumulus Convection Using Large-Eddy Simulation. Journal of Advances in Modeling Earth Systems, 9(8), 2798–2821. https://doi.org/10.1002/2017MS000981 Wang, H., & Feingold, G. (2009). Modeling mesoscale cellular structures and drizzle in marine stratocumulus. Part I: Impact of drizzle on the formation and evolution of open cells. Journal of the Atmospheric Sciences. https://doi.org/10.1175/2009JAS3022.1 Xue, H., Feingold, G., & Stevens, B. (2008). Aerosol Effects on Clouds, Precipitation, and the Organization of Shallow Cumulus Convection. Journal of the Atmospheric Sciences, 65(2), 392–406. https://doi.org/10.1175/2007JAS2428.1

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