Response to reviewer comments, "Identifying meteorological influences on marine low cloud mesoscale morphology using deep learning classifications", Mohrmann et al.

Response to reviewer #2:

We thank the reviewer for their time and helpful comments on the manuscript. Based on their feedback, we have the following response and revisions to the manuscript:

Comment on l101, l133, l202: By mass continuity, the horizontal divergence at 700 hPa would only give us the *local* vertical divergence dw/dz (or dω/dp in pressure coordinates) at 700 hPa, and while we do not expect very strong gradients of large-scale divergence with height in the marine lower troposphere, this value will be somewhat sensitive to the level chosen. The quantity w_{700}/z_{700} results from the integration of dw/dz from the surface to 700mb, and so represents the mean horizontal divergence over that layer, making it more suitable as an estimate of large-scale divergence. To make this point clearer, we have amended this paragraph as follows:

"Note that this large-scale divergence is not the horizontal divergence at 700 hPa, but rather the mean divergence from the surface to the 700 hPa level; this follows from the mass continuity equation by considering a column of air from the surface (where vertical motion is 0) to 700 hPa. The terms large-scale divergence and 700 hPa subsidence are used interchangeably throughout; divergence is plotted instead of subsidence to allow for more straightforward comparison with surface divergence. As surface pressure varies with time, the second equality is only approximate."

Comment on l218: Corrected typo, thank you.

Comment on l218-220: The reviewer correctly interpreted this sentence and is perhaps wondering if there was anything more to this point; we have added a parenthetical "(as expected)" to this sentence to indicate there is no deeper point being made.

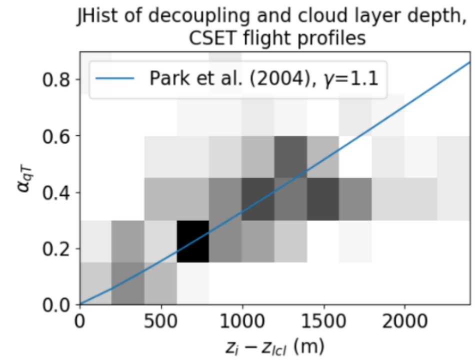
Comment on l234: Corrected by adding reference.

Comment on S3.5: regarding alpha_q, we have added the following text: The parameter α_q is a measure of relative resemblance of upper boundary layer moisture to the lower FT and lower boundary layer, with a value of 0 indicating a perfectly well-mixed boundary layer and a value of 1 indicating a perfectly decoupled boundary layer where the upper BL moisture is equal to the lower FT moisture.

$$\alpha_{qT} = \frac{q_T(upper\ BL) - q_T(lower\ BL)}{q_T(lower\ FT) - q_T(lower\ BL)}$$

For a given profile, the thermal inversion height is estimated using the maximum lapse rate, with the inversion being the layer where the lapse rate deviation from a moist adiabat exceeds 25% of maximum deviation (this was tuned to agree with a visual assessment of the inversion layer and worked well for all profiles). Upper and lower BL in the q_T equation are taken as the top and bottom 25% of the BL depth, while the lower FT starts 500m above the inversion top.Regarding question 2 on this section, this is clarified in the text added above; the inversion used for diagnosing MBL depth is always the strongest thermal inversion, which tended to occur at the top of the remnant cloud layer in trade-like shallow Cu profiles. The result is that the boundary layers appeared highly decoupled as this layer has been subject to ample dry entrainment during the transition. There were some cases where the upper layer was thoroughly eroded by dry entrainment making the diagnosed inversion was much shallower.

Regarding question 3 on this section, we reprocessed the profiles to include a surface mixed layer depth using the (near-)surface-derived LCL, consistent with Wood and Bretherton (2004). This allows for a more apples-to-apples comparison with the figures referenced by the reviewer showing model results of the same quantities. We include the fit from Park et al, 2004 shown in all 3 previous papers (though we note that Neggers et al. (2017) used a lower gamma value). It certainly seems that our profiles are consistent with the results in Wood and Bretherton, though we do not have enough sample size to say anything more insightful regarding the slight disagreements between the LES results and the plotted fit in de Roode et al. 2016.



Regarding the comparison to Lock (2009), we did briefly attempt to validate those results, but could not find a strong correlation between kappa and low cloud fraction. The most likely explanation for this is that in our observations, both kappa and cloud fraction are measured roughly simultaneously, whereas the simulations in Lock allow for cloud adjustment. It is also possible that kappa is sensitive to noise resulting from messy real-world profiles, and so we cannot say anything one way or another about its role in controlling cloud fraction.

Comment on Fig 2: added "Image scale is roughly 100 km across." to caption.

For the discussion of clustered vs disorganized MCC, the following text has been added to section 2.1: "The primary difference between these two types is that disorganized MCC represents a regime with cellular convection at some characteristic scale, though not organized clearly into open- or closed-cell regimes, while clustered Cu represents aggregated convection at a variety of scales within a scene. When distinguishing between these two types during manual labelling, scene large-scale context proved helpful."

Comment on Fig 5: This may be a screen issue, though we do agree that for Figure 5, the colors are not as easy to distinguish, though as the reviewer notes it is a moot point as no disorganized MCC or clustered Cu occurs in this scene. The colors are selected for consistency with the rest of the plots, where they do not present an issue, and so we will keep them as is.

Regarding the differing color scales between the two divergence plots, we have updated the plots so that both ASCAT and MERRA surface divergence are on the same color scale.

Comment on l342: typo corrected, thank you.