Response to Reviewer 1

We thank the reviewer for the constructive comments and suggestions that have helped improve the manuscript. Below we address each of the reviewer's comments and include any proposed changes to the manuscript that follow from addressing the comments.

Review of "Aircraft observations of cold pools under marine stratocumulus" by Terai and Wood.
Overview
This manuscript presents aircraft observations of cold pools in the stratocumulus topped boundary layer from the VOCALS-REx field campaign. The observations show that the observed cold pools tended to form in environments associated with a deeper marine boundary layer, thicker clouds and lower aerosol concentrations, all factors that make the stratocumulus more susceptible to drizzle. The importance of precipitation as the key driver for cold pool formation is highlighted. Individual cold pool observations are then composited to show how various thermodynamic and chemical variables change across the cold pool edge, and mechanisms to explain the observations discussed. These observations will provide useful constraints for testing high resolution model simulations of precipitating stratocumulus, which are required to better understand the role that cold pools play in modifying the mesoscale cloud field. Overall I found the manuscript very interesting, it was well written and is suitable for publication in ACP. I do have a number of comments below that I would like to see the authors consider when producing a revised manuscript.

Comments
Section 2.1 pp 11027 line 11: change "(REX) is" to "(REX) was".

We have corrected the text as suggested.

Section 2.2 pp 11030 line 4: It is not clear at this stage in the paper (i.e. Fig 1) that drops in θ typically coincide with thick clouds and heavy precipitation. For example the first drop in θ in Fig 1 is associated with a thin cloud layer and relatively low radar reflectivities at cloud base. Presumably this could be due to a time lag between the measurement and the precipitation event that created the cold pool. It may be worth noting here that thicker clouds and heavier drizzle are shown to be typical when all cold pools are analysed later in the paper.

We have included the following sentence in the text:
The drops in θ also coincide at times with thick clouds, which are more likely to produce heavy drizzle. **Although there are drops in θ in Fig. 1 that do not correspond to increases in thickness, possibly due to the lag between the precipitation event and aircraft sampling, we discuss in Sect. 3.3 and 4.3 how cold pool occurrence correlates with cloud thickness.**

Section 2.2 Equations 1 to 4 and Fig 2: Can you rewrite the equations or modify the figure so that they are consistent. For example Eq. 1 has θ (t − t2) − θ (t) which corresponds to Δθa in the figure. Also in Equation 1 you have θ2 whereas when you define this in the text below you just have θ 2 = 0.12K i.e. no Δ.

In the text, we have added Δ to θ_1 and θ_2 such that they read $\Delta\theta_1$ and $\Delta\theta_2$. We have also changed Eq. 1 – 4 so that they now read:

$\Delta\theta_a = \theta(t\text{-}t_2) - \theta(t) \geq \Delta\theta_2$,
$\Delta\theta_b = \theta(t\text{-}t_2) - \theta(t) \geq \Delta\theta_1$,
$\Delta\theta_c = \theta(t) - \theta(t+t_1) \geq \Delta\theta_1$, and
$\Delta\theta_d = \theta(t) - \theta(t+t_2) \geq \Delta\theta_2$.

Section 3.1 and Fig 3: Here you discuss the algorithm to calculate cold pool size. I have difficulty reconciling this algorithm with the colour bars presented in Fig 3 and think some clarification is needed. For example taking the first cold pool edge in Fig 3 at approx 7 km, there is a blue bar identifying a cold pool that extends to another identified edge at approx 12 km. Assuming that the algorithm is run in the forward direction I don't see how the second edge at 12 km is identified as the same cold pool because the potential temperature hasn't recovered to that of the first edge. I can see how this would have been identified as a distinct cold pool if the algorithm was run in the reverse direction. In contrast, when looking at the cold pool identified from approx 46 to 56 km I can only see the algorithm detecting this if it was run in the forward direction. So I can only reconcile the results in Fig 3 with the algorithm if it was run in both the forward and reverse directions. However, the implication from line 2 pp 11032 in section 3.1 is that the algorithm was only run in the forward direction. Perhaps I am missing something?

We apologize for the confusion. The text does not reflect the actual method. Unlike what is implied in the text, we ran the algorithm in both directions to identify cold pools and estimate their size. The text was modified to reflect this:
~~If we run our algorithm in reverse direction, the differences are found to be negligible.~~
We run the algorithm in the forward direction and then in the reverse direction to identify all the unique cold pools.

Section 3.1: When calculating the distance travelled by the aircraft the authors assume that the aircraft is flying at 100 ms−1 as stated in the caption of Fig 1. Why not use the measured true air speed of the aircraft? This would give a more accurate measure of the distance travelled. I suspect that the error may be minor, but it makes sense to use the true air speed, particularly when one of the cold pool diagnostics analysed is size.

The average true airspeed of the C-130 is 105 ms^{-1} with a standard deviation of 2ms^{-1}. If we were to apply the change to the size distribution, we only see a slight change in the size distribution (see figure below). We have included this in the text that the true airspeed more closely resembles 105 ms^{-1}, but because the change is small compared to other corrections (e.g. $4/\pi$ correction), we have not modified Fig. 4.

We have modified the text to say:

Since the aircraft flew straight legs at rate of ~100 ms^{-1} **(average airspeed of 105 ms^{-1} with standard deviation of 2 ms^{-1})**, the time interval is then converted into a distance **using the 100 ms^{-1} approximation**.

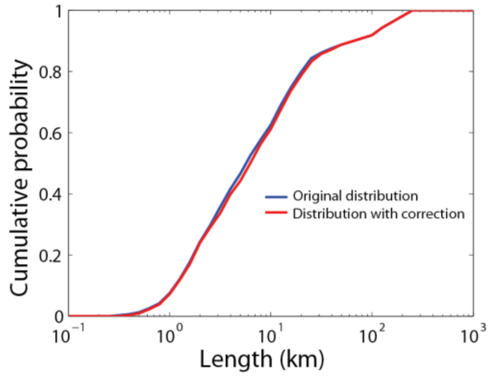


Fig 4: The caption refers to Eq 6 when it should be Eq 5.

This has been corrected.

Section 3.1 pp 11032 line 18 and Fig 4: Would a better lognormal fit be possible in Fig 4, where the slope is shifted to the left so that it passes through the median point of the distribution. It would therefore better represent the first 90% of the data at lower sizes where the statistics are better. It would be worthwhile including the fit parameters in the text as well to compare against future LES model output etc.

The fit parameters $\mu = 2.45$ and $\sigma = 1.25$ have been inserted into the body of the paragraph and added to the caption of Fig. 4. These fit parameters were calculated from calculating the μ and σ parameters from the data, and therefore, we find that the fit given in the paper more accurately represents the data than one that is made to go through the median values.

In reality, as one would expect, the fit is not lognormal, apparent in the plot below. Part of the shift of the lognormal distribution may be due to undersampling issues at the smaller sizes, but a shift of the distribution to a smaller median will result in largely underestimating the larger cold pools in the >100 km range.

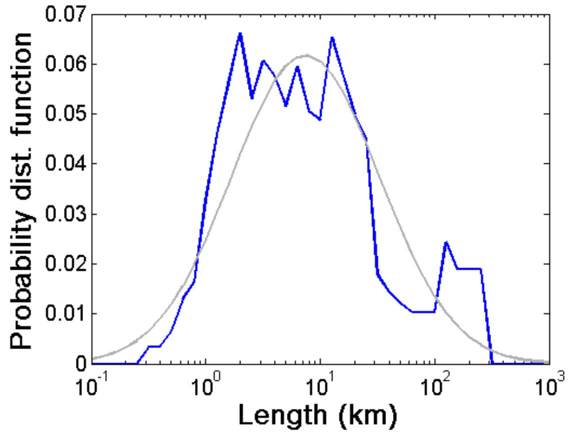


Figure: Binned data of cold pool size (blue) and the lognormal fit (gray - $\mu = 2.45$ and $\sigma = 1.25$).

Section 3.3 pp 11034 line 11: It is mentioned that cold pools are expected to form preferentially in deeper MBLs. How is this different to the factors that affect the discussion in the previous paragraph about cold pools preferentially forming when thicker clouds are present as these are more susceptible to drizzle formation? At least in the VOCALS region, measurements along 20S show that thicker clouds are typically associated with a deeper MBL (Bretherton et al., 2010).

Thicker clouds do tend to form in deeper MBL depths. However, the two follow a one-to-one relation only if the boundary layer is coupled. As the MBL deepens, the boundary layer tends to decouple, and the formation of cumulus clouds developing into stratocumulus clouds becomes more likely. The mean cloud thickness does not capture this, because it only captures the mean thickness of the clouds and not its variations. And indeed, the correlation between MBL depth and cloud thickness is 0.65. The correlation only explains roughly 40% of the variability because the boundary layer can be decoupled.

The two graphs are organized as they are because unlike cloud thickness and precipitation rates that vary widely over scales of tens of km, the boundary layer depth and PCASP aerosol concentrations tend to vary at larger scales and at longer timescales, except for extreme examples as pockets of open cells. As the study of Berner et al. (2013) show, the CCN concentration and boundary layer depth also better describe the regime of the evolving boundary layer than the cloud thickness and precipitation rate, and hence provide a way of looking at how cold pool formation fits into the transitions between the different regimes.

Berner, A. H., Bretherton, C. S., Wood, R., and Muhlbauer, A. (2013), Marine boundary layer cloud regimes and POC formation in an LES coupled to a bulk aerosol scheme, Atmos. Chem. Phys., Discuss., 13, 18143-18203.

Section 3.3 pp 11034 line 24 and Fig 8: You show cases where cold pool formation occurs under moderate PCASP aerosol concentrations up to 200 cm-3. Are these cases those that have the higher cloud thickness (presumably LWP which you could look at *using the data from the microwave radiometer) and drizzle rates shown in Fig 7? One would imagine that if the LWP was high enough the cloud could precipitate easily at these CCN concentrations and then potentially lead to cold pool formation. Presumably it is a combination of LWP and cloud droplet number concentration that will dominate whether the cloud can precipitate and hence form cold pools. After all, those are the key factors that will determine if the cloud droplets can grow sufficiently to form drizzle drops. In order to highlight additional dependencies in these plots you could perhaps modify the symbols in Fig 8 so that the size of the symbols represented either the cloud thickness or the amount of drizzle. Similarly in Fig 7 you could relate the size of the symbols to the PCASP aerosol concentration.*

For the legs with cold pools where the PCASP aerosol concentration is nearly 200 cm^{-3}, the mean cloud thickness is ~400 m, which is on the higher end of thicknesses. However, we find that changing the size of the symbols will not be helpful since that would suggest the increased importance of larger points than others and result in overlapping data points, which is not the intent of the figure. Experimenting with color instead of size, we have divided up the cold pool and non-cold pool data into two graphs below and colored the data by the cloud thickness and PCASP concentrations. We find that the information gained from splitting the data into two graphs does not outweigh the loss in the main point that while certain variables do a better job at explaining cold pool formation, none taken independently or in combination can clearly separate the legs where cold pools were observed from those where cold pools were not observed. Therefore, we have kept with the original configuration of the graphs.

We have, however, modified the text to acknowledge that the cloud thickness is higher for the 'polluted' cold pool cases:

However, cold pools can exist for aerosol concentrations as high as 200 cm^{-3}. **The mean cloud thicknesses in those particular legs are roughly 400 m (not shown), but as Fig. 7 indicates, none of the factors, including precipitation rate, taken separately, are perfect predictors of cold pool formation.**

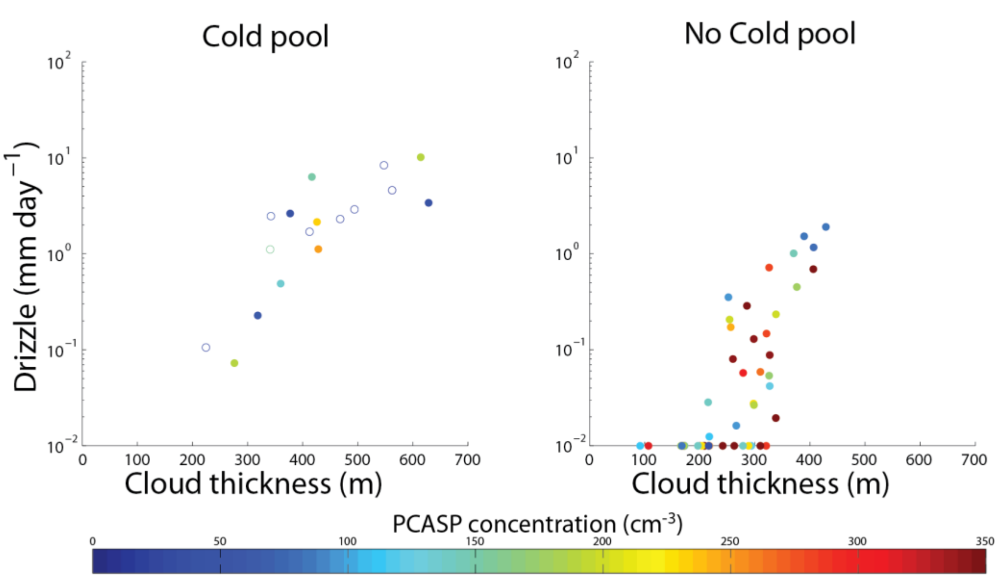


Figure: Leg-mean drizzle rate and cloud thickness for legs where cold pools were observed and legs where cold pools were not observed. Each data point is colored by the leg-mean PCASP concentration.

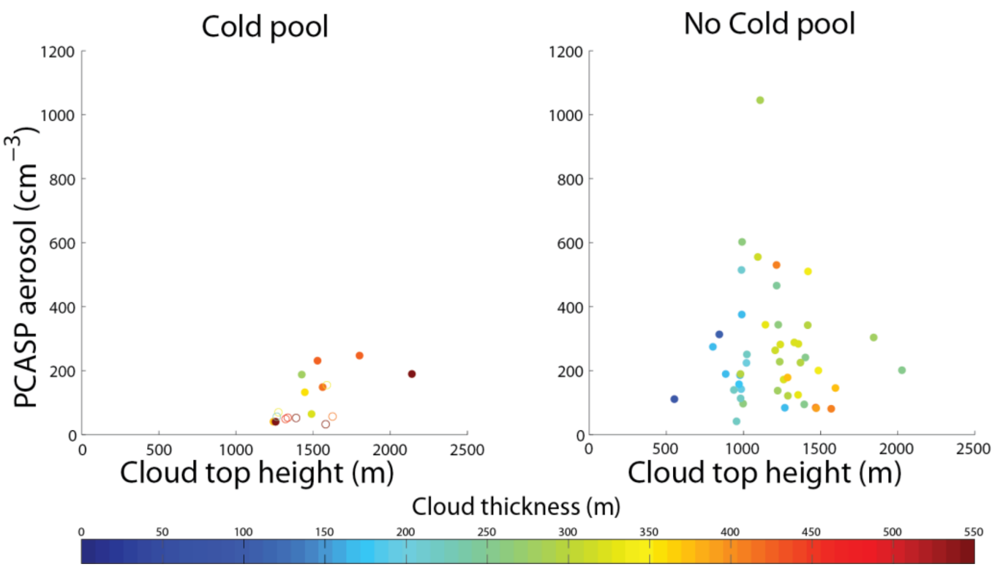


Figure: Leg-mean PCASP aerosol concentration and cloud top height for legs where cold pools were observed and legs where cold pools were not observed. Each data point is colored by the leg-mean cloud thickness.

Section 3.4 pp 11035 line 11: Would it be better to centre the 1 x 1 degree box on the centre of each cold pool rather than the centre of each flight leg? This would better capture the mesoscale variability around the cold pool. From Fig 3 it is clear that this would lead to more mesoscale variability for the cold pools to the west of the flight leg where the cloud is more broken.

Since each data point represents a flight leg, rather than a cold pool, we believe that each box should be centered on the leg and not the cold pool features. Especially for cases where cold pools are observed at the edge of the flight leg, we have no way of knowing how far off the other edge of the cold pool lies.

Section 3.4 pp 11035 line 23: It is stated that legs with cold pools have a markedly different TB distribution (Fig 9). When looking at the results in Fig 9, I think this is a bit overstated given the low statistics (22 flight legs with cold pools out of 87 - section 2.2). In fact you then go on to say that cold pools are not necessarily associated with broken cloud fields, again suggesting that the prior statement perhaps needs to be toned down.

We agree with the reviewer that the word 'markedly' does not accurately describe what is found in Fig. 9. Therefore, we have deleted the word 'markedly different' and replaced it with 'broader'.

Section 3.4 pp 11036 paragraph related to Fig 10: I think the paragraph describing the analysis of satellite LWP with ECMWF back trajectories and the corresponding Fig 10 *could be removed as I am not sure it adds anything to the paper. You have already shown that cold pools preferentially form under thicker clouds, and so I would assume that these typically have higher LWP values. In fact why not just look at the microwave LWP retrievals from the aircraft.*

The intent of this paragraph is to look at some of the history of the clouds of the airmass sampled by the C-130. The C-130 observations only provide a snapshot of boundary layer conditions at the time sampling. The satellite retrievals allow us to ask whether the cloud fields over cold pools were systematically different the day or night before sampling. Therefore, the satellite retrievals provide information that the microwave LWP retrievals from the aircraft cannot provide.

The following sentences have been added to emphasize this point:
We can pursue the relationship between drizzle and cold pools further by looking at the satellite passive microwave dataset. **Whereas the aircraft only provides a snapshot of the cloud field that lies over the observed cold pools, the satellite retrievals allow us to look at the temporal evolution of the clouds leading to sampling the cold pools and associated clouds.**

Section 4.1 pp 11037 line 20: Suggest you change the equation $C_P\ \theta = -Lq_v$ to $C_P \Delta\theta = -L\Delta q_v$ to make it obvious that θ and q_v correspond to a difference in the various parameters with respect to the cold pool edge.

The Δ has been added to both θ and q_v.

Section 4.1 Eq 6: The text states that _ is the in situ density when this should be _. The incorrect symbol is used a few lines below this as well.

θ has been replaced by ρ, where appropriate.

Section 4.1 pp 11038 line 18: Can you postulate why the pressure is sometimes lower in the cold pool? How often did this occur?

The mean difference in pressure inside and outside of the cold pool is 4 Pa when the standard deviation in pressure is 12 Pa (mostly from background pressure perturbations). We see the signal only when we composite the data. Of the 90 edges, the cold pool has a lower pressure in 29 cases.

We have modified the text to say:
Because the background variations in *p* are larger than the contribution from the cold pool, p is not always greater inside the cold pool on every flight, and if we attempt to calculate the cold pool depth for each transect, we obtain estimates of negative depth.

Section 4.2: From here on the figure numbers in the text are incorrect and need changed.

All figure numbers have been corrected to match the figures.

Section 4.2 and Fig 12: It looks like the data in Fig 12 does not have the 4/_ correction factor applied as the numbers in the text don't correspond to the results in the figure. Can you state this in the text or modify the figure caption.

We have stated the lack of correction factor in the figure caption.

Section 4.2 pp 11039 line 20: Is the derived Froude number an average over those cases where h estimates were possible from Eq 6? What is the variability in the Froude number from the different cases?

The derived Froude number is derived from averaged values of h, ρ, $\Delta\rho$, and V_f. Since we did not calculate h for each cold pool case, we have not obtained the variance in the Froude number.

Section 4.3 pp 11041 line 4: It is stated that cold pools have the potential to lift the decoupled surface layer above its LCL to form cumulus clouds. Are these cumulus clouds formed at the cold pool edge where there are enhanced vertical velocities (Fig 12)? Can this be observed from the aircraft data?

No, we do not see a dip in the cloud base height just at the cold pool edge. Instead we see a gradual decline of cloud base height over the cold pool, starting at the edge.

Section 4.3: How is the in-situ drizzle water content calculated in Fig 14? Is this from the 2DC?

Yes, the drizzle water content is taken from the 2DC probe. We have added in the text that the insitu precipitation is based on measurements from the 2DC.

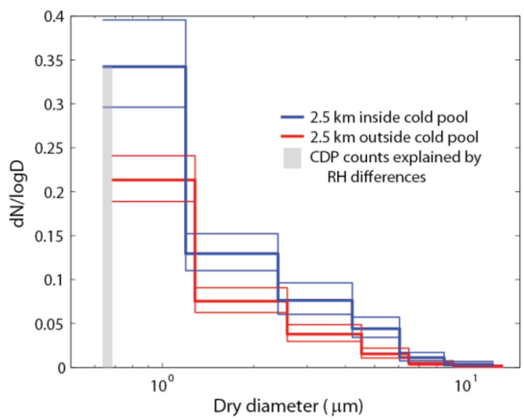
Fig 14 c) caption: Change "reflectivity does not follow (a) Gaussian" to "reflectivity does not follow a Gaussian".

We have corrected this mistake.

Section 4.4 and Fig 15 b): The composite CDP measurements show an increase in coarse mode aerosol concentration in the cold pool. Is the relative humidity in the cold pools typically higher than outside of the cold pools? I would imagine this is the case if drizzle evaporation is the key driver in their formation. If so I could envisage that there is enhanced hygroscopic growth of accumulation mode aerosol particles inside the cold pools, such that they grow large enough to be measured by the CDP. Can you show that the impact of this is not the major factor that leads to the increase in concentration shown in Fig 15 b)?

The plot below shows the number weighted size distribution of the 'dry' aerosols inside and outside of the cold pools, as measured by the CDP. The following steps were used to produce the plot. The mean RH inside the cold pool was 78%, compared with 71% outside the cold pool. After producing a size distribution from the CDP (bins were combined to increase the counts per size range), the 'wet' diameters were then corrected to dry diameters by assuming the particles had a growth factor comparable to NaCl. Looking at the plot below, the counts that can be attributed to the different threshold is shaded in gray. The concentration that lies in the gray shaded region is approximately 0.010 cm^{-3}. This is 14% of the difference that we observe between inside and outside the cold pools.

We have inserted the sentence the following sentences in the text to explain this: The increase in PCASP aerosol concentration is not significant at the 95% confidence level, whereas the increase in CDP aerosol concentration is. **Because the CDP measures the particles in ambient air, we must account for the increased number of aerosols measured purely due to the increase in relative humidity in the cold pool (from 71% to 78%) and existence of a minimum measurable size cutoff. Assuming that the particles have a growth factor similar to sodium chloride (Tang, 1996), the increased swelling of the aerosols in cold pools can explain 0.01 cm^{-3} of the 0.07 cm^{-3} increase of CDP aerosol concentration in cold pools.** Along with the coarse mode aerosol concentration, the mean DMS concentration also increases across the cold pool edge (Fig. 15c and Table1). **Taken together, the enhancements in CDP-measured aerosols and DMS inside the cold pools suggest the effect of cold pool-induced stratification in trapping surface fluxes.**



Section 5 pp 11046 line 14: It is stated that the significant drop in LCL inside the cold pool is largely due to increases in qv instead of the θ decreases (not shown). As this is a statement made in the conclusions I think some evidence should be presented in the paper.

In the body of the paragraph in Section 4.3, we have added the following sentence discussing the relative importance of variations in mixing ratio and temperature in explaining the drop in LCL.

Figure 14a also shows that the LCL dramatically decreases inside the cold pool, evidence of the cooler and moister subcloud layer in the cold pool. **Furthermore, we assess the relative importance of the cooling and moistening inside the cold pool in explaining the decrease in LCL. To look at the effect of q_v variations on the LCL variations, we can fix the temperature from each transect across the leg to the value at the edge and then calculate the LCL based on the temperature at the edge and the q_v variations across the edge. The same can be done to look at the effect of temperature**

on the LCL variations. Of the 161 m difference in the LCL between the 2.5 km segment inside and outside of the cold pool, 113 m can be explained by q_v variations, while 48 m can be explained by temperature variations. The mean LCL also lies …

Section 5 line 17: Change "cloud bases is a result" to "cloud bases are a result".

The suggested change has been applied.