Interactive comment on “Top-down and Bottom-up aerosol-cloud-closure: towards understanding sources of uncertainty in deriving cloud radiative flux” by Kevin J. Sanchez et al.

Anonymous Referee #4

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This paper presents results from a variety of measurements during an intensive field campaign at Mace Head in Ireland. It is perhaps unique in comparing estimates of cloud drop number concentration and radiative fluxes at cloud top based on several significantly different methods for a handful of cases during the campaign. Given the disparity among the cases (i.e. cumulus/stratocumulus; coupled/decoupled; adiabatic/sub-adiabatic), as well as the presentation of the results, it is a little unclear how to generalize the results of the study. The most substantive result seems to be the successful application of method for adjusting a parcel model calculation of the cloud top radiative flux to account for dilution of the cloud by entrainment that results in a flux estimate that agrees better with in-situ measurements of cloud extinction. The paper is appropriate for publication in ACP after addressing some minor revision.

We thank the reviewer for their comments. Please see below responses to each of the authors comments and suggestions.

The authors want to note that values in Table 3 have slightly changed. These changes were brought about by reviewer #2’s comment to present cloud optical thickness. It was noticed that the ‘observed’ optical thickness was not consistent between calculations that including and excluding cloud top entrainment. The observed optical thickness is calculated from the observed cloud droplet extinction. The observed droplet extinction is calculated by subtracting the simulated cloud droplet extinction and fitted difference in droplet extinction ($\delta\sigma_{ext}$) (Figure 8b,d,f). This was necessary to take into account the fact that the UAV’s often missed portions of the cloud. The linear fit made it possible to fill the gaps. Since the observations should be consistent, the observations from the fit that excluded entrainment was compared to simulations with entrainment.

In a couple of places some fairly arbitrary adjustments were made with inconclusive results. For example, in lines 319-322 the authors describe a test where the aerosol concentration imposed on the parcel model is arbitrarily reduced by 50% based on the notion that the aerosol concentration in the cloud layer of a decoupled boundary layer is likely to be less than what was measured at the surface. Yet the the change resulted in little change in the cloud-top radiative flux. How do the authors reconcile the small change in radiative flux for such a larger perturbation of the imposed aerosol concentration with their ultimate conclusion that the main source of error in their bottom-up radiative closure for the decoupled boundary layer cases is the lack of measurements to constrain the CCN concentration in the decoupled cloud layer?

Figure 9 shows the OPC concentration reduced by almost 50% in decoupled layer (compared to the surface based mixed layer), though this is not the same case. The choice of 50% was loosely based on this given there were no other measurements to base this choice on. We have now referred to Figure 9 in the text:

“ACPM simulations were conducted using aerosol concentrations based on the approximate average decoupled to coupled aerosol concentration ratio (50%, Figure 9) to estimate the difference in shortwave radiative flux. “

Previous literature has shown there are cases were CDNC is sensitive to aerosol concentration (aerosol limited) while others are sensitive to updraft velocity (updraft limited). The manuscript discusses the results of decreasing the aerosol concentrations in simulations of both the D05Sc and D06Cu cases. The D06Cu case which has a large range of updraft velocities (0-1.6 m/s) had significantly fewer (42%) CDNC after reducing the aerosol concentration. The D05Sc has significantly lower updraft velocities, ranging from 0-0.3 m s-1, and therefore, is updraft limited. The CDNC is very sensitive at these low updraft velocities, so it is likely that the combined modeled updraft resolution of 0.1 m s-1 and error in updraft velocity measurements is the cause for the large error in shortwave radiative forcing (δRF) of 33 W m-2 (Table 2) for the D05Sc case, after accounting for cloud top entrainment.

The following text has been changed to incorporate this information:

"For the D05Sc case, simulations with 50% decreased cloud-base aerosol concentrations show only slight differences in δRF of 2 Wm^{-2} and decreases in CDNC of 10%. The decrease in aerosol concentration resulted in increased supersaturation due to the low water uptake from fewer activating droplets. The increased supersaturation caused smaller aerosols to activate (Raatikainen et al., 2013) and therefore, little change in CDNC. The D05Sc case has very low updraft velocities (0-0.3 m s^{-1}). At low updraft velocities, the CDNC is often updraft limited (Reutters et al., 2009). This means the CDNC is very sensitive to the updraft velocities and less sensitive to aerosol concentration. Small errors in updraft velocity and low modeled updraft resolution (0.1 m s^{-1}) likely contributes significantly to the error in this case. The D06Cu was not influenced as much by low water uptake because the CDNC was much higher at 171 cm^{-3} compared to 86 cm^{-3} for D05Sc. The D06Cu the CDNC decreased by 42% and δRF decreased by 18 Wm^{-2}. The updraft velocity range for the D06Cu case is significantly higher than the D05Cu case (0-1.6 m s^{-1}). The higher velocities for the D05Sc and greater sensitivity to aerosol concentration suggest this case is aerosol limited (Reutters et al., 2009). Both decoupled cases still have a δRF greater than the coupled cases."

For the D06Cu case, the 42% decrease in CDNC, significantly reduced δRF from 74 to 56 w m-2. A δRF of 56 w m-2 is still high compared to the decoupled cases. It is possible that the difference in aerosol concentration between the coupled and decoupled boundary layer is greater than 50%. We do not have aerosol concentration measurements in the decoupled layer for this case. Also, it is possible that this case experienced some cloud top entrainment. The measured lapse rate for this case was slightly higher (0.1 K km-1) than the adiabatic lapse rate, however this was within instrument error, so cloud top entrainment was not explored. If the heating is offset by long wave cooling (not considered in this paper), then the effect of entrainment may be significant. Note, the two entrainment cases studied both had measured lapse rates that were 1 K km-1 higher than the adiabatic lapse rate.

The following text has been changed to incorporate this information:

"The UAV observations show that both C11Sc and D05Sc have sub-adiabatic lapse rate measurements, compared to simulated moist-adiabatic lapse rates within the cloud (Table 2). The difference between the observed and simulated lapse rates therefore suggests a source of heating in the cloud. The sub-adiabatic lapse rate is attributed to cloud-top entrainment by downward mixing of warmer air at cloud-top. The D06Cu case has a slightly sub-adiabatic observed lapse rate (Table 2), however the difference with respect to an adiabatic lapse rate is within instrument error. For this reason, cloud top entrainment is not explored for this case, though it may contribute slightly to the error."

In the conclusion it is stated that cloud-top entrainment is only observed on 2 out of 13 flight days, and a decoupled boundary layer on only 4 of 13 flight days. It might be valuable to include this in the abstract. While reading the paper, I was struggling to understanding the broader implications. Is there sufficient data to draw a tentative conclusion about the overall sign and/or magnitude of errors in bottom-up forcing calculations based on the surface station data at this location? If this can be addressed in any manner by the authors, then the paper will have substantially greater importance.

After revisiting the statement (that cloud-top entrainment is only observed on 2 out of 13 flight days, and a decoupled boundary layer on only 4 of 13 flight days) we have decided to reworded this statement to more clearly what these statistics are based on:

"Based on airborne observations with UAVs, decoupling of the boundary layer occurred on four of the 13 flight days (two decoupled cloud cases were not discussed due to the lack of in-cloud measurements). However, cloud drop entrainment was only observed on two of those days, limited by the ability to make in-situ measurements. These measurements occurred during the summer, so additional measurements are needed to look at seasonal trends."

Because the entrainment statistic is limited by measurement capabilities we have decided not to include this in the abstract.

The main broader implications of these results are that cloud-top entrainment and decoupling of the boundary layer lead to over estimation of cloud-top shortwave radiative forcing when using the adiabatic and well mixed boundary layer assumptions, respectively. While we have indicated the magnitude of these errors for the cases presented, there are only a limited number of cases in this manuscript to draw statistics on the occurrence of these scenarios. In order have a Many more case studies are needed to conclude more specific implications for the Mace Head location. Furthermore, similar studies at other locations are necessary to understand global implications.