Response to Referee #1

I'd like to express my gratitude to the reviewer for providing thoughtful and insightful comments on this work, which have led to an improved paper.

Major comments:

1. The following discussion regarding the fixed number concentration has been added to Section 2:
   In this work the cloud droplet number concentration (CDNC) is constant. Observations of the shallow cumulus case described in Section 2.1 show an approximately constant droplet concentration with height (vanZanten et al. 2011). Slawinska et al. (2012) demonstrated the reason behind the observed constant CDNC being due to significant in-cloud activation of cloud condensation nuclei. Using a bin microphysics LES, Wyszogrodzki et al. (2013) showed that while CDNC were constant with height for the majority of occurrences in their simulations, there is variability in the CDNC fields. Therefore, while the use of a constant CDNC is a good assumption, variations in CDNC will likely affect the development of precipitation and this will not be captured in the simulations presented in this work.

2. The discussion on neglecting the effects of turbulence on the collision efficiencies has been expanded to read:
   Limited data are available for the effects of turbulence on collision efficiencies.Currently the DNS data only provide two data points, for dissipation rates of 100 and 400 cm^2s^{-3} (Wang et al. 2008). To include the turbulent collision efficiencies in this work would require them to be extrapolated out to dissipation rates of 1500 cm^2s^{-3}. The collision kernel results show that the effects of turbulence do not scale linearly with dissipation rate and two data points does not provide enough information to represent this process with any certainty for the high dissipation rates. Therefore, the decision was made not to include the turbulent collision efficiencies until more DNS data become available.

3. Wyszogrodzki et al. (2013) speculated that the dynamical enhancement was due to the off-loading of greater rain water contents acting to increase the buoyancy, but they did not analyse the buoyancy fluxes. In this work the buoyancy is examined and shows that in fact the greater rain water does not increase but rather decreases the buoyancy due to the increased water loading. Further analysis has been undertaken that examines the cloud properties in the cloud cores – the cloudy regions that are positively buoyant. The following discussion and figure have been added:
   To further examine the buoyancy characteristics of the clouds and estimate the entrainment rates, the conditional averages of vertical velocity, total and liquid water contents within cloud cores are analysed. Cloud cores are defined as the cloudy regions that have positive buoyancy as compared to the slab average. Figure 6a shows that the simulation with the turbulent microphysics has a larger area of cloud cores throughout the mid and upper cloud levels as compared to the simulation with the non-turbulent microphysics. However, comparing these profiles to Fig. 3e, we see that the turbulent microphysics case has a smaller proportion of positively buoyant cloud regions in the levels above 1300 m. The average vertical velocities in the cloud core are very similar in the simulations with the non-

turbulent and turbulent microphysics schemes, with the turbulent case having slightly weaker updrafts in the upper cloud core levels. This result together with the vertical velocities averaged over all cloudy regions illustrated in Figure 5f, shows that the turbulent microphysics simulation has increased vertical velocities in the cloudy regions that are not positively buoyant. This demonstrates that in this simulation it is not the reduced water loading associated with greater precipitation that acts to increase the buoyancy and hence the vertical velocities. Figure 6c shows that the turbulent microphysics simulation has larger average total water contents in the cloud core upper levels and this applies to the cloud liquid water as well (Fig. 6c). Diagnosing the mass flux and fractional entrainment rates using equations 11 and 16 of Stevens et al. (2001) and the total moisture mixing ratio, shows that the mass flux in the upper levels is larger for the turbulent microphysics simulation (Fig. 6d) and this is due to the greater area of the cloud cores in this case. The turbulent microphysics simulation has a smaller entrainment rate throughout the vertical compared to the simulation with the non-turbulent microphysics parameterisations, in agreement with the larger water contents in the simulation that includes the effects of turbulence on the droplet collision rates. Note that the application of the mass flux approach with a simple entraining plume model breaks down in the inversion at about 2 km (Siebesma et al. 2003) and explains the sharp gradient in Figure 6f.

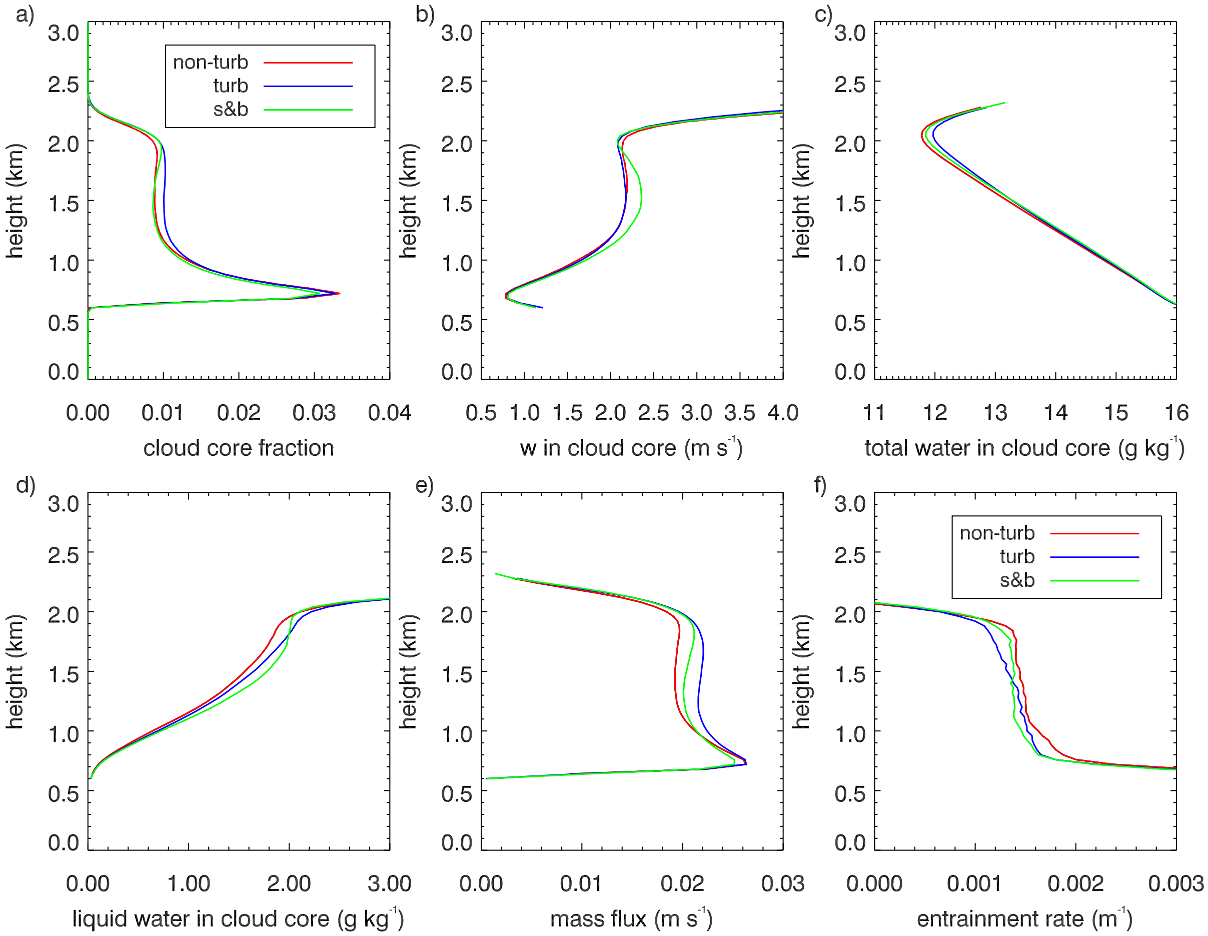

Figure 6. As in Fig. 3 except for, a) cloud core fraction, b) conditional average of vertical velocity inside cloud cores (m s^{-1}), c) conditional average of total water inside cloud cores (g

kg^{-1}), d) conditional average of cloud liquid water in cloud cores (g kg^{-1}), e) mass flux (m s^{-1}), and f) entrainment rate (m^{-1}).

Further to this, an additional comment is made in the summary section regarding the effects of refined resolution on entrainment. Matheou et al. (2011) found that the negative buoyancy surfaces that occur at the cloud-environment interface are unresolved for typical LES resolutions and discussed the impact that this may have on modelling the entrainment process.

4. Comparing the non-turbulent and turbulent microphysics results from both case studies shows that the rain water differences are statistically significant for the RICO case, with the mean rain water paths more than 1 standard deviation apart. For the DYCOMS II case it is the different CDNC that produce statistically significant differences in rainfall, not the inclusion of the turbulence effects. Therefore, for these two cases we find that turbulence has a larger effect than cloud droplet number concentrations on shallow cumuli, however, it is the opposite for the stratocumulus with CDNC having the largest control on the rain water. These statements been added to the abstract, results and summary sections.

   While the RICO case is non-stationary, the thermodynamic profiles between the simulations are very similar. If rather than calculating the rain water path statistics over the last 4 hours (mean and standard deviation of the rain water path over the last 4 hours for the turbulent and non-turbulent microphysics simulations are 7.9 ± 3.3 and 1.9 ± 0.9) and instead different time periods are used that correspond to the same inversion heights (a slightly shorter time period for the turbulent microphysics simulation), the results hardly change with the turbulent rain water path average equal to 7.8 ± 3.4. This is to be expected as the averaging period of 4 hours was chosen to be much longer than the time periods of the precipitating clouds or cloud clusters that give rise to the large variability, which is on the order of tens of minutes to an hour (vanZanten et al. 2011).

   The revised figures (10 and 11) have the standard deviations plotted on the figures. The other figures only include the standard deviation if the results are statistically significant and the means lay outside of the ± 1 standard deviation (rain water and rain water evaporation for the shallow cumuli case). Throughout the paper the results are discussed in terms of the variability and statistical significance, or lack thereof, and we note that the large variability is in agreement with other studies that have investigated the effects of aerosol concentrations on cloud properties (e.g. Xue and Feingold 2006).

Specific comments:

1. This has been corrected to read liquid water potential temperature. The approach used in the UCLA-LES is consistent with the approximation of most LES models that are used to study both precipitating and non-precipitating clouds. Stevens et al. (2005) describe that all of the 10 LES models participating in that particular intercomparison study use liquid water potential temperature as a prognostic variable.

2. The two panels in each of Figures 1 and 2 have been replotted to be larger in size and run vertically in the revised version. The labels on all of the figures have been increased in size for readability.

3. This has been rewritten to read 6.6 km square.

4. The comparison with the other microphysics schemes is now addressed for each of the case studies, as well as in the summary section. It is well established that different microphysics schemes produce distinctly different cloud properties so the fact that we see this result is not surprising. This is why the focus in this present work is on the comparison between the two Franklin (2008) schemes that are derived in the same manner except for one including the effects of turbulence on droplet collisions, and the other not including these effects.

5. The figures referred to (10 and 11 in the revised paper) have been replotted so that the larger symbol refers to the lowest cloud droplet number simulation and the +/- standard deviation has been included for each point. The liquid water in these figures is both cloud and rain.

Response to Referee #2

I'd like to express my gratitude to the reviewer for providing thoughtful and insightful comments on this work, which have led to an improved paper.

Major comments

1. The parameterisations of Franklin (2008) only consider the effects of turbulence on small collector cloud droplets with radii between 10 and 30 microns. This is different to the study of Ayala et al. (2008) (the work which the Wang-Ayala parameterisation is based on) who consider droplets up to 60 microns in radius. For the small droplets considered in Franklin (2008) it is the dissipation range turbulence that governs the droplet motion (e.g. Wang and Maxey 1993) and, therefore, the dissipation rate is the dominant flow property that determines the collision rate, with the Reynolds number effect of significantly less importance. This is clearly illustrated in Figure 4. of Ayala et al. (2008) who show that Reynolds number effects are only apparent for droplets of radius 40 microns and larger, which are larger than the size of droplets considered in Franklin (2008). This result is also described in Wyszogrodzki et al. (2013) who state that small drops with radius less than 30 microns are not affected by the root mean square velocity, or Re_λ. This is the reasoning behind the parameterisations of Franklin (2008) being a function of the dissipation rate only, where equation (4) of Franklin (2008) has been used to eliminate Re_λ. An additional discussion on why the dependence on dissipation rate only is an appropriate assumption is included in the revised paper.

   Making an equivalent non-dimensional equation for equation (4) of Franklin (2008) for Re_λ is trivial. One only needs to divide by a reference value of the dissipation rate, for example we could rewrite this as $Re_\lambda = 2100\left({}^{\varepsilon}/_{\varepsilon_0}\right)^{0.12}$ where ε_0 is taken to be 100 cm^2s^{-3}. In fact this approach is taken by Seifert et al. (2010), who use the Wang-Ayala parameterisation to generate an autoconversion scheme for their bulk model that includes the effects of turbulence. Although the derivation of their autoconversion scheme is very different to that used here, they take the same approach in the sense that the dissipation rate and Re_λ are functionally dependent in their calculation of the Reynolds number from the LES flow field, which means that turbulence is only characterized by one of these quantities, the dissipation rate. The parameterized collision kernel that is used to develop the autoconversion parameterisation was shown by Franklin (2008) to represent the DNS data well for the much wider range of dissipation rates used in this work as compared to others (between 100 and 1500 cm^2s^{-3}). The kernel parameter in Seifert et al. (2010) includes a linear dependence on the dissipation rate, which is shown by Franklin et al. (2007) and Ayala et al. (2008) to be an incorrect assumption, with the turbulent enhancement of the collision kernel showing a strong non-linear dependence, increasing as the dissipation rate increases. This dependence is captured in the parameterisations used here.

   It is also worth noting that in the work of Seifert et al. (2010) and Wyszogrodzki et al. (2013), the representation of the turbulent enhancement of the collision efficiency is a linear function of dissipation rate only, there is no dependence on Re_λ .

2. The cloud water evaporation rate is not a function of the cloud droplet number concentration as the reviewer rightly points out. However, the rain water evaporation is a function of both the mass and number concentration of rain water. Including the standard deviation in Figure 9 shows that the variability of the liquid water path is much larger than the difference between the simulations with differing CDNC. Therefore, the argument that the nonmontonic behavior is a statistical artifact rather than a physical one is correct and this point is made in the revised manuscript.

3. The general point here ties in with a couple of Reviewer 1’s comments. The differences between the stratocumulus simulations are not statistically significant and this is shown explicitly in the figures that have been replotted to include the standard deviations about the mean. Discussions on the lack of statistical significance and the difference between the bulk schemes are included in the revised manuscript. It is also noted that for the two cloud regimes studied we find that turbulence has a larger effect than cloud droplet number concentrations on shallow cumuli, however, it is the opposite for the stratocumulus case with CDNC having the largest (statistically significant) control on the rain water. These comments have been added to the abstract, results and summary sections.

4. It is important to note that while an older version of the model has been used, the correction to the calculation of the dissipation rate of TKE (which was in error in version 1.1) has been included in all of the simulations. The aim of the work presented in this paper is to compare simulations that use two microphysics schemes that have been developed in exactly the same manner except for the inclusion of turbulent effects on the collision kernel in one of the schemes. To achieve this goal the model version used is not important as long as the model and the simulation set up are the same. The reviewer’s idea of comparing two different bulk turbulent schemes (i.e. Franklin (2008) and Seifert et al. (2010)) is a great suggestion for the next step in this work.

Minor comments:

1. This study is now mentioned in the introduction, thank you for the reference.

2. This spelling mistake has been corrected.

3. The value of the cloud-rain water threshold radius used in the simulations is 40 microns for the Seifert and Beheng scheme and the two Franklin schemes. This is different to that used by Savic-Jovcic and Stevens (2008) and is clarified in the text.

4. The reason for using Khairoutdinov and Kogan for the DYCOMS-II case is because this scheme was developed based on large-eddy simulations of stratocumulus and is widely used to model this particular cloud type. Because this scheme was developed for stratocumulus, it neglects processes that are important for shallow cumuli such as self collection and, therefore, we do not use this scheme for the RICO case.

Response to Referee #3

I'd like to express my gratitude to the reviewer for providing thoughtful and insightful comments on this work, which have helped to improve the paper.

Specific comment:

The parameterisations of Franklin (2008) only consider the effects of turbulence on small collector cloud droplets with radii between 10 and 30 microns. This is different to the study of Ayala et al. (2008) (the work which the Wang-Ayala parameterisation is based on) who consider droplets up to 60 microns in radius. For the small droplets considered in Franklin (2008) it is the dissipation range turbulence that governs the droplet motion (e.g. Wang and Maxey 1993) and, therefore, the dissipation rate is the dominant flow property that determines the collision rate, with the Reynolds number effect of significantly less importance. This is clearly illustrated in Figure 4. of Ayala et al. (2008) who show that Reynolds number effects are only apparent for droplets of radius 40 microns and larger, which are larger than the size of droplets considered in Franklin (2008). This result is also described in Wyszogrodzki et al. (2013) who state that small drops with radius less than 30 microns are not affected by the root mean square velocity, or Re_λ. This is the reasoning behind the parameterisations of Franklin (2008) being a function of the dissipation rate only, where equation (4) of Franklin (2008) has been used to eliminate Re_λ. An additional discussion on why the dependence on dissipation rate only is an appropriate assumption is included in the revised paper.

The approach of relating the dissipation rate to Re_λ is also taken by Seifert et al. (2010), who use the Wang-Ayala parameterisation to generate an autoconversion scheme for their bulk model that includes the effects of turbulence. They take the same approach in the sense that the dissipation rate and Re_λ are functionally dependent in their calculation of the Reynolds number from the LES flow field, which means that turbulence is only characterized by one of these quantities, the dissipation rate. The advantage of the current approach is the wide range of dissipation rates used in the DNS, ranging up to 1500 cm^2s^{-3}, which is much larger than the rates considered by others. The kernel parameter in Seifert et al. (2010) includes a linear dependence on the dissipation rate, which is shown by Franklin et al. (2007) and Ayala et al. (2008) to be an incorrect assumption with the turbulent enhancement of the collision kernel showing a strong non-linear dependence, increasing as the dissipation rate increases, which is captured in the parameterisations used here.

It is also worth noting that in the work of Seifert et al. (2010) and Wyszogrodzki et al. (2013), the representation of the turbulent enhancement of the collision efficiency is a linear function of dissipation rate only, there is no dependence on Re_λ .

In addition, it is important to note here the reasons for the difference between the results of Ayala et al. (2008; which I think are the red circles in the figure provided in this comment) and Franklin et al. (2007): different schemes used to force the large-scale flow and different values of viscosity and density ratio between air and water. Ayala et al. (2008) noted the role that the different forcing schemes (stochastic in Ayala and deterministic in Franklin) may have in generating differences in the turbulent collision kernels in the two studies. Kunnen et al. (2013) use a different approach whereby turbulence is synthetically generated and they show good agreement with the results of Franklin et al. (2007). Kunnen et al. (2013) also used similar values of the momentum viscosity coefficient and density to Franklin et al. (2007), those that correspond to typical atmospheric warm cloud conditions.

As noted in the final summary section of the paper, there needs to be further work done to improve the existing parameterisations that include the effects of turbulence on cloud droplet collision rates. The work by Rosa et al. (2013) shown in this comment is an important step towards that goal.