Reply to anonymous Referee #2

We thank the reviewer for his/hers comments and remarks. Reviewer's comments are listed below in black, our replies (R) are written in blue.

In this manuscript, the author analyse the effect of turbulent mixing on drizzle formation in stratocumulus using a Lagrangian-Eulerian model. They find that mixing has two opposing effects: first mixing delays the initial formation of drizzle drops by diluting high LWC parcels, but later mixing is essential to create an environment in which drizzle drops are able to develop further and therefore reach the sub-cloud layer.

The Lagrangian-Eulerian model is a great tool to analyse drizzle formation and I think that the manuscript can contribute to better understand the puzzling role of mixing in drizzle formation in Sc. However, I have two general comments which should be taken into account before publication.

General comments

I understand that the LEM, which is used for this study, has been developed and described in earlier papers and that turbulent mixing has been included as a process in the LEM by the same group of authors (Magaritz-Ronen et al. 2014). In that 2014 study, the authors simulate and analyse a different research flight (RF01 instead of RF07) from the same field campaign (DYCOMS-II). Although RF01 is a non-precipitating case and RF07 develops more pronounced precipitation, in their 2014 paper the authors already conclude that "turbulent mixing leads to an increase in the effective radius and facilitates and accelerates drizzle formation" (this is from the 2014 abstract). I think that the current analysis shows some new results compared to the 2014 paper, especially concerning the opposing effects of mixing. However, some of the analysis overlaps, e.g., Fig.6 in the 2014 paper and Fig.3 in this manuscript. Please point out more clearly, where the currents study builds on (or reproduces) results from the earlier study and where it contributes new insights. Please skip overlapping analysis if necessary.

(R) The previous paper (Magaritz-Ronen et al., 2014) concentrated on the affects of turbulent mixing on the averaged properties of the stratocumulus cloud. It discusses the overall structure and variability of different parameters such as LWC, concentration and temperature inside the cloud. In the simulation presented in the paper it was seen that more large drops form more rapidly and that the effective radius seen in the cloud layer was higher in the case turbulent mixing was included. These results indicated that turbulent mixing and entrainment have an effect on the drizzle formation process in the cloud.

The current paper is a continuation of that work and aims at understanding the mechanism leading to the enhanced drizzle formation in the mixing case. Figure 3 has been removed.

In the manuscript, a collision parameter is defined as $N^2 r_e^5$. In the stochastic collection equation, the collision rate depends on the droplet concentration, the size of the droplets and on the velocity difference of pairs of droplets. Assuming that drops fall with terminal fall velocity, the velocity difference can be related to a size difference. Therefore, I would expect that a collision parameter should be highly sensitive to the DSD width, which characterises droplet size differences. However, in Fig.11 there is only a small dependence of the collision parameter on the spectrum width. Please discuss, how and why you define the collision parameter as you did. Which assumptions are in the formulation? Why does it not depend on the DSD width? How does the formulation effect the interpretation of the results?

(R) Collisions are described using the stochastic equation for collisions. The rate of collision is determined by the concentration of the colliding drops (N^2) and the collision kernel. In a study by Freud and Rosenfeld (2012) analysis of data from many field campaigns showed that the collision kernel is proportional to $\sim r_v^5$ ($r_v^{4.8}$). The relationship between the rate of collisions and the DSD width is discussed. It is shown that increase in the DSD width does not automatically lead to increase in the rate of collisions. Moreover, in our particular cases, DSD broadening due to mixing leads to decrease in the rate of collisions. The corresponding comments are included into the revised paper.

Freud, E. and Rosenfeld, D.: Linear relation between convective cloud drop number concentration and depth for rain initiation, *J. Geophys. Res. Atmos.*, 117(D2), D02207, doi:10.1029/2011JD016457, 2012.

Specific comments

1. Throughout the text, e.g., p.24132, l.20: If several references are listed to support one statement, they are usually sorted by year, not alphabetically.

(R) Corrected

2. p.24133: The first paragraph is hard to read because it jumps between different topics, please restructure. Maybe have two paragraphs: one about the processes that foster drizzle, and one about the difficulties of LES to simulate drizzle.

(R) The paragraph has been rewritten

3. p.24135, l.3: Is the model version used in this study exactly the same as described in Magaritz-Ronen et al. (2014)? Or are there differences to that version?

(R) The version used in this study is the same as the one used in Magaritz-Ronen et al., (2014), only different simulations are used.

4. p.24135, l.5: 2D turbulence is known to have a quite different structure from 3D turbulence. (See, e.g., Stevens, B., Feingold, G., Cotton, W. R., and Walko, R. L. (1996). Elements of the microphysical structure of numerically simulated nonprecipitating stratocumulus. Journal of the atmospheric sciences, 53, 980-1006.) What are the limitations of using a 2D model? What might be the effect on the results?

(R) The statistical properties of the simulated velocity field are as in 3D turbulence. For instance, the velocity field obeys the -5/3 law and as such we do not simulate 2D turbulence. Similarly, mixing between parcels is based on the Prandtl approach also developed for 3D turbulence.

The utilization of 2D geometry for the model means that the convective (large eddies) structure represent roll vortices elongated along the background wind. A great number of studies showed that such a structure provides the same heat fluxes and has the same critical Rayleigh numbers as in 3D geometry. At the same time, 2D geometry allowed us to use high resolution, and to use very accurate description of microphysical processes needed for description of such a fine feature as drizzle formation.

5. p.24136, l.6: Does the formulation of Pinsky et al. 2001 include turbulent enhancement in the collision efficiency? If so, I think it would be worth to state that here. If not, what is the effect of neglecting turbulent enhancement?

(R) No, the tables of the collision efficiency presented by Pinsky et al. (2001) present gravitational collision efficiencies. Our estimations showed that low dissipation rates used in the model do not lead to any significant increase in the collision rate. The turbulent-induced increase in collision rate in Sc clouds supposedly takes place in zones of imbedded convection. However, in this paper the turbulence-like wind field does not contains such convective elements. We showed in the study that even gravitational collision kernels can lead to drizzle formation and reasonable drizzle fluxes.

6. p.24136, l.21: In that formula, why is K a function of l? Is ε a function of l? Later, in section 3 it is said that ε is set constant (in the BL).

(R) We used the Richardson law to describe the turbulent coefficient. In the equation $K(l) = C\varepsilon^{1/3}l^{4/3}$: the turbulent dissipation rate ε is constant in the BL, and set at the beginning of the simulation; l is the distance between Lagrangian parcel centers and changes as the parcels are advected in the computational domain. The increase in K with increase in mixing length can be explained by the fact that at larger distances, turbulent vortices of larger size can participate in the mixing. The utilization of the Richardson law does not mean the increase in turbulent fluxes and the fluxes are determined by spatial gradients of the variables. The gradients decrease with the increase in the distance.

7. p.24136, last paragraph: Do inconsistencies arise from those "two kinds" of diffusional growth?

(R) There are difference between resolved and subgrid processes of diffusion growth. Cloud resolved diffusion growth takes place in a parcel that ascends or descends with time. This process is accompanied by transport of air mass (parcels) upward or downward. Subgrid (small scale turbulent) diffusion growth has no net updraft or downdraft. It represents small scale fluctuations up and down. It is equivalent to introduction of small-scale fluctuations of vertical velocity around the mean updraft or downdraft.

8. p.24137, l.3: Is SST fixed? At what value?

(R) Yes. SST is set to $19°C$

9. p.24137, l.11: Please add references here or skip that sentence.

(R) Removed.

10. How long is the simulation? What is the timestep?

(R) The simulation is for 8 hours. There are several timesteps used in the model. For diffusion growth a small time step of 0.01s is used, collisions and mixing is calculated using 1s time step.

11. It might be my personal taste of style so please ignore this comment if you feel strongly about it: I think figure caption should not be repeated in the text, e.g., Fig.3. on p. 24140, l.9-13 or Fig.11 on p.24146, l.8-11. Skipping them would shorten especially section 4, which is sometimes a bit lengthy to read.

(R) Accepted, changed throughout the text

12. p.24140: This paragraph is hard to follow. At several points I am not sure how the sentences are relating to each other: l.7 what kind of changes in the variable field? l.17 How does homogenisation making the processes (which?) adiabatic? l.17 Which two limits? Cloudy and inversion layer from two sentences ago? l.20 What do you mean by magnitude of q_t and θ_l?

(R) The paragraph has been rewritten.

13. p.24144, l.14-15: I think the statement of the second half of the sentence is too strong. Looking at the last panel of Fig.7a, it is not the parcels with the highest initial humidity that have the highest LWC, but the parcels with the highest LWC that preferable start from the highest initial humidity.

(R) Agreed, not all parcels with high humidity will have maximum LWC. Corrected in the text.

14. p.24144, l.24: What is "the maximum value of the DSD"? Maximum re?

(R) It is the peak of the distribution, corrected in the text.

15. p.24145, l.3: It would be interesting to see how much LWC a parcel loses through sedimentation. Looking at Fig.8b the contribution might be small.

(R) Sedimentation is mostly effective for the larger drops at the right tail of the DSD, and yes, the fraction of LWC lost should not be very large.

Most of the drops are lost by sedimentation at the next stage of cloud development when drizzle falling from above collects smaller droplets in parcels located below.

16. p.24146, l.22-23: This sentence does not makes sense to me. Why would drizzle drop formation continue because something has happened before?

(R) The sentence was unclear and has been rewritten. If in the parcel that mixes with the inversion air collisions were already sufficiently strong, larger drops will be present. In this case efficient collision can continue also after the parcel mixes with the dry air and the smaller drops in the spectrum evaporate.

17. p.24150, l.25-27: This paragraph/sentence appears somehow unrelated. Please skip or relate to the following text.

(R) Removed

18. p.24151, l.2-17: The recirculation of aerosols and the importance of large aerosols for the drizzle drops seems somewhat speculative to me. Please back this up with analysis or skip it.

(R) The full analysis of this mechanism and its effects on the cloud microstructure has been previously presented in Magaritz et al., (2010). In the study it was shown that collisions also lead to larger aerosol particles in the cloud layer and that subsequently the largest drops contain these large aerosols.

Magaritz, L., Pinsky, M. and Khain, A.: Effects of stratocumulus clouds on aerosols in the maritime boundary layer, Atmos. Res., 97(4), 498–512, doi:10.1016/j.atmosres.2010.06.010, 2010.

19. p.24151ff: Section 5 is rather a summary of section 4 than a discussion. Although I like the conclusion section 6 in its current (concise) form, please think about combining section 5 and 6 (or section 4 and 5).

(R) Accepted

20. p.24152, l.10-18: The comparison of Sc and Cu appears here out of the blue. I would recommend to skip it because Cu are not the topic of the manuscript. If you want to keep it, a thorough discussion of literature on lucky parcels in shallow cumulus is needed (e.g. studies by Lasher-Trapp, Cooper, etc.).

(R) The comparison has been removed

21. p.24153, l.29f: Larger compared to what? Fig. 13 does not show spectrum width.

(R) In the cloud layer, larger values of spectrum width are seen in CON case than in the NoMI. This is seen in fig. 11 (corrected in the text)

22. Fig.1: Please use the same color scale to make the figs comparable.

(R) Corrected

23. Fig.2: Labels of the x-axis are wrong. Is concentration the concentration of cloud droplets?

(R) Labels corrected. The concentration is total droplet concentration (added)

24. Fig.3: Why do you show data for different height layers from the model and the observation?

(R) The figure has been removed.

25. Several figure (e.g., 2, 3, 5, ...) show model data from different point or periods in simulation time. For what reason did you chose those (different) time frames? It seems a bit arbitrary to me.

(R) There are many processes described in the model which affect the microphysical structure of the cloud and the investigated mechanisms described in the paper are at times masked by one another. The time steps selected for the different figures are the ones in which the discussed affect is most clearly presented.

26. Throughout the text and e.g. in Fig.6 and Fig.7: Is humidity and qt the same in manuscript? Please clarify.

(R) Humidity in figures 6 and 7 (and in the text) refers to the water vapor in the parcel. q_t is the total water mixing ratio and is the sum of the humidity and LWC. q_t is a conservative value in adiabatic processes and its average profile is constant in the BL.

27. Fig.8b: Please explain the y-axis. M is never mentioned.

(R) This figure presents the mass distribution in the parcel. Changed to LWC.

28. Fig.11: How do you calculate the spectrum width?

(R) The spectral width here is the standard deviation of the droplet size distribution.

Technical corrections

1. p.24132 l.25: comparatively to what? - *corrected*

2. p.24134, l.12: of the droplet size distribution - *corrected*

3. p.24137, l.3: Lagrangian-Eulerian Model (LEM). - *corrected*

4. p.24137, third paragraph: Stay in present tense for the model description. - *corrected*

5. p.24139, l.29: model by using - *corrected*

6. p.24140, l.13: Concentration of cloud droplets? - *Yes, corrected in the text*

7. p.24142, l.11: Insert a paragraph break here. - *corrected*

8. p.24143, l.5: layers - *corrected*

9. p.24143, l.16: t = 150 min? - *corrected*

10. p.24144, l.27: g m−3 - *corrected*

11. p.24145, l.14: to investigate - *corrected*

12. p.24146, l.4: substantially, leading - *corrected*

13. p.24152, l.20-22: This sentence is grammatically not correct, please rephrase. - *corrected*

14. p.24153, l.23: as a result of - *corrected*

15. Fig.16: Add "in cloudy parcels" to the caption. - *corrected*