

## Reply to Anonymous Referee #1

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.

General comment: This manuscript describes a new modeling approach in order to understand the formation of drizzle in stratocumulus fields. A unique feature of the model is the possibility to follow individual cloud parcels in a Lagrangian sense and to analyze which are the favor conditions for the onset of drizzle production and the further drizzle dynamics until the drizzle drops partly reach the ground. Before I start with my comments I have to point out that my personal background are in-situ cloud experiments and, therefore, I cannot make detailed/specific comments on technical aspects of this kind of modeling.

My overall impression is that this manuscript provides unique details of the development of drizzle production in Sc and can provides insight in the favorable conditions under which drizzle formation takes place which is terrific. In particular the different contributions of turbulent mixing on this procedure is evaluated in an interesting and clear way. From my point of view the manuscript is clearly written although at a few places careful rewording is needed. Furthermore, the part about the aerosol in Sec 4.5 is interesting but a little bit separated from the previous parts. If the manuscript should be shortened I suggest skipping this paragraph.

I highly recommend this manuscript for publication in ACP after my (minor) comments - which are given below - are considered/discussed.

We thank the referee for the positive remarks and recommendation.

### Detailed comments:

Page 24134; Line 19: This sentence should be reworded – it is somewhat confusing. The two main questions are very important and maybe it is better to make shorter sentences.

**(R)** The sentence has been rewritten.

P24135; l 16: What metric is important from the second-order structure function? Do you assume inertial sub-range scaling and derive the energy dissipation? Be more precise here.

**(R)** The shape of the structure function characterizes the correlation properties of the turbulent velocity field. The relations between amplitudes of different harmonics were derived using the structure function. The structure function used in the model produces a  $-5/3$  energetic spectrum in the horizontal direction. Intensity of turbulence is evaluated using measured root mean square values of vertical velocity fluctuations. The dissipation rate is set constant using a typical value of  $10\text{cm}^2\text{s}^{-3}$ .

P 24135, l 22ff: Can you briefly describe the input aerosol size distribution and number concentration of CCN

**(R)** The aerosol distribution is described in the following section 3. It is derived from measurements with a total concentration of  $\sim 200\text{cm}^{-3}$  in the radius range of  $0.01 - 1.3\mu\text{m}$ . The distribution is presented in Magaritz et al. (2009).

Magaritz, L., M. Pinsky, O. Krasnov, and A. Khain (2009), Investigation of Droplet Size Distributions and Drizzle Formation Using a New Trajectory Ensemble Model. Part II: Lucky Parcels, *J. Atmos. Sci.*, 66(4), 781–805, doi:10.1175/2008JAS2789.1.

P 24136, l 22ff: 1st, during DYCOMS-II; was the energy dissipation rate measured? I assume you can estimate it at least as a mean value from the wind measurements? What was the value? A typo in Monin / Yaglom (see also reference list)

**(R)** In the model the dissipation rate has been set to a constant value below the inversion of  $10\text{cm}^2\text{s}^{-3}$  which is typical of maritime Sc (Katzwinkel et al., 2011). Above cloud top the dissipation rate in the model diminishes linearly to zero at the height of 1100m.

The reference has been corrected.

Katzwinkel, J., Siebert, H. and Shaw, R. A.: Observation of a Self-Limiting, Shear-Induced Turbulent Inversion Layer Above Marine Stratocumulus, *Boundary-Layer Meteorol.*, 145(1), 131–143, doi:10.1007/s10546-011-9683-4, 2011.

Page 24137, l 6&7: I have no idea what you mean with this statement – please explain.

**(R)** The LEM is a 2D model. The 2D computational area can be considered as a cross-section perpendicular to the large-scale flow within the boundary layer. The large eddies that are simulated in the 2D model can be considered as a cross-sections of a roll vortices that are known to be elongated along the direction of the background large-scale flow. Such consideration was used in many studies (e.g. Ginis et al. 2004; Shpund et al. 2012, 2014). Since background flow is directed perpendicularly to the 2D computational area and all derivatives along the direction of the flow are equal to zero, the existence of such wind does not change the structure of the velocity field within the computational area. At the same time, the background wind increases the fluxes from the surface. The effect was taken into account assuming that the background wind is 10m/s near the surface.

Ginis I., Khain, A. P., and Morosovsky E., 2004: Effects of large eddies on the structure of the marine boundary layer under strong wind conditions. *J. Atmos. Sci.*, 61, 3049-3063.

Shpund, J., M. Pinsky, and A. Khain, 2011: Microphysical structure of the marine boundary layer under strong wind and spray formation as seen from simulations using a 2D explicit microphysical model. Part I: The impact of large eddies. *J. Atmos. Sci.*, 68, 2366–2384.

Shpund, J. J. A. Zhang, M. Pinsky, and A. Khain, 2014: Microphysical Structure of the Marine Boundary Layer under Strong Wind and Sea Spray Formation as Seen from a 2D Explicit Microphysical Model. Part III: Parameterization of Height-Dependent Droplet Size Distribution. *J. Atmos. Sci.*, 71, 1914–1934

Page 24137, Line 13 ff: You mention that there is no large-scale subsidence in your model but then you argue that this subsidence sharpens the gradients? Maybe I misunderstood your statement – please clarify in the manuscript.

**(R)** The paragraph has been rewritten more clearly in the revised text. In nature, the gradients of humidity and temperature above cloud top are affected by large-scale air subsidence. Stratocumulus clouds can have been observed to have a strong temperature jump and also a more gradual change of temperature. In the model the slope of the dissipation rate profile in this area determines the gradient seen in the simulation. In the simulations presented in the paper, the inversion is not very strong and turbulence induced entrainment leads to an increase of cloud top height.

On page 24135 you mention that the longitudinal structure function is taken as input, on page 24137 (l 23) you take the lateral component?

**(R)** The input for the model is the lateral structure function taken from Lothon et al. (2005). Corrected in the revised paper.

Lothon, M., Lenschow, D. H., Leon, D. and Vali, G.: Turbulence measurements in marine stratocumulus with airborne Doppler radar, *Q. J. R. Meteorol. Soc.*, 131(609), 2063–2080, doi:10.1256/qj.04.131, 2005.

End of sec 3 on Page 24138: Up to here it is not clear to me if you take data from the cited literature as input for your model run or measured values. Where exactly does the mean dissipation rate mentioned on page 24138 (line 4) comes from? I suggest to change the reference "Siebert et al. 2006" to Siebert et al 2010, *JAS*, Statistics of small-scale velocity fluctuations and internal intermittency in marine stratocumulus clouds. The 2006 paper is about shallow cumulus clouds.

**(R)** The input for the model includes:

- The lateral structure function  $D_{NN}$  taken from observations (Lothon et al., 2005)
- The vertical velocity variance profile taken from observations (Stevens et al., 2005)
- Aerosol distribution taken from measurement data. This distribution is set equal for all parcel in the boundary layer at  $t=0$  min.
- Initial temperature and humidity profile selected to correspond to measured values once the boundary layer is well mixed.

The dissipation rate profile is set to a characteristic constant value in the boundary layer and sharply decreases above cloud top height to zero.

Reference changed to Siebert et al, 2010.

Fig 1: Axis labels are weak, mark the two discussed positions. I suggest for the x-axis: “x / m” and for y-axis “z / m”. Can you include up- and downdrafts into Fig. 1 Also the adiabatic LWC should be included for reference.

**(R)** The figure has been corrected

Page 24139, line 24: Is it helpful to include a figure to illustrate this feature?

**(R)** Figure 2 has been replaced and now includes the temperature and total water mixing ratio profiles as well.

Page 24139, line 26: "minor underestimation of temperature and humidity gradients"- Can you provide numbers? What is "minor"?

**(R)** Figure 2 now presents the vertical profiles allowing to evaluate the underestimation of the temperature and humidity gradients near the upper cloud boundary. As was stressed in the paper as a response to this comment, the gradients depend on the assumption of the height dependence of dissipation rate within inversion just above cloud top. Supplemental simulations indicate that comparatively low sensitivity of results to this choice. At the same time, we wanted to investigate effects of mixing at the cloud base and decided to, supposedly, overestimate the rate of such mixing by the choice of linear profile of dissipation rate within the inversion layer. Note that in many observations including RF03 in DYCOMS-II, the gradients of T and q are similar to those simulated by the model. As such our choice can be considered realistic and typical for these clouds.

Page 24140, 15 ff: I feel that a somewhat more detailed explanation of the Paluch diagram would help the reader to follow your arguments.

**(R)** Since the Paluch diagram was presented and discussed by Magaritz-Ronen et al. (2014), Figure 3 has been removed in the revised manuscript

In line 9 you write that the data is for cloud top but in the next sentence you write that it is for "in and near" the interfacial layer - this is confusing and I suggest to be more precise in your wording.

**(R)** As mentioned above, Figure 3 has been removed in the revised manuscript

Page 24140, Line 19: Can you quantify this statement? What does the slope exactly tells me about the mixing process?

**(R)** As mentioned above, Figure 3 has been removed in the revised manuscript

Page 24141, 13: Please provide numbers, what are large droplets?

**(R)** Large droplets are larger than 30  $\mu\text{m}$ .

Page 24141, l 18ff: I understand that larger droplets close to cloud base can be a result of downward mixing of large droplet originally formed in cloud top regions but what is the effect of the ascending volume (line 18)? Maybe just a misunderstanding but please clarify.

**(R)** We clarify the sentence in the revised text. DSDs in ascending and descending parcels are not symmetrical (Pinsky et al., 2013), and larger drops can be found in descending parcels when they reach cloud base. The mechanism suggested by Korolev et al. (2013) proposes that these larger droplets near cloud base can laterally mix into ascending parcels at cloud base. The ascending parcels will have a wider DSD and will be more likely to produce large drops through collisions. In the model results (Fig. 3) we point to the existence of larger values of  $r_e$  near cloud base in the mixing case which could point to this mechanism. In addition, we stress the role of droplet collisions that increase the aerosol size within the drops. As a result, parcels containing larger haze particles at cloud base have more intense collisions.

Korolev, A., Pinsky, M. and Khain, A.: A New Mechanism of Droplet Size Distribution Broadening during Diffusional Growth, *J. Atmos. Sci.*, 70(7), 2051–2071, doi:10.1175/JAS-D-12-0182.1, 2013.

Pinsky, M., Mazin, I. P., Korolev, A. and Khain, A.: Supersaturation and Diffusional Droplet Growth in Liquid Clouds, *J. Atmos. Sci.*, 70(9), 2778–2793, doi:10.1175/JAS-D-12-077.1, 2013.

Page 24142, l 5: This sentence is difficult to understand, I suggest rewording.

**(R)** The sentence has been rewritten.

Page 24142, l 27: I like the phrase "lucky parcel" but I remember a paper by Alex Kostinski about "lucky droplets" and at some place you should definitively cite and discuss this paper in depth. Alexander B. Kostinski and Raymond A. Shaw, 2005: Fluctuations and Luck in Droplet Growth by Coalescence. *Bull. Amer. Meteor. Soc.*, 86, 235–244. doi: <http://dx.doi.org/10.1175/BAMS-86-2-235>

**(R)** Yes, Kostinski and Shaw (2005) introduced the concept of lucky drops. Droplets in clouds experience different amount of collisions. Some rare droplets experience more collisions which result in formation of a small number of drizzle (or rain) drops. The formation of such lucky drops is a fully stochastic, random process.

However, lucky parcels (or lucky cloud volumes) are those which have favorable conditions for formation of largest LWC and drops. Formation of lucky parcels is a deterministic process, and we can predict which volume will become "lucky".

Page 24143, l 7: Maybe you could show the velocity field for a certain time step?

**(R)** The velocity field has been added in figure 1.

Page 24143, l 8ff: this sentence is quite complicate; do you just consider the integral time scale?

**(R)** In Magaritz-Ronen et al. (2014) the lifetime of a single parcel is determined by the time it takes a conservative property in the air volume (total water mixing ratio) to become equal to that of the air volume's environment value. It was found that this life time is on the order of 15-20 min. The sentence has been rewritten more clearly in the revised paper.

Page 24143, l 14ff: Does this make sense? You consider the ratio of ql at 150 min and 140 min but take the location at 15 min - probably a typo and you mean 150 min?

**(R)** Corrected

Page 24144, l 8: What do you mean with "..a slope forms.."?

**(R)** The sentace was unclear, corrected

Page 24145 l 1: This sentence is somehow incorrect..

**(R)** Corrected

Page 24145, l 20: Can you calculate the ratio of the droplet concentration in the considered height versus the cloud base which should be a better parameter showing how adiabatic the considered parcel is?

**(R)** The adiabatic properties of the parcels in zone 1 are demonstrated by the almost constant droplet concentration. Parcels near cloud base (~400 m, blue color) have the same concentration as parcels near cloud top (~700 m, yellow).

Page 24146, l 4ff: At this point the concept of inhomogeneous (and homogeneous) mixing should be introduced/discussed. As I understand this mixing concept the data is perfect to show that both, homogeneous and inhomogeneous mixing occurs in Sc but at different levels or more precisely in parcels with different history – right? Exciting result!

**(R)** In the classical concept of homogeneous and inhomogeneous mixing the hypothetical final equilibrium stage of a cloud and environment volume pair is considered. Our approach differs from the classical approach in two main aspects. First, we consider the history of each parcel and look at changes in the cloud parcel and in initially dry parcel separately. Second, because the parcels move and their adjacent parcels change the final equilibrium stage is not reached.

The mixing between parcels in the model is inhomogeneous and gradients between neighboring parcels remain throughout the simulation. In case homogeneous mixing was represented all microphysical parameters in neighboring parcels would become identical during one time step and spatial gradients of all quantities between would tend to zero during this short time, this does not occur.

The model allows us to follow the history of each parcel. We suppose that such an approach better describes natural processes occurring in clouds than can be derived from standard mixing diagrams. The analysis in fig. 9 shows that all parcels can be separated into three groups with different LWC-N relationships. Formal application of such dependencies using the concept of the mixing diagrams to characterize the type of mixing imply that parcels belonging to group 2 mix homogeneously, while parcels belonging to group 3 mix inhomogeneously. However, considering that the presented results are only a single time step in an ongoing process reveals a different interpretation which is not limited by the assumptions of the mixing diagrams as mentioned above. In initially cloudy volumes mixing decreases the LWC but the concentration remains high through penetration of small droplets from the initially dry volume. Also, evaporation of droplets is only partial that leads to decrease LWC, but not droplet concentration.

Despite that the evolution of the DSD resembles the concept of homogeneous mixing the mixing is inhomogeneous. The apparent impression comes from simplifications of "classical" approach that assumes monodisperse DSD. In this case inhomogeneous mixing should not lead to change in the DSD shape. In our more realistic case, the width of DSDs in cloud parcels is quite wide and mixing leads to the DSD broadening. Such broadening was found in several recent studies of inhomogeneous mixing. Besides, in reality all of these features describe different stages in of inhomogeneous mixing process between cloudy and dry environment air. Corresponding comments are included into the revised paper.

Page 24146, l 7: Please define "spectral width"

**(R)** The spectral width here is the standard deviation of the droplet size distribution. Definition added in the text.

Page 24146: l 25 ff: I am a little bit confused at this point. Cloud base should be basically defined by the difference between the dewpoint and actual temperature at surface level - right? Does it mean that the water vapor and sensible heat fluxes decrease this difference? Maybe this point should be explained a little bit more detailed, although I think you are right.

How much is the upper boundary influenced by entrainment? Is it significant? Can you provide numbers?

**(R)** Yes, during the simulation sensible and latent heat fluxes from the ocean surface increase the humidity in the sub-cloud layer and alter the cloud base height. Entrainment-mixing leads to an increase of the cloud top height of ~100 m during the simulation. Both of these changes can be seen in fig. 4 (top panel). This has been written more clearly in the text.

Page 24147, l 4 ff: Do you really need lin and log representation in your Figures? Next sentence: delete one "peak" (line 5)

**(R)** Only the log scale is presented in the revised figure, text corrected

Page 24147, l 13ff: can you specify - what is the humidity level of these lucky parcels? Are you talking about absolute humidity or relative humidity/supersaturation?

**(R)** Lucky parcels are characterized by high absolute humidity, and will be the most humid air volumes in the cloud. This feature of lucky parcels is demonstrated in fig. 6.

Page 24148, l 1 ff: Why is humidity maximal at surface – adiabatic implies well-mixed SCL - right?

**(R)** Humidity fluxes from the surface maximal values near the bottom of the domain (fig. 5). These fluxes lead to an increase in the parcels closest to the surface. The added humidity is mixed into the BL during the parcel's ascent and the entire BL becomes more humid. The boundary layer is considered well mixed but still not all parcels have the same properties. In the mixing case (CON) adiabatic parcels are the ones that lose only a small portion of the humidity during their ascent in the boundary layer.

End of page 24149: you mentioned that increased turbulence (and turbulent fluxes) result in a moister SCL and the LCL is lower so there must be a further effect: drizzle has a shorter way in subsaturated air (LCL to surface) and this path is moister which should increase the drizzle rate at surface - right?

**(R)** Yes we agree, in a moister sub-cloud layer there will be less evaporation of drops and an increase of drizzle amount at the surface.

Page 24149, l26: Is this true? I thought that updrafts are smaller but with stronger vertical velocity compared to larger downdraft areas with smaller negative values of the vertical velocity (keeping the mass balance)? You mention that areas of up and down drafts are equally distributed?! Are there references?

**(R)** The sentence has been removed.



Page 24150, l 10: Why is aerosol size is increasing during droplet collisions? I don't understand this procedure or misunderstood... Do both nuclei stick together?

**(R)** For droplets with dissolvable aerosols, collisions lead to an increase in the mass of both the water and salt in the drop. If the drop evaporates a single larger aerosol releases. Actually, evaporation of droplets leads to formation of haze particles in equilibrium with environment. The increase in the aerosol mass by collisions leads to an increase in the radius of the equivalent "dry" aerosol. This problem was considered by Magaritz et al (2010). Corresponding comments are added into the text.

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.

End of Sec 4.5: I have the feeling that this is a subsection which is a little bit speculative and if one wants to shorten this manuscript I suggest to remove this section. Is the formation of larger aerosol due to collision of cloud droplets a feature of the numerical model?

**(R)** The formation of large aerosols due to collisions is represented in the model. This is a specific feature of the advanced model microphysics. Most models are not able follow the drop salinity (salt mass) in the process of drop growth and collisions. The LEM tracks the aerosol mass also when it is inside the drops. A detailed study concerning the changes to the aerosol distribution during the cloud evolution and the effects of collisions on the maximum aerosol size has been presented in Magaritz et al. (2010).

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.

Discussion section: Most of the "Discussion" section is a summary because many aspects have been discussed in the previous sections and no more discussion is added here. Why not combining Sec 5 and 6, shorten it and call it "Summary and conclusion"?

**(R)** Accepted

Page 24152, Sec 5.1: why do you introduce shallow Cu at this point - all the paper is about Sc? Fig 17 is nice but it lengthen the manuscript and I suggest to delete Fig 17 and completely focus on Sc - shallow Cu is a different story and I wouldn't mix them.

**(R)** The comparisson has been removed

Please check carefully the reference list in terms of typos.