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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Drizzle formation in stratocumulus clouds: effects of turbulent mixing" *by* L. Magaritz-Ronen et al.

Anonymous Referee #1

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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.

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.

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.

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

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)

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

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.

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

End of sec 3 on Page 24138: Up to here it is not clear to me if you take date 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.

Fig 1: Axis labels are weak, mark the two discussed positions. I suggest fort he 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.

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

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

Page 24140, l5 ff: I feel that a somewhat more detailed explanation of the Paluch diagram would help the reader to follow your arguments.

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.

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

Page 24141, l3: Please provide numbers, what are large droplets?

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.

Page 24142, l5: This sentence is difficult to understand, I suggest rewording.

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

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

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

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?

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

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

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?

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!

Page 24146, l7: Please define "spectral width“

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

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

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?

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

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?

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?

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?

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?

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"?

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.

Please check carefully the reference list in terms of typos.

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