Review of “Impact of high and low vorticity turbulence on cloud environment mixing and cloud microphysics processes” by Kumar et al. (acp-2021-101)

The manuscript analyzed the broadening of droplet size distributions in a cloud filament using direct numerical simulation. By analyzing the results for high and low vorticity regions, the authors can show that high vorticity regions cause spectral broadening due to inertia effects, which is a well-known phenomenon, and has been analyzed in a wide range of modeling and observational studies already (Shaw 2003; Grabowski and Wang 2013). However, the novelty of this study lies in the analysis of this process in the context of cloud environment mixing.

Accordingly, I like to support the manuscript’s publication in Atmospheric Chemistry and Physics, subject to one major and several minor revisions detailed below. Furthermore, the text contains several spelling and grammar mistakes, which do not impede its comprehensibility, but should be removed in a revised version.

Major Revisions

Criticism of Shaw et al. (1998): I think the overall criticism of Shaw et al. (1998) in this manuscript is unjustified and misleading. Shaw et al. (1998) laid the foundation for understanding droplet clustering in clouds and its effect on cloud microphysics. Furthermore, Shaw et al. (1998) did not conduct DNS, as falsely claimed in the current manuscript (ll. 54 – 59); they only reviewed previous DNS literature. In fact, Shaw et al. (1998) only used an idealized model based on a Rankine vortex to explain how inertia effects can increase supersaturation, i.e., modeling far from DNS. Moreover, it is very much misleading to claim that the results of the present study are “completely different” from Shaw et al. (1998) (ll. 129 – 133). In fact, the present study actually confirms the original work of Shaw et al. (1998) by showing that clustering is taking place in filaments at the cloud edge and contributes to spectral broadening. Finally, how did the authors determine the “volume fraction of high vorticity” (Tab. 1) that has been used by Shaw et al. (1998)?

Minor Revisions

L. 6: No need to defined DNS in the abstract.

Ll. 23 – 24: Although one can see collision and coalescence as two processes, they are often treated as one process, especially in the droplet size range considered in this study. In fact, so do the authors. Therefore, it is awkward to talk about three instead of two processes here.

L. 26: The reference to “Pruppacher 2000” is wrong. The last edition of the book is from 1997, and the authors are Pruppacher and Klett.

Ll. 34 – 35: You should add Grabowski and Wang (2013).

Ll. 39 – 40: “Particle response time” is ambiguous. I prefer “inertial response time” since this is what you mean. A reference on how it is defined would be helpful. Maybe Clift et al. (1978)?

Ll. 43 – 46: The work from Marcia Baker beginning in 1979 with several subsequent publications is missing here (e.g., Baker and Latham 1979).

L. 55: Define DNS.

Ll. 60 – 61: All the recent work by Katarzyna Karpińska and Szymon Malinowski on vortex tubes and their effect on cloud droplet distributions is missing here (e.g., Karpińska et al. 2019).

L. 77: DNS should have been defined already.

Ll. 79 – 81: Why do you mention the output format of your data files? This feels highly irrelevant here.

Ll. 82 – 83: The initial setup is not shown in Fig. 1a.

Ll. 84 – 88, ll. 247 – 255, Tab. 2: I would omit to discuss the monodisperse case in the manuscript. Its discussion feels like an unnecessary addendum to the discussion of the more realistic poly-dispersed droplet size distribution, without any additional value. Furthermore, it is highly unrealistic to find a monodisperse distribution in cloud edge filaments.

Ll. 84 – 88: Please state that the case with 22 % is unrealistic. In a real cloud, the air in the direct vicinity of a cloud edge filament (< 15 cm) should be moister. However, I think the case still adds to the study since it reveals interesting features of the entrainment-mixing process.

Ll. 84 – 88: Since the poly-dispersed distribution is obtained from measurements, a plot of its shape needs to be provided. Even the cited paper of Kumar et al. (2017) does not contain such a plot.

Ll. 93 – 94: A vortex needs at least 4 grid boxes to be represented on a numerical grid.

Ll. 104 – 107: I do not think it is necessary to state the definition of vorticity here. It is not used in the following, and a simple reference should be sufficient.

Ll. 116 – 117: How do you define a box? Is it a cube? Or a rectangular cuboid? This definition might matter since high vorticity regions are often organized in tubes, which shape might not always fit a “box”.

L. 118: “tubular” not “tabular”

L. 116 – 124: How often do you look for high vorticity clusters? Each timestep? Are these high vorticity clusters tracked in time?

L. 134: It is not initially clear that you are discussing the cases with the polydisperse droplet distribution first.

Ll. 138 – 140: What do you mean by these sentences? Please clarify.

L. 142: “The **cloud** volume lies […]”

Fig. 3: If the slab and the edge occupy the same volume of the model domain, how is it possible to have different kinetic energies at the beginning of the simulation? I would assume that the initial kinetic energy is uniformly distributed since evaporation processes only start after the beginning of the simulation. The same applies to the mean vorticity.

L. 159: Clarify the qv is the water vapor mixing ratio.

Fig. 4: At what size is a particle considered a droplet? Or, in other words, how do you determine Nd?

L. 194: How do you calculate the spectral width? There are several ways to do it, and they can result in significant differences.

Ll. 194 – 197, Fig. 5: An additional plot showing the dispersion of the droplet size distribution (σ_r/r_{mean}) is necessary to make the statement about different droplet size widths more robust! Since the mean radius is also changing significantly between the high and low vorticity regions, changes in the spectral width are inevitable, even in the absence of a process that contributes to broadening.

Ll. 206 – 208: A reference to Tölle and Krueger (2014) or Luo et al. (2020) might be appropriate here.

L. 219, and several other places: It is odd to use the word supersaturation when you write about subsaturations. I suggest using “saturation ratio“ instead.

Fig. 6: State the time at which these spectra are calculated in the plots.

Ll. 224 – 229: This is a great paragraph. It shows clearly that high vorticity regions can be identified as zones of entrainment. I would state this more explicitly.

Fig. 7, lower panel: The label on the ordinate of the plot should read σ_S and not σ_r. Is σ_S shown in percent?

Ll. 234 – 235: You are writing about Fig. 8, not Fig. 7.

L. 235: How do you define the degree of homogeneous mixing? Do you use the approach by Morrison and Grabowski (2008)?

L. 235 and several other places: Damk**ö**hler not Damkohler.

Ll. 237 – 240: It might be more appropriate to use the droplet evaporation timescale here since you are interested in changes in the droplet size distribution and not the thermodynamics.

Fig. 8: The entire discussion is based on panel d, although panels a to c provide valuable information. What does “line at nh” (panels a and b) mean?

L. 280: Define urms.

Ll. 282 – 283: Where do we see this in the current study? I think this statement is true, but the presented results do not confirm this directly.

L. 285: Change σ to σ_r in accordance with the notation of the manuscript.

L. 287 – 289: This is interesting. However, it is probably also the increased transport of less-moist air into these regions, i.e., entrainment of environmental air, which you are observing.

Technical Corrections

There are many spelling and grammatical errors which need to be addressed.

References

Baker, M. B., & Latham, J. (1979). The evolution of droplet spectra and the rate of production of embryonic raindrops in small cumulus clouds. *Journal of the Atmospheric Sciences*, *36*(8), 1612-1615.

Clift, R., Grace, J. R., & Weber, M. E. (2005). Bubbles, drops, and particles.

Grabowski, W. W., & Wang, L. P. (2013). Growth of cloud droplets in a turbulent environment. *Annual review of fluid mechanics*, *45*, 293-324.

Karpińska, K., Bodenschatz, J. F., Malinowski, S. P., Nowak, J. L., Risius, S., Schmeissner, T., ... & Bodenschatz, E. (2019). Turbulence-induced cloud voids: observation and interpretation. *Atmospheric Chemistry and Physics*, *19*(7), 4991-5003.

Kumar, B., Bera, S., Prabha, T. V., & Grabowski, W. W. (2017). Cloud-edge mixing: Direct numerical simulation and observations in I ndian M onsoon clouds. *Journal of Advances in Modeling Earth Systems*, *9*(1), 332-353.

Luo, S., Lu, C., Liu, Y., Bian, J., Gao, W., Li, J., ... & Guo, X. (2020). Parameterizations of Entrainment-Mixing Mechanisms and Their Effects on Cloud Droplet Spectral Width Based on Numerical Simulations. *Journal of Geophysical Research: Atmospheres*, *125*(22), e2020JD032972.

Morrison, H., & Grabowski, W. W. (2008). Modeling supersaturation and subgrid-scale mixing with two-moment bulk warm microphysics. *Journal of the Atmospheric Sciences*, *65*(3), 792-812.

Shaw, R. A., Reade, W. C., Collins, L. R., & Verlinde, J. (1998). Preferential concentration of cloud droplets by turbulence: Effects on the early evolution of cumulus cloud droplet spectra. *Journal of the atmospheric sciences*, *55*(11), 1965-1976.

Shaw, R. A. (2003). Particle-turbulence interactions in atmospheric clouds. *Annual Review of Fluid Mechanics*, *35*(1), 183-227.

Tölle, M. H., & Krueger, S. K. (2014). Effects of entrainment and mixing on droplet size distributions in warm cumulus clouds. *Journal of Advances in Modeling Earth Systems*, *6*(2), 281-299.