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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Droplet Clustering in Shallow Cumuli: The Effects of In–Cloud Location and Aerosol Number Concentration" *by* Dillon S. Dodson and Jennifer D. Small Griswold

Anonymous Referee #1

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Review of "Droplet clustering in shallow cumuli: The effects of in-cloud location and aerosol number concentration" by Dodson and Small-Griswold submitted to Atmospheric Chemistry and Physics (ACP).

Recommendation: reject and encourage resubmission

General evaluation: This paper reports analysis of observations applying a statistical technique that aims at documenting cloud droplet clustering. I found this subject interesting and fitting the ACP scope. However, I am confused by the specific detail of the analysis (PCF normalization) and I feel the way analysis is performed amounts to throwing the baby out with the bathwater. Specifically, forcing the PCF to approach

zero at large scale is not appropriate as there are likely large-scale heterogeneities both at the cloud core (i.e., due to different updraft across the cloud base and thus fluctuations of cloud droplet concentrations above) and at cloud edges due to turbulent mixing and filamentation. I have to admit that I started to read the paper with large expectations, and my enthusiasm went down and down as I kept reading. I admit that I stopped reading at the end of section 4.1. I do feel that the analysis is flawed because of the normalization that forces the cloud to look homogeneous at large scales. There are plenty of cloud observations showing that such an assumption is simply not valid! Thus, I recommend the paper to be rejected and then resubmitted with the discussion based on PCFs without normalization. There are numerous other problems and their sheer number (see specific comments) also suggests the need for a significant rewriting.

Major comments:

1. This comment is arguably more to the ACP technical staff than to the authors. The collection of figures at the end of the manuscript is unacceptable: figures are way to small and the only way to review their details is to go to the electronic version of the paper and zoom in. I feel the journal staff should request the authors to revise the submission before publishing the paper online to have each figure legible (e.g., one figure per page).

2. I do not feel the introduction is appropriate. First, it touches on some issues only remotely related to the specific focus of the paper (e.g., the indirect effects). The selection of references is incomplete or simply inappropriate. Second, the review of previous studies concerning clustering misses important publications as there are studies showing relatively small clustering (in contrast to the papers cited right now). Overall, I agree that there is likely some clustering at the cloud microscale, but its magnitude is relatively small and thus difficult to extract from observations.

3. The discussion in the introduction and in section 2.1 excludes the impact of gravi-

tational acceleration on droplet clustering. This is a serious omission as I would argue that droplet sedimentation is the mean reason for a relatively small droplet clustering. I provide detailed comments below, the Grabowski and Vaillancourt (1999) comment on Shaw et al. (1998) in particular.

4. The way PCF is calculated (section 2.3) need to be better explained. First, I am not sure why normalization is needed. Second, it is not explained how the normalization is performed (a simple shift?). I think there might be interesting differences between cloud cores and cloud edges due to entrainment and small-scale filamentation for the latter (this is what is illustrated in Fig. 1, correct?) In other words, cloud core and cloud edge may look different at different scales. For instance, near the edges, there may be differences at large scales (say tens of centimeters and meters, see Fig. 1), but similar clustering may take place at small-scales (the latter is the focus of the manuscript, correct?). Moreover, normalizing PCF does not allow estimating the magnitude of small-scale concentration fluctuations. The fact that Shaw et al. (2002) did the renormalization is not convincing (although I had just a quick look at that paper without getting into details). I feel this has a significant impact on the results and their interpretation.

Specific comments (those requiring special attention - more serious - marked with *).

1. P. 1, L. 20. I think it would be appropriate to cite Grabowski and Wang (2013) that reviewed the progress in this area a decade after Shaw (2003).

2. P. 1, bottom paragraph: This is a very pessimistic message. I think there are many realistic cloud simulations showing relatively fast rain formation (e.g., vanZanten et al. 2011, Seifert et al. 2010, Wyszogrodzki et al. 2013, Khain et al. 2013).

3. P. 2, L. 5: Earlier references than Khain and Lehman would be appropriate here. For instance, the exchange between Telford and Chai on one side and Jonas and Mason on the other in QJ in 1983 is worth referring to (the great phrase comparing the impacts of entrainment to turning down the gas to boil water faster!). Perhaps some papers of

Charlie Knight at about the same time are also of relevance.

4. P. 2, L. 6: For GN and UGN, there is the Illingworth (1988) paper way before the Knight et al.

5. P. 2, L. 8: “no one theory” is simply incorrect, please see 2 above.

6. P. 2, L. 9-18: this paragraph should be deleted as irrelevant to the results presented. Referring to Small et al. (2009) rather than to original papers (Twomey, Albrecht, etc.) is simply inappropriate. Also, Xue and Feingold (JAS 2006) is a more appropriate first reference to the impact of cloud-edge evaporation impact.

7*. P. 2, L. 19-26: Brenguier and Chaumat (2001) has to be cited here to show that not all studies indicate significant clustering at small scales! In fact, I would argue that the clustering is relatively small (e.g., see Fig. 3 in Kostinski and Shaw 2001 and Fig. 5 in Vaillancourt et al. 2002).

8. P. 2, L. 34: there are many more references than Pinsky pointing out to the significance of droplet clustering for collision/coalescence. Perhaps as reference to a review by Grabowski and Wang (2013) would be appropriate here.

9*. P. 2, L. 35. I do not think the impact of the Reynolds number (i.e., the range of spatial scales) is important for droplet clustering at the microscale. However, the difference in the eddy dissipation rate between laboratory experiments and natural clouds is the key. That was pointed out by Grabowski and Vaillancourt (1999) who also discuss the role of droplet sedimentation and referred to observations that were subsequently reported in Brenguier and Chaumat (2001). Discussion of the latter aspect, droplet sedimentation, is completely missing from the manuscript.

10*. P. 3, last paragraph of the introduction. I think the introduction should lead to the questions posed in this paragraph. The way introduction is written right now does not do that. For instance, why one should expect clustering to depend on the aerosol concentration? Because clustering is expected to depend on the droplet size. Why it should be different between cloud edge and cloud center? Is that because of the droplet size (arguably smaller at the cloud edge) and intensity of the turbulence (larger at the edges)? Similar for the dependence of the distance from cloud base. Etc. Etc. I feel a complete rewrite of the introduction addressing all points above and leading to these questions is needed.

11. P. 3, L. 23: Kolmogorov scale is around 1 mm in atmospheric turbulence.

12. P. 4. Eq. (1) is valid only for the case without gravity, correct? If so, it is not appropriate for cloud droplets.

13. P. 4, center: the discussion here should include the effects of droplet sedimentation, see Grabowski and Vaillancourt (1999) and perhaps other papers (e.g., from Prof. Lian-Ping Wang?).

14. P. 4, L. 24: the reference to Shaw (2003) is not correct here. The shear near cloud edges comes from cloud dynamics, not evaporation, see original study of Grabowski and Clark (1993) and more recent support from Park et al. (2017).

15. P. 5, L. 1: Should Twomey's or Squiers' old classical papers on droplet activation and growth be referred to here instead of Small and Rosenfeld?

16. P. 5, L. 9: Rather than Pinsky and Khain, the original inhomogeneous mixing papers (John Latham, Marcia Baker, etc.) should be brought here.

17*. P. 6, L. 23. Please explain how the normalization is done. Is the PCF simply shifted up or down to have zero at large scales? If this is an entrainment zone and droplets are clustered at large scales (as clearly illustrated in the left panel of Fig.1), the analysis should show that! This aspect is completely missed by the normalization. If the purpose of the analysis is to show small-scale clustering, then estimating the absolute magnitude of such small-scale clustering is impossible with the normalization.

18*. P. 7, L. 17. Fig. 1 clearly shows that the patchiness is at large scales (meters), not at small scales. Renormalization takes this aspect away. Is the focus of the analysis

on concentration fluctuations at large scale (meters) or at small scales (centimeters) scale? I would think the latter. To me this is the key flaw of the analysis.

19*. Fig. 5. First, I am curious how the figure looks without the normalization (see major point 3 above). The specific discussion in lines 15-20 on p. 9 may change if no normalization is performed. For instance, I think the statement on line 21 ("droplet spacing shifts from non-homogeneous to homogeneous at larger spatial scales") is incorrect as I expect the cloud edge to be quite heterogeneous at large scales (meters and up). The interpretation the authors provide comes from the normalization and it is counterintuitive.

20. Fig. 6. I do not understand what the value of the PCF is shown. Is that the asymptotic values at small scales (i.e., the left edge in Fig. 1)?

As I stated in the overall evaluation, I stopped reading the paper around page 11.

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