

Review of the revised manuscript “Droplet clustering in shallow cumuli: The effects of in-cloud location and aerosol number concentration” by Dodson and Small-Griswold resubmitted to Atmospheric Chemistry and Physics (ACP).

Recommendation: accept after major revision

General evaluation: This is a resubmission of a manuscript I reviewed previously recommending rejection. I am happy to see that the authors took my comment seriously, modified the analysis, and rewrote the manuscript entirely. In the revised manuscript, the PCFs are no longer adjusted and thus my major objection is no longer valid. Also, the figures are now large enough. The introduction is also completely rewritten and I feel it is more appropriate. Unfortunately, the new manuscript leaves many questions unanswered as detailed below. In general, my comments need to be treated as a review of a new manuscript. I have a few major comments and suggestions, and several minor and more serious technical comments. Overall, I feel the manuscript still requires significant work to put the results on a better footing.

Major comments:

1. One of significant results of the study is the difference between PCFs for the L2 case and the other three cases, the asymptotic value at small scales. I strongly feel that some interpretation of this difference is needed. One possibility would be to expand Fig. 1 so the left panel is replaced with two panels showing PCFs with different asymptotic values similar to the difference between L2 and other cases. Right now, the asymptotic value at small scales in the left panel of Fig. 1 is about an order of magnitude smaller than in Figs. 5, 9 and 10. Why? Should the artificial data be generated in such a way that the PCF has the asymptotic value similar to that in the real data? I simply have no intuition what the asymptotic value means and why there is a difference between L2 and other cases. This needs to be better explained in the revised manuscript.
2. There are numerous unclear statements, phrases, and figure details that make reading and understanding the manuscript difficult. I list several of them in the specific comment section. These needs to be corrected.
3. Section 5.1. This section is a pure speculation that is not supported with any data you present. I really do not know what to suggest. One possibility is to leave this subject out and perhaps replace L2 with another flight that has PCFs similar to L1, H1, and H2. This is also related to 1 above as the manuscript really does not provide any explanation what the difference in the asymptotic value means. I also feel the discussion in section 5.2 (edge versus center) is speculative. Perhaps the discussion in the two sections can be put on a better footing if other cloud parameters are included in the analysis (e.g., adiabatic fraction, spectral width of the droplet size distribution, etc.). At the moment, the PCF analysis alone and the way it is presented in the paper is not sufficient.
4. In view of the above criticism, I am not surprised that the conclusion section 6 is relatively short and thin on merit.

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

1*. In general, I do not like when the authors refer to concentration inhomogeneities at large scales as “clustering”. Yes, the authors make it clear early in the paper that one needs to distinguish between

inertial clustering at sub-cm scales, and concentration heterogeneities at larger scales. However, applying the term “clustering” for the latter is not appropriate in my view. I would prefer to refer to them as “heterogeneities” as they likely result from entrainment and filamentation during turbulent stirring. The authors suggest that these are because of “cloud holes”, but I would prefer to think about them as “filaments”, not holes. This point is related to my major point 1 above.

2. P2L3 (page 2, line 3): “climate sensitivity” should be explained or a reference that explains this term should be provided.

3. Fig. 1. It is difficult to see that the symbols at the very bottom of the lower graphs are squares. Perhaps replacing squares with crosses or just vertical bars would be better.

4. Beginning of section 2.1. The role of the Stokes number should be brought here. The Stokes number is discussed later, but bringing it in the first paragraph would be more appropriate.

5. P6L2 and 3: “...represents: given...”: please rephrase; “time bin”: please define.

6. Fig. 5: lower and upper panel heights are different. Please replot.

7*. Figure 6 and its discussion on p. 10 is unclear. Specifically, I do not understand the boxplot, its horizontal scale, and the phrase “that is \geq to the 0.998 quantile”. The relation of the boxplot to PCF is unclear. Please explain how one gets the boxplot from the PCF. Or maybe these are different ways to represent the same sequence of droplet positions?

8*. Fig. 7 and its discussion. I think the boxplots are similar to those in Fig. 6, correct? As with Fig. 6, I do not understand what they show. Also, what are the numbers in each panel? The caption does not explain that.

9. Fig. 8. PCF is a function, so I do not understand what Fig 8 shows. Is that the asymptotic value? Also, how come the RH is over 100%? This is also repeated later in the paper (e.g., Table 2 and Fig. 11). This is not possible. Please explain and/or check the data.

10. P11L23: what is the “average PCF value”? Is this different from the asymptotic value I refer to above? Again, I do not know what the difference between the mean values of 0.54 and 0.43 means. And where do they come from (i.e., what does the difference represent)? Again, this goes back to my major comment 1.