

Replies to the Reviewer 2 comments

Overview:

The main contribution of this paper apparently is to demonstrate the relationship between different moments of the size distribution for the limits of homogeneous and extreme inhomogeneous mixing. Analytical results are compared with the results from a parcel model. The conceptual picture of inhomogeneous and homogeneous mixing is well illustrated in Figure 1 and the central analytical expression is validated in Figure 2. Figures 3-8 then show the response of different moments of the cloud droplet size distribution to idealized mixing processes. Figures 9-11 describe a conceptual model of a cascade of mixing events between a dry parcel and the cloud, which is a step toward making comparisons between the theory and observations within an evolving cloud environment. Section 4 and Figures 12-15 provide a brief analysis of observational data in the context of the conceptual models developed in the previous sections. The analysis is useful in attempting to connect the concepts and idealized models to the more complex situation observed in real clouds. The paper ends with a discussion of characteristic time scales, which seems somewhat disconnected. It is not clear how this integrates with the previous sections, and perhaps it should be either moved closer to the introduction or separated as an appendix. If kept in this location, its logical flow with the rest of the paper needs to be improved. Overall, my sense is that the expanded view to consider different moments of the size distribution is a valuable contribution, especially for the experimental cloud physics community, but perhaps also for applications to radiative transfer, remote sensing, etc. I am not aware of other papers where different moments are considered thoroughly as here, so this seems to be original. Comment regarding disconnect of the section 5 discussing the characteristic time scales.

Reply: Authors highly appreciate the Reviewer's comprehensive comments and time to read our manuscripts. Special thanks for thoroughly going through equations and revealing numerous typos.

The manuscript underwent major revision and modification. The text was shortened to make it concise, sections were rearranged, some of them were re-written, the variable names were modified to be consistent with part 2 and 3. We agree that Section 5 is in many ways disconnected, and it was excluded from the manuscript.

General criticisms:

1. The application to size distribution moments is original, as far as I am aware (Jeffery gave a brief discussion of how the second moment is affected by mixing, but the treatment here is much more thorough and covers all typical moments). But much of the conceptual model is written more like a textbook. Maybe this is nice for readers new to the field, but the authors take a risk in expanding the length of the paper, especially when combined with the other two parts. Much more important, and definitely missing from the introduction as it currently stands, is some kind of overview of how the three part series fits together. What are the different levels of complexity treated? Why are two specialized papers on homogeneous and inhomogeneous mixing needed if part 1 already treats both cases? Now that I have read all three parts I have an idea, but this needs to be clear from the outset. It is especially important to motivate why part 1 should be connected at all. Currently it is disconnected in its approach, in its use of observational data, and even in its notation. The use of observational data is nice, but it is somewhat confusing given the title "theoretical study..." The notation is a major problem that needs to be corrected... the physics is difficult enough by itself, without having to translate symbols from one paper to the next.

Reply: The authors shortened several pages of the text in order to reduce the size of the manuscript and make it concise. A number of cross references were added in all three parts in order to link them together. As it is seen now, part 2 is closely related to part two and it uses the same approach. Part 3 utilizes the results of part 2. The first part uses experimental data to demonstrate the how the theoretical outcomes could be verified from in-situ measurement. In our opinion such comparisons with experimental results are natural, and if it is not there, it probably would be requested by reviewers. So, since
 We also checked Jeffery’s works on mixing. However, no discussions of the effect of mixing on the DSD second moment were wound. We appreciate if this reference could be provided.

2. After a long preliminary discussion, the most important paragraph in the introduction is on page 30214 starting at Line 26: “Besides the effect on N and r the type of mixing is anticipated to manifest itself in relationships between other moments of the droplet size distribution...” It should be further explained in that paragraph why it is valuable to analyze different moments. Are they expected to be more insightful than the traditional mixing diagram methodology; is it making applications of mixing to other fields clearer; etc?

Reply: The paragraph explaining importance of the effect of mixing on the DSD moments was added in the introduction following the Reviewer’s comment: “It is shown that the newly obtained relationships between the moments provide a more robust identification of type of mixing from in-situ measurements as compared to conventional $N - D_v^3$ relationships used in mixing diagrams. Relationships between moments may be useful for parameterization of mixing in numerical simulations of clouds and climate, interpretations of remote sensing measurements.”

3. In Fig. 9 and after, a multiple-step mixing process is envisioned. The approach is to consider mixing between a cloud and the dry environment, and then to consider subsequent mixing events between that parcel and the cloud again. Why did the authors choose to take this view instead of considering a cloud parcel progressively mixed with clear air? Some motivation for that choice is needed and some discussion of how the results would be expected to differ. For example, if one were to focus on the dry air first, dots should be concentrated at lower end in Figure 10.

Reply: The modeling of the progressive mixing presented in the paper corresponds to the case when the entrained dry air is interacting with the cloud environment. The final state of this interaction is diluted cloud, which continues its life cycle. The progressive mixing of the cloud environment with the environmental dry air corresponds to detrainment, which ultimate state is dry cloud free air. It can be show that during detrainment the relationships between moments will be the same as during primary mixing. The authors consider that the case of detrainment is less interesting, and left it outside the frame of the manuscript in order to keep it concise. Following the reviewers suggestion a paragraph was added in the text in order to explain the motivation of our choice: “It is worth noting that progressive mixing with the dry air does not break the functional relationships between the moments. This case is equivalent to detrainment of cloudy environment into dry air. It can be shown that Eq.(14) remain valid at any stage of progressive homogeneous mixing with dry air only, i.e. $N_j/N_1 = \mu^{(1)} \dots \mu^{(j-1)} \mu^{(j)}$ where $\mu^{(j)}$ is the mixing

fraction at the j -th stage of mixing. Eqs. (15)-(24) also remain valid for the progressive mixing with the dry air only. ”

4. There are many mistakes in the paper, including errors in the equations, at least according to the derivations as I am able to follow them. Again, the physics is difficult enough by itself, without having to make corrections. Please thoroughly check all results and the typesetting.

Reply: The authors highly appreciate the Reviewers efforts to improve our paper and pointing out numerous typos. All specific comments listed below were addressed and the text of the manuscript was thoroughly checked.

Specific comments

1. Eq. 1, page 30218: As monodisperse cloud droplets are used in this part of the study, the droplet size distribution $f(r)$ will confuse people. Especially Equations 2 and 3 only work for monodisperse droplets theoretically. Please explain and be consistent.

Reply: The relationships between moments are valid for relatively narrow non-monodisperse droplet size distributions. However, the modeling was performed for monodisperse size distributions. The confusion about assumption of monodisperse droplets during deriving relationships between the moments is probably coming from mentioning monodisperse size distributions in section 2.2. This section was modified in order to avoid possible confusion about the assumption about monodispersity.

2. Eq. 5: prefactor should be $(c_p R v T_{mo}^2 / L^2)$? T_{mo} not T_2 ?

Reply: Corrected.

3. It is difficult to connect Eq. 8 to Eq. 5. How do you prove Eq. 5 is $(1-\mu)$ Eq.8, when $T_1=T_2=T_{mo}$?

Reply: The term $(1-\mu)$ appears as a result of expansion in series. Appendix B was added to clarify the derivation of this equation.

4. Line 21, page 30218: q is liquid water mixing ratio (g/kg), not liquid water content (g/m³).

Reply: Corrected

5. Line 6, page 30220: The neglect of latent heat is a strong assumption that removes possible important factors such as negative buoyancy production. It is valid in the range specified by the authors, but the limitation should be discussed. Does it restrict the results to certain environments or cloud types (e.g., shallow convection)?

Reply: If fact the latent heat was accounted during derivation of Eq.3 (old Eq.8) (see Eq.A7 in Appendix A). The confusion regarding disregarding the latent heat is coming from inaccurate statement on page 30220 as indicated by Reviewer. The original purpose of this statement was to indicate that the temperature remains constant. In order to demonstrate that δq^* and δq_m allow accurate depiction of the temperature depression during mixing-evaporation process, the air temperature formed after mixing calculated from the analytical expression Eq. 6a,b was compared with the modelled temperature in Figs. 4h and 6h.

6. Line 7, page 30220: “comparisons of with numerical...” needs to be corrected.

Reply: Corrected.

7. Line 13, page 30220: missing space between “on” and “delta_q”

Reply: This sentence was deleted.

8. Line 17, page 30220: the volume change due to temperature change should not affect liquid water mixing ratio, because it’s connected to mass not volume as mentioned in point 4.

Reply: This paragraph was deleted.

9. Eq. 8: prefactor should be $(c_p R v T^2 / L^2)$?

Reply: Corrected.

10. Eq. 13: left side should be r_{33}/r_{303}

Reply: Corrected.

11. Eq. 14b: I think the right side should be $(q/q_0)^{2/3}(q+\delta q^*/q_0+\delta q^*)^{1/3}$

Reply: Corrected.

12. Eq. 16: I believe the exponent should be $-1/3$, and inside the parentheses should be N_0/N .

Reply: Corrected.

13. Eq. 20: right side should be $q^{2/3}(q+\delta q)^{1/3}/q_0$

Reply: Corrected.

14. Fig. 3: it looks like panels a and b are mixed up. Also the caption refers to liquid water mixing ratio but the axis label states LWC; needs to be consistent.

Reply: Corrected.

15. Figs. 3 and 4: should use same format for S through the whole paper (e.g. 20% as in Fig.4 or 0.2 as in Fig. 3)

Reply: Corrected. In the revised manuscript S is replaced by RH.

16. Lines 12-15, page 30224: Lots of problems here. Where are the black stars in Fig. 4? Do you mean the stars in panels a and b of Figure 3, or should there be stars in Figure 4 too? And by the way, the stars in Figure 3 are very difficult to see... I had to search for them. And again, regarding text on line 14, the question of LWC versus q comes up. Finally, on line 15 it is not obvious to be that the statement is for Figs. 3 and 4. Do you mean to include Fig. 2 also?

Reply: The problems with the figures numbering and incoherency of the associated text were fixed.

17. Line 25, page 30226: q_0 is not liquid water content.

Reply: Corrected.

18. Line 9 page 30227: Fig.17 should be Fig. B1?

Reply: Corrected.

19. Fig. 7: why changes from $r_0=10\mu\text{m}$ (Fig. 4,5,6) to $r_0=5\mu\text{m}$. And also changes the S from 50% to 90%?

Reply: The sizes $10\mu\text{m}$ and $5\mu\text{m}$ were selected to demonstrate mixing for the cases $T_1 = T_2$ and $T_1 \neq T_2$ in a most pronounced way. For the case $\text{RH}_2=50\%$ no supersaturation will be formed. Positive supersaturation may occur only at $\text{RH}_2>80\%$ and $\Delta T<15\text{C}$. Larger ΔT seems to be uncommon for the tropospheric clouds.

20. Fig. 8: My understanding is that homogeneous and inhomogeneous mixing coincide with each other for $S_{\text{mo}}>1$? It's hard to see this phenomenon in Fig. 8 (might use different colors or symbols?) also line 5 page 30228: unclear, should be "exceed those for inhomogeneous mixing for $\Delta T=0$ and $\Delta T=5\dots$?"

Reply: Corrected. Inhomogeneous mixing for $\Delta T=10\text{C}$ was indicated by the grey circles.

21. Line 5, page 30228: in Fig. 8, ΔT is negative, here it's positive.

Reply: The sign of ΔT was corrected.

22. Line16, page 30228: could you explain why "the effect.. is more pronounced when $T_1>T_2$ compared with $T_1<T_2$."

Reply: When the entrained air is colder ($T_1>T_2$), it results in additional condensation of the cloudy air due to its cooling compared to the case when the dry air is warmer ($T_1<T_2$). This statement is supported by the results of numerical simulations. This explanation was not included in the text for the sake of conciseness.

23. Line 27, page 30229: "becomes denser towards the top right corner" Is it because the mixed volume is mixed with cloud volume, not environmental volume?

Reply: Yes. The mixing with the cloud environment results in approaching of the properties of mixing environment to the cloud properties. Eventually the entrained air is dissolved in the cloudy environment.

24. Fig. 11: why use $r_0=5\mu\text{m}$, not $10\mu\text{m}$. It's better to use the same radius through the paper, except you want to do the sensitivity test.

Reply: During the paper preparation the authors tried different r_0 . Unfortunately it does not work well for the same r_0 . Different r_0 ($5\mu\text{m}$ and $10\mu\text{m}$) were used in order to demonstrate the most pronounced effect of mixing on microstructure. A relevant comment was embedded in the text to address this issue.

25. Line 13, page 30231: missing space between "q" and "beta"

Reply: Corrected.

26. Line 14, page 30231: define Sc, Ac, Cu, Cb

Reply: This sentence was deleted in the revised version.

27. Line 1, page 30232: missing space between “N” and “q”
Reply: Corrected.

28. Fig. 13: caption T=-12 not -120
Reply: Corrected.

29. Line 13, page 30233: how does sample averaging affect homogeneous versus inhomogeneous mixing?
Reply: This is a good question. It was debated over years: how the averaging scale affects identification of the type of mixing, i.e. homogeneous versus inhomogeneous? The single instrument approach used in this and the majority of previous studies does not allow judgement about type of mixing at scales smaller than the averaging scale L_{av} . In part 2 it was shown that for typical cloud environmental conditions the upper spatial scale of homogeneous mixing is limited by few m. Inhomogeneous mixing depending on the conditions may cover a wide range of scales from cm to km. A discussion of spatial scales of homogeneous and inhomogeneous mixing is provided in parts 2 and 3.

30. Fig. 14a: y axis unit (g/m³) not (km⁻¹)
Reply: Corrected.

31. Fig. 14: what’s the dash line in a,b,d
Reply: The explanations for the dashed lined was implemented in the caption for Fig.14.

32. Line 9, page 30237: $Da \gg 1$ is for inhomogeneous mixing, while $Da \ll 1$ is for homogeneous.
Reply: This section was removed in the revised manuscript.

33. Line14, page 30237: Andrejczuk is misspelled both here and in the reference list.
Reply: Corrected.

34. Lines
35. Lines17-22, page 30238: λ_{ev} , λ_v , and $\lambda_{\Delta V}$ need to be defined, and the assumptions in calculating them clarified (e.g., evaporating distance assumes droplet always falling at terminal speed corresponding to time-dependent radius?).
Reply: This section was removed in the revised manuscript.

36. Line 6, page 30240: S2 approximate 1 not 0?
Reply: This section was removed in the revised manuscript.

37. Line 6, page 30240: missing space between “concentration” and “nev”
Reply: This section was removed in the revised manuscript.

38. Fig. 16: define A and B in the text or caption
Reply: This section was removed in the revised manuscript.

39. Line 13, page 30240: missing space

Reply: This section was removed in the revised manuscript.

40. Line 15, page 30240: missing space

Reply: This section was removed in the revised manuscript.

41. Eq. B4: left side should be T_{mo} not T_m

Reply: Corrected.

42. Eq. B8: There seem to be mistakes here. I believe the prefactor should be $(c_p R v T_{mo}^2 / L^2)$ and T_{mo} not T_2 ?

Reply: Corrected.

43. Line 14, page 30244: “is hold” should be “holds”?

Reply: Corrected.

44. Line 15, page 30244: Figure B1 is Figure 17.

Reply: Corrected.

45. Table A1: there are two τ_{ev}

Reply: The variable related to the time scale section were removed from the table.