Reply to Reviewer #1 comments on "Theoretical study of mixing in liquid clouds – Part 1: Classical concept" by A. Korolev et al.

General comments to all three parts (repeated in all three reviews). I read the papers with considerable interest mostly because this seemed to be a popular topic some time ago, in both observations and modeling. I was curious to see what new these manuscripts bring. Frankly, I was disappointed. First, the analysis concerns a highly idealized problem, with little applications to real clouds. Turbulent mixing in clouds is by far more complicated that situations depicted in Fig. 1 of part 1 (and then repeated in different shapes as Figs. 1 in Part 2 and 3). Second, I am aware of study in which the authors developed a fairly sophisticated model of microphysical evolution during turbulent stirring (Jarecka et al., JAS 2013) aiming at prediction of the homogeneity of mixing. They applied the model to LES simulations of shallow convective cloud field. The impact was surprisingly small and the authors of that paper argued why this might be so (the entrained air comes from the descending shell and is not far from saturation). So in a sense the subject is "old news". Finally, the lengthy discussions, full of unnecessary caveats and references to details of small multi-panel figures, made the reading frustrating. All three parts read like a student dissertation, not a concise scientific paper highlighting key points and leaving the rest for the reader to follow. Thus, I read the manuscripts with decreasing interest, and my comments are more detailed for the part 1, and get more general for parts 2 and 3.

Overall, I do not believe that the subject matter deserves close to 100 pages and close to 50 figures. I feel that the material deserves a single, short and concise manuscript, with new material clearly separated from what I feel has been discussed in the past, perhaps not at such a level of detail. Reading introductions to all three parts made me mad, because all three say basically the same thing with different language and organization. Part 1 is mostly trivial in my view, with some parts speculative and other repeating already published material (see detailed comments). Parts 2 and 3 have some aspects that perhaps deserve to be published, but it is not clear to me how useful these are (not very much in my opinion). References to aircraft observations are vague and missing the key aspect, which is the irrelevance of an idealized problem considered by the authors to low-spatial resolution observations of a complicated multiscale natural system.

Reply to general comments:

Authors appreciate the Reviewer's time and efforts to review our manuscript.

The overview sections, which were copied and pasted for all three different reviews, can be summarized by the following claims:

a) The problem of turbulent mixing in clouds "seemed to be a popular topic some time ago", but now "the subject is old news".

b) This study addresses a "highly idealized problem" and uses simplified models in order to describe cloud mixing.

c) The results presented in the papers are not new and are "repeating already published material".

The authors strongly disagree with the above statements of Referee 1.

In response to the first claim: the mechanism of mixing is still not well understood and continues to be a highly relevant problem in the cloud physics community, especially given the high rate of recent publications on this topic. We believe that the three papers contribute significantly to the theory of interaction of cloud droplets with turbulent environment and present novel techniques of investigating the effect of mixing both from a theoretical standpoint and through in-situ observations.

Second, in contrast to the reviewer, we support the common practice of using idealized models of complex cloud processes, in order to investigate physical mechanisms without being bogged down by the multitude of other processes involved. Idealized considerations (e.g. adiabatic assumptions) are widely used in cloud physics community as well as in physics in general. The assumptions are clearly articulated at the beginning of each paper in order to let a reader judge about the level of idealization of the utilized approaches.

Third, as regards to novelty, the following new results have been obtained:

a) The first paper suggests a new technique for identifying type of mixing (homogeneous or inhomogeneous) based of the analysis of the moments of droplet size distributions. It was shown that homogeneous mixing breaks functional relationships between the moments. Nothing like that has been done before. The comments obtained by the authors from their colleagues showed that the proposed technique start to be utilized by other research groups.

b) The second paper considers *homogeneous* mixing. One of the important finding of this paper is an analytical universal solution describing the rate of evolution microphysical parameters as well as the final equilibrium state (mixing diagram). It is shown that in case of polydisperse droplet size distributions evolution of droplet spectra can lead to increase in characteristic size of droplets in contrast to widely accepted "classical" view, when the characteristic droplet size is decreasing.

c) The third paper is dedicated to *inhomogeneous* mixing. A theoretical framework for a time dependent mixing of two volumes that accompanies by cloud droplet evaporation is developed. A new turbulence-evaporation model of time evolution of ensemble of droplets under different environmental parameters is proposed. In contrast to previous studies the Damkoller number is introduced as a result of re-normalization of mixing-evaporation equation, rather than empirically. It is shown that any mixing leads to droplet spectrum broadening. For the first time the scientifically grounded demarcation between homogeneous and inhomogeneous mixing in the space of environmental parameters is performed.

The authors are confused that Referee 1 overlooked all these novelties.

The authors also believe it is impossible to follow the recommendation of Referee 1, to combine all papers into one single, summary paper. While the papers all consider the same subject, they perform completely different functions with regard to investigating the issues of mixing.

Comment:
A small technical comment: I think the terminology the papers use is not correct. The limiting cases should be referred to as homogeneous and extremely inhomogeneous mixing. Everything between the two is the inhomogeneous mixing.

Reply: Corrected.

Specific comments to Part 1:

1. The title should include “concepts”, not “concept”.

Reply: Corrected

2. I feel the proper start to the discussion is to recognize that bulk properties (moist static energy and total water) are sufficient to calculate the final thermodynamic state (i.e., once the mixing is completed). However, the transformation of the droplet spectrum may lead to different spectra with the same final liquid water. Extremely inhomogeneous mixing leads to the final spectrum as given by (1), that is, number of droplets in each bin is simple reduced in the same proportion. Homogeneous mixing leads to a shift of the spectrum towards smaller sizes. In such a case, the shift may lead to a complete evaporation of the smallest droplets in the initial spectrum. Note that such a simple interpretation makes the first sentence in the abstract to the Part 2 trivial.

Reply: The statements about independence of the final state of the bulk parameters on the type of mixing are scattered throughout the text of Part 1. One more statement was implemented in Section 2.1 following the reviewer's comment.

3. The main problem with the observations is the insufficient spatial resolution. If the diluted cloud consists of filaments of cloud-free and undiluted cloudy air, averaging such a structure gives an impression of the extremely inhomogeneous mixing (this was pointed out long time ago, perhaps in on of the papers involving Charlie Knight). In fact, aircraft in-situ observations seldom allow looking at homogenized volumes, at least not at scales that the observations are able to resolve. Moreover, there are additional processes that affect droplet spectra, such as updraft and downdraft, activation of additional cloud droplets, collision/coalescence, etc.

Reply: The problem here is not as much as in the particle probe resolutions, as in the identification of the stage of mixing. For example, Beals et al. (2015) demonstrated existence of cloud free zones in clouds down to cm scale. That's the highest possible spatial resolution available nowadays. However, the results of this study and other similar studies do not provide answer, whether this is a final stage of mixing and whether the mixing is extremely inhomogeneous, or it is an interim stage of homogeneous mixing. To address this question a collocated high spatial resolution (~1cm scale) measurements of temperature and humidity are required. Unfortunately, airborne instrument capable of such measurements are not available at that stage. The discussion about it is added in the text.
Yes, there are limited number types of clouds suitable for identification of type of mixing, which are free of "non-mixing" processes (i.e. collision-coalescence, mixed phase, activation of interstitial CCN). There is a discussion about it in the manuscript. Authors do not think that it should be expanded in the manuscript more than it is.

4. Reference to Jarecka et al (JAS 2013) needs to be included in the paragraph starting at line 20 on p. 30213. Note that the review by Davenish et al. was published prior to that paper.

Reply: The reference was added.

5. Section 2.2. Figure 1 shows processes occurring at a constant volume. Does it make the difference that atmospheric processes typically take place at a constant pressure?

Reply: Consideration of the effect of pressure (e.g.$u_z \neq 0$) is not included in the text. This was stated in section 2.2. A potential effect of the vertical ascent was discussed in section 6.

6. Section 2.3. Does the conservation of moist static energy and total water lead quickly to the answer?

Reply: The derivation of δq was done based on the mass and energy conservation. Yes, it leads quickly to the answer for q.

7. I do not understand the statement below Eq. 9. Latent heating is included if one follows what I suggest in 6 above.

Reply: The mentioned statement is misleading and it was excluded from the text of the revised manuscript. The original meaning of this statement was to indicate that the temperature in Eq.9 is used as a constant. The modified statement in the modified manuscript was moved to Appendix A.

8. Section 2.4. The initial paragraph provides information that needs to be stated at the onset of the analysis (see 2 and 6 above).

Reply: The sequence of sections was rearranged in order to improve the flow of the text.

9. Eq. 15. The phase relaxation time scale goes back to Squires.

Reply: The reference to Squires was added.

10. Section 3. First, I do not think there is anything to model. Is the comparison between a specific model used by the authors (no details provided) and the analytical solutions the purpose of this section? Sections 3.1 to 3.4 should be compressed into a short section and a single figure should be selected. These sections are exactly what I mean by my statement that the paper reads like a student dissertation.

Reply: The sections were shortened and rearranged. Figs.4-6 were converted into one figure.

11. Section 3.5 is perhaps a good start to a follow-up investigation. At the moment, it does not belong to this paper.

Reply: This section has a strong link to the subject of the paper, which might not be well articulated in the original text. The text and the sequence of the sections were rearranged in order to address this issue.

12. Section 3.7. This is really not a summary.

Reply: The title of the section was changed. This text of this section was moved into Sections 5 in the revised manuscript.

13. Section 4 is long and does not bring anything new in my view. What is the point of having it here? I was not able to follow detailed discussion in section 4.1 and references to the specific figures. Section 4.2 can be omitted. I question the link between in-cloud observations and the results of theoretical analysis that the previous sections provide.

Reply: The results of this section create a basis for a new approach for identification of type of mixing from in-situ measurements. All previous attempts of identify homogeneous mixing were based on the comparisons of measurements with the $N - r_v$ calculated for the first stage of mixing. Such attempts have a limited success and may be misleading. More explanations were added in the text in order to clarify the results of this section and link it to in-situ measurements.

14. Section 5 discusses aspects that have been beaten up in other papers. Just a short paragraph with proper references would be sufficient.

Reply: The entire section on time scales was removed in the modified manuscript to make the paper more focused.

15. Conclusion section is short, perhaps not surprisingly.

Reply: Nothing to comment.