Response to Reviewer 1 to the paper : Theoretical investigation of mixing in warm clouds – Part 2: Homogeneous mixing authored by M. Pinsky, A. Khain, A. Korolev, and L. Magaritz-Ronen

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.

® 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 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. A novel approach for identifying mixing from in-situ observations was proposed. 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. It was shown that evaporation time can be expressed in terms of time of phase relaxation. This is important for definition of reaction time in Damkoller number.

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 regret 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.

General comment. Part 2 provides detailed analysis of the temporal evolution of the thermodynamic homogenization after the mechanical mixing quickly homogenizes the initially separated cloudy and cloud-free (i.e., sub-saturated) volumes. I feel Part 2 presents some relevant results, but the paper is way to long for the outcomes it provides. I hope my comments below will help the authors to convert this part into a section of the rewritten new manuscript.

(R) We appreciate this comment of Reviewer. However, we do not see the possibility to compress all parts of the study into one paper. The topics discussed in these parts as well as the approaches used are too different to combine them into one paper.

(C) 1. Towards the end of the introduction, the paper (once again) introduces the relevant time scales. What I find interesting and worth pursuing in my view is that the time scale describing droplet evaporation, taken as the phase relaxation time scale in the current study, is not the only possibility. Others (including the Jarecka et al. paper I think) have taken this time scale as the time required to evaporate a droplet with the sub-saturation of the cloud-free volume. I think some studies considered both and took the smaller (or the larger?) of the two (Feingold?). Note that the phase relaxation time scale has no information about the sub-saturation of the initially cloud-free volume, but the other definition does. Is this relevant? It should be for the extremely inhomogeneous mixing I think (I will comment on that in my review of Part 3).

(R) We know that some authors indeed use evaporation time of individual drop under given sub-saturation as a characteristic time of mixing process. In our opinion only phase relaxation time should be used as a characteristic time scale of the process since we should consider behavior of a large amount of droplets. In the paper (Pt 2) is shown that the phase relaxation time is a natural time scale of mixing problem. This is clearly seen from universal renormalized evaporation equations, in which the phase relaxation time plays the role of time unit. Adiabatic consideration of the total volume in which mixing takes place leads to strict relationships between the changes of supersaturation (or sub-saturation) and liquid water content in the volume, that makes absolutely impossible to consider the changes in the size of individual droplet and in supersaturation independently. These two quantities (supersaturation and liquid water content) are tightly linked.

Note that supersaturation (or sub-saturation) is not in the list of parameters that determine phase relaxation time. Thus, the consideration of evaporation of individual droplet (under unchanged supersaturation) is not physically grounded and cannot be a time scale related to the mixing process. This comment is added to conclusion of Pt2.

2. Top of p. 30274. The Damkoehler number was introduced much earlier that Lehmann (2009), not using the name. Old paper by Latham, Baker and others should be cited here.

(R) Done

3. P. 30275. I think the fact that S can be taken as a linear combination of the supersaturations between the two volumes after the end of the mechanical mixing is an interesting observation. However, the small temperature difference assumption may be valid for the cumulus, but it is likely invalid for the subtropical stratocumulus. The inversion jump there is typically around 10K and the very top of the cloud may be exposed to several degree temperature differences between cloudy and cloud-free air.

(R) Very high temperature jumps like 10C above cloud top of stratocumulus cloud indicate, supposedly, the absence of intense turbulent mixing between cloud and dry air. In many cases temperature jumps is a few degrees C (H. Riehl). Penetration of clouds from BL to the inversion layer and their evaporation within this layer indicates the existence of strong interaction between Sc and the inversion layer.

A comment is included: At temperature differences of 5-10^{o}C, the deviation from the analytical solution (6) increases, which requires using more precise formulas for supersaturation (see Pt. 1).

4. Section 3 should be compressed. Section 3.1 shows formulas already introduced and used earlier in the paper (Part 1).

Formulas presented in Appendix A of Pt 1 which are the same as used in 3.1 of Pt 2 are deleted (since they are not used in Pt 1).

(C) Section 3.2 should be combined with 3.3 focusing on the key outcome (shown in Fig. 3). Section 3.3 provides a universal (i.e., using nondimensional parameters) analysis of the problem.

As much as it is interesting, it is not very useful in my opinion. My suggestion is to compress it into an appendix in the rewritten paper.

(R) We disagree with Reviewer. We believe that obtaining of universal dependencies is one of the most important results of the study. This universal solution clearly shows the comparative role of different microphysical and thermodynamical parameters. This role is hardly possible to get from numerical studies. Note that Formula (6b) and Fig. 6b show actually the analytical expression for widely used mixing diagrams. Thus, we cannot shorten this section or move it to an appendix.

(C) The conclusion at the end of section 3.3 that the time scale is the phase relaxation time scale is trivial. Is the phase relaxation time scale about a response of the system to the supersaturation perturbation? The end of the mechanical mixing is exactly such a perturbation. So what is new here?

(R) As was mentioned in our response to Comment C1 of Reviewer, we wanted to stress that the phase relaxation time is a natural time scale of mixing problem in contrast to other time scales considered in literature.

(C) Figs. 3 and 4 and their discussion are not needed. Of course the time scale based on the initial radius changes as droplets evaporate and the mean radius decreases.

(R) Figs. 3 and 4 (and corresponding discussions) show:

a) time dependencies of microphysical characteristics during process of homogeneous evaporation under different initial parameters. Note that in standard mixing diagrams only final equilibrium states are considered. The figures show that the droplet relaxation time is the characteristic time scale.

b) the figures clearly demonstrate the existence of two different possible final regimes which depend in the initial relative humidity (or on universal non-dimensional parameter). These regimes are: total droplet evaporation and termination of evaporation by reaching saturation state; curves separating these two regimes are also presented.

c) it is shown that the theory developed in the study agrees well with the numerical solution of more complicated non-linear equation for supersaturation.

Thus, these figures hardly can be excluded.

(C)5. Section 4 considers a polydisperse case. As before, I did not attempt to follow the derivations, but as I stated before I consider this problem trivial: you know how much water has to be left at the end (i.e., after the parcel is brought to saturation), so the only problem is how much shift of the spectrum is needed. I am not sure if obtaining detailed formulas for the evolution of various quantities is of significance in this highly idealized problem.

(R) This part illustrate non-trivial features of homogeneous mixing in case of initial polydisperse DSD. It is shown that (in contrast to general opinion)

a) Homogeneous mixing can be accompanied by a decrease in droplet concentration;

b) Homogeneous mixing may lead to an *increase* in the mean volume and an increase in the effective radii. The increase or decrease in the effective radii depend on the characteristic of initial DSD.

Since in real clouds DSDs are polydisperse, the widely accepted believes that homogeneous mixing always decreases effective radius keeping droplet concentration unchanged is wrong. These conclusions are main ones of the Pt2.

We would like to stress that consideration of such "highly idealized problem" showed results that were not recognized by investigators for many years. This study shows, therefore, that even such simplified model has direct relation to real clouds in which DSDs are polydisperse and time dependent.

6. Section 5 does not belong to this paper, not in this form. First, the authors talk about "parcels". Which fluid flow model applies parcels in its formulation? I think the authors mean "grid volumes", not "parcels". Second, finite-difference fluid flow models (with some exceptions) typically assume that a grid volume is homogeneous. Any flux in and out (advection, turbulence, etc) leads in instantaneous homogenization of such a grid volume after completion of the time step. So obviously such a model does "homogeneous mixing". This is trivial. One can design a model (or scheme) that includes (i.e., assumes) a subgrid-scale structure of model-predicted variables, but this is a different story. Overall, section 5 is perhaps a start for a new scheme development, but it does not belong to this paper.

(R) This is not so trivial as reviewer assumes. The mixing that is simulated in any model is inhomogeneous at resolvable scales (the text is clarified) and typically homogeneous at the subgrid scale. Since we consider the understanding of this point is actual for cloud community, we decided to keep this section as Discussion, because it is important for cloud modelers.

7. Conclusion section is again short. As I said before, I consider point 2 a trivial consequence

of the bulk evaporation in case when small droplets in the initial distribution
have to evaporate completely.

(R) We suppose that if we would follow all recommendations of Reviewer concerning shortening and deleting entire sections, the conclusions would be indeed short. We believe, however, that in our responses we managed to show that results obtained are new and not trivial. As regards to specific comment of Reviewer to point 2 of conclusions, we agree that indeed the result is natural and easy to understand. The problem, however, is that during many years investigators believe (see, for example, comments of reviewer 2) that homogeneous mixing always decreases drop size, mean volume radius, but keeps droplet concentration constant. This is a wrong believe, because in contrast to the classical concept, the DSDs in real clouds are polydisperse.