The response to comments and remarks of Referee# 1 to the paper "Theoretical analysis of mixing in liquid clouds – Part 3: Inhomogeneous mixing" by M. Pinsky et al.

We express our gratitude to Referee#1 for his comments and remarks

Interactive comment on "Theoretical analysis of mixing in liquid clouds – Part 3: Inhomogeneous mixing" by M. Pinsky et al.

Anonymous Referee #1

Received and published: 23 January 2016

Review of "Theoretical analysis of mixing in liquid clouds", in three parts. Overall recommendation: reject and encourage rewriting and resubmission. 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 forparts 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.

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® 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.

©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.

® Strictly speaking, as follows form the analysis in the paper, the limiting case referred to as extremely inhomogeneous (at which drop radii do not change) corresponds to the absence of mixing (actually some mixing remains that corresponds to the pure molecular mixing). That is why we introduced the concept of extremely inhomogeneous mixing as the case when the mixing process is very slow (very high Da). Mixing with smaller Da is referred to as inhomogeneous. We also reveal parameters at which homogeneous mixing dominates in some sense over inhomogeneous. Such cases we refer to as homogeneous mixing.

Specific comments to Part 3:

General comment:

© Part 3 discusses an idealized case of the 1D diffusion between initially monodisperse condensed water volume and subsaturated cloud-free volume. Such a problem is supposed to

mimic the homogenization process in the inhomogeneous mixing scenario. The authors develop a nondimensional equations and solve them. I really run out of steam to read this part carefully. Thus, my comments are even less detailed than in the case of Part 2. That said, the diagram shown in Fig 16 is interesting and with proper exposition may become useful in the development of subgrid-scale schemes for LES.

® We agree that the delimitation introduced between regimes of mixing can be useful for LES. Corresponding discussion about the perspectives of mixing investigation is added into the conclusion section.

© How the transitions between various mixing scenarios compare to the outcome of DNS simulations reported in Andrejczuk et al (2009)? That paper is not even mentioned, but I think it is relevant, like the Krueger's EMPM model mentioned in the conclusion section.

® The approaches are quite different. In our study time evolution of DSD and their moments as well as other parameters like RH in course on mixing are investigated. Andrejczuk et al (2009) present only final results of DNS simulations in the form resembling the standard mixing diagrams.
The references to the studies by Andrejczuk et al (2009) and Krueger's (EMPM model) are included.

©And what about the Jensen et al. (JAS, 1985) predictions (not mentioned either)?

® The study by Jensen et al. (1985) assumes that cloudy air has two sources: air coming from cloud base and air coming from cloud top. A method of calculation of cloud parameters in the case of multiple penetrations of dry volumes into the cloud air is proposed. The approach is tested using a model of ascending cloud parcel. The approach and subject of investigations in the study by Jensen et al. (1985) and in the present study are quite different.

Overall, I find Part 3 the most promising and I feel that focusing on results discussed in Part 3 should be the goal of the new paper.

® We also suppose that Pt 3 contains interesting results. As regards to the new paper, see our response above.

Specific comments:

© 1. The time scale describing droplet evaporation is again taken as the phase relaxation time scale (and used to define the Domkoehler number applied in the investigation). Part 2 shows (not surprisingly) that this is the correct time scale for the homogeneous mixing. I am not convinced that the same applies to the inhomogeneous mixing.

® In our response to corresponding comment of Reviewer to Part 2 we showed that the phase relaxation time is a natural characteristic time of the mixing process, because this time determines both the time evolution of sub-saturation over ensemble of droplets and also time evolution of liquid water mass.

The fact that the drop relaxation time is the natural time scale at any type of mixing is clearly follows from the non-dimensional form of diffusion-evaporation equation showing that Da is the ratio of mixing time to the drop relaxation time.
Corresponding comments are included into the text and in the Conclusion section.

© In the limiting case of the extremely inhomogeneous mixing (which in the current setup corresponds to the mixing coefficient taken as the molecular diffusivity), the rate of the homogenization progress depends also on the initial humidity of the cloud-free volume, doesn't it? Thus, the time-scale of droplet evaporation should be some combination of the information provided by the phase relaxation time scale and the humidity of the cloud-free volume.

® The reviewer is correct. Yes, it is shown that the mixing -evaporation process depends on two parameters, Da and R. The parameter R is related to the initial humidity of the cloud-free volume. These two parameters determine process of mixing at any regime, not only in the case of the extremely inhomogeneous mixing.

© Note that the other time scale that can be used (calculated as the time required for the total evaporation of a single droplet as used in Jarecka et al. I think) excludes droplet concentration. However, droplet concentration clearly is a relevant parameter in the problem of the cloud interface propagation due to molecular diffusion in the 1D problem considered by the authors. Thus, I feel that 1D results applying molecular mixing can be used to calculate

the proper time scale for the homogenization and to explore which time scale (the phase relaxation or the evaporation) is more appropriate.

® As we showed above, the characteristic time scale of the evaporation process is the phase relaxation time. This time is automatically follows from non-dimensional equations containing Da. In our 1-D problem we use averaged equations, so effects of turbulence are described using coefficient of turbulent diffusion. At the averaging scales there are clear definitions of concepts of droplet concentration, supersaturation, etc. The scales, where molecular diffusion dominates (Kolmogorov scales), are of the same order as the distance between two droplets. At these scales the concepts of concentration, supersaturation over the droplet ensemble become uncertain. We do not consider mixing at such small scales, as it is mentioned in the paper.

We deal with turbulent mixing and with turbulent diffusion. The interface between initially cloud and droplet-free volumes in our problem is of turbulent nature. Corresponding comments are added into Section 2.

© 2. Sections 2 and 3 are in my view incomprehensible. Details of the mathematical derivations should be moved to the appendix and only key formulas should be left in the main text. Section 4 can be shortened to just a few sentences.

® We shortened Section 3. Expressions in Sections 2 should be kept in the main text because they show the physics of mixing process. Section 4 is very short. The further shortening would lead to loss of important information about method of solution.

© 3. I was not able to read through section 5. However, I noticed that the title of section 5.3 is practically the same as section 5.3.2. Shortening (!) and reorganizing is needed.

® The title of section 5.3.2 is changed. We do not see the ways to shorten the section 5 without loss of clarity.

© 4. Can the results be further synthesized? For instance, figures 6 to 9 show time evolutions of profiles across the simulation domain. Can just one such a figure be shown and outcome of other simulations be simply presented applying some measure(s) of the evolution? I think this

is what Fig. 16 is showing, but honestly I was too tired reading the three parts to get the points clearly. Please simplify the discussion and streamline the presentation.

® As was shown in the study, the process of mixing depends on two non-dimansional parameters: Da and R. So, we showed time evolution of most important microphysical parameters at 3 values of Da and 3 values of R (to cover the range of changes of these parameters). Note that the time evolutions clearly show that times of reaching of final equilibrium state are quite different, so that in-situ observations hardly show only final states (as it is generally assumed in analysis of observed data using mixing diagrams).
So, we do not see the ways to shorten the Section.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 30321, 2015.