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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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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 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 multi-scale natural system.

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.

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

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 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. 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.

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.

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.

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.

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