Atmos. Chem. Phys. Discuss., 15, C11945–C11949, 2016
www.atmos-chem-phys-discuss.net/15/C11945/2016/

Interactive comment on "Theoretical investigation of mixing in warm clouds – Part 2: Homogeneous 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 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 2:

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.

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).

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.

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.

4. Section 3 should be compressed. Section 3.1 shows formulas already introduced and used earlier in the paper (Part 1). Section 3.2 should be combined with 3.3 focus-

ing 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. 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? 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.

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.

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.

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.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 30269, 2015.