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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Theoretical study of mixing in liquid clouds – Part 1: Classical concept" *by* A. Korolev 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 1:

1. The title should include "concepts", not "concept".

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.

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.

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.

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?

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

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

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).

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

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.

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

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

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.

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

15. Conclusion section is short, perhaps not surprisingly.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 30211, 2015.