Atmos. Chem. Phys. Discuss., 11, C4142–C4144, 2011
www.atmos-chem-phys-discuss.net/11/C4142/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Resolving both entrainment-mixing and number of activated CCN in deep convective clouds" *by* E. Freud et al.

Anonymous Referee #2

Received and published: 1 June 2011

This manuscript proposes a new method to derive the average number of activated drops in cumulus clouds, which is interesting because it is not a value that can be measured directly. The overall idea seems sound and publishable and will be a good contribution. However, the way the paper is written and the results presented generates substantial confusion and uncertainty in the mind of the reader, so I believe some fairly substantial changes should be made before it is ready for publication. Also, I believe that some sense of the uncertainty in this method is warranted.

Main comments:

1. The use of both effective radius R_e and volume mean radius R_v is seems to make things much more complicated than necessary. Please change to using only R_v only since that's what naturally comes out of the equations. R_e is primarily used for radiation

calculations. I believe a short section near the end of the paper could address the relationship between R_v and R_e to connnect these two concepts, but throwing α into the mix and swtiching from R_e to R_v in different plots in the course of describing the method just adds far more confusion than needed.

2. Why do the data sets used change from Fig 2 to Fig 3? The perception is that something is being hidden from the reader. To alleviate this, use the same data set for all steps so the reader can see how each step transforms an initial data set.

3. The authors state that it is difficult to do this, but essentially the method is an exercise in curve fitting so I think it's possible and important to do. I believe it's important because there does seem to be substantial sensitivity of the final result to the method, as even small changes in RH_best lead to large (order 10%) changes in N_a. Expressing this seems important. I understand that some of the uncertainties related to the measurements themselves are hard to tackle, but the Lance et al. 2010 paper in AMTD which characterizes the CDP should be useful in this regard. At the very least, the uncertainty associated strictly with this method should be quantified, i.e. uncertainty assuming the measurements are perfect.

4. It's unclear if the authors really took the coalescence problem as serious as they should. They say (p. 9691, lines 23-24) that no more than 5% of the liquid water should have converted to hydro-meteors. How do they define hydro-meteors? What is the size threshold? How was this measured (since the CDP doesn't measure drops in the larger size range)? I think a much more rigorous filtering process to make sure that these really were clouds where collision-coalescence was not active is needed.

5. Why do Figs 1 and 3 have adiabatic fraction AF on the x-axis, but Fig 2 has it on the y-axis? It seems like these should stay consistent. Also, I strongly believe that Figs 1 and 3 should be plotted in log-space so that the fitted curves become straight lines. Or alternatively, plot mean drop volume R_v^3 (as in Fig 4) since this will also produce a straight line. It's easy to do and also the right way to present data, in my opinion. And

ACPD
11, C4142–C4144, 2011

why does Fig 4 plot R_v^3 where the others plot R_v?

6. There's some inconsistency in the plots as to whether they are 1-sec data, or penetration averages, or daily averages. And it's not always clear which is being plotted. Figs 2, 4, 6 and 7 have no statement for what length of time each point represents, and it's hard to figure out what they really represent. Figs 4 and 5 give averages over single penetrations, which seems like a reasonable choice for a fundamental averaging period. Why do Figs 6 and 7 have much less data, then? How are these data averaged?

7. Does Fig 3(b) imply that all 1-sec data points always fall along a single homogeneous mixing curve, i.e. if one were to plot the points on a similar plot to Burnet and Brenguier 2007, that all the points would sit along a single curve? I find this surprising since no other study finds such behavior. Usually there is substantial scatter in 1-sec data within such a diagram, indicating that no single choice for an effective "homogeneous mixing" RH is applicable. See Burnet and Brenguier 2007 (Fig 8) and Lehmann et al. 2009 (Fig 5) for Cu examples that clearly behave very differently. What makes these data behave so much better than these previous studies?

8. The short section on alpha needs to be fixed. The value 62.03 clearly has units but it's never given, which makes it all the more confusing once that same value is used for computing alpha.

9. Lastly, I'm not sure the data in this study are enough to fully support the last two points in the conclusions. The second sentence in point #3 "It appears like the entrained air..." is addressed but, I don't think, with enough thoroughness to warrant a concluding statement. Same for point #4 (lines 20 to 22).

ACPD

11, C4142–C4144, 2011

Interactive Comment