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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Constraining mass–diameter relations from hydrometeor images and cloud radar reflectivities in tropical continental and oceanic convective anvils" *by* E. Fontaine et al.

Anonymous Referee #3

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Review of "Constraining mass-diameter relations from hydrometeor images and cloud radar reflectivities in tropical continental and oceanic convective anvils" by Fontaine et al. The authors have conducted an interesting research project looking at mass dimensional relationships using novel methods. They have used particle imagery from 2D optical array probes and mathematical techniques as well as radar reflectivities to constrain the mass dimensional parameters. The research is very interesting and provides some interesting results and I feel is worthy of publication in ACP. There are a number of substantial issues that the authors should address before publication.

1. The authors state that their work was partially inspired by the work of Schmitt and Heymsfield 2010. While there are substantial similarities in some of the particle probe simulations, the work by Schmitt and Heymsfield did not use any remote sensing data. The authors of this work state that Schmitt and Heymsfield used remote sensing data from ARM to constrain the relationship between alpha and beta. The ARM data presented were another aircraft particle probe dataset. The authors should re-read the right column on page 1612 of Schmitt and Heymsfield to understand how the alpha factor was mathematically (not empirically) determined in that study. I would also encourage the authors to try this method and see how it compares to their alpha values. 2. The authors state that they only used the 2DS probe for area dimensional relationships even though PIP data was available. How much difference was there between 2DS and PIP area measurements at the largest sizes that the 2DS was seeing? There will obviously be discrepancies at small (for the PIP) sizes, but there shouldn't be too much difference at larger sizes (more than 20 pixels for the PIP). My concern is that in not using the PIP area information for the largest sizes, you may be losing valuable information on the fractal properties of the particle population. The density values determined for large particles suggest that there should be a lot of graupel or hail present, and it is likely that the PIP would show that better. Also note, that aircraft probe data at large particle sizes aren't necessarily randomly oriented. This could affect your results as well. Larger particles are naturally oriented due to aerodynamic affects and this orientation may not be disturbed enough by the airflows near the probe for the orientation to be considered random. 3. The choice of mostly pristine particle shapes for use in determining the relationship between the power is not really realistic. The shapes that you show in figure 6, how often to you see these shapes in the 2DS data or, more important, in the PIP data? This is probably part of the reason that your equation 11 is so substantially different from the results found in Schmitt and Heymsfield. For each random orientation, it is possible to calculate a density. How do those density values compare to those determined by your mass dimensional relationships? 4. Using the alpha and beta values given in the summary for the mass dimensional relationship, I

get m=0.0244*D^2.44 or m=0.0266*D^2.44. This is really heavy for large particles, and substantially different for published mass dimensional relationships. The authors need to explain why there is such a substantial difference. Is it specifically related to your dataset? Or, are all of the others wrong, and if so, why?

Further comments: Page 2984 line 2: Ice hydrometeors (without the word "ice" you could be talking about rain as well). Page 2987 lines 9-12: Relationships derived in SH2010 were derived numerically, and tested with aircraft data. (See major point #1 above). Page 2987 line 13: There should be a period at the end of this line. Page 2988 line 21: Was the Nevzorov probe used a standard version or modified with a deeper groove? The standard version likely underestimates CWC when there are high concentrations of larger particles which can shatter and partially bounce out on impact. (See Korolev's 2011 BAMS article) Page 2990 line 8: TSD is not defined. Could you mean AsD? Page 2990 line 24: How do the measured aspect ratio values from the probe measurements compare to the average aspect ratio calculated for the theoretical particles (in the appendix)? (Densities as well) Page 2991 line 24: You assume that the reflectivity at the aircraft is the value linearly interpolated between the value 300m above and the value 300m below. How different are these values typically? Difference and standard deviation and how much uncertainty does this cause in the results? It might be interesting to look at the difference between 300 and 900 meters above and see if that difference is similar to the difference between 300 above and 300 below. Page 2992 & figure 4: Doesn't 5 g/m3 CWC seem rather high? Your dBZ values are very high. My concern is that the Protat 2007 parameterization appears to include data only up to about 10dBZ. Could higher values be influenced by graupel or hail? This should show up in the PIP data. Figure 4 should also have the dBZ value derived for the pass. Page 2994 line 13: Suggest that you try the Schmitt and Heymsfield method for calculating alpha directly from the particle area data and compare to your results. Page 2995: Sigma for each particle shape is calculated, but it is unclear how. Is the area and maximum dimension determined for each random orientation, then a fit done to maximum dimension versus area for all of the individual rotations? If so, then

your sigma value may be more related to the orientation rather than the size to area relationship. Page 2995 line 21-22: using 1.0 for sigma yields a value of 0.6 for beta (outside the range presented on line 21). Page 2995: How many of your theoretical particles are truly irregular? How many of your observed particles are truly irregular? Page 2997, line 17: Consider comparing either Brown and Francis and/or Heymsfield et al 2004. There are no assumptions on shapes in these as well. Page 2998 line 25: From here, there is a lot of discussion of the basic properties of the clouds measured during the campaigns. It isn't clear why it is important to discuss this now. Much of it isn't relevant to the study. Page 3001 line 3: It would be interesting to plot some typical density values from your alpha beta pairs as compared to density values from the literature. Given the extremely high dBZ values recorded, can these results be generalized? Page 3004 line 4-5: When I compare these alpha and beta pairs to BF, the results show a similar density predicted for 200 micron particles, then for larger (3000 um) up to a factor of 5 higher density for your results. This difference (with BF and others) needs to be shown and explained. Page 3006 line 9: It would be good to show typical plots of the shape of the PSDs so that they can be compared to other data. Gamma fit parameters (lambda, mu, No) as are commonly shown would be helpful.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 2983, 2014.