

Interactive comment on “A novel approach to characterize the variability in mass-Dimension relationships: results from MC3E” by Joseph A. Finlon et al.

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The statistical analysis that is conducted in this study is thorough in many regards, lacking clarity in some places, and requires some further discussion and analysis yet in other places. The general premise of the study is that further evaluation is needed of the mass-diameter equations that are used to derive cloud water from remote sensing measurements. This is an extension of the analysis that has been done by others, and is amply referenced in the introduction. Although I consider this a useful study, worth of publication, I am reluctant to immediately recommend it for publication without having my primary concerns addressed, as well as some of my lesser ones that are listed

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after I enumerate the more important questions (to me). 1. The methodology by which TWC and reflectivity (Z) are derived from the size distributions need to be explicitly stated, in equation form, at the first of the paper. TWC is never shown and Z is not shown until the discussion of how the equally plausible surfaces (EPS) are computed. It seems more logical to have the equations on TWC and Z prior to the error term rather than afterwards. 2. When the computation of TWC and Z is introduced, there has to be a more vigorous discussion of the expected uncertainties when deriving the size and the mass from the 2D images. This is also why it becomes confusing at a later point when there is a discussion of the effective density and its impact on the EPS. What effective densities were used to derive TWC in the first place? Wouldn't that bound the uncertainty in the a&b coefficients? 3. How was the 10 second averaging period derived? Wouldn't it have been much more consistent to use variable sampling periods that always ensured statistically significant number of particles? 4. What is the rationale for equally weighting the TWC and Z error terms? To me, this is a significant assumption that needs a more thorough discussion. The Nevzorov TWC probe has a sample area much smaller than the 2D and HVPS while all three instruments sample a volume many orders of magnitude smaller than the radar. How do you reconcile these differences? If you construct the EPS from the TWC and Z independently, are they similar, or does TWC drive the minimization some of the time and the Z others? I understand, and appreciate, the care that was taken to obtain homogeneous samples from the Z data, but ask that my question be addressed in the manner I suggest. 5. I found the summary somewhat incomplete in that it concludes that there will always be a broad region of EPS in any given situation. If I am a radar or satellite scientist, this would lead me to throw up my hands, put the EPS chart on the door, and throw a dart. Is this what the authors suggest we do? If not, then I strongly suggest that the paper end on a more positive note that can recommend to the remote sensing community what should be done. Less major comments (that still require attention) 1. Why was the CIP not used? It is introduced as one of the probes on the aircraft, it has twice the number of diodes as the 2D-C, 100 mm between arms rather than 63, and hence

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more sample volume, yet it isn't used. Why? 2. How are the 2D-C and HVPS size distributions combined? Do they always overlap well? If not, how is this reconciled? 3. Error analysis, error analysis, error analysis. What are the expected uncertainties in the EPS due to choice of size and effective density? 4. Page 5, line 17. "Following Heymsfield and Baumgardner (1985) and Field (1999), only particles with a center of mass within the OAP's field of view were considered as otherwise there is too much uncertainty in particle shape." Several corrections/questions here. First of all, is the center of mass being located in the field of view or center of the measured image? In either case, how is this determined? Finally, using the "center-in" technique mostly reduces uncertainty in size, not in shape. 5. Equation 3 shows TWC being averaged not Z, but don't the derived Zs also get averaged?

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