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Interactive Comment

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 #2

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Review of "Constraining mass-diameter relations from hydrometeor images and cloud radar reflectivities in tropical continental and oceanic convective anvils" (E. Fontaine et al.)

Summary:

This paper presents a method to derive the coefficients in the power-law relationship between ice-particle mass and diameter by means of constraining them to direct observations from ice probes and observations and simulations of cloud-radar reflectivity.

The methods presented mostly build on existing theory, but the combination of various observations allow for a novel approach. The science and methodology appear sound and the results are worthy of publication. The paper in its current form is not suitable for publication due to excessive length, as it includes numerous figures and discussions which distract from the main result. Therefore, major revisions are advised, based on several points outlined below.

Major comments (structure):

1. Introduction (p.2989, lines 14-22): This section requires more elaboration on the motivation of the study. What is the actual problem with fixed mass-diameter-relationship coefficients? Do we need them to vary with temperature? How will it help Megha-Tropiques retrievals or numerical simulations, especially when cloud-ice probe data and radar data are unavailable? Addressing the purpose more strongly in this section will give the paper clearer focus and should allow for better understanding of why certain figures and discussions appear later in the paper.

2. Presentation of method/results: Sections 3 and 4 should be swapped around. Section 4 currently is heavy on methodology and its result, its linear relationship between beta and sigma, will help focus section 3 and understanding of the results in that section. Since the remainder of the results concern beta_sigma, the authors should consider ignoring the use of beta_i on pages 2992 and 2993. Although interesting, the beta_i do not re-appear once the beta_sigma have been introduced. The discussion on pages 2992 and 2993 could be shortened and focus on the derivation of alpha_sigma.

3. Section 5: This results section is currently overflowing with figures which are poorly introduced, and various discussion points seem irrelevant to the paper's main focus. The only results related to the section title ("Mass-diameter relationship") are figures 10 and 11b and 11c; it is unclear why the other figures are included. The authors should consider reducing this section to "Retrieved mass-diameter relationships", which discusses figures 10, 11b, and 11c. This discussion can then continue with figure 9, which

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compares the CWC simulated from the retrieved mass-diameter relationship with theory and observations. The remaining figures (11a, 11d-f, 12, 13, 14, 15) all show interesting results, but these have no immediate purpose in this paper. If the authors are adamant that these figures should be included, they are advised to combine them in a separate section, for instance "Altitude relationships of cloud-ice properties".

Major comments (science and method):

4. Matching of observations in time and space (pages 2989-2990): It is currently unclear over how many observations the particle size distribution is calculated; what is the stretch of time? And what is Lˆ-1 (line 9, pages 2989)? The authors should include a paragraph at the end of section 2 to describe the radar data: how are the radar observations matched in time and space to the PSD detected by the aircraft? This currently partly appears elsewhere but the information is required here for the reader.

5. Equation 4 (mean aspect ratio): At a later stage, the authors mention that <As> is calculated only for Dmax within the 94GHz radar sensitivity - is this true for equation 4 as well? If so, please adjust this in the summation. Since the mean aspect ratio is used for radar reflectivity calculations, it is worrying that the summation is weighted by particle number concentration, and not by mass or mass-squared. The radar reflectivity will be dominated by large particles, so the effect of flattening observed in Z should mostly come from large particles. The <As> however is weighted towards the more numerous (likely smaller) particles, which are expected to be more spherical, thus <As> might be closer to 1 than what would be observed by the radar. Could the authors consider changing the equation to weight it with mass or mass-squared instead of number, or at the very least consider this option in the text?

6. Vertical trends of mass-diameter coefficients: This appears to be only weakly supported by the results, but is stated as a major conclusion in both the abstract and the conclusions. In a revised section 5, the authors are advised to more carefully establish these "vertical trends": what is the relationship of alpha and beta individually with tem-

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perature, and how significant is this relationship? These trends look rather vertical in figures 11b and 11c and certainly within the error bounds presented.

7. Use of a single m(D) relationship to calculate CWC (page 3003): This seems a missed opportunity to test the effect of having a variable m(D) relationship. The authors have the tools to assume a single m(D) relationship (e.g. beta=2.44, page 3005) and calculate a Z-CWC relationship, or even use BF95 on their observations to calculate Z-CWC. This will test how advantageous it is to have a variable relationship, rather than comparing with P2007. Using their own data to test this, the authors could possibly add a major conclusion and scientific advance to this paper.

Minor comments:

8. p.2986, line 24-25: "Retrieved relationship are finally used..." - by whom? By Lawson et al.? By the authors? 9. p.2987, line 2-3: "vertical profiles" - of what? Radar reflectivity? 10. p.2987, line 6: What numerical simulations? Of scattering properties? 11. p.2987, line 13-16: What was the strong relationship from H10 based on? Theory, observations, simulations, something else? 12. p.2989, eq.2: How good is the PIP at Dmax<950, if it will only measure 9 pixels across? 13. p.2991, line 21-22: The authors are advised to call alpha_i here alpha_j, and use alpha_i only for the alpha which minimizes the reflectivity difference. (Though this part of the text may be removed if the revised discussion solely focuses on alpha_sigma). 14. p.2992-2993: Are there no error calculations for alpha, beta, and CWC? The uncertainty from the reflectivity differences when finding the alpha_i could be used to weight-average CWC in equation 8. 15. equation 10: It is not clear which measurements are used to find gamma and sigma. 16. equation 11: Is there any evidence in literature of such a fit? Should we expect a linear relationship between beta and sigma? A bit more discussion is required here. 17. p.2996 lines 9-16: What type of growth speed do the authors consider? Growth in time? Growth with change in diameter? 18. p.2997 line 14: Where is this Sierra Nevada? 19. p.2997 lines 9 and 17-18: These statements appear related and should be combined in a single sentence (exponent close to 1 and good correlation).

20. p.2998 line 5-6: Is this correlation between alpha and beta expected from theory, or is it a result of the methods used in this paper? 21. p.2998 line 14: How would the different beta-calculation of H10 affect the slope? 22. p.2998 line 17: How does this (weak) variation with temperature relate to CWC(Z,T) relationships? 23. equation 12: Why not use a single exponent for the constant and the beta-dependence? 24. p.2999 line 17: What is this horizontal variability? Horizontal across the width of an anvil? 25. equation 13: Note that this equation is very similar to that for f_ice, that is, rho_eff = rho_ice * f_ice. Any reason why? 26. p.3001 line 12-13: "most of the total mass resides in the range" - This is a confusing statement, as the total mass referred to here is actually the sum of M(Dmax) over the different Dmax, whereas the authors have already defined M(Dmax) to be total mass. Better to define M(Dmax) as the mass of particles of size Dmax, not as "total mass". 27. p.3002: How do the authors' findings relate to existing CWC-Z relationships, and why do they think there is no CWC-Z-T relationship? Is this because the temperature dependence is incorporated in alpha and beta, which both affect CWC and Z? (also p.3004, line 9-16). 28. p.3005 line 4-5: Is there any significance in the MT2010 and MT2011 sharing the same beta? How do these average alpha and beta compare with literature? 29. p.3005 line 10-11: "Since <As> increases with altitude, the reflectivity of the larger diameter particles decreases with altitude" - a large particle's reflectivity will change with altitude if its own As increases with altitude, not necessarily the mean As. The mean As could simply change because there are more numerous small (spherical) ice particles. The current statement is confusing and should be rewritten. 30. Figure 16: What is the purpose of this figure and why is it introduced at this stage? Its discussion on p.3005-3006 reads as a description of observations and would have made more sense in section 2.

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