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Interactive Comment

Interactive comment on "Developing and bounding ice particle mass- and area-dimension expressions for use in atmospheric models and remote sensing" *by* E. Erfani and D. L. Mitchell

Anonymous Referee #1

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Review of "Developing and bounding ice particle mass- and area-dimension expressions for use in atmospheric models and remote sensing" by Erfani and Mitchell

Recommendation: Accept after revision of manuscript.

This paper presents self-consistent ice particle mass and projected-area dimension relationships that are not power laws, but that can be easily reduced to power laws, that are valid over a much larger range of D than are power laws. This is done through analysis of data collected by a 2-dimensional stereo probe and Cloud Particle Imager in synoptic and anvil clouds at similar temperatures, and it is shown that the developed relationships are in good agreement with m-D power laws developed from recent field

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Interactive Discussion

Discussion Paper

studies. The unique contribution of this paper is that it develops m-D and A-D relations that aren't power laws that cover larger ranges of particle sizes. Further, for implementation in schemes that require such power laws, the formalism can be converted to power law. As such, I feel that this paper is worthy and should be published in ACP subject to the suggested modifications below.

In terms of an historical perspective on the development of power laws, the authors reference the papers by Mitchell (1996, 2000, 2002) and Mitchell et al. (2006), and later on a series of papers by Heymsfield et al. (2004, 2007, 2010). It is important to note that many groups in addition to that of Mitchell and Heymsfield have been involved in the development of such power laws, and a more balanced list of references should be provided (e.g., Fontaine et al. 2014). It is also important to note that authors have derived power law relationships from measurements of bulk reflectivity in addition to measurements of bulk total mass (e.g., McFarquhar et al. 2007, MWR, and Locatelli and Hobbs 1974 should be referenced more early on in paper). I recommend that such studies also be referenced.

Why are the results from the Sierra Cooperative Pilot Project used to assess the aircraft results. Since these observations were obtained before the advent of the anti-shattering tips, there could be some contamination in the results. But, as Jackson and McFarquhar (2014) showed the shattering does not make a significant contribution to the mass measured by the probe, so perhaps the use of the SCPP data are ok. In any event, it would be good to have some discussions somewhere in the paper about the uncertainties associated with the probe measurements, and why those uncertainties do not affect the principal conclusions of the study.

The authors state on page 28523 that the objective of their study Is to develop m_d and A-D expressions that are representative of all ice particles for a given cloud type and temperature interval, suitable for use in climate models. I also note that the authors do talk some about measurement uncertainty. But, one question that they do not thoroughly address is variability. Past studies have shown that there is a lot of variability in derived m-D and A-D relations. Further, there is some variability even for a given cloud type of a fixed temperature. Given this, do the authors expect that representative relations can be found that are representative for all particles? Some discussion about variability and how the authors address such variability should be given.

As noted in specific recommendations in the detailed comments below, one other recommendation I would give for this paper is to include some more detailed error or uncertainty analysis. In particular, assigning some uncertainties to the estimated mass amounts would have been beneficial. This would go beyond the uncertainty analysis that is done for the polynomial fit expressions for the m-D and A-D relations, but rather relate more to the uncertainties in the fundamentally measured quantities.

Detailed comments:

Abstract line 10. The authors claim that field measurements of individual particle m are used from the 2DS/CPI. There are no measurements of mass from these probes. Further, the measurements of mass that are used to derive m-D relations come from integrated measurements of masses of several particles combined rather than from measurements of mass of single particles (this is correctly stated on page 28520, line 14).

Page 28522, line 15. It is stated that the CPI data are used for sizes less than 100 micrometers. But, is there any diameter below which the CPI data are not used? For example, McFarquhar et al. (2013) showed that it was difficult to extract any information from CPI images for particles with D smaller than 35 micrometers.

Page 28522, line 19. How is the CPI mass estimated?

Page 28522, line 25. How is maximum dimension defined? It is important to note that past studies have defined maximum dimension differently so it is important that the exact definition of maximum dimension (or methodology used to compute maximum dimension) be given.

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Page 28523, line 21: Are particles from the smallest size bin in the 2DS used in the analysis? Jensen et al. (2013) reported that the 2DS may overestimate concentrations of particles with D < 15 micrometers due to a poorly defined depth of field. Further, the sample volume for such sized particles is very small so only a few counted particles can dominate the concentrations. Further, for the larger particles that occur on the edges of the photodiode, how much does reconstruction of partially images particles affect the estimated areas (and hence impact the calculated masses)? For what fraction of particles is reconstruction used?

Page 28524, lines 6-11: It might be worthwhile also referencing the paper of Jackson et al. (2012) who found that the application of habit specific m-D relations applied to size/shape distributions measured during the ISDAC field campaign gave better agreement with the bulk measured masses than did application of the Baker et al. (2006) approach to the measured size distributions.

Page 28524, lines 25-29: There is a limitation to this technique in that the probe sample volume will fall off rapidly with particle size for the entire in technique meaning that very few completely imaged particles will be available for the analysis. This limitation should be specifically stated. Second, what specifically is the "most accurate estimate for maximum dimension" and how is it obtained. Presumably this is very different than the length along the direction of travel which is very different than some of the definitions of maximum dimension that have been used in the literature. Some further comments on both of these points are warranted.

Page 28525, lines 3-7: This averaging procedure over temperature intervals will mean that your contributions from PSDs with larger mass contents will dominate. In their classic study, Marshall and Palmer (1948) averaged PSDs with similar rainrates so that different mass contents would not dominate the comparisons.

Page 28525, lines 14 and 15: Can a more quantitative description of relatively be given?

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Page 28526, line 26: This assumption about hexagonal column geometry seems to be very different than past studies that have assumed small particles are typically quasi-spheres such as droxtals, Gaussian random spheres, Chebyshev particles or budding bucky balls. Can you justify this assumption? How important is this assumption to your final results?

Page 28527, line 4-5: I am a bit unclear on what is meant by not using any mass estimation technique for the size range of 100-200 micrometers. Does this mean that these particles are not considered in your calculation?

Page 28529, line 9: How useful are temperature-dependent curves? In a model scheme typically a single m-D relation is adopted, so how useful is it to have curves as functions of temperature?

Page 28532, line 17: It is worth noting that agreement will always appear good on a logarithmic scale. How good does the agreement have to be in order to be considered good?

Page 28536, line 24: Can you be more quantitative on what you mean by “valid over a limited range of D”. This would help those wishing to apply such relationships.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 28517, 2015.