The material in this manuscript is suitable for publication in amt. It gives a useful comparison between an older particle probe, the 2DC, and the newer probe, the 2DS, thought to provide more accurate ice crystal information. A compilation of the parameterization and normalization of many ice crystal size distributions measured by both probe types is used in an attempt to adjust the older probe data to make that data more reliable.

1. The paper needs a careful review concerning the lack of definition of some given variables. For example, what is a_mi and b_mi in Eq. (4), what is D_eq in the Figures, what is subscript I ?

2. The accuracies of the density/dimension and mass/dimension relationships used in the paper are not discussed, even though they may affect the conclusions reached. A comment on such a possible affect.

3. The data for D05/D014 is listed as starting at 25 um; whereas the data for the 2DS starts at 15 um. Is this taken into account in the comparisons?

4. The author points out the difficulty of the probes measuring the smallest ice crystals, given that the probes can create errors due to uncorrected crystal shattering and other reasons. His sentence associated with small crystals (line 181) "It is therefore felt that the averaging approach is justified" is inconsistent with this difficulty.

5. The paper only deals with integrated ice-crystal properties, but it also points out that the nature of the ice-crystal size distribution should also play a significant role in probe performance. The latter is not dealt with in the paper. It would be helpful for the author to comment on what might be done to improve the size information on the smallest ice crystals that can dominate under certain atmospheric conditions (e.g., Heymsfield et al., 2010, JAS, 67, 3303-3318). For example, can forward scattering probes that respond to small particles be used for ice crystal measurements (e.g., Gerber and DeMott, 2014, JTECH, 31, 2145-2155)?

6. The impressive Appendix is not essential for the conclusions reached in the paper. Deletion of the Appendix is recommended.

Response to Anonymous Referee #2

Thank you for your thoughtful and helpful review. I will address your remarks in order.

1. This point is well taken. I have gone over the text and have removed inconsistencies (viz., that Deq and De are supposed to be the same), have explicitly described each variable and its subscript, and have removed the error of always using the letter "i" for every subscript. Rather than document each change here, I've attached the marked-up manuscript to this reply.

2. In fact, there is an unfortunately high amount of uncertainty in these relations. It was felt that the best that could be done was to use the same relations in this paper as in D05/D14 so as to keep that part of the comparison consistent. This, of course, assumes the same overall mix of particle habits was encountered between the PSD datasets. This is now noted in the discussion section.

3. No, it is not. In light of the difference found, that is well worth pointing out and is done so in the final section.

4. I think perhaps that I've not worded that sentence well and that it is redundant. The "averaging approach" is adopted for smoothing out Poisson counting noise, not for ameliorating measurement problems such as shattering. The shattered particle removal post-processing (performed by the instrument team) is aimed at that. The sentence in question has been removed, and the following sentence has been inserted at line 157 (given with the sentence prior for context).

"In the first exercise, fifteen-second temporal averages were performed along with truncating zero through two of the smallest size bins while only the unimodal fits (chosen according to a maximum likelihood ratio test [Wilks, 2006]) were kept. This exercise was performed first so as to prevent the most spurious size bins' interfering with the smoothing out of Poisson counting noise."

5. This matter is now dealt with in the final section.

1) Finally, it is important to note that this study does not specifically consider PSD shape. (For a more detailed discussion on cirrus PSD shape and on the efficacy of the gamma distribution, please refer to Schwartz [2014].) This is a critical component of the answers to Korolov et al.'s (2013b) original two questions. Mitchell et al. (2011) demonstrated that for a given effective diameter and IWC, the optical properties of a PSD are sensitive to its shape. Therefore, PSD bimodality and concentrations of small ice crystals are critical to realistically parameterizing, cirrus PSDs, to modeling their radiative properties and sedimentation velocities, and to mathematical forward models designed to infer cirrus PSDs from remote sensing observations (Lawson et al., 2010; Mitchell et al, 2011; Lawson, 2011). In order to improve knowledge on PSD shape, as well as to develop statistical algorithms for correcting historical PSD datasets so that PSD shapes are corrected along with computations of bulk properties, it will be necessary to make use of instruments that can provide reliable measurements of small ice crystals beneath the size floors of both the 2DC and the 2D-S. Recent studies such as Gerber and DeMott (2014) have provided aspherical correction factors for particle volumes and effective diameters measured by the FSSP. However, the author expects that this problem will ultimately be resolved by the continued technological development of new

probes such as the HOLODEC.

6. The Appendix has been removed.