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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Understanding effective diameter and its application to terrestrial radiation in ice clouds" *by* D. L. Mitchell et al.

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Review of "Understanding effective diameter and its application to terrestrial radiation in ice clouds" by D.L. Mitchell et al.

Recommendation: Should be acceptable for publication after revisions are made.

This paper investigates whether the effective diameter or radius can, in combination with the cloud total water content, be uniquely used to characterize the optical properties of the particle size distributions. Although the paper is a bit on the long side (it could be shortened), for the most part it is technically sound and easy to follow. Further, the conclusion that treating optical properties solely in terms of effective diameter

and total ice water content leads to errors on the order of 25% for optical properties of terrestrial radiation in the window region is large enough to be significant for climate modeling and remote sensing studies. Thus, I think the paper should be published. However, there is some misleading information in the manuscript and a couple of other problems that must be corrected before the paper can be published.

1. The paper states that "optical property parameterizations . . . based on historical PSD measurements may exhibit errors due to previously unknown PSD errors (i.e. the presence of ice artifacts due to the shattering of larger ice particles on the probe inlet tube during sampling). This, and other statements throughout the manuscript, gives the misleading impression that all prior PSD measurements are biased by shattering. The statements about the CEPEX PSDs could be especially misinterpreted by other readers. In their analysis of CEPEX data, McFarquhar and Heymsfield (1996, JAS), compared the size distributions from a Video Ice Particle Sampler (VIPS) with that from an FSSP. They noted that the FSSP significantly overestimated the number of small ice crystals, especially in the presence of large ice crystals (though they did not attribute this overestimate to shattering). Based on this analysis, when McFarquhar and Heymsfield (1997, JAS) produced a parameterization of the size distributions measured during CEPEX they did not use data collected by the FSSP, but rather used data from the VIPS. Thereafter, Ivanova (2005) produced a parameterization that included the FSSP data even though it had been previously been shown to be unreliable. By using the Ivanova (2005) parameterization and by not referencing any of the other papers on the CEPEX data, this manuscript provides the misleading impression that the CEPEX data are contaminated by shattering events. This is not the caseâĂŤthe McFarquhar and Heymsfield (1997) parameterization does not suffer from inclusion of shattering events. There are also many other examples of PSDs in the literature where the FSSP was not used because of knowledge of its problematic function in ice clouds well before this shattering problem was identified as the source of the problem. Appropriate references and text should be added to clarify this misconception. This is important as those not familiar with cloud microphysics measurements are under the

mistaken impression that all past microphysical measurements are impacted by this shattering problem. Instead, a careful examination of past data is needed to determine which previous results can be trusted (i.e., don't throw out the baby with the bath water).

2. The original definition of effective radius (Hansen and Travis 1974) was defined to give a mean radius for scattering. This should be mentioned somewhere in the manuscript.

3. Replace PSD's with PSDs. The use of the apostrophe implies possessive.

4. I have an additional comment regarding the assumption of the ice particle shapes in the CEPEX PSDs. The authors assume that the ice particle shapes were roughly the same as those in the TC4 PSD in terms of their mass and area attributes. McFarquhar et al. (2002, JAS) developed a parameterization of the optical properties of the CEPEX anvils based on a shape classification scheme identified from the two-dimensional crystal images. Although this parameterization is not relevant for the terrestrial wavelengths used in this study, its use would be more relevant for determining what the optical properties actually are using the observed shapes. I am a bit puzzled why additional assumptions about particle habits need to be made beyond what is observed. McFarquhar (2002, QJRMS) lists actual fractions of different particle habits that were observed during CEPEX.

5. On page 29414, the authors note that the de values for each bin from the Yang et al. (2005) database were used to obtain accurate optical properties. However, I am a bit puzzled as to why this is so important. Given that the PSDs bins and Yang et al. bins also can be matched by maximum dimension, why is it so important that the de be matched? Is this because the observed mixture of habits does is not being assumed in the development of the optical property database? In the development of the McFarquhar et al. (2002) parameterization it made no difference whether we matched the de values or the maximum dimension values.

6. On page 29414, it is stated that droxtals were assumed due to the quasi-spherical nature of the observed habits (and bullet rosettes for the bullet rosette PSD). However, I am a bit concerned about the assumption of droxtals. Quasi-spherical particles dominate for maximum dimensions less than 100 um, but they are much less common for particles larger than 100 um where mixtures of other sized particles are prevalent. I think more discussion about the difference between the observed and assumed particle shapes is warranted.

7. On page 29415, line 25, comments are made about a CEPEX PSD based on observed particle shapes. Earlier in the text it was stated that the shapes for the CEPEX PSD were based on the mass/area attributes that were similar to those for the TC4 PSD. Thus, I am a little confused about the meaning of the observed particle shapes here. Are these the same as the observed mixture of shapes that were assumed in the McFarquhar and Heymsfield (1997) and McFarquhar et al. (2002) parameterizations?

8. I would recommend streamlining and shortening the discussion in Section 3.

Provided that these recommendations are dealt with, I think that this paper should be published.

Greg McFarquhar

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