Review of "Technical note: Bimodal Parameterizations of in situ Ice Cloud Particle Size Distributions", by Irene Garcia and coauthors, submitted to EGUSphere.

This study uses a massive set of in-situ aircraft observations collected from high latitude to equatorial ice clouds and collected in the Julia data base to investigate the size distributions of the ice and mixed-phase clouds over a wide range of conditions. Figure 1 and the cloud descriptions nicely shows the locations of the sampling, which clearly shows where the clouds were sampled. The particle probes included the CDP, FCDP, and the NIXE-CAPS, which is a CAS and grey scale CIP. The acronyms for these probes are identified in the text. Normalized size distributions, derived as a function of the melted equivalent diameter are evaluated. The interpretation of bimodality draws heavily on the data from the small particle probes. It is shown that bimodal particle size distributions (PSD) fit the observations much better than a single mode.

I have several major comments that I would like the authors to consider. A few are as follows.

1. Are the actual size distributions bimodal? Your assumed mass dimensional (m(D)) relationship is poorly constrained for small particles. This could affect your interpretation.
2. I've attached a figure showing PSD measured with balloon-borne ice crystal replicators, with very high resolution, no particle breakup, and unequivocal detection of small particles. There is little evidence of bimodality. It would be interesting to see if your assumed mass dimensional relationship (based on Mitchell et al., 2010) could change this result.
3. I'm very uneasy about your use of the small particle probe data. Shattering is a serious concern. The CAS is known to yield PSD that have major contributions from shattering. This issue could certainly create the bimodality you find. This issue needs to be discussed in more detail, not just in the references cited.
4. Lines. 122-124. Mass dimensional relationship. Some of your measurements are at temperatures considerably warmer than for cirrus. Is there some reason to think that you can apply the modified m(D) relationship of Mitchell to the warmer temperatures?
5. Line 131 and Eq. (3). What is the advantage of using the melted equivalent diameter (from the measured PSD) versus the physical diameter. The former uses an assumed mass diameter relationship which may not be valid under certain conditions.
6. Eqs. (3) and (4). Is it valid to assume that the PSD extends from 0 to infinity, rather than a partial gamma? Does this affect the IWC?
7. Normalizing as a function of N_{ICE}. The value of N_{ICE} is subject to considerable uncertainty and potential error.
8. Line 199. D05 and D14 use the Brown and Francis m(D) relationship. How will this affect your comparison with their normalized PSD.
9. Lines 265-267. "minimize the impact of shattering effects". Down to 3 microns? This is difficult to agree with.

Minor Comments. I feel that the comments above and the few minor comments below are the ones that need to be addressed in the revised article. I'll identify more minor comments after I see the revised manuscript.

1. Line 3. based on aircraft in situ
2. 8. consists of
3. 71. What is the averaging time as that is the relevant time.
4. Eq. (2) what is the [m]
5. 149: remove studied
6. 176: "fast" to "strong"
7. 255. Parameterization