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Interactive comment on "A Statistical Comparison of Cirrus Particle Size Distributions Measured Using the 2D Stereo Probe During the TC^4, SPartICus, and MACPEx Flight Campaigns with Historical Cirrus Datasets" *by* M. Christian Schwartz

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This study is a logical and complementary follow-up of the Delanoë et al. (2005,2014) evaluations that provided parameterizations of cirrus size distributions based on a large set of measurements taken in both mid-latitude and tropical environments. The author has provided a detailed analysis using more recent measurements with a more modern imaging probe to address an important question:

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"Given what we now know about the impact of crystal shattering on measurements by cloud particle spectrometers, can historical data sets be trusted"?

I think that this study has answered that question, at least with respect to cirrus clouds. In addition, even though the instrument that is used in this study has a faster response time than the earlier 2D-C and 2D-P, and marginally larger sample volume, the results of the current study would suggest that such instrument improvements really have minor impact on the overall statistical robustness of the previous measurements and may also only be marginally more accurate, especially given the many other uncertainties that the new instrument has not overcome. In particular, there remain major uncertainties due to unknown ice density and shape n the third dimension that lead to large error bars in derived bulk parameters.

It is only at the very smallest sizes where there is a clear difference between current and previous measurements; however, even when there are several orders of magnitude difference in concentration at these sizes, the propagated error in effective radius, IWC and reflectivity is surprisingly small.

What I think would be a useful, and perhaps even necessary, addition to this paper would be to include in Figs. 7&8 the relative errors and standard deviations that are reported in Delanoë et al. (2005,2014) where they compare their data sets against the parameterization. That would then put into context the current comparison with the parameterizations with the original, hence bringing closure.

The other very important source of uncertainty that the author side steps is that of oversizing of out-of-focus ice crystals (Korolev, 2007). Although a correction for this issue has not yet been provided, such as has been done for water droplets, measurements in cirrus clearly show crystal images that are out of focus and that should be size-corrected. These might even be the source of the "bump" in the size distributions, i.e. a certain fraction of the particles in that size interval most certainly are smaller crystals out of focus. This bump is also seen in the Delanoë et al. (2005,2014) studies;

however, whereas the bump occurs in the current study at a Deq/DM <1, in Delanoë et al. (2005,2014) the bump is right at 1. How does the author explain this?

Lastly, the author refers to three of his papers that have not yet been published. These references should be removed since, as a reviewer, I was unable to access them.

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