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Interactive comment on "Evaluation of the reflectivity calibration of W-band radars based on observations in rain" *by* Alexander Myagkov et al.

Anonymous Referee #2

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Summary ——— This paper looks into the calibration of W-band FMCW cloud radars using a variety of methods. The primary contribution uses self consistency and polarimetric spectral data to select a region of the spectra that is believed to be primarily rayleigh scattering, and use this for the calibration by fitting several self consistency curves. There are also comparisons to using disdrometer scattering as a calibration source. Overall the paper is well written, covers an interesting subject, ande has an actual need in the field. I'm recommending major revision as I believe the handling of the uncertainty could be better, and I have some concerns about the effect of other confounding variables on the accuracy. The paper is however important, and the methodology does appear sound. Overall it was a pleasure to read the paper and I believe after addressing my issues this paper will be an important contribution to the field.

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Discussion paper

I apologize to the authors for the delay in the review as Covid has mixed a few things up.

Major Comments ——- Section 3.2: One large question I have here is how this varies by temperature, DSR, and choice of canting angle distribution. You discuss in the appendix how it is fairly stable with rotation (and so likely not dependent on DSR and canting angle) but that is only for the back scattering coefficients. Is this true for the forward scattering coefficients and delta upon which this method relies?

The choice of fitting functions (both parametric form, and the actual form of f(x) feels incredibly arbitrary and over parameterized on first read. I'm sure a lot of thought went into this, so maybe a sentence or two justification on why this form and so many parameters over something like a polynomial or power law?

Looking at the fit parameters they seem to vary fairly drastically based on temperature (for instance changing both in order of magnitudes, and in sign).

Section 3.3: There seems to be no discussion of air effects on the fall speed here. Is the argument just that at ~240 meters there is no vertical air motion, or that it is bounded such that it won't effect the choice of 0-2 m/s choice for spectra? This assumption should be stated. If it is not the case, it should be shown that realistic wind speeds at this height don't effect the methodology.

Section 3.5: I am okay with most of the uncertainty characterization, but page 11 first paragraph uses a value of 0.5 standard deviation for the separation of delta and DP. This feels a little low to me, but can the authors provide some justification for these values?

Minor comments ——- p3.10: "Calibration with a point target does not take into account the volumetric scattering" -> I don't understand this point, nor do I think it matters. The calibration process is only concerned with transmitted/retrieved power and as long as the IF filters are set appropriately, volumetric vs point does not matter.

p6.4: I think you mean 50% of terminal fall speed. The wording behind a factor of 2 is a little ambiguous (for instance 5 m/s , a factor of 2 would mean you reject anything between 0 and 10 m/s)

p7.20: You should define Z0 here by name at least. I know it is done in the appendix, but it took a little bit to track down.

Eq(7): I assume the two dielectric terms are just to account for differences in assumed dielectric at the radar vs the actual measured based on temperature. A sentence should be added just to clarify this.

Section 3.6: Applications to radar 2-> Did I miss applications to radar 1? Later on you bring up radar 1, but maybe change how you refer to them as it is a bit confusing to start with radar 2 in evaluation.

P12.10 The hyphens to stand off the 0.3 deg and 0.06km should be removed, it reads as negative values.

Lapse rate is mispelled as laps rate.

P11.28: Convoluted should be convolved.

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