The authors describe a method for estimating the path integrated attenuation due to hydrometeors in an observed profile. The method uses two techniques, the first which uses measurements of surface backscatter in clear-sky profiles in the vicinity of the observed profile, and the second which used a scattering model to estimate the surface backscatter at the location of the observed profile. While CloudSat's 2C-RAIN-PROFILE product adopted a similar method, in this method the clear-sky profiles are allowed to be much further from the observed profile (~30 km for CloudSat versus 100-200 km for this method).

The paper is useful to document the methods used for EarthCARE products and so is likely appropriate for an EarthCARE special issue. I have one primary concern and that is that I think the uncertainty for the retrieved PIA has been estimated improperly. This estimation is described in lines 109 to 143.

In their approach, the authors compile a "large set" of clear-sky profiles. For a selected profile, differences described in equation (8) are computed against every other profile in the dataset and then decomposed by wind speed and separation distance. For each wind speed and distance bin, the standard deviation of the differences is taken, computing over all of the data points in the bin.

There are two issues I see:

1) The actual algorithm doesn't use a large number of points to estimate the NRCS. Only five points are used.

2) The standard deviation of the differences (over a large number of differences) is likely not a good estimate for the uncertainty in a single estimated NRCS.

I think both of these issues would contribute to substantially underestimating the uncertainty. I am skeptical that the uncertainties in estimated PIA could actually be as small as 0.25 to 0.5 dB when the calibration points are 100 to 200 km away.

An approach that would more faithfully estimate the error resulting from the single application of the algorithm on a single profile would be to:

1) Compute the difference using only five calibration points (as is done in the algorithm) selected randomly, and produce multiple realizations of each difference.

2) For each wind speed and distance bin, estimate the uncertainty by using the absolute value of the differences in that bin and taking the expected value or mean of those absolute differences.

Since the PIA uncertainty has direct influence on the uncertainties in hydrometeor retrievals, it is important that the NRCS uncertainty be estimated accurately. I'd like to see a more rigorous estimate of NRCS uncertainty used in the article.

Below are comments about details of the manuscript followed by editing corrections.

L38-39: Please clarify here. Are you referring specifically to liquid precipitation particles, or does this hold for both ice and liquid particles. If only liquid, it would be more clear to say "rain" rather than "precipitation particles".

L106: In what way has N=5 been tested and determined to be "optimal"? I will see if Section 2.3 sheds light on this.

L133-L136: There are a couple of statements here that say that interpolation yeilds sigma_0 values with "lower" uncertainties. Lower than what? It is not clear if you are comparing against uncertainties in other parts of the distance-windspeed space, or if you are comparing against the model-driven method.

L136: This statement doesn't seem correct based on Figure 1. When wind speeds are small, the PIA uncertainties appear generally to be larger than about 3 dB, regardless of distance. For these windspeeds, it is only for the cases of very small distances are the PIA uncertainties very small.

L144: Using sigma to represent both surface backscatter cross section *and* uncertainties most likely will lead to some confusion. Consider using "s" to represent uncertainty - it is not uncommon to do this.

L161-L163: I think it is not accurate to say that equation 6 "accounts" for the modulation due to the surface properties. If anything, equation 6 ignores the dependency on surface properties, opting instead to simply weight the calibration point sigma_0s based on distance from the profile of interest and to compute the uncertainty using the distance-based weights.

L171: There is no prior description or reference for X-MET.

L190: Is this correct? As written, if f_surf is negative, r_surf is larger (farther away) than r(n_surf). This seems to conflict with what is stated in L187-L188.

L222-L226 and Figure 3, caption: Per Figure 3, there are broad areas of the midlatitude storm tracks, subtropics and tropics where calibration point fraction (CPF) is 0.7 or larger. Per the definition, the CPF is the "ratio of valid calibration points to the total number of radar profiles within each 1-deg x 1-deg grid cell". My understanding, then, is that 70% or more of the observed radar profiles in these regions are valid calibration points, which are clear profiles. That seems like a very large clear fraction. If my understanding isn't correct, some additional explanation is needed in the text.

L230: Bibliographic information is not provided for the works by Cox and Munk, Wu, and Freilich and Vanhoff. Please make sure your bibliography is complete.

L246-L49: This seems inconsistent with the results shown by Haynes et al. (2009), their Figure 3. At small wind speeds, they found the standard deviations of sigma_0 to range up to 2.3-2.6 dB, depending on SST. The standard deviations presented in this work, as described for Figure 5, are substantially smaller. The standard deviations in Figure 5 seem inconsistent with Figure 4 of this work also, where the 25th and 75th percentile ranges are about +/- 5 dB at small wind speeds. Please provide some explanation and provide commentary in the text.

L258, Figure 6: As noted earlier, bib info has been omitted for Cox and Munk, Wu, and Freilich and Vanhoff.

L281-L282: I think "extensive" might be a better description than "persistent". While I agree that this stratocumulus deck likely *is* persistent (long in time duration), that can't be deduced from the radar observation.

Figure 10, caption: The second and third sentences are partial duplicates.

I won't comment on these further, but please make sure articles ("a", "an", "the") are used where needed in the text. This seems to be an issue starting mainly in Section 4.

L319-L321: Averaging using further-removed calibration points may reduce the *occurrence* of transitions, but this is likely at the expense of accuracy. It would be appropriate (and fair, I think) to show sigma_0,gas results from the EarthCARE approach in which there are transitions between the model-based and interpolation methodologies. I suspect there are similar non-physical jumps in those results.

L352-L356: I don't see the logical path by which the positive bias of EarthCARE's PIA estimate relative to CloudSat's would be due to differences in the frequencies with which each use the Wind/SST method. More explanation is needed here about how that conclusion was reached.

L359: I think you should apply the same adjective used when this result was presented at lines 334-335: "slightly"

L388-L390: See my prior comment regarding L352-L356 and adjust this text to match changes made there.

Editing comments:

L77: "selection" should be "selecting".

L112: "chose" should be "chosen".

L164: "it's" should be "its".

L227: "Section" should be "section".

L243, Figure 4: The color bar is unlabeled.

L250, Figure 5: Again, the color bar is unlabeled.

L298-L299: "at cloudy region" might be better as "at the cloudy profile".

L300: "and second one" should be "and the second one".

L301: "surrounding cloudy profile" should be "surrounding the cloudy profile".

L326: "Section" should be "section".