Final Response to Referees and Reviewers

December 27, 2025

Manuscript egusphere-2025-3573 ”The Estimation of Path Integrated Attenuation for the EarthCARE Cloud Profiling Radar”

1 Referee Comments (RC1)-Matthew Lebsock:

1. Line 42: Add Lebsock and Suzuki 2016 DOI:10.1175/JTECH-D-16-0023.1.

   Thank you for pointing this out. Added the reference in the revised manuscript.

2. Equation 6: I agree that the fact that there are differences in the equation is an advantage IF the water vapor profile and the surface wind speed are approximately equal at points x and x_1. However, in the presence of cloud at point x and clear sky ‘calibration point’ at x_1 we don’t expect this to necessarily be true, especially for small scale unresolved by the model fields from which water vapor is derived. For example, Lebsock and Suzuki 2016 show using an LES that water vapor attenuation is larger in the cloudy targets than the clear targets, which makes physical sense.

   Thank you for your comment. We understand that Equation 6 relies on modeled water vapor and surface conditions, so its accuracy depends on how well the model represents these fields at both points. Small-scale differences, such as higher water vapor over clouds compared to clear-sky points, may lead to slight underestimation of hydrometeor attenuation. This limitation is inherent to all PIA retrieval approaches, since gas attenuation must be estimated from model-based water-vapor profiles in any method.

3. Line 112: ’chose’ → ’chosen’.

   Thank you for the comment. Corrected in revised manuscript.

4. Figure 1: I need help with this figure. First, I think you should show another panel with both the ‘model-method’ and ‘interpolation-method’ error plotted as a function of wind speed. Second, I think you should label on the existing panel which region is best for each method for clarity. Third, I can’t quite understand why the interpolation method is better for a much greater distance between x and x_1 when the wind speed at x_1 drops below about 3 m/s. I actually would expect the opposite – that the interpolation would work better over greater distances for higher wind speeds . Fourth, the residuals should be a function of both the wind speed at x and the wind speed at x_1 since they each influence one of the σ_{0e} terms. Can you comment on points 3 and 4?

   Thank you for this suggestion. In response, we carried out additional analysis to better characterize the PIA uncertainty.

   Regarding the influence of calibration-point wind speed, we agree that in principle the residuals depend on both the wind speed at the cloudy profile (x) and at the calibration point (x_1), since each influences one of the σ_{0e} terms. To capture this, we segregated residuals by wind speed at x and x_1 and constructed two separate PIA uncertainty look-up tables (LUTs):

   - Using calibration points for which $|\text{Wind}(x) - \text{Wind}(x_1)| \leq 2$ m/s (Fig. 1).
   - Using calibration points for which 2 m/s $< |\text{Wind}(x) - \text{Wind}(x_1)| \leq 4$ m/s (Fig. 2).

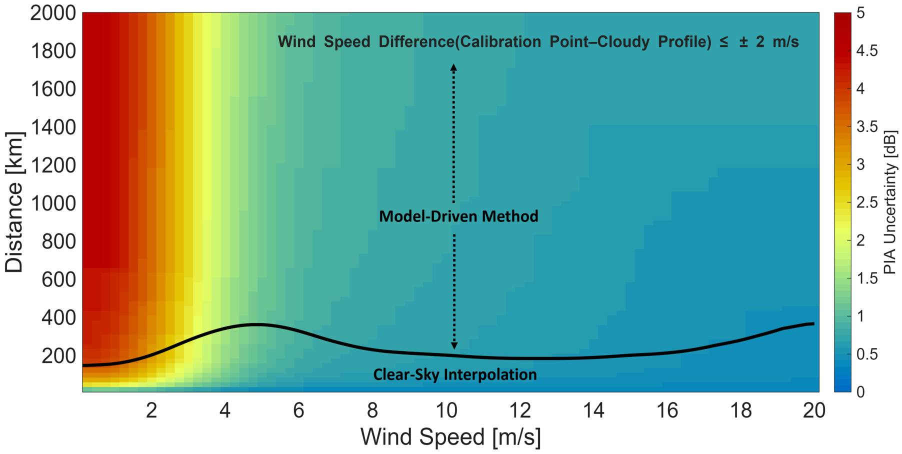

Figure 1: Lookup table of PIA uncertainty constructed using calibration points whose absolute wind-speed difference from the cloudy profile is ≤ 2 m/s. The x-axis shows the wind speed at the cloudy profile, and the y-axis shows the distance to the calibration point. The black line indicates the region where the model-based method becomes preferable, as its uncertainty is lower than that of the interpolation approach.

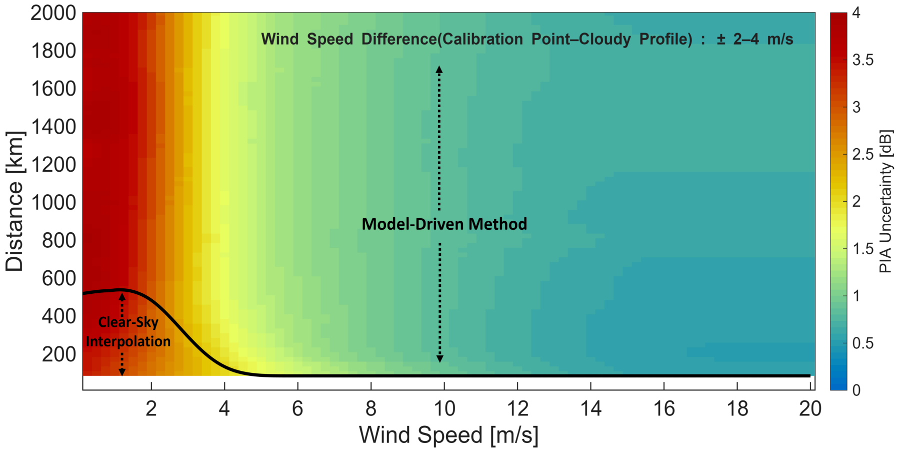

Figure 2: Lookup table of PIA uncertainty using calibration points whose absolute wind-speed difference from the cloudy profile is between 2 m/s and 4 m/s. The x-axis shows the wind speed at the cloudy profile, and the y-axis shows the distance to the calibration point. The black line marks the threshold beyond which the model-based method becomes preferable, as its uncertainty is lower than that of the interpolation approach.

Within $\sim$85 km of the cloudy profile, winds are generally correlated, with most calibration points lying within ± 2 m/s limits.

To address your point 1, we are including two panels comparing PIA uncertainty from the model-driven and interpolation methods as a function of the wind speed at the cloudy profile:

- Using calibration points for which $|\mathrm{Wind}(x) - \mathrm{Wind}(x_1)| \leq 2$ m/s, for varying distances. (Fig. 3).
- Using calibration points for which 2 m/s $< |\mathrm{Wind}(x) - \mathrm{Wind}(x_1)| \leq 4$ m/s, for varying distances. (Fig. 4).

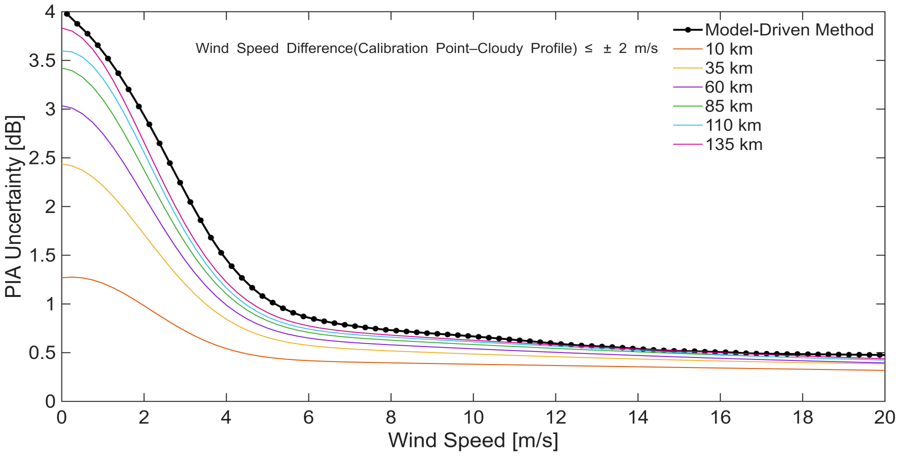

Figure 3: PIA uncertainty estimated from the model and clear-sky interpolation using calibration points whose absolute wind speed differs by no more than 2 m/s from the cloudy profile, evaluated for different calibration point distances. X-axis: wind speed of the cloudy profile; Y-axis: PIA uncertainty.

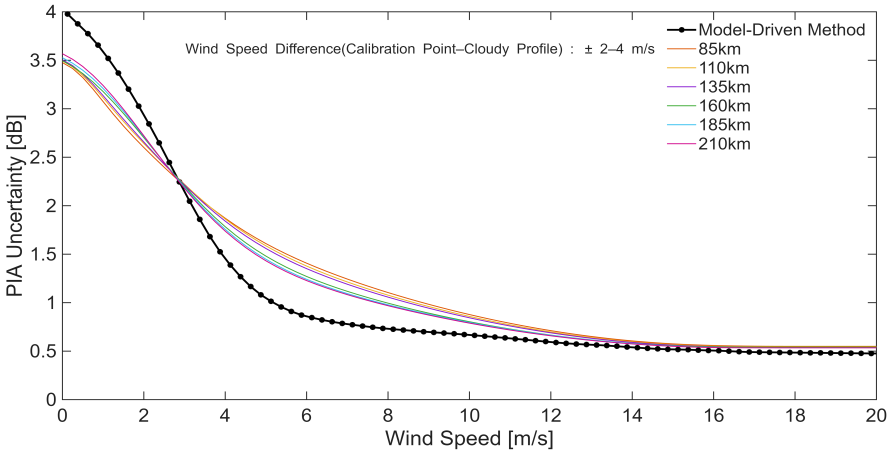

Figure 4: PIA uncertainty estimated from the model and clear-sky interpolation using calibration points with an absolute wind speed difference greater than 2 m/s but no more than 4 m/s from cloudy profile, evaluated for different calibration point distances. X-axis: wind speed of the cloudy profile; Y-axis: PIA uncertainty.

According to Fig.3, selecting calibration points with wind speeds closely correlated to the cloudy profile substantially reduces uncertainty compared to the model, particularly for distances within 50 km, where interpolation performs markedly better than model. The solid black curve in Fig.1 indicates that using closely correlated calibration points in wind speed enables interpolation over an average calibration point separation distance of 250 km. But, as the distance to calibration points increases, the advantage of interpolation diminishes, and the uncertainty approaches that of the model-driven method.

Fig. 4 shows that when calibration points with an absolute wind speed difference of 2–4 m/s from the cloudy profile are used, interpolation generally performs worse than the model, except at very low wind speeds (<2.5 m/s), where it yields slightly lower uncertainty. However, using calibration points that are both distant and have uncorrelated wind speeds can lead to a significant degradation in PIA estimates. Since uncertainties from both the model and interpolation are high (3–4 dB) at low wind speeds, the model-driven approach will be generally preferred. Hence if wind speeds are uncorrelated at calibration point and cloudy profile, model-driven method has to be preferred.

To address point 3, the apparent improvement of interpolation over the model at very low wind speeds and large distances in the previous LUT is primarily a numerical effect. In these cases, the errors from the interpolation and model methods are very similar, so the LUT shows a slight advantage for interpolation, but the difference is not really significant.

Finally, although residuals depend on both the wind speed of the cloudy profile and the calibration points, our analysis indicates that PIA uncertainty is primarily determined by the distance to the calibration points. At short distances, winds tend to be more similar, resulting in lower uncertainty, whereas at larger distances, greater wind difference leads to higher uncertainty. We tested PIA estimation using two separate look-up tables for PIA uncertainty and compared the results with those obtained using a single look-up table. The resulting PIA histogram distributions (see Fig. 5) show no statistically meaningful difference when two look-up tables are used. Given the absence of clear performance gains, we adopt a single look-up table in the final algorithm to maintain simplicity and robustness. In addition, another panel showing the uncertainties from both the model-driven and interpolation-based methods as a function of wind speed has been included in the revised manuscript.

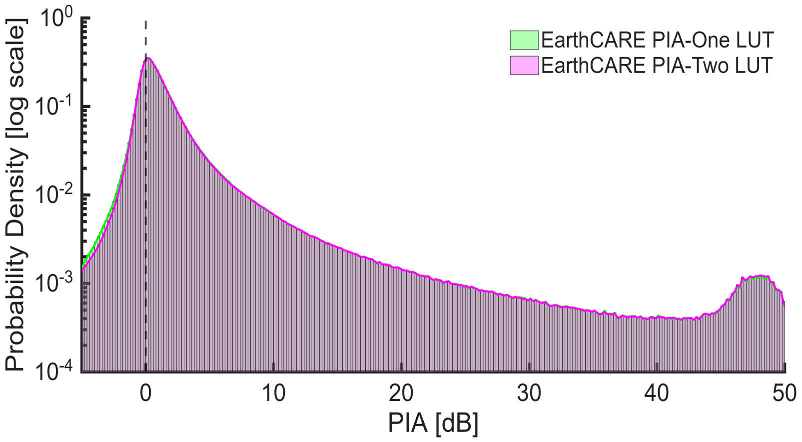

Figure 5: Statistical comparison of PIA estimates derived using one and two PIA uncertainty look-up tables.

5. Line 159: Related to point above about low wind speeds here you say you exclude the low wind speeds from interpolation which is what I would expect. ‘In contrast, the method used here allows interpolation even when the calibration points are 200 km to 100 km from the cloudy pixel in wind speed conditions between 4 and 15 m/s’.

   In practice, interpolation is not applied at very large calibration distances in low wind speed regimes, since in these conditions the uncertainty from both methods is already similar. Extending interpolation to such large distances can actually degrade the performance.

6. Line 164: I think you will get an even better uncertainty estimate if you bin by wind speed at both x and x1. ‘Each calibration point used in the PIA estimation is weighted based not only on it’s distance from the point of interest 165 but also on the potential uncertainty associated with wind speed at that location’.

   Thank you for your comment. We agree that both wind speeds (at x and x_1) can influence the uncertainty. To account for this, we binned the residuals by wind speed at both locations and constructed separate PIA-uncertainty lookup tables based on the wind-speed difference between x and x_1. This approach incorporates the influence of both wind speeds into the uncertainty estimate. Please see our detailed response to Comment 1, where the full analysis and corresponding figures are presented.

7. Equation 14: Several terms are not defined: lambda, c, tau_p.

   Thank you for the comment. We have defined all previously undefined terms in Equation 14.

8. Lines 288-300: The ‘model’ used in precip-column is actually an empirical look-up-table derived from clear sky observations not the li model. ‘The first approach, referred to as the Wind/SST method, estimates the NRCS at cloudy region in absence of hydrometeor and presence of gaseous attenuation (σ gas 0) as a function of surface wind speed and SST using geophysical models (Li et al., 2005)’.

Thank you for your comment. We acknowledge this correction. The initial description followed the 2C-PRECIP-COLUMN documentation, but in the revised manuscript we will clarify that the approach relies on an empirical look-up table derived from clear-sky observations, and not directly on the Li et al. (2005) geophysical model.

2 Referee Comments (RC2)

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.

1. 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.

   Thank you for the comments. I would like to clarify the method used to estimate the PIA uncertainty and how it is applied into our analysis.

   In the implementation of the methodology, the five calibration points can be located at varying distances from the cloudy profile. The contribution of each calibration point to the total error is weighted based on its distance from the cloudy profile (with nearer points receiving higher weights) using the PIA uncertainty look-up table (LUT), see Figure 1 in the manuscript.

   The uncertainty LUT provides the uncertainty associated to a given wind speed (or equivalently a given σ_0) in correspondence to a fixed separation distance of the calibration point.

   To quantify this uncertainty, a large set of clear-sky profiles with measured NRCS (σ_0^{gas}) and wind speed is compiled based on one year of EarthCARE observations.

   All the profiles with wind speed u_j in a given interval j ($(j-1)\,\delta u < u(x) < j\,\delta u$, $\delta u = 1$ m/s) have been paired with profiles whose distance d_k falls in a class $k((k-1)\,\delta d < d(x, x_i) < k\,\delta d$, $\delta d = 25$ km); for all pairs the residuals

   $$\Delta\sigma_0(d(x, x_i), u(x)) = \Delta\sigma_0(x, x_i) = \sigma_0^{gas}(x, x_i) - \sigma_0^{gas}(x) \tag{1}$$

   are computed based on the estimate of $\sigma_0^{gas}(x)$ and $\sigma_0^{gas}(x, x_i)$ defined by Eq.(5) and Eq.(6), respectively (refer the manuscript).

   For each bin of wind speeds falling in the $j-$bin and separation distances falling in the $k-$bin the standard deviation of the residuals

   $$\sigma_{\text{uncer}}(d_k, u_j) = \text{std}\,\{\Delta\sigma_0(d, u) \mid (k-1)\,\delta d < d < k\,\delta d,\ (j-1)\,\delta u < u < j\,\delta u\} \tag{2}$$

   is assumed to represent the uncertainty associated to an estimate of $\sigma_0^{gas}(x, x_i)$ at a location x with wind speed u_j based on a calibration point x_i separated by a distance d_k.

When multiple points are used for the estimation of $\sigma_0^{gas}(x)$ like done in Eq.(7) (refer the manuscript) the uncertainties of the different calibration points can be used to weight differently each $\sigma_0^{gas}(x, x_i)$. The weights w_i assigned to each calibration point x_i are defined as the inverse of the squared uncertainty associated with each calibration point $(\sigma_{\text{uncer}}(d(x, x_i), u(x))$, such that points with lower uncertainty contribute more strongly to the estimate: $(w_i = 1/(\sigma_{\text{uncer}}(d(x, x_i), u(x)))^2)$.

The improved description of methodology for estimating PIA uncertainty described here has been incorporated into the revised manuscript.

While the algorithm typically uses five points, more calibration points can be incorporated, but they will be weighted lower and won't contribute much to the PIA estimate.

In constructing the LUT, the five-point selection criterion was not applied, since the goal was to characterize the uncertainty as a function of distance by sampling as many points as possible. Using a larger number of calibration points provides a more robust distribution of residuals.

Following your suggestion, a LUT using the mean of the absolute differences instead of the standard deviation has been constructed. This approach resulted in a underestimation of the uncertainty enabling interpolation over larger distances. The LUT generated (Fig.(6)) using this method is attached for r reference.

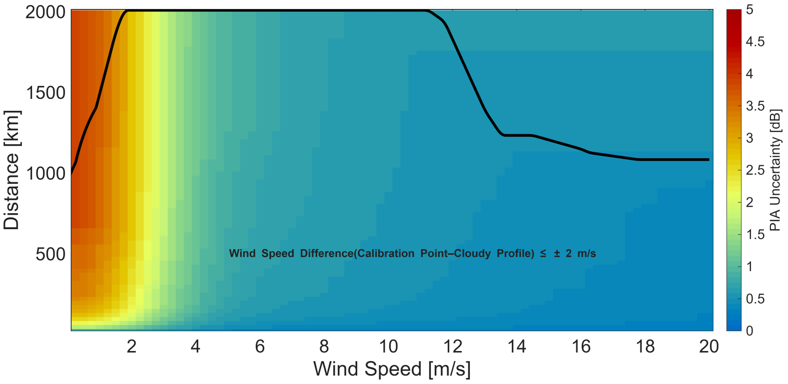

Figure 6: PIA uncertainty Look-up-table created using mean of absolute differences of residuals. The solid black contour delineates the transition boundary beyond which the model-driven method yields lower PIA uncertainty.

Regarding the concern about unrealistically low uncertainties when calibration points are located 100–200 km away, I would like to clarify that the uncertainty is highly dependent on wind speed. For calibration points within 100 km, the uncertainty is approximately 0.8 dB at higher wind speeds (8–15 m/s) and increases to about 1–3.5 dB at lower wind speeds (4–0.5 m/s). Refer Fig.(7) for clarification.

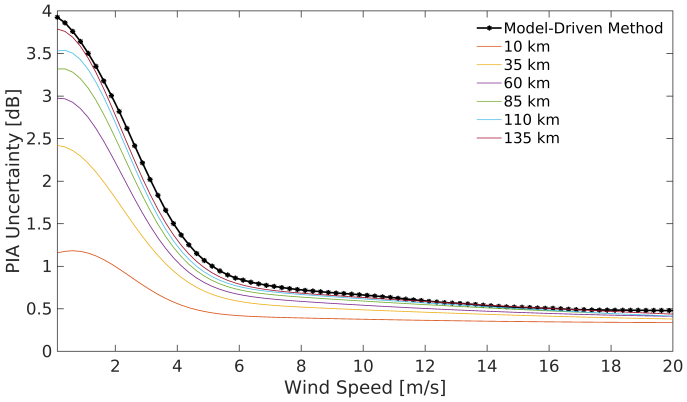

Figure 7: PIA uncertainty as a function of wind speed at the cloudy profile for different separation distances between calibration and cloudy points.

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

2. 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”.

   Thank you for the comment. We are referring specifically to liquid precipitation. We will revise the text in the new version to use “rain” instead of “precipitation particles” for clarity.

3. 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.

   We selected N = 5 based on practical considerations. Calibration points are determined following the criteria described in Section 2.3: at least six clear profiles must be present within a 10 km along-track segment, the standard deviation of the measured NRCS must be less than 0.3 dB, and the calibration NRCS is defined as the mean NRCS of those clear profiles. When selecting five calibration points, we maintain a 10 km separation between each, ensuring that no two points fall within the same 10 km segment used for NRCS averaging. Consequently, when the cloudy region is adjacent to a continuous clear-sky area, the farthest calibration point is located approximately 50 km away. We also tested configurations using 5, 10, and 15 calibration points, and the resulting differences in PIA estimates were marginal (Mean of 0 dB and standard deviation of 0.05 dB), using five calibration points provided a good balance between accuracy and computational efficiency, and was therefore selected as optimal. The corresponding Fig.(8) is attached for reference.

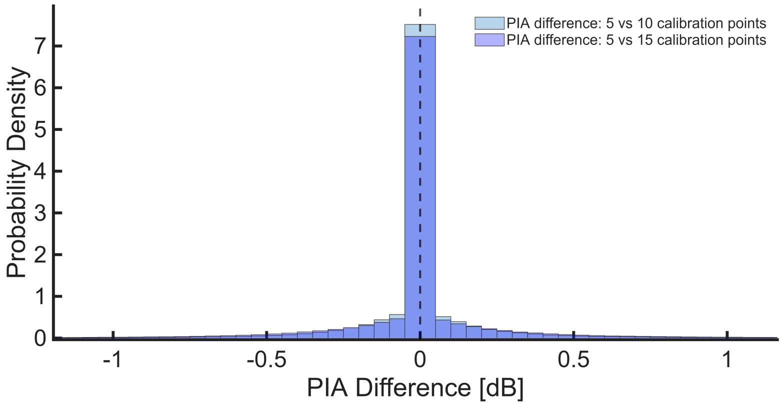

Figure 8: Differences in PIA estimates obtained using 5 calibration points, compared to those derived using 10 and 15 calibration points.

4. 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.

   Thank you for your comment. The comparison refers to the uncertainties from the model-driven method. We will clarify this in the revised manuscript.

5. 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.

   Thank you for your comment. At low wind speeds, PIA uncertainties are generally large for both clear-sky interpolation and the model. The statement that interpolation yields lower uncertainty refers specifically to a comparison with the model, though the difference is marginal. This will be clarified in the revised manuscript.

6. 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.

   Thank you for the comment. We will use a different symbol to represent the uncertainty.

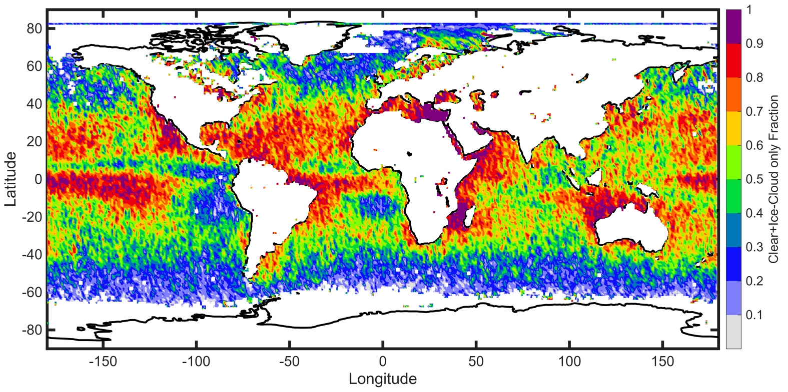

Figure 9: Global distribution of the clear profiles and ice-cloud-only fraction within each 1° × 1° grid cell.

7. 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.

   Thank you for your comment. The statement refers to the fact that Eq.(6) (refer the manuscript) includes σ_{0e}, the effective NRCS, which represents the surface backscatter in the absence of atmospheric and gas attenuation and is inherently a function of surface wind speed and SST. Therefore, the influence of surface properties is implicitly accounted for through σ_{0e} in the formulation.

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

   Thank you for the comment. We will provide a clearer description in the revised manuscript.

9. 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.

   Thank you for pointing this. The description in L187–L188 was inaccurate, and we will revise it for consistency and clarity in the revised manuscript.

10. 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.

    Thanks for the comment. To clarify, the calibration point fraction (CPF) shown in Figure 3 (refer the manuscript) represents the six-month mean fraction of profiles within each 1-deg x 1-deg grid cell cell that satisfy the calibration point criteria described in Section 2.3 (which will be better explained in the revised manuscript). Therefore it is an average number. If we compute the "clear-sky plus ice-cloud only" fraction from the EarthCARE CPR data it can indeed reach values as high as 70% in the tropical regions in the sinking branches of the Hadley cell (Fig.(9)). The clear-sky and ice-cloud only profiles are identifies using significant detection mask in EarthCARE and to identify ice-clouds, a cloud base temperature of less than $-263.15K$ is imposed. Of course this value would be reduced when considering other observations that include visible and infrared radiation (thus more sensible to thinner clouds) like in (Bertrand et al., 2023).

11. 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.

    Thank you for the comment. We will add complete bibliographic information for Cox and Munk, Wu, and Freilich and Vanhoff in the reference list.

12. 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.

    Thank you for the comment. The apparent discrepancy arises because Figure 4 and Figure 5 in the manuscript represent different types of variability. In Figure 4 (refer the manuscript) the error bars show the standard deviation of NRCS within each wind-speed bin (aggregated across all SST values). These wind-bin standard deviations range from 0.4–0.5 dB at high wind speeds (8-12 m/s) to 1–4 dB at low wind speeds (4-0.5 m/s). These values are generally consistent with the results in Haynes et al.(2009). By contrast, Figure 5 (refer the manuscript) displays the distribution of standard deviations calculated for individual 10 km along-track clear-sky segments, and we plot the median of those per-segment standard deviations within each wind bin. This standard deviation is much smaller because it reflects variability within short homogeneous clear-sky segments, not across the full population of scenes.

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

    Thank you for your comment.

14. 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.

    Thank you for the comment. We agree and will use “extensive” instead in the revised manuscript.

15. 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.

    Thank you for the comment. We will remove the duplicate sentences in the caption and review the text to ensure correct use of articles throughout the manuscript.

16. 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.

    Thank you for the comment. As noted in the manuscript, the EarthCARE approach also exhibits transitions when switching between the methods. We will include an example case demonstrating the corresponding jumps in the EarthCARE-based estimate for comparison in the revised manuscript.

    In the revised manuscript, a new case study is attached which presents jumps in both EarthCARE and Cloudsat methodology (Fig.10).

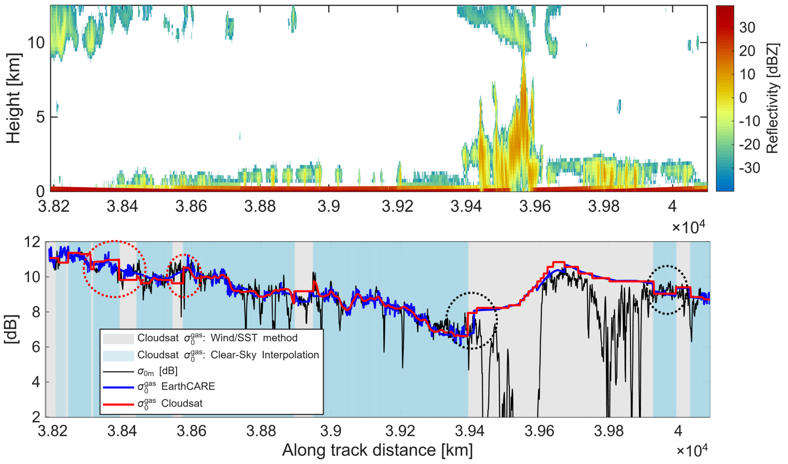

Figure 10: CloudSat Case Study. Top panel: vertical reflectivity profile as a function of the along track distance. Bottom panel: the measured normalized radar cross Section (NRCS), denoted as σ_{0m} (black curve), along with the estimated clear-sky NRCS (σ_0^{gas}) within cloudy regions, derived using the proposed EarthCARE method (blue curve) and the CloudSat-based estimate (red curve). The red circle highlights the jump in σ_0^{gas} in CloudSat estimate and black circle highlights jumps in both EarthCARE and Cloudsat σ_0^{gas} estimate. The grey and blue shading in second panel represent the two estimation methods employed in CloudSat methodology, which are Wind/SST method and clear-sky interpolation method. The jumps present in σ_0^{gas} CloudSat generally occur when there is switch in two methodologies.

17. 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.

    Thank you for your comment. We agree that the explanation for the positive bias requires further clarification. After further analysis and discussions with the CloudSat team, the study was repeated, with the main difference being the use of the correct NRCS look-up table. Even so, a slight positive bias in PIA estimates using the EarthCARE method remains (Fig.11). Updated figures reflecting this revision are included in the manuscript.

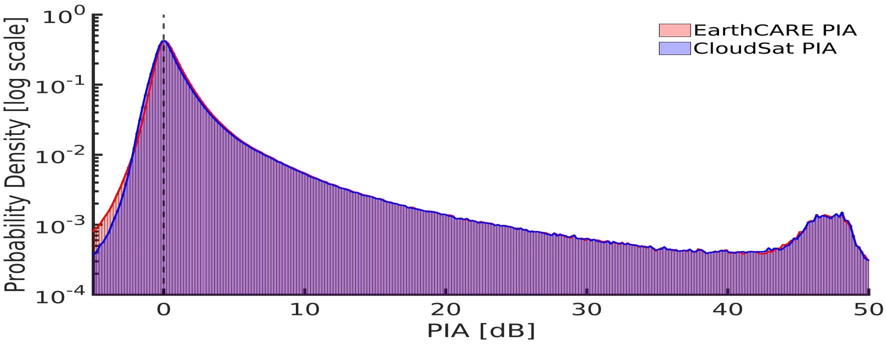

Figure 11: The global probability distributions of PIA estimates obtained from the EarthCARE retrieval methodology applied to CloudSat observations (January–April 2007) and from the PIA from CloudSat retrievals are compared on a logarithmic scale.

When applied, the EarthCARE PIA methodology leads to a greater dependence on clear-sky interpolation, accounting for 78% of the profiles, compared to 33% for the CloudSat approach.

Fig. 12 shows the differences in PIA estimates between the EarthCARE and CloudSat methods for various scenarios: (i) when both CloudSat and EarthCARE use clear-sky interpolation, (ii) when EarthCARE uses clear-sky interpolation while CloudSat uses the model-driven method, (iii) when both CloudSat and EarthCARE use model-driven method. In the histogram for the scenario (ii), a slight positive bias in PIA differences is visible (0.3-0.5 dB), which likely contributes to the overall positive bias in EarthCARE PIA estimates.

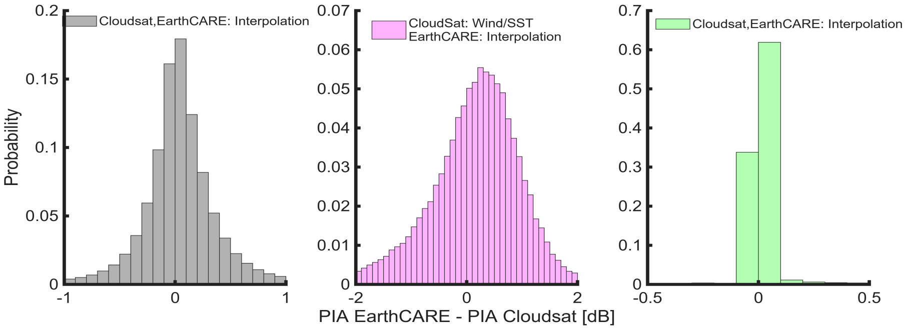

Figure 12: Probability distributions of the differences between PIA estimates from the EarthCARE and CloudSat methodologies. The gray histogram shows cases where both CloudSat and EarthCARE use the clear-sky interpolation method, with a narrow distribution centered near 0 dB. The purple histogram corresponds to profiles where CloudSat uses the wind/SST method and EarthCARE applies clear-sky interpolation, exhibiting a broader distribution with a peak around 0.3–0.5 dB. The green histogram represents cases where both EarthCARE and CloudSat use the wind/SST method, with a distribution centered at 0 dB.

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

    Thank you for the comment. We will change the adjective accordingly to maintain consistency in the manuscript.

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

    Thank you for your comment.

    Editing comments:

20. L77: "selection" should be "selecting".
21. L112: "chose" should be "chosen".
22. L164: "it's" should be "its".
23. L227: "Section" should be "section".
24. L243, Figure 4: The color bar is unlabeled.
25. L250, Figure 5: Again, the color bar is unlabeled.
26. L298-L299: "at cloudy region" might be better as "at the cloudy profile".
27. L300: "and second one" should be "and the second one".
28. L301: "surrounding cloudy profile" should be "surrounding the cloudy profile".
29. L326: "Section" should be "section".

    Thank you for all the editing comments. We will correct these issues in the revised manuscript.

References

Leah Bertrand, Jennifer E. Kay, John Haynes, and Gijs de Boer. A global gridded dataset for cloud vertical structure from combined cloudsat and calipso observations. *Earth System Science Data*, 2023. doi: 10.5194/essd-16-1301-2024. URL `https://essd.copernicus.org/articles/16/1301/2024/`.