Atmospheric Measurement Techniques Discussions

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Comment on amt-2022-213

Anonymous Referee #2

Referee comment on "In-orbit cross-calibration of millimeter conically scanning spaceborne radars" by Alessandro Battaglia et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-213-RC2, 2022

This manuscript utilizes the GPM KaPR and CloudSat CPR data records to address a critical question for the continuation of cross-calibrated cloud and precipitation radar observations with future spaceborne radars: how can natural targets be utilized to provide a precise comparison of the calibration of two independent instruments on different spacecraft in different orbital geometries? While the specific use case considered here is that of conically scanning cloud and precipitation radars at large off-nadir incidence angles, the method appears generally applicable and would apply to a number of upcoming spaceborne radar missions with different viewing geometries. The authors present a compelling statistically based approach in which PDFs of physically separated ice cloud regions are compared using a scalar metric (the Jensen-Shannon distance) that quantifies PDF similarity, and thereby demonstrate that relaxed coincidence criteria for observations from the respective instruments can be afforded in such comparisons when restricting analysis to stratiform ice cloud regions. They further extend this idea to purely climatological based calibration assessment and suggest that such an approach may enable calibration without the need more measurement coincidence from two platforms, but that more investigation is needed to assess the impact of natural variability across time scales (diurnal, seasonal, etc.). The methods and results are generally well presented and constitute an important contribution to the field. However, before the manuscript is accepted for publication, I believe several important comments/concerns/confusions need to be addressed. These are outlined in the “major comments” below, and less critical items are outlined in the “minor comments” section. I thank the authors for their submitted manuscript and look forward to their response.

Major comments:

- Section 2: I'm not sure I agree with the methodology used to calculate the expected number of calibration points per unit time presented in the manuscript, and will outline my concerns here, though I welcome input from the authors clarifying if I have missed a step or am misunderstanding their approach. The first point I would like to clarify is that the quantity referred to as "monthly intersections" throughout the paper is in fact more aptly described as the monthly-average number of instrument footprints satisfying the coincidence criteria gridded at 2 x 2 degree resolution. That is to say, for a single intersection of the orbit tracks of the two spacecraft (assuming nearly temporal coincidence), there are many points that will satisfy the spatiotemporal criteria and be logged as many "intersections" in a single 2 x 2 deg grid cell. If this is true, the problem for me arises when this "weekly/monthly intersections" variable is multiplied by the "mean number of ice layers" variable to get the number of weekly/monthly calibrating points. Specifically, I interpret the distributions in figure 5 as the mean number of ice cloud range bins per 2 x 2 degree grid cell, and not the mean number per horizontal radar footprint gridded at 2 x 2 degrees. Thus, to properly calculate the number of coincident ice cloud range bin detections within a given 2 x 2 degree grid cell, I would calculate it as: (mean number of ice cloud bins per 2 x 2 deg)*(number of footprints within the 2 x 2 deg grid cell satisfying the coincidence criteria per unit time)/(number of footprints per 2 x 2 deg grid cell).
- Lines 130-135: The authors state that due to the low attenuation in ice at Ka and W-band, the measured reflectivity of a given ice cloud target will be nearly identical from near-nadir and large off-nadir viewing geometries. This seems to neglect the possibility of backscatter dependence on angle for oriented ice crystals, which are known to exist in certain stratiform regimes. The potential impact of this effect on the proposed calibration method should be addressed in this manuscript.
- I find the description of the Z-PDF method at the beginning of Section 2.3 somewhat confusing. My interpretation of the procedure is that a correlated pair of PDFs is calculated for 5-km-long along-track swaths of radar reflectivity profiles that are separated by \Delta s, and this pair could be labeled with an index "i", say f_i(Z) and g_i(Z). The JS distances are calculated for these specific PDFs f_i(Z) and g_i(Z) with index i and then an ensemble of such realizations is made by finding N (i = 1, 2, …, N) pairs of such spatially separated PDFs. In lines 185-188, the single-swath, spatially separated PDFs are defined, but then it is said that the PDFs are accumulated up to a specified sample size before the JS distance is calculated between "the two PDFs". It isn't totally clear what is meant by this accumulation, and could be interpreted as accumulating counts across many spatially separated observation points to form a single PDF consisting of reflectivity observations from N swaths. This is confusing since it seems to be the case that the JS distance can only be meaningfully calculated on isolated pairs of single swath PDFs. I think cleaning up the wording and notation in this section would make the manuscript much more easily readable.

Minor comments:

- Line 31: It is not clear why conical scanning permits larger domain coverage, unless the statement is more about the difficulty of reaching the same large incidence angles using a cross track technique vs using a conical scanning approach.

- Line 31: The considerations around surface clutter at large off nadir vs near nadir incidence are very different in nature, and it is not a given that one offers lower clutter free heights in general than the other. Specifically, beam width determines the clutter height at large off nadir angles, and the problem is essentially independent of the radar waveform parameters/ the range weighting function. The situation is completely different for near nadir scattering where the beam width has little to no impact on the clutter free height, and it is the range weighting function as well as the transmitter phase noise (for high time-bandwidth product PC systems) that determines the clutter. This statement on line 31 should be clarified or removed.
- Line 4 and Line 35: 10 degrees vs 12 degrees for surface scattering-based calibration?
- Line 79: More justification is needed for this particular choice of statistical metric, the JS distance, in assessing the similarity between two PDFs. What are the strengths/sensitivities of this metric to PDF structure?
- Line 42: The authors highlight the statistical cross-calibration approach of Protat et al. (2009) that utilizes CFADs to perform a calibration comparison including information about the vertical distribution of observed reflectivity, but the approach implemented in this work collapses the distribution into a 1-D PDF with no vertical information retained. This difference should be addressed with at least some discussion in the manuscript.
- Line 132: The authors state that only ice clouds away from deep convection are used in this analysis, but do not state how the observational data sets have been filtered to ensure such ice cloud detections are excluded. Please include these details.
- Section 2.2: the definition and description of statistical quantities that are intended to quantify the intersection occurrence rate could be improved. Most importantly, the gridding resolution of 2 deg x 2 deg seems rather arbitrary, and for instance, the "mean number of layers" plots in fig would scale (likely proportionally) to this choice of angular area. Furthermore, it seems slightly misleading to refer to this quantity as a number of ice cloud layers, when my interpretation is that this is actually referring to the mean number of radar range bin volumes (e.g., 5 x 5 x .25 km for GPM) that have returns meeting the ice cloud criteria laid out in the paper. It would seem more appropriate to use for this statistical quantity a mean area density of ice cloud bins that would not depend so strongly on the choice of grid resolution. Furthermore, this would allow for a more straightforward calculation of expected mean number of intersection bins per unit time by multiplying this density by the mean inter-satellite swath overlap and the mean rate of overpass "coincidence" for that lat/lon (see major comment 1 above). Assuming my interpretation of the calibrating target definition in Section 2.2 is correct, I suggest that the ice cloud layer variable be renamed to something like "ice cloud radar bins" to ensure that the reader is not confused by this. The term ice cloud layer evokes an extended cloud morphological feature that would nominally encompass a very large number of radar range bin volumes. If my interpretation of this "ice cloud layer" definition is wrong, I suggest that the explanation in this section be reworded for clarity.
- The vertical black and blue dashed lines in Fig. 4 (right panel) are difficult to distinguish
- Section 2.1: More details are needed on how orbits are propagated for the purposes of calculating intersection points