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Interactive comment on "An integrated approach to monitor the calibration stability of operational dual-polarization radars" *by* M. Vaccarono et al.

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General Comments

The paper describes four methods to monitor radar calibration and presents strategies how these methods can be combined. The methods are self-consistency check, ground clutter return observation, solar monitoring, and reflectivity inter-calibration of two radars' data in overlapping areas.

The method description and result presentation is in general quite clear. The results demonstrate that the proposed methods are useful monitoring tools. This is in particular evident from one radar having various calibrations issues and the other one not.

Parts of the algorithm description and the methods itself should be improved. This affects in particular the following subjects:

a) The intensity of ground clutter return is not only depending on weather condition and vegetation (as the authors write), but also significantly on the vertical distance between beam axis center and ground, i.e. on the effective elevation angle. The effective elevation angle is not necessarily constant; it depends on anaprop conditions and also on the limited pointing accuracy of a radar system. In particular when discussing the long-time variability of ground clutter monitoring (as shown in figures 13 and 15) one needs to know the approximate influence of elevation angle error on clutter intensity. If for example 0.5 degree nominal elevation angle data are used for monitoring, one could provide the ECDFs (as in figure 12) once for 0.5 deg data and once for 0.6 deg data (using a sufficiently large data base, e.g. a couple of hours in clear air), and discuss the clutter differences resulting from a 0.1 degree elevation difference (which potentially amounts the typical accuracy of the effective elevation angle).

b) It is not clear why one method, namely the self-consistency, was performed only at the beginning and at the end of the observation period, and not repeatedly during the many weeks in between. Also, the self-consistency method strongly depends on exact differential reflectivity calculation. It is somewhat questionable that the most promising method for Zdr calibration, namely vertically pointing in rain, seems to have not been performed, although the radar systems in question are able to do so.

c) For the inter-calibration method, attenuation seems not to be considered properly. While dry-radome conditions and sufficiently high RhoHV only are considered (but unfortunately this is explained only in the results section 4.2.1 and not already in the method describing section 3.2), attenuation as evident from differential phase shift seems not to be considered, but can have significant impact on the reflectivity data of one radar only, in particular in convective situations and for the C-band frequencies used here.

d) The authors seem to confuse solar monitoring of differential reflectivity with differential reflectivity calibration. On page 12 line 17 (when describing figure 17) they write: "This bias is considered to correct the Zdr measurements in the radar post-processing chain." But what does this mean? A negative Zdr average of the solar monitoring is not necessarily an indication of a Zdr mis-calibration. Instead, it may just be the compensation of a difference between the calibration constants C in eq. (8) of the horizontal and vertical channels, respectively.

e) Results are shown only for a period when precipitation at ground level and low atmospheric levels is liquid. The radars used for this study are operated in a region where a significant amount of such echoes is from solid precipitation during the winter months. If the authors cannot provide some results for such cases, they should at least discuss on potential limitations of each particular calibration monitoring method during winter conditions.

Specific Comments

Methods should fully be described in the corresponding sections 3.1 to 3.4. Page 9 lines 2 to 7 belong to section 3.1 and not 4.1. Page 10 lines 18 to 24 belong to section 3.2 (as a refinement of the method description) and not to section 4.2.1. More such examples follow below.

Page 4 line 28 states "Rdp(Kdp, Zdr)", but in eqs. (2) and (3) it is Rdp(Kdp) only.

The clutter mask mentioned in section 3.3 should be better described. Is it based on reflectivity data only, or are polarimetric moments considered? How exactly is determined if an actual measurement is clutter only? By considering reflectivity only, or by considering polarimetric moments as well? If such details were described in the cited references (Silberstein, Wolf), the authors should at least outline them here.

Page 7 line 17: The sun's apparent angular diameter is not constant at 0.54 degrees but varies by about 3 percent (largest in December, smallest in June). Would that have influence on the solar calibration monitoring results?

Page 8 line 24 and figure 15 caption: In the text, "Fit residual standard error" is not clear. The caption mentions "square root of the differences between the measured solar power and the theoretical model". Is both the same? Does "theoretical model" in the caption refer to the "Nonlinear Least Square method" of the text? (On a side note, the text is section 3.4 and figure 15 belongs to section 4.2.3.)

Page 9 line 13: "Zdr < 0" is not a good indicator of attenuation. Such may happen either if the system is not properly calibrated, or be due to random measurement accuracy. Instead, differential phase shift should be used as a measure of total path attenuation. Note again that such details should be mentioned with the method description and not with the results only.

Page 9, around line 10: How is "data collected in rain" determined? Manually? Using a hydrometeor-classification? Also, this belongs to the method description in chapter 3.

Figures 6 and 7 (and text page 9 around line 30): What means the "dBR > 11" threshold: both Rdr and Rdp above threshold, or only one (which one)?

Bottom of page 10: Instead of describing "warm" and other colors, authors should give a color scale to figures 6, 7, 9, 10 and 11.

Page 11, around line 15: removing all data below 20 dBZ significantly (?) alters the ECDF and thus potentially the monitoring stability. The authors should comment on that. And again, this belongs to chapter 3 and not 4.

Page 11 line 32: The sun's "received power in dBm" is from both the sun's emission and the clear air thermal noise. The latter is somewhere between -120 and -110. How is the thermal noise determined and subtracted? Figure 14 shows contour lines for the sun's emission only, but are the radar measurements also sun's emission only, or measurements including the thermal noise?

Page 12 line 11: "The daily PTOA value of the received solar power is generally comparable with the DRAO reference". This is no good statement (comparisons can almost always be made). Instead, the authors should write e.g. "the mean difference is X dB, and the correlation is Y".

Page 12 lines 25 to 32: this describes Figure 18, but the self-consistency results are not included in Figure 18.

Page 13 lines 5 to 7: If the corruption of solar signal by radio interference was observed, why was it not corrected? At least the "solar" measurements in question should have been removed before calculating the results of Fig. 18. And why are these results for the Monte Settepani radar so much worse than for the Bric della Croce radar?

Figure 12 (ECDFs): What is the meaning of the many lines with different colors?

Figure 13 (time series of ECDF): Instead of "Mean values of the daily values of the 95th quantile" (which probably means "daily mean values of the 95th quantiles of all day's scans"), one could also have derived one 95th quantile value using all data of one day together. The proper English term here is "95th percentile", not "95th quantile".

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