

Interactive
Comment

Interactive comment on “A novel approach for absolute radar calibration” by C. Merker et al.

C. Merker et al.

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Response to interactive comments of Referee 3

The authors describe a novel method for calibration of radars. A special setup of the involved radar systems is required. The manuscript is well prepared and certainly deserves publication.

We thank the reviewer for the constructive remarks and appreciate the suggestions to improve the quality of the manuscript. The answers to the comments are outlined in the following.

General comments:

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1. *Could you state about the effects of wrong estimation of the DSD by MRR? How large would be the influence in the presence of vertical air motion (e.g. 0.5 m/s) or spectral broadening by turbulence?*

Vertical air motion is indeed one major possible source of errors when it comes to applying the method to a real data set. An error in the estimated specific attenuation yields an error of the same factor in the calibration (see eq. 7). Vertical wind effects on MRR measurements have been studied in Peters et al. 2005. Table A1 lists the impact of vertical wind on attenuation. The relative error (LEM) is nearly linear and can be approximated to 3 dB per ms^{-1} (this means specific attenuation k_3 is overestimated by a factor 2 for 1 ms^{-1} vertical wind). The remaining bias (LET) in k_3 is found to be 0.8 dB per m^2s^{-2} . However, a test using 10s vertical wind values at a height of 50 m (measurements at the Wettermast Hamburg, May and June 2014) yields a standard deviation of 0.49 ms^{-1} when considering rainfall events. This strongly reduces the possible error to about a factor 1.4 for LEM and 0.05 dB for LET. Since convective precipitation events should not be considered for calibration anyway (strong inhomogeneities), the typical variance of vertical wind in considered cases should be even lower. Furthermore, the MRR considers a measuring volume, which also reduces fluctuations. Still, this remark is justified and certainly requires attention in future work.

Changes in manuscript: A paragraph was added to the conclusion in order to highlight the issue.

2. *I think I didn't understand completely the setup in chapter 4. Did you use 'only' the reflectivity profile of a horizontal looking radar to estimate a profile, ignoring real attenuation and real DSD (i.e. non Marshall-Palmer type)*

The beginning of Section 4 was clearly missing some more details for easier understanding, thank you for pointing this out. Synthetic, intrinsic reflectivity fields are created from measurements of horizontally pointed MRRs. From these fields, all theoretical analysis is done using Marshall-Palmer type DSD.

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Changes in manuscript: ‘In order to obtain realistic precipitation fields, reflectivity measurements from both horizontally oriented MRRs are used to generate synthetic, intrinsic reflectivity fields along the path. These synthetic, intrinsic reflectivity fields are created by comparing and combining measurements from R_1 and R_2 such that the highest reflectivity value of both is selected in each range gate. Using measurements from just one MRR would yield synthetic, intrinsic reflectivity fields showing a systematic decrease in reflectivity toward one side of the measuring path, as an artefact of attenuation present in real measurements. From the obtained reflectivity fields, rain rate and synthetic, attenuated reflectivity for all three radars are simulated according to the procedure described in Sect. 3. All devices are still considered...’

3. *In case the measurement setup in Lindenberg is suited for your method, it would be nice if you could show at least one event with real data, i.e. the temporal evolution of C3.*

We agree on the importance of testing the method using real, measured data. However, even if the setup of an appropriate network looks simple, it is not trivial in detail. The network in Lindenberg was build up in order to validate the method, but the pointing of both horizontally oriented MRRs had to be improved, which was done by the end of 2014. At the same time, a rain gauge for validation against a proved method was installed next to MRR_3 . The data set collected since this necessary improvement of the setup is not large enough yet to provide a satisfying amount of cases for calibration. Furthermore, the application of the method on real, measured data would require further preprocessing (selection of suitable measurements) which still remains to be optimised. A proper analysis of the effect of the integration time on results is also required. In our opinion this would be beyond the scope of this article, focusing on the theoretical formulation, but will be done in future studies. The title of the manuscript was changed to be less confusing about that.

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Changes in manuscript: Adapted title

4. *Could you give some statements, how the method would perform if you use for R1 and R2 less attenuating radars (e.g. X-band)*

The horizontal radars must of course be operated at attenuated wavelength because the method is based on attenuation. This can be any frequency provided that the attenuation from rain along the section of interest is high enough to be detected. In practice, the attenuation for X band, for example, would be approx. one order of magnitude smaller then for MRRs using K band (Atlas and Ulbrich, 1977), making it difficult to get a clear attenuation signal when measuring uncertainty is of about 2 dB. So using any device with lower frequency than K band for the horizontally oriented radars is probably not possible. Both should operate at the same frequency in order to be subject to the same attenuation.

Specific comments:

- p. 1673, l. 3 *rain gauges are certainly not accurate in windy conditions*

Changes in manuscript: Added typical measurement error for rain gauges: ‘...have achievable measurement uncertainties of about 5% (Vuerich et al., 2009).’

- p. 1675 and 1676 *it's obvious, but maybe you can affirm that C_3 in Eq. (2) is the same as C_3 in Eq. (4)*

Changes in manuscript: ‘...and C_3 the same as in Eq. 2.’

- p. 1677 *Eq. (7) is only valid, if you assume that DSD is constant between $z=0$ and $z=s_3$; thus the whole method depends on this assumption (?)*

Specific attenuation is assumed to be constant, but the DSD is not necessarily constant with height. A height dependent attenuation only induces a small error here due to the relatively short vertical path (especially compared to the considered horizontal path) and is probably not a strong constraint. One could also use

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a weakly attenuated frequency for R_3 .

Changes in manuscript: ‘This implies the necessary condition of a constant specific attenuation in the vertical section between R_3 and the height of the measuring path (about 40 to 80 m, depending on the network setup).’

p. 1682, l. 3 *what are the criteria to define the TDBZ threshold at 1.4 dBz?*

Changes in manuscript: ‘This threshold is defined by studying the quality of calibration results running a Monte Carlo simulation as presented in Sect. 3 for the 4220 chosen time steps. 90% of the cases having at the most 10% error in the determination of the correction factor and standard deviation below 1.0 have to show TDBZ lower than the threshold.’

p. 1684, l. 20 *shouldn't it read: 'over a 3246 sample of filtered time steps'?*

Changes in manuscript: ‘over a 3246 sample of filtered time steps’

p. 1693 *the colorscale of Fig. 4a is confusing*

Changes in manuscript: The colorscales of Figure 4 and 5 has been changed in order to make plots clearer.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 1671, 2015.

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