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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 1

The manuscript 'A novel approach for absolute radar calibration' by C. Merker et al. raises an important issue and is well written. I recommend the manuscript for publication, but the authors have to describe and discuss the uncertainty of their method in more detail. An overall error of 1% is certainly too small.

Many thanks to the reviewer for the detailed and constructive comments on the manuscript. The points raised helped and provided useful suggestions for improving its completeness and comprehensibility. Our detailed list of answers (including the points mentioned above) can be found below.

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Interactive Discussion

Discussion Paper

General comments:

1. *I think the most important issue is that the authors take N(D) as measured by MRR no. 3 for granted. However, vertical air motion can lead to strong biases of N(D) and extinction (The authors should know e.g. Peters et al. 2005). How do these errors propagate to the calibration estimate and how does this affect the uncertainty estimate of 1% to 11%?*

   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 $3\,\mathrm{dB}$ per ms^{-1} (this means specific attenuation k_3 is overestimated by a factor 2 for $1\,\mathrm{ms}^{-1}$ vertical wind). The remaining bias (LET) in k_3 is found to be $0.8\,\mathrm{dB}$ per $\mathrm{m}^2\mathrm{s}^{-2}$. However, a test using 10s vertical wind values at a height of $50\,\mathrm{m}$ (measurements at the Wettermast Hamburg, May and June 2014) yields a standard deviation of $0.49\,\mathrm{ms}^{-1}$ when considering rainfall events. This strongly reduces the possible error to about a factor 1.4 for LEM and $0.05\,\mathrm{dB}$ 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. *If the MRR is mounted horizontally on a mast, the main-lobe will hit the ground after some km due to the beam width of 1.5 degrees (and the side-lobes even earlier). This can cause additional attenuation due to the surface. Can this potentially bias the results?*

Taking a suitable setup as example (e.g. Lindenberg, see answer to comment 5), the distance between R_1 and R_2 is assumed to be about 6000 m. The setup presented in the manuscript considers range gates of 200 m width. Above R_3 in the middle of the measuring path, the beam diameter is approx. 78 m and the radius $r = 39$ m. The measuring path is situated in about $s_3 = 60$ m height above R_3. Assuming a gaussian beam and a flat and non-reflective ground, it is possible to estimate the attenuation. A simple, upper estimate of the loss is given by the error function $V = 1 + \text{erf}(-x)$ with $x = \sqrt{ln(2)}s_3/r$. Thus, $x = 1.3$ and $V = 0.066$ in this case. The important parameter here is not the loss itself, but its change dV/dx along the path, which can be calculated and takes the value 0.2 at $x = 1.3$. In the middle of the path, a step of one range gate induces a change in distance of $\delta x/x = 0.06$. Therefore, the gradient of the ground attenuation in the surrounding of the middle of the path is approx. 1.2% per range gate. Since the loss itself there is only 0.066, the relative power change $\delta P/P$ per range gate caused by the ground attenuation is $\delta P/P = (1 - V \cdot dV/dx \cdot \delta x/x) = 0.9992 \approx 3.4 10^{-3}$dB and can probably be neglected along this path.
Changes in manuscript: A paragraph was added to the conclusion in order to highlight the issue.

3. *There are some additional, minor sources of uncertainty which the authors should at least mention. (a) How well is the pointing of the MRRs, do the volumes really match? (b) Does the different size of the observation volumes due to the different distance to the antenna matter?*
Yes, this points should be mentioned. Since one of the major assumptions for the theory's method is homogeneity of the rainfield within the area of interest, the effects of different observation volumes for cases fulfilling this condition should be negligible.
Changes in manuscript: A paragraph was added to the conclusion in order to highlight the issue.

4. *I'm not sure whether I understood section 4 correctly. The authors used the measured, attenuated reflectivity field and assumed it to be real. Then they applied Gaussian noise to each pixel and checked whether they get a correction factor of 1.0? Maybe the beginning of the section can be updated to make the general concept more clear.*

The beginning of Section 4 was clearly missing some more details for easier understanding, thank you for pointing this out.

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...'

5. *What happens if the method is applied to the Lindenberg data? How do real, measured distributions compare to Figure 6? In case a calibration offset is found for the MRR, is the offset stable?*

We agree on the importance of testing the method using real, measured data. However, the setup of an appropriate network is not trivial. The pointing of both horizontally oriented MRRs had to be corrected, 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. **Changes in manuscript:** Adapted title

6. *I would expect interference if three FMCW radars are operated with the same frequency. Why is this study not affected by that?*
Interferences between the radars is no problem here. The frequency modulation and tolerance of carrier frequencies of the individual radars lead to mutual frequency differences far outside of the analysed beat frequency range.

7. *How do the results depend on used integration time?*
This is a very good point and certainly needs to be studied with regard to a stable and correct calibration result. The theoretical study presented here can not provide an answer to that question and real, measured data will have to be analysed. This is not a problem specific to the presented method, that just requires consistent measurements from three devices. In general, it will be a trade off between minimising noise through a long sampling time and still resolving the variability of the measured rain field. Looking at MRRs 10s values provide good measurements, but averaging over a longer time could help reducing effects from temporal and spatial mismatch.
Changes in manuscript: A paragraph was added to the conclusion in order to highlight the issue.

8. *How was the data of MRR no 1. and 2 processed? as far as I know, the Z provided by the standard MRR software is calculated from the measured $N(D)$ and not by simple integration of the power spectrum. For a slanted MRR, $N(D)$ cannot be estimated correctly and therefore the Z would be wrong. In this case,*

a method like e.g. Kneifel et al. 2011 or Maahn and Kollias 2012 should be used.
Thank you for this remark. This information is of course important and was missing in the text. In order to obtain correct reflectivity measurements for the horizontally oriented MRRs in Sec. 4 the power spectrum is integrated as done in the article mentioned above. Z from the MRR software is not used. An explanation was added in the article along with the references.

Changes in manuscript: 'For slanted devices R_1 and R_2, reflectivity is calculated directly by integration of the power spectrum, as done before by e.g. Maahn and Kollias (2012); Kneifel et al. (2011). The values given by the standard MRR software are derived from the measured DSD and are only valid for vertically pointing MRRs.'

Specific comments:

p. 1672, l. 14 *Please highlight that 30 dBz refers to reflectivity. For attenuation, the unit would be wrong.*
Changes in manuscript: '(reflectivity above approx. $30\,\mathrm{dB}Z$)' in the abstract

p. 1672, l. 24f *I think stating an error of only 1% is non-credible, see also general comments. At least I would recommend to add the found range, i.e. 1 to 11% error.*
Changes in manuscript: '... the estimated uncertainty of the calibration factor is in the order of 1% to 11%, depending on the chosen interval width.' in the abstract

p. 1673, l. 3 *'Most accurate' sounds like gauge measurements would be actually accurate, but they are not. Please provide a typical error.*
Changes in manuscript: '...have achievable measurement uncertainties of about 5% (Vuerich et al., 2009).'

p. 1673, l. 17 *Recent weather radars use also polarimetric variables for precipitation measurements which are far more accurate and not affected by calibration.*

Changes in manuscript: 'Although recent weather radars making use of polarimetric variables for precipitation estimates are more accurate and not affected by calibration, polarimetric methods are not applicable in all conditions and a demand for absolute calibration remains.'

p. 1674, l. 4 *Please provide a short introduction for the MRR. At least the operation frequency should be included.*
Changes in manuscript: 'Vertically pointing micro rain radars (MRR), using a frequency modulated continuous wave (FM-CW) measuring principle and operating at K band (24.1 GHz, $\lambda =$12.4 mm) (Peters et al., 2002), allow a comparison...'

p. 1675, l. 2 *When calibrating the other MRRs I see one problem: As far as I know, the MRR is calibrated with a wet antenna. Calibration with a dry antenna can result in a difference of some dB (Would be actually interesting to study this with the novel method). The antenna of no. 3 is certainly wet, but are the antennas of no. 1 and 2 also wet? This depends not only on the question whether it is raining at 1 and 2 as well, but also on the mounting of the two vertically pointing MRRs. From Fig. 1 it looks like the antennas were mounted such that the antennas cannot get wet during precipitation.*
The antenna of the vertically pointing radar R_3 is inevitably wet, so C_3 is valid for a wet antenna. The state of the antennas of R_1 and R_2 does not matter since C_1 and C_2 cancel out in this procedure.

p. 1675, eq. 1 *introduce s_0 and s_{max}*
Changes in manuscript: 'The positions of R_1 and R_2 are denoted s_0 and s_{max}, respectively.'

p. 1676, l. 3 *Please explain the method by Atlas et al. 1973 briefly. In addition, please highlight that $Z \propto N$. What assumptions are required for this approach?*

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Changes in manuscript: '...according to the method of Atlas et al.(1973), using an analytical relation between drop terminal velocity and drop size in the absence of vertical winds.'
'Because N is proportional to Z,...'

p. 1676, eq. 4 *Doesn't N_3 depend on h as well?*
Changes in manuscript: $N_3(D_j, s_3)$ was replaced by $N_3(D_j, s_3, h)$ in all equations

p. 1678, l. 18 *Do the results change when this condition is removed? I.e. apply the Gaussian function to smaller bins and then average Z to the MRR resolution.*
This assumption is done in order to fit measured data obtained from MRRs, where one averaged value describes the rain within one range gate. The variability within one range gate can possibly affect the results when comparing different observation volumes, but was not studied here.

p. 1678, l. 22 *I would say Z depends on DSD and the backscattering cross-section*
Changes in manuscript: 'Then, the theoretical, intrinsic reflectivity $Z(i)$, depending on DSD and backscattering cross-section, is calculated. '

p. 1679, l. 15 *Even though mathematically correct it is slightly confusing to use the inverse of C_3. Maybe this can be introduced earlier?*
Changes in manuscript: 'In the following, the inverse of C_3 is considered, since C_3^{-1} is the factor with which data is corrected after calibration.' added at the end of the theory Section

p. 1682, l. 3 *Why was 1.4dBz chosen as threshold?*
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. 1683, l. 13 *Please include that this result is valid only for Lindenberg*
Changes in manuscript: '...the most appropriate setting for calibration in the network considered here.'

p. 1684, l. 7f *This would be true if precipitation would be normal distributed, but is that true?* The distribution of precipitation at one point does not matter here. It is important that precipitation is uniformly distributed in space within the area of interest, which can be assumed.

p. 1684, l. 24 *The numbers in abstract and conclusions should be the same.*
Changes in manuscript: Numbers changed in abstract

Table 1 *Can these numbers be included in Figure 6?*
Changes in manuscript: Figure 6 is updated to show mode, 25% and 75% percentile.

Figure 4 *I find the colorscale of the left plot 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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