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

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Response to interactive comments of Referee 4

The paper addresses the question of absolute radar calibration, which is a topic highly relevant to hydro-meteorology. Paper on this issue are welcomed. Some innovative methodological developments are introduced which is a good point and validated by synthetic rainfall fields. The manuscript is interesting and deserves to be published. However I believe that some aspects should be improved before publication and that the modifications needed require a minor revision.
Many thanks to the reviewer for the detailed comments on the manuscript and the provided suggestions which contributed to the improvement of its quality. Our answers to the comments are listed in the following.

General comments:

1. *There is a need to emphasize more explicitly and discuss the validity of the various assumptions made in the development of the method. See detailed comments below.*
   This is certainly correct and we are thankful for the recommendations. Please see the details below.

2. *I did not understand very well what was done in section 4. Real data? Synthetic?*
   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...'

3. *It would be very interesting to actually test the developed method on a real case (not clear to me whether it is possible with the data in section 4, its seems the radar configuration is OK). If possible, I would suggest to slightly reorganize the manuscript with section a 'proof of concept' that would include tests with homogeneous rainfall, very regular pattern, and more realistic one (current section 4) and a new section with actual implementation.*

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Interactive Comment

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.
Changes in manuscript: Adapted title

Specific comments:

p. 1672-1673, l. 28-1 *'this implies... heigth': some references that quantify this effect should be added (ex : in a radar context Jaffrain and Berne 2012, or more generally Gires et al. 2014, or Moreau et al. 2009*
These references should indeed be mentioned.
Changes in manuscript: 'This implies... height (e.g. Gires et al., 2014; Jaffrain and Berne, 2012; Moreau et al., 2009).'

p. 1674, l. 1-2 *disdrometers could also be mentioned*
Changes in manuscript: 'or disdrometers (e.g. Nielsen et al., 2013; Lee and Zawadzki, 2006) at ground level.'

p. 1675, l. 5 *'a strongly attenuated frequency', some quantitative elements should be*

given (what wave length are needed)
Changes in manuscript: '...strongly attenuated frequency (e.g. K band)...'

p. 1676, eq. 4 *may be more explanations for eq. 4 would be needed*
Changes in manuscript: 'Because N is proportional to Z, the relation between measured and intrinsic DSD is analogous to the expression for the reflectivity:...'

p. 1677, eq. 7 *there is a strong assumption that DSD is constant in the vertical profile above the MRR. It should at least be explicitly mentioned. What is the typical height where the three beams cross (for example in the configuration of section 4)?*
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 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. 1677, l. 5-6 *'specific attenuation.... particular section', again this is a strong assumption, especially given that n=8 is advocated after which corresponds to a section of more than 3 km, over which rainfall is highly variable moreover during one 10s time step (see examples in Jaffrain and Berne 2012 and Gires et al. 2014, or Mandapaka et al. 2009) of small scale rainfall variability. The limitations of this assumption should be discussed more explicitly.*
Changes in manuscript: 'Again, constant specific attenuation along this particular section is required. Considering the high spatial variability of rainfall on small scales (e.g. Gires et al., 2014; Jaffrain and Berne, 2012b; Mandapaka et al., 2009), one important challenge of the method becomes obvious here.'

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p. 1677, l. 5-6 *The differences in the volumes scanned by the MRR and the radars should be mentioned and discussed.*

Accurate alignment, particularly of the horizontally pointing radars, is very important since inhomogeneities of the rain field is probably the dominating error source. Even in case of perfect alignment the different shapes of the observation volumes remain. Optimum agreement is obtained in the middle of the measuring path. Regarding the vertically oriented MRR, range gates (minimum resolution of 10 m) within the beams of the horizontally pointing radars can be averaged in order to provide a more consistent comparison. One of the major assumptions for the theory's method is homogeneity of the rainfield within the area of interest. For cases fulfilling this condition the effects of different observation volumes should be negligible.

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

p. 1679, l. 28 - p. 1680, l. 1 *Ok according to the graph but for large n, the homogeneity assumption is much less valid...*

We mentioned this explicitly in the updated manuscript.

Changes in manuscript: 'Notice that when considering inhomogeneous DSD along the measuring path as it would be the case in real conditions, the assumption of homogeneity within the section of interest is less valid the largest n is chosen.'

p. 1680, l. 4 *'possible... results', it remains a very regular patterns with regards to actual one*

'Heterogeneity' maybe was the wrong word in this context.

Changes in manuscript: 'investigates possible impacts of precipitation maxima or minima on the calibration results.'

fig. 4-5 *could you explicitly mention in the text that colour scales for the mean are not* *the same on both figures (it would help the reader, at least me :-)). It enables to see that with homogeneous rainfall the influence of n and R is actually quite limited.*

Changes in manuscript: '(note that the color scales of Figure 5a and Figure 4a are different)'

section 4 *Test on synthetic data with realistic precipitation pattern - I do not really understand what is done in the section ? Real data just to obtain rainfall (and only rainfall) patterns over the section ? Then why adding a random noise ? Could you clarify.*

Changes in manuscript: The beginning of Section 4 was updated to make this clearer, please see the response to comment 2 for more detail. Random noise is added to the synthetic, attenuated reflectivity fields in order to simulate measuring uncertainties.

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