Response to the review by Anonymous Referee #1 to the manuscript *"Prediction of rainfall measurement errors using commercial microwave communication links"* in AMTD, 2010

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General comment

1. Many assumptions used to conduct the proposed calculations are not justified enough. It is difficult to judge if they are relevant or not, and this is a serious issue for a scientific article. Here is a list of assumptions which validity should be more rigorously demonstrated:

Response: *We appreciate the comments made by Anonymous Referee #1, and attempted to address the raised issues as much as possible in the revised version of the manuscript. Please find the author's comments in the italic below.*

(a) Page 7, eq.13: it is supposed that the interpolation technique employed allows a correct description of the attenuation baseline, so that the noise η is 0 on average.

Response: *Interpolation of the baseline according to the pre/post-event measurements is a natural way to reconstruct the baseline change along the event, provided that there is no other data available. However, we agree that it is difficult to distinguish which baseline variations are described by the interpolation and which aren't. Actually, it is not essential for the paper which way the baseline is estimated; it is essential to correctly estimate the baseline variation given any baseline estimation technique.*

In the revised version, we simplify the definition, removing the interpolation: now baseline is estimated as an average attenuation before and after the event, without interpolation. Consequently, the sample variance of the baseline is now estimated as a deviation from this constant baseline value.

(b) Page 8, eq.15: which terms are neglected and can they be neglected?

Response: *Neglected are the terms of the Taylor series expansion of non-linear* $a_w(A_M - \hat{A}_0 - n_0 - n_q)$ *around the estimate of the rainfall-induced attenuation* $A_M - \hat{A}_0$ *involving noise terms* $-n_0 - n_q$ *of orders two and higher. They can be neglected under an assumption that the magnitude of* $A_M - \hat{A}_0$ *is much higher than that of the noise terms. While this is the case for high rain rates where* a_w *is nearly constant (otherwise, the reduction of Eq. (14) to the conventional Eq. (17) similar to the one used, for example, by Leijnse et al. (2007b) would be impossible), this approximation can lead to the additional errors in the estimation of* $\hat{A}_R$ *and* $\hat{\sigma}^2\left[A_R + n_w \middle| A_M - \hat{A}_0\right]$ *for weak rain rates where signal to noise ratio is low and more Taylor expansion terms may be required to accurately represent* a_w*. Noting that the second derivative* $a_w''(x) < 0$*; the direct consequence of this is that the conventional estimation of* $\hat{A}_R$ *using Eq. (17) leads to overestimation of weak rain rates due to neglecting of the term with* $a_w''(x)$*. By retaining this term, (17) transforms into*

$$\widehat{A}_R \cong A_M - \hat{A}_0 - a_w\left(A_M - \hat{A}_0\right) - \frac{a_w''\left(A_M - \hat{A}_0\right)}{2} \cdot E\left[\left(n_q + n_0\right)^2\right]$$

One can see that zero-mean noise $n_q + n_0$ *leads to a bias if the last term in the above equation is dropped. However, this issue can be put aside as long as the empirical function* a_w *is calibrated for the model given by Eq. (17) to provide unbiased rain rate estimation (see Sect. 3.2).*

This explanation has been added into the revised version.

(c) Page 8, eq.17: idem...

Response: *The Eq. (17) is obtained by taking expected value of the Eq. (16); no further assumptions have been involved.*

(d) Page 12, eq.26: the authors should demonstrate that the assumption of the rain gauge being representative of the (link) path-integrated rain rate is valid?

Response: *Due to the difference in the nature of observations, a single point gauge is not necessarily representative of the link path-averaged rain rate due to spatial rainfall variation. Spatial variations may lead to considerable differences in path-averaged rainfall amount in the link's location and point rainfall amount at the gauge's location, even though the link and the gauge are installed in close proximity; in this case, the link-gauge difference in rainfall intensities and baseline errors will be absorbed into the wet antenna attenuation coefficients when the latter are calibrated using Eq. (26). However, in the climatological scale, the gauge records can be considered representative of the areal average rain rate; the link-gauge differences in the recorded rainfall amount will decrease with increase of the number of different realizations used for calibration. Eq. (26) requires therefore minimization over much data that comes from multiple links, oriented in various directions w.r.t. rain gauges. We added a figure, demonstrating that for the link length ranges where much data are available (e.g. 2…3 km), the link-gauge differences in total recorded rainfall amount per event are widely scattered around zero, that indicates that realization-specific differences in rainfall intensity at link-gauge locations have little effect on the resulting wet attenuation coefficients.*

This discussion has been added into the revised version of the manuscript (Sect. 3.2). Following this analysis, we excluded 2 parallel links (L1 and L3) from consideration in the revised text, since their records are available for only one event. We also excluded 9 out of 13 link-gauge pairs with 7.16 links since, due to considerable differences in link-gauge rainfall records, wet antenna attenuation cannot be calibrated using Eq. (26). The respective explanation has also been added into the text. The issue of the representativeness of rain gauges for calibration of wet attenuation, leading to overfitting, is also reflected in the conclusion.

(e) Page 15, l.2: is the assumption of isotropic spatial structure of rainfall field valid?

Response: *This assumption is central for the presented analyses; its validity is discussed in the Sect. 5.2, where the experimental results (real world observations) show that the isotropic model allows explanation of great deal of errors of the link-gauge comparisons.*

We added a note into the revised version in the beginning of the Sect. 4.2.

(f) Page 18, l.1-2: on what is based the assumption of constant expected value of rainfall intensity?

Response: *This assumption is what the ordinary kriging (Schabenberger and Gotway, 2005) technique is based on. In the climatological scale, the expected rainfall intensity in an area depends on the location (constant in our case, per link) and the area size (determined by the link length and the link-gauge distance, constant per link in our case as well).*

This explanation has been added into the text.

2. The proposed analytical expression of the uncertainty affecting microwave link rain rate estimates is evaluated by comparison with point measurements from (nearby) rain gauges and "spatialized" using a climatological variogram. There are sources of errors in the process, so the quantification made is not very reliable and accurate.

Response: *Of course, any modeling process involves errors; a central goal of the paper is evaluation of the proposed models using real-world data. In this real-world setup, the possibility to verify the proposed models are somehow limited, as discussed in Sect. 6. The verification of the models goes as follows:*

1. *The self-consistency of the proposed analytical expression of the uncertainty affecting microwave link is evaluated using the original (modeling) data in Sect. 3.3 and Table 2.*
2. *To verify the conditional semivariogram model standalone, we conducted an additional simulation; Sect. 4.4 with a respective discussion have been added into the revised version of the manuscript.*
3. *The complete system, combining the models together, has been evaluated and analyzed using the real-world data in the Sect. 5.*

We also add into the Sect. 6 (conclusions) that verification of most of the models shows errors of approximately 5-20%, so that the quantification made is not very accurate. However, we believe that it still allows understanding typical magnitude and relative contribution of various error sources.

3. Only 3 rain events are considered, this is really a very limited sample to draw robust interpretations...

Response: *In the first version of the manuscript, there are 63 different data samples (link-gauge time series realizations), recording different parts of the same 3 rainstorms.*

In the revised version, we added 3 more rainstorms, total of 96 different link-gauge pairs have been used in the analyses.

4. Page 20, l.1-2: there is a confusion between rainfall intensity variability and DSD variability: they are not independent, as the rain rate is a weighted moment of the DSD. If the rain rate is variable, then the DSD is variable. The total concentration and the shape of the DSD can change (while the rain rate may remain constant). This list of sources of error should be rephrase. And also in the abstract.

Response: *Obviously, in terms of nature of the effects, DSD variation along a link and rainfall intensity variations in the link's neighborhood are DSD variations. However, in terms of error sources, they are different entities: DSD variation along a link affects the accuracy of measurement of the true path-averaged rainfall that, ideally, can be improved by choosing optimal link frequency. The spatial variability of rainfall intensity in the link's neighborhood affects the ability to estimate rain rate out of the link path that is a different task.*

In the revised version, we clarified this point, as well in the abstract.

3 Specific comments

1. Title: which characteristic of rain fall is investigated? I suppose it is the rain rate or rainfall intensity... This should be clarified in the title.

Response: *The name has been changed to "Prediction of rainfall intensity measurement errors …".*

2. Page 4, l.7: A_0 results from the scattering of the link signal by atmospheric gases, and mostly by water vapor. So the term "dry air" is a bit confusing.

Response: *We removed the term "dry air" from the context and the entire paper.*

3. Page 5, l.1: the Rayleigh approximation is not valid for the considered range of frequencies. The Mie regime is more appropriate.

Response: *Indeed, the Rayleigh scattering gives exactly power low (D^6); Atlas and Ulbrich (1977) talk about approximation of the scattering cross-section by a power law for frequencies where Rayleigh scattering is not applicable. We removed the reference to Rayleigh from the text, thank you.*

4. Page 6, l.7: other factors can influence the attenuation of a link signal: temperature effect on transmission/reception electronics, wind effects on antennas and masts,...

Response: *Thank you, we added these points into the text.*

5. Page 6, eq.11: ε is no defined.

Response: *Indeed, missed. We add the definition into the text, thank you.*

6. Page 6, l.21-22: what does "before and after a rainstorm" exactly mean?

Response: *The time of rainstorm starts and ends in the area has been determined according to nearby rain gauges with 10 minute margins, to compensate for link-gauge physical distance. Then, the measurements of* A_M *of 2...27 hours length (depending on data availability) have been used for calculations.*

We added this explanation into the text.

7. Page 6, l.23: Rahimi et al. (2003), Upton et al. (2005) use dual frequency links if I am correct. Commercial links cannot be considered as dual-frequency links (because the frequencies used for the different channels are too close).

Response: *The same technique, based on the correlation between parallel links (high correlation means rainfall) has been successfully adopted for commercial microwave links (Goldshtein et al. 2009). To apply the technique, there is no need for link to be dual-frequency as for rainfall intensity measurements.*

We added one more reference describing a different technique (Schleiss and Berne, 2009).

8. Page 7, l. 1-2: why a cubic spline can accurately capture the behavior of the attenuation baseline?

Response: *Smooth interpolation of a baseline between dry periods is a natural technique to use unless other information about baseline change during rainfall is available. Cubic spline interpolation is just a convenient tool, answering this definition.*

In the revised version, the cubic spline interpolation has been replaced with a simpler technique – just average baseline before and after the event.

9. Page 7, l.15: for (relatively) high frequency (about 38 or 58 GHz) commercial links, are the effects of non-linearity still negligible?

Response: *At frequencies around 34 GHz where the attenuation-rain rate relation becomes nearly-linear, the difference between averaging of rain rate and attenuation nearly vanishes.*

We added this comment into the text.

At 58 GHz, the relationship is non-linear again, so non-linearity will definitely distort the measurements if attenuation is averaged to get temporal accumulated rainfall amounts, instead of rain rate. The power law coefficients at 38 GHz are similar to the ones at 34 GHz.

10. Page8, l. 5-7: this paragraph is not clear to me. Why does the term n_q appear in the attenuation baseline term? Especially if the attenuation baseline is estimated by interpolation...

Response: *This paragraph talks about bias, caused by quantization, if it is not rounding but either ceiling or flooring. If this is the case, the quantization noise (not zero mean) naturally appears in* A_M *(since the estimate* $\hat{A}_0$ *is averaged* A_M *), and therefore* $\hat{A}_0$ *is biased. However, the same quantization noise appears in the samples of* A_M*, captured during rainfall. Therefore, this (depending on quantization type) bias is cancelled for* $A_M - \hat{A}_0$*, and the residual quantization noise* n_q *is zero-mean.*

We rephrased this paragraph to be clearer in the revised version of the text.

Accordingly, if the attenuation baseline is estimated by interpolation, the same bias presents in $\hat{A}_0$ *both before or after the rainfall as well as during the rainfall, no matter which way the baseline is interpolated…*

11. Page 8, l.13-14: n_q and n_0 are supposed to be 0 on average, so individual realizations can still be significant.

Response: *We gave the justification of the linearization in the response to the major comment 1(b).*

12. Page 10, section 3: which values have been obtained and how? Are they consistent with literature values?

Response: *A table with resulting power law coefficients for typical frequency bands has been added into the revised text. The detailed description of how the power law coefficients have been obtained is given in the Sect. 3.1 in the first version of the manuscript. The power-law function has been found similar to the lognormal (Zhang and Moayeri, 1999), even though the actual coefficients differ due to different ways of optimization (non-linear optimization in Eq. (25) vs. linear in log-domain by Zhang and Moayeri (1999)).*

A table with the resulting wet attenuation coefficients for different link lengths has also been added into the revised text, and a respective discussion has been added into the end of the Sect. 3.2.

13. Page 16, l.11-12: 4 points in space is very limited to estimate the experimental variogram at 3 interdistances. How the values at different time steps have been combined?

Response: *In general, it is possible to get the experimental data over a larger span of distances from even single rain gauge record at 1 minute resolution, by invoking the Taylor hypothesis (using climatological average rainfall advection velocity 14.6 m s^{-1}) and transforming the time series into a spatial profile at the spatial resolution of 0.88 km. For generation of temporally averaged data for a Δt minutes interval, one should pass the time series via a rectangular moving average filter of Δt samples length. However, this operation introduces unrealistic correlation between adjacent samples. To avoid this, one can the subsample the filtered time series at Δt samples rate, but then the temporal (and, consequently, spatial) resolution becomes Δt-dependent. To preserve the consistency between experimental semivariograms for different Δt, separate realizations using different rain gauge pairs have been used instead of applying Taylor hypothesis.*

We added this comment into the text.

14. Page 17, eq.41: please provide a detailed description of the derivation. *e* is not defined.

Response: *You probably missed: the paragraph between Eq. (40) and Eq. (41) contains the definition of e and explains that the Eq. (41) is obtained by substituting the Eq. (39) into Eq. (40). By simply following these directions, the Eq. (41) is derived.*

15. Page 19, eq.46: $\hat{\sigma}_{tj}^2$ is given by eq.44?

Response: *You probably missed: the paragraph following Eq. (46) contains the explanation that* $\hat{\sigma}_{tj}^2$ *is the predicted MSE, given by Eq. (44).*

References:

Schleiss, M. and Berne, A.: Identification of dry and rainy periods using telecommunication microwave links, Proc. 34th AMS Conf. Radar Meteorol., Williamsburg, VA, USA, 2009.

Zhang, W. and Moayeri, N.: Power-law parameters of rain specific attenuation, IEEE 802.16cc-99/24, 1999.