Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2019-51-RC3, 2019

Atmospheric Measurement Techniques
Discussions

Interactive comment on "Estimating raindrop size distributions using microwave link measurements" *by* Thomas C. van Leth et al.

Anonymous Referee #3

Received and published: 16 May 2019

General comments:

The manuscript concerns estimation of drop size distribution (DSD) from attenuation of radiowaves at different frequencies/polarizations. The focal point of the manuscript lays in validation of proposed methods by numerical simulations, nevertheless applicability of the method is demonstrated also during a single rainfall event on attenuation measurements obtained from an experimental setup with dual polarized 38 GHz and horizontally polarized 26 GHz microwave links and array of disdrometers. The topic is relevant and the methodology is scientifically valid. The manuscript is also well structured and very well written.

My major concern is in applicability of the presented approach on real attenuation measurements obtained from commercial microwave links (CMLs), which is where the pro-

posed methods have the highest potential. The DSD estimation is thoroughly tested on simulated attenuation observations, which are to my understanding ideal (not perturbed by any errors). This should be clearly stated probably already in the Method section because it is very important for interpretation of the results. The authors are apparently aware of different limits and pitfalls when it comes to application of the method on CML data, nevertheless the discussion of these limitations and pitfalls could (should) be more specific. This is important, because some of the conclusions based on numerical experiments (e.g. that dual frequency method is insensitive to difference between frequencies) are to my understanding only valid for ideal CMLs with high precision and accuracy. Detailed sensitivity or uncertainty analysis which would enable to quantify effect of inaccuracies in attenuation measurement on the efficiency of the proposed methods is probably out of the scope of this study, nevertheless the manuscript would clearly benefit from more robust discussion of the results in the context of real attenuation measurements from CML networks. This issue is further discussed in the specific comments.

Despite my concern regarding applicability of the method in real CML setting I consider the authors' work as a valuable contribution to DSD research and research related to exploitation of CMLs for environmental monitoring and believe that authors can address this issue by relatively minor revisions.

Specific comments:

P1L8 – 9 Abstract: Isn't the accuracy of the method highly dependent also on precision of the measurements as noted in the Conclusions or more general on accuracy of identified rainfall induced attenuation?

P3L10 – 14: Why do you use transect of 20 km when typical length of CMLs operated at frequencies 15 – 38 GHz (and esp. 26 – 38 GHz) is substantially shorter? Moreover, you later demonstrate the method on 2.2 km CML. Could you indicate the reason for simulating CML over entire length of the field?

P7L2: The reasoning should probably refer to eq. 14 instead of Eq. 11 – 13 to apply not only for dual polarization setting but also for dual frequency setting. Furthermore, variables used in eq. 15 are defined in eq. 14 and not in Eqs. 11 – 13.

P13 – 15: Please comment on spells with no results which can be seen on Fig. 10 (and later also on fig. 16). Are they due to not identified parameters, or due to measurement outages? Please, comment on these 'outages' also on P20.

P18L12 – P19L5 (the whole section): It is not clear if the offset is applied to both frequencies resp. polarizations. If yes, then the whole analysis is not much informative. And what if bias evolves in time differently for both polarizations? This might be the case e.g. due to wet antenna attenuation when droplets on the surface of antennas start to transform themselves into rivulets (see e.g. Mancini et al., 2019).

As noted in the Conclusion section, stability of baseline level is crucial in practice for utilizing the proposed technique. In practice, the feasibility of your DSD estimation approach will be probably very much sensitive to ratio between rainfall induced attenuation and other sources of attenuation which cannot be easily identified, i.e. it is reasonable to expect that method will be applicable only to CMLs relatively sensitive to rainfall (longer CMLs, higher frequencies) where effect of wet antenna attenuation and limited quantization is not so much pronounced. Given this, it might be much more informative to investigate sensitivity of the methods to precision of CMLs, e.g. simple averaging might very well simulate quantization of CMLs.

I fully acknowledge that detail sensitivity or uncertainty analysis on the whole dataset might be sufficient for stand-alone work and it is out of the scope of this investigation, however, performing such analysis on a subset of data (e.g. the event used for demonstration on real data) would be probably sufficient to enable discussing applicability of the method in real CML network in more specific manner (see the next comment to the P23L1 – 11).

In any case, the limitations of recent sensitivity analysis should be addressed either in the section itself or within discussion section. It would be also valuable to i) refer to typical values of baseline offset and ii) to include into discussion of this issue also wet antenna effect. Finally consider presenting results in dB/km. This would make them applicable also to other link lengths.

P23L1 – 11: The issues discussed in this paragraph are of crucial importance for application of the method in a real CML network. It would make sense to discuss in here also quantization of CMLs (which is raised at the end of the Conclusions without previous deeper discussion). Also discussion on wet antenna attenuation might be deeper, e.g. to which extend we can expect it will differ for two frequencies or polarizations? Finally, the results presented in the section Dependence on link frequency should be discussed in a view of inaccuracy of real CML measurements. It is likely that by lower frequencies (e.g. 17 GHz) only relatively long CMLs will have sufficient sensitivity to rainfall (and thus sufficient precision) to be suitable for DSD estimation. Similarly, insensitivity of DSD estimation on an offset between frequencies (now presented as one of the conclusions) does not apply for real data with limited precision. The attenuation difference will be for typical CML frequencies and lengths at least for light and moderate rainfalls below quantization of the CML records.

P23L33 – P24L7: The second paragraph of the Caveats section lacks exactness of the previous text with vague formulations like 'potentially more serious', 'disdrometer measurements is used more directly', or 'somewhat in favor of'. Detailed investigation and quantitative assessment of discussed issues is probably out of the scope of this manuscript, nevertheless, authors might consider reformulating the second paragraph to provide more specific reasoning why and to which extend can the mask influence the results and overall applicability of the proposed methods.

P23L29: typo, 'too' instead of 'to'

P25L15 – 17: The limited precision of CMLs due to quantization is a very important limitation which should be discussed in more detail already in the Discussion section (see comment to P23L1 – 11).

Some further questions I would be curious about (no need to answer):

- The three parameter method which uses as an input three attenuation measurements ($k_{1,2,3}$) provided less accurate DSD estimate than two-parameter method. Have you tried to apply two-parameter method on different combinations of attenuations (k_1 / k_2, k_1/k_3, k_2/k_3) and use the redundant parameter estimates for improving the estimation accuracy (e.g. identifying outliers)?

- There is probably some autocorrelation in parameters of DSD function, have you thought about using autocorrelation structure of DSD observations to constrain the optimization procedure?

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2019-51, 2019.