REBUTTAL

amt-2019-51, Submitted on 08 Feb 2019

Estimating raindrop size distributions using microwave link measurements

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Associate Editor Decision: Publish subject to technical corrections (25 Jan 2020) by Saverio Mori

Comments to the Author:

Dear Authors, In my opinion this work can be published; nevertheless several important corrections are absolutely required. Anonymous referees have given precise indications in this respect, both on minor comments and on major and substantial ones. Without an adequate addressing of the issues indicated, this work is not suitable for publication.

Dear editor, thank you very much for communicating your decision. We have addressed all issues indicated. See our detailed replies to the referees' comments and suggestions, as well as our revised manuscript (with adjustments with respect to the previous version indicated in red).

Non-public comments to the Author:

Dear Authors, first i apologize for the delay within the publication process. I have had several doubts on how proceeding, because of the analysis mine and of the anonymous referees. I have decided to proceed, nevertheless i condivide the most critical comments of referees #1 and #3, and you should adequately address them. The proposed approach is interesting but based on idealistic assumptions: this must be clear in all the paper, and the assumptions clearly indicated and described; also conclusions have to be corrected. All The Best

Dear editor, we apologize for the delay in our response. This was due to personal circumstance beyond our control. That said, in the revised version of our paper we have followed your recommendation to make it even more clear to the reader that the adopted approach represents conditions where measurement errors and uncertainties associated with microwave link measurements do not play a role. We would like to stress, however, that we did test the proposed methodology on measurements from actual microwave link instruments during one event (Section 6). In any case, our assumption of idealized conditions has now been articulated even better throughout our paper, although we would like to recall that we already discussed the practical limitations of our method in the previous version of our paper (e.g. in the last sentence of the abstract and quite extensively in the discussion section). To avoid any misconception on the part of the reader, we have extended the title of our paper to "Estimating raindrop size distributions using microwave link measurements: potential and limitations".

Anonymous Referee #1

Assessment:

This is the second time I review this paper and my overall impression of it remains rather negative. I'm particularly disappointed in the way the authors handled my previous comments. Some superficial changes were made but the really important issues regarding feasibility and validation remain the same (see below for more details). In their rebuttal, the authors say that "the suggestion to dig deeper into the experimental data is not helpful and further analysis over the whole experimental dataset would not yield meaningful results." I don't agree with this assesment and encourage the authors to reconsider their position. In particular, I don't think that there is enough scientific evidence to support the feasibility of the retrieval methods yet. The simulation study is interesting but highly idealized and far away from reality. Given that there is still no proper uncertainty/error analysis, it is hard to judge the soundness of the retrievals. Unfortunately, since the authors do not seem to be interested in performing more detailed and rigorous investigations, I cannot recommend publication at this point.

We respectfully disagree with this referee. We believe that our previous revision included significantly more than "some superficial changes". We would like to stress that our paper is certainly not meant to be the last word on estimating raindrop size distributions using microwave link measurements. Rather, it should be seen as an extensive feasibility study, both under simulated conditions (using simulated DSD fields derived from polarimetric radar observations for two rainfall events) and real conditions (using a nine-month dataset from a line configuration of five laser disdrometers). We also tested the proposed methods on measurements from actual microwave link instruments during one event along the same path as the disdrometers. Finally, we sincerely believe that our paper does involve "detailed and rigorous investigations", although we admit that a full error analysis of the proposed DSD retrieval methods is beyond the scope of the current paper. We aim to address this in future work.

Main arguments against publication:

1. There is no rigorous and realistic assessment of the uncertainty affecting the retrieved DSD parameters (e.g., no error bars and no benchmark for comparing results). The simulation studies are performed in idealized conditions which do not reflect reality.

Although we have not included a full error analysis (see our response above), we do present a discussion of the sensitivity to attenuation bias (Section 5.3) and power quantization error (Section 5.4). As mentioned above, a full error analysis of the proposed DSD retrieval methods is beyond the scope of the current paper. Note that the simulation studies are based on real radar or disdrometer data, where the latter span a period of nine months representative for rainfall in the Dutch climate.

2. The presented evidence does not always match the conclusions/statements made by the authors. There are several inconsistent and contradicting sentences (see below). The general conclusion of the paper regarding feasibility remains unclear.

We thank the reviewer for identifying occasions where our statements could be interpreted as favoring the potential rather than the limitations of the proposed methodology. We have rephrased these statements wherever relevant. Moreover, we changed the title to better reflect the limitations.

3. The writing is biased towards highlighting potential rather than providing a fair objective scientific assessment of feasibility and accuracy.

See previous response.

A. Feasibility

A1. The authors base most of their conclusions on a few, highly idealized simulation studies. But to me, these are of little practical and scientific value. In reality, there are serious issues due to the instability of the baseline, quantization and wet antenna attenuation which make the proposed techniques very unlikely to be ever applicable to commercial microwave networks. Indeed, Figure 5 shows that the relationship between the attenuation ratio and the value of mu is almost flat. To get a good accuracy on mu, one therefore needs a very high accuracy on the attenuation ratio. *"To achieve a non-convergence ratio of 10%, quantization errors of 0.001 dB would be required."* However, current accuracies are 0.1 dB at best, which is several orders of magnitude lower than what is actually required. Higher accuracies are unlikely to be ever available in commercial networks due to the high cost of measuring power more accurately and other technical limitations (e.g., additional uncertainty due to baseline and wet antenna).

We respectfully disagree with the referee that we "base most of [our] conclusions on a few, highly idealized simulation studies". As mentioned above, we test the feasibility of the proposed DSD retrieval methods both under simulated conditions (using simulated DSD fields derived from polarimetric radar observations for two rainfall events), real conditions (using a nine-month dataset from a line configuration of five laser disdrometers) as well as measurements from actual microwave link instruments during one rainfall event along the same path as the disdrometers. That is significantly more than "a few, highly idealized simulation studies". Indeed, it will be a challenge applying the proposed methods to commercial microwave link networks. But one first needs to perform a feasibility study such as the one we have performed to learn that such is actually the case. In addition, we believe that reporting less favorable results is also important for scientific progress. Even then, our methodology may be applicable to dedicated research microwave link configurations such as the one described in Section 2.2 of our paper. Such a setup may then be seen as a (very) large disdrometer.

A2. Figure 14b clearly shows that the attenuation ratios derived from actual data are extremely noisy and poorly correlated with the true attenuation ratios. And this is for a "good" case without any quantization noise. Sure, you can cherry pick a few decent retrievals in there. But these might as well be coincidences and there is not enough hard evidence to prove feasibility. Please consider more cases and/or perform a more systematic and rigorous assessment.

As was indicated in the first sentences of Section 6 of our paper: "The baseline power level of the links showed considerable fluctuations over the course of the measurement period. Therefore, it was not feasible to perform retrievals for the entire 9-month dataset". This is an unfortunate situation which we are, alas, not able to revoke at this stage. The measurement campaign with the microwave link setup took place between 1 April 2015 and 1 January 2016. The experimental setup has since been disassembled. Actually, the building where one end of the microwave link setup was installed no longer exists. To accuse us of "cherry picking" at this stage does not feel fair. We believe we have made a serious attempt to demonstrate the challenges of applying the proposed DSD retrieval methods to actual microwave link measurements. Furthermore, we have selected the event that we present based on a relatively stable baseline around the event, rather than on the performance of the retrieval, as was clearly stated on p.21, line 18. An assessment on a more extensive dataset would mean setting up a new microwave link measurement campaign. That is certainly beyond the scope of the current work.

B. Validation and assessment:

B1. The approach used to validate the DSD retrievals using MOR, MAD and AD95 on N(D) and R is inadequate. At best, it's incomplete. On their own, these values don't mean anything! A proper validation requires a benchmark against which the reported performances can be compared. For example, if the goal is to retrieve the rainfall rate from the links, then you should validate against the alternative model of retrieving R through the power-law relation $A = aR^b$ (without any knowledge of the DSD). If your method does not perform better than that, then there is no skill in the retrieved DSDs for the rainfall estimation problem. Similarly, if your goal is to retrieve the Dm or mu values, then you should validate against the alternative model which assumes a constant value (e.g., the climatological mean). In any case, error bars and a rough estimate of the uncertainty affecting the retrieved quantities need to be provided!

As the title of our paper indicates, our aim was not to retrieve R alone, but rather to retrieve DSD parameters. The simulation framework we employed, based on two events with spatial DSD fields derived from polarimetric weather radar and nine months of disdrometer data, allowed us to explore the feasibility of DSD retrieval in a controlled environment, i.e. under conditions where one knows the true DSD (and hence the true R). In such a situation, the reference is not the climatological mean but the true value of a DSD parameter. Therefore, we think that the validation approach is adequate given the goal of this paper. Finally, concerning the actual microwave link measurements discussed in Section 6, we have not included a full error analysis, but – as we mentioned above – we do present a discussion of the sensitivity to attenuation bias (Section 5.3) and power quantization error (Section 5.4). Once more, a full error analysis of the proposed DSD retrieval methods is beyond the scope of the current paper.

B2. It is not 100% clear how MOR, MAD and 95AD were calculated. Please provide unambiguous expressions/equations for your performance scores and clarify the difference between the "normalized" and non-normalized versions.

We believe the definitions of MOR, MAD and 95AD provided in Section 3.4 represent unambiguous descriptions of our performance measures. As stated in the same section, "all metrics are normalized with respect to the median of the original quantities". We added ", hence they are dimensionless" to this sentence and "(also dimensionless)" after "the failure ratio" to further clarify the employed metrics. We believe that should be clear to the reader. In addition, none of the other referees asks for "expressions/equations" of the statistical measures we use.

B3. It would be good to show a few cases in which the retrievals failed in order to have a better understanding of the numerical issues involved and the type of measurements that cause the algorithm(s) to fail. Right now, the paper mostly focuses in highlighting good cases, which is only one side of the story.

We state toward the end of Section 3.4 that "we also compute the fraction of non-convergent retrievals compared to the total number of retrievals. This 'failure ratio' is necessary for a complete picture of the robustness of the method since the other metrics naturally exclude these intervals". Tables 2 and 4 report the values of this failure ratio for rainfall retrievals based on microwave link simulations for the nine-month disdrometer dataset. Finally, Section 6 provides an application of the proposed retrieval methods to actual microwave link measurements for a complete rainfall event, clearly showing the challenges and limitations of the proposed methods. Hence, in all honesty, we believe we pay ample attention to "the numerical issues involved and the type of measurements that cause the algorithm(s) to fail".

B4. The sensitivity study in 5.3 is based on unrealistic assumptions. The use of an equal offset for both frequencies/polarizations is much too optimistic. In reality, the errors/offsets on the individual measurements are likely to be independent. Indeed, the final offset is the result of many error/noise terms from multiple factors such as electronics, baseline attenuation, wet antenna and quantization effects. By assuming the same offset for both measurements, you are dramatically underestimating the uncertainty affecting the attenuation ratios. Please use independent offsets during the sensitivity study or justify why you think it is appropriate to use correlated noise terms.

We respectfully disagree with the reviewer that the offsets are likely to be independent, as baseline fluctuations and wet antenna attenuation will affect multiple links in a similar manner. Of course, we realize that the assumption of an (equal) offset for all attenuations is hardly ever completely met in practice. However, it provides a reasonable first order appreciation of the effects of attenuation biases, which was the purpose of the analysis presented in Section 5.3.

B5. Page 18, ll.16-17: Why don't you take the effect of noise into account in the simulations. Please explain!

Because that is beyond the scope of the current paper. As stated, "it is expected that this would influence the retrieval the most when the frequencies are close together". We have decided to study the effect of offsets because this is known to be the largest source of error in microwave link rainfall monitoring, and it is hence expected that noise would have a smaller effect than offsets. It would certainly be very interesting and relevant to study this effect in detail, but that would effectively lead to an additional paper. We are motivated to address this issue in future work.

B6. Page 13, Figure 7: There are important conditional biases in the retrievals of Nt and mu. But almost no explanations are given to what caused them. A more detailed discussion is needed to understand these results and how they affect the quality of the DSD retrievals.

We agree that it would be interesting to learn more about the reasons for the mentioned biases, but unfortunately we have no clear explanation for them. As mentioned in Section 4.2 "These outliers do not seem to correspond with any particularly high or low precipitation intensity, but they do correspond with high drop concentrations". There is not much more we can meaningfully say about this.

C. Inconsistent and/or misleading statements:

C1. Page 1 (abstract): *"Simulations show that a DSD retrieval on the basis of microwave links can be highly accurate."* This is a strong statement that is not aligned with the evidence presented in the paper. In reality, the simulations show that even under idealized conditions, the retrievals can fail. Please reformulate.

We have adapted the formulation in the revised paper by removing "highly" and adding "under idealized conditions" at the end of this sentence. Note that we already stated in the last sentence of the abstract *that "in practice, the accuracy and success rate of any retrieval is highly dependent on the stability of the base power level as well as the precision of the instruments and in particular the quantization applied to the recorded power level".*

C2. Page 25, ll. 8-9 the authors write that: *"This provides a hopeful perspective for the application to commercial networks."* However, this is not really consistent with the other statements made in the paper. For example, on page 21, l.7 it is said that *"This limits the prospective of successful application to current networks".* On page 25, ll. 5-6, *"the links examined in this study lacked stability […] and wet antenna attenuation was an intractable problem".* On page 25, ll. 11-13, *"The only way to apply such retrievals to currently operational unmodified link networks consistently is to install dedicated data-loggers at selected link locations to read out the analog signal directly, which might not be feasible."*

That particular statement ("This provides a hopeful perspective for the application to commercial networks") refers to the sentence before, which reads: "we found that a former commercial microwave link had a much stabler baseline and furthermore the effect of wet antennas was much more manageable for that particular device". In other words, this was not a general qualification of the potential of commercial microwave links for DSD retrieval, but rather a statement about the (perhaps surprising) stability of the baseline of a former commercial microwave link as compared to a dedicated research link. To clarify this further, we added at the end of the mentioned statement: ", in particular if data could be logged with high precision (Chwala et al., 2012)". Apart from this, we do not see a good reason to rephrase this statement.

C3. Page 25, ll.2-3: *"No such problem exists in principle with regard to the phase difference; it is independent of any baseline as long as that baseline is indifferent to polarization."* Yes, but there is no evidence that the baseline is actually indifferent to polarization and wet antenna attenuation. Please reformulate the sentence to avoid misunderstandings.

We thank the reviewer for pointing this out. The condition that the baseline is indifferent to polarization is not necessary. We meant that the base line issues which affect rainfall retrievals based on measurements of signal amplitude do not play a role for retrievals based on signal phase differences between orthogonal polarizations. Wet antenna attenuation will affect the received signal amplitude, but not the phase difference at the receiver. We shortened this sentence to: "[...]; it is independent of any power baseline".

C4. Page 27 (conclusions): *"... we have shown that a DSD retrieval on the basis of multiple microwave link variables can be successful and highly accurate, but only when precise high-resolution records of received power are available."* This is a very misleading statement. Firstly, the conditions under which a retrieval can be made and the uncertainty affecting the retrieved values remain unclear (i.e., due to the lack of a proper uncertainty analysis). Secondly, what is really needed for a successful retrieval is a precise measurement of the "rain-induced attenuation" and not the "received power". That's a big difference because in practice, it is almost impossible to get a precise rain-induced attenuation estimate, even if you could measure the received power accurately. Please reformulate to convey the right meaning.

You are right. We have removed "highly" and we have replaced "received power" by "rain-induced attenuation".

D. Others:

D1. The discussion about computation time is not really relevant. There are no real challenges associated with the numerical optimization techniques used in this study and real-time implementation would not be a problem. I suggest to shorten this part or remove it in favor of a more detailed uncertainty assessment.

This "discussion" is actually only one paragraph in Section 7.1. Because the computational burden is an important distinction between the two-parameter and the three-parameter retrieval method, we think it is relevant for the reader to know about this. In fact, this distinction may even become more relevant in an operational setting, where real-time computations are required, especially if this is done on embedded link hardware (Chwala et al., 2012).

D2. Page 4, section 2.2: The temporal resolution of the DSD data are missing. I assume it's 30s?

Indeed, thanks. We added this in Section 2.2, also for the microwave links.

D3. Page 8, l.13: the figure number is missing

Thanks. That should have been Fig. 4b. This is now corrected.

D4. Page 10, l.17: What do you mean by "real outliers?" As opposed to imaginary ones?

Haha. We meant the actual "outliers", which fall outside the core of the error distribution. We replaced "real outliers" by "true outliers".

D5. Page 12, ll.1-2 *"These outliers do not seem to correspond with any … with high drop concentrations". Why? Can you elaborate?*

Unfortunately, no. As indicated above, we would have liked to understand this issue ourselves, but have not succeeded in doing so.

D6. Page 12, l.10: of the rain intensity ~~is~~ <u>are</u> given in Table 1

Thanks. "is given in Table 1" has been replaced by "are given in Table 1".

D7. Page 19, Figure 11: Please provide units for MOR, MAD and 95AD and specify what quantity is considered here (N(D) or N(D)/Nt?).

As indicated in the caption, "all statistics are normalized with respect to the median of the moment of the original measured DSD". Hence, MOR, MAD and 95AD are dimensionless in Fig. 11. This has now also been explicitly stated in Section 3.4. Also, as indicated in the caption, what is considered here is "the third order moment of the DSD [...] as a function of carrier frequency". Hence, this is neither N(D) nor N(D)/Nt.

D8. Page 21, l.12: "This ~~is~~ can be attributed"

Thanks. We removed "is".

D9. Page 26, the threshold used to select DSDs for inferring the mu-lambda relationship is not what I call a "compromise". It's a fixed threshold imposed by the authors based on a previous paper without any justification or optimization. Please reformulate.

It is a "compromise" between the desire to be comprehensive (take all measured DSDs into account no matter how small the sample size) and the desire to be selective (only take those DSDs into account that correspond to significant sample sizes and hence rain rates). One could call this threshold an "educated guess", but we believe "compromise" actually reflects best what we mean. Therefore, we have decided to keep it.

D10. Page 26: "Considering the small spatial scale of the measurements we considered and the high spatial correlations therein this is an acceptable loss". This sentence does not make any sense.

We replaced this sentence with: "Considering that the employed disdrometers were located relatively close together and therefore that their measurements are strongly correlated, we accepted this potential bias".

D11. Page 26-27: The discussion about the truncation of the gamma distribution on page is besides the point. The real issue is not the truncation but the fact that real DSDs are never perfectly gamma. Even if they were distributed according to a gamma at the point scale, the average DSD along the link path would be a mixture of gamma distributions with different shape parameters which is not a gamma anymore. To me, the whole discussion about the truncation issue seems to be a minor issue in this story. Instead of obsessing about it, the authors could provide more details about the sampling uncertainty affecting the retrieved DSD estimates or the sensitivity to the temporal resolution of the link data.

We are not "obsessed" with truncation effects; we simply discuss it as one of the caveats (Section 7.2) of our approach. Nothing more and nothing less.

D12. Page 27: "… but the effects of this on high order moments is minute". Does not make any sense. Please reformulate.

We meant "minute" in the sense of "(very) small", "minimal" or "marginal".

Anonymous Referee #2

REVIEW REPORT

Review of amt-2019-51-manuscript-version4

By Thomas C. van Leth, Hidde Leijnse, Aart Overeem, and Remko Uijlenhoet

Manuscript Title – Estimating raindrop size distributions using microwave link measurements

MINOR COMMENTS

In my opinion the Authors have addressed all my concerns/question improving the quality of the manuscript. I suggest minor revision before the publication. The main revision regard several sentence within the text that probably refer to the figures reported in the previous version of the manuscript and therefore describe something that is not shown in the referred Figure. It can be solved modifying the sentence or adding *(not shown)* in the text.

We thank the referee for his/her positive evaluation of our revised paper as well as for the suggestions for further improvement. We have taken all of them into account in the revised version of our paper (see below for our detailed responses).

The latter happen at:

- Page 12 3rd line: "The temporal evolution…."

We added "(not shown)" to the end of the sentence "The temporal evolution of μ is very close to the temporal evolution of Λ, with a correlation coefficient of 0.86."

- Page 12 last 3 lines: "The retrieval gives an …."

We added "(not shown)" after "The retrieval".

- Page 15 "Furthermore, fort both the retrieval methods,…."

We added "(not shown)" after "the retrieval".

- Page 22 "The resulting DSD is very similar…."

We added "(not shown)" to the end of the sentence "The resulting DSD is very similar in shape to that obtained in the simulations, with overestimations especially at smaller diameters, but with the general shape of the DSD preserved".

- Page 27 1st line: the list of the Figure is wrong and all the sentences until the end of the Section need to be checked.

We replaced "Figs. 6a, 8b and d, 9c, 10 and 15c" with "Figs. 6a, 10a and 13". We checked the other sentences and concluded that they make sense with this revised numbering. The only change we made here is that we added a comma before "which suggests that the gamma distribution is a valid approximation".

Please check if there are other sentences in similar conditions that I did not notice within all the manuscript.

We checked the entire manuscript, but could not find other instances that required attention.

Below some few suggestion:

- Page 8 last paragraph: "….(as shown in Fig??)…."

See response to referee #1. That should have been Fig. 4b. This is now corrected.

- Page 10, line n. 5: please quantify "significantly". Which is the allowed differences between retrieved and TS96 parameters to consider that the gamma distribution is not a good fit?

We employ the qualification "significantly" loosely in this context. We refer to the correspondence (or lack thereof) between the estimated and the 'true' parameters upon visual inspection rather than as a result of *sound statistical testing. To avoid confusion, we replaced the first instance of "significantly" with "appreciably" (l.5) and the second instance with "considerably" (l.6).*

- Page 10, line 6: please quantify "significantly"

See previous response.

- Page 10, lines 7-8: Which is the number of time that the measured DSD deviate significantly from the TS96 DSD? Which is the number of time that the estimated DSD deviate significantly from the measured DSD? Please add this information

Note that this is still the Methods section (3), where we do not want to present actual results. The requested information is provided in the Results sections (4 and 5), in particular in Figs. 7 and 8 (Section 4) and Fig. 9 (Section 5) and the corresponding text.

- Table 2 (and all the tables that report the same statistics) : For sake of simplicity please add the unit. fail is a percentage?

All tables report normalized (and hence dimensionless) statistics, as is clearly indicated in the captions. The failure ratio is a (dimensionless) fraction (multiply with 100 to obtain a percentage). This has now also been explicitly stated in Section 3.4.

Anonymous Referee #3

General comment:

The manuscript concerns a retrieval of drop size distribution (DSD) from attenuation of microwave links. Its focus is primarily on numerical validation of a DSD retrieval concept. This rose major concerns of all three reviewers to the initial submission as it was unclear to which extent were the proposed methods applicable for real microwave link observations affected by different sources of errors. The authors took an effort and substantially revised the manuscript. They provided additional numerical analyses investigating effect of signal quantization (precision) on the DSD retrieval and also changed several figures (time series plots to scatter plots) to better illustrate the results.

We thank the referee for his/her positive evaluation of our revised paper as well as for the suggestions for further improvement. We have taken all of them into account in the revised version of our paper (see below for our detailed responses).

The additional analyses show that accuracy of microwave link observations in real networks will greatly affect reliability of the results and substantially limit the feasibility of a DSD retrieval in practice. The authors discuss the feasibility issue in the Discussion section, however, the Conclusions and Abstract section lacks clear and unambiguous statements about feasibility. E.g. the Conclusion section starts with the statement: "Using both simulations and actual link data we have shown that a DSD retrieval on the basis of multiple microwave link variables can be successful and highly accurate, but only when precise high-resolution records of received power are available."

See our response to referee #1 and our response to this referee's next remark. Following his/her suggestion (C4), we have removed "highly" and replaced "received power" by "rain-induced attenuation" in this sentence.

Yes, the authors showed, that a DSD retrieval can be highly accurate on ideal data, but definitely did not demonstrate this high accuracy on real data. And of course, if precise high resolution records would be available, the method would be highly accurate also on real data, but this is not the case. I think that partly negative results are not a reason preventing the manuscript to be accepted for a publication, however, the authors should be much more careful not to give an impression that retrieving accurate DSD estimates from real microwave links is highly feasible. This concerns especially the Conclusion section and Abstract section.

We thank the referee for his/her constructive feedback. We agree that any misconception should be avoided. Following the recommendations of the editor and of referee #1, our assumption of idealized conditions has now been articulated even better throughout our paper, although we would like to recall that we already discussed the practical limitations of our method in the previous version of our paper (e.g. in the last sentence of the abstract and quite extensively in the discussion section). To avoid any misconception on the part of the reader, we have changed the title of our paper to "Estimating raindrop size distributions using microwave link measurements: potential and limitations". In addition, we have replaced the statement "Using both simulations and actual link data we have shown that a DSD retrieval on the basis of multiple microwave link variables can be successful and highly accurate, but only when precise high-resolution records of received power are available" with "Using simulated link data we have shown that a DSD retrieval on the basis of multiple microwave link variables can be successful and accurate, but only when precise high-resolution records of rain-induced attenuation are available. This was confirmed when applied on actual link data, where baseline variations prohibited accurate DSD retrievals".

Furthermore, the manuscript would clearly benefit form more specific conclusions with respect to limitations of the method in practice. For example, the figure 5 together with the table 4 indicates pretty clearly what is the minimal required accuracy of observed attenuation and the range at which it is feasible to estimate the 'mu' parameter. This might be relatively easily linked with minimal rainfall intensity and minimal microwave link length (which affects the sensitivity to rainfall) required for a DSD retrieval.

This is discussed to some extent in the last paragraph of Section 5.4. In particular, we state the following: "To achieve non-convergence ratios of less than 10% a quantization of 0.001 dB or less is required, which makes this not achievable with current generation operational networks. It should also be noted that taking into account the quantization error in the analysis favors the dual-frequency method over the dual-polarization method. This can be attributed to the steeper slope of the attenuation-ratio–μ relationship within the band of common DSD shapes as shown in Fig. 5". A more specific statement regarding the *"minimal rainfall intensity and minimal microwave link length [...] required for a DSD retrieval" can unfortunately not be provided, because this does not only depend on the instrumental characteristics of the microwave link at hand (the length of which in our case was a fixed 2.2 km), but also on the space-time properties of rainfall (in particular of the DSD). This is definitely something we would like to study in further detail in future work.*

Finally, it is difficult to interpret if the obtained results are in fact good or bad. The two-parameter method relies on calibrating relation between parameters of gamma distribution Lambda and mu using disdrometer data (or DSD typical for a local climate), i.e. typical values of Lambda and mu have to be known. It is therefore not clear, what is the real information gain, when using attenuation data compared to the baseline scenario when average (or most common) DSD pdf is assumed to be same for all the events. The results for such baseline scenario (or similar) should be, therefore, included into evaluation. The comparison to '’baseline scenario' might also explain, why three-parameter method (which works independently on calibrated relation between 'Lambda' and 'mu') perform substantially worse than two-parameter method.

The gamma parameterization for the raindrop size distribution (DSD) contains three free parameters (N_T, μ and Λ). Hence, in principle three independent measurements (in this case with microwave links) are needed to retrieve each of the parameters. However, decades of research concerning the parameterization of DSDs has taught us that the three gamma parameters are correlated to each other, suggesting that the effective number of free parameters is actually less than three. This is also the scientific basis for radar remote sensing of rainfall, both using conventional single-parameter radars (which use measurements of radar reflectivity Z to estimate rain rate R) and using dual-parameter radars (which typically use observations at orthogonal polarizations to estimate R and DSD parameters). The use of a climatological μ–Λ relationship is a cornerstone of many rainfall retrieval algorithms for dual-parameter weather radar (see e.g. the papers by Zhang et al., 2001, 2003, cited in our paper). Here, we employ exactly the same approach in our dual-parameter rainfall retrieval algorithm, but now applied to microwave links rather than weather radars. In fact, the climatological μ–Λ relationship we derived from nine months' worth of DSD data from The Netherlands (Eq. (17)) is quite similar to the one proposed by Zhang et al. (2003). Hence, the proposed approach has firm footing in the (radar meteorological) scientific literature. The main reason why the three-parameter method has trouble beating the two-parameter method is probably related to the fact that, besides numerical convergence issues, the effective number of free DSD parameters is rather two than three (due to the mentioned correlations between the parameters). With three measurements the estimation problem then becomes overdetermined. We feel this is discussed in sufficient detail in Section 5.1 of our paper. With respect to the question about the information content compared to assuming a climatological DSD shape, it is clear from the literature that the two parameters of a gamma distribution with μ correlated to Λ are nearly orthogonal, and both provide a significant amount of information. Hence, as stated above, having two parameters does add a significant amount of information as compared to just having one parameter (which would be the case if a climatological DSD shape would be assumed).

I recommend the manuscript for a publication after minor revisions concerning mostly presentation and interpretation of the results.

Again, we thank this referee for his/her constructive feedback.

Specific comments:

Fig. 7, 8, and 9: Cannot see orange dots. Maybe transparency of points would help. In addition, consider showing correlation coefficients for all three methods.

The orange dots are indeed invisible in Fig. 7d, can hardly been seen in Fig. 8d and 9d, and clearly fall on the 1:1 line in Fig. 15d. This is a consequence of the fact that the method of moments (of which TS96 is a special case) works very well for the estimation of the rain rate R. Therefore, the use of transparent points would not help. In addition, we do not see the need to show correlation coefficients, as the statistics corresponding to Fig. 7d and 8d are shown in Table 1, those related to Fig. 9d in Table 2, and those related to Fig. 15d in Table 4.

P18L1-3: Is it so, that tree-parameter method can work also without disdrometer data used for calibrating relation between gamma distribution parameters Lambda and mu? If yes, this might be worth to note, because it is important benefit of this method compared to two-parameter method.

This is correct, as has already been noted in Section 3.3 ("In order to still solve for the two parameters an additional equation is required for the relationship between μ and Λ"). Note, however, that (as noted in Section 5.1) "the addition of a third microwave link variable does not improve the retrieval (in many cases it actually harms the retrieval) and is unnecessary". Also see our response to one of the previous remarks.

P22L5-6: Consider noting that resulting DSD is not shown.

As suggested by referee #2, we added "(not shown)" to the end of the sentence "The resulting DSD is very similar in shape to that obtained in the simulations, with overestimations especially at smaller diameters, but with the general shape of the DSD preserved".

P26L23-28: The evaluation of baseline scenario with most common Lambda and mu parameters used for all the events would enable more robust reasoning clearly separating effect of DSD observations from the information gain obtained from microwave link attenuation.

As noted on l.25–26, "the μ–Λ relationship [is] determined from the total of all 9 months of disdrometer measurements, not from the specific event in question". Hence, this is a fixed climatological relationship employed for the entire nine-month period. As such, our method does already separate the "effect of DSD observations from the information gain obtained from microwave link attenuation", as requested by the referee.

P27L11-13: The phrasing gives misleading impression that highly accurate retrieval of DSD was demonstrated also on real data, which is not correct.

We agree. Hence, we decided to replace "both simulations and actual link data" with "simulated link data". Also see our response to referee #1 and our response to one of this referee's previous remarks. Following the suggestion (C4) of referee #1, we have replaced "received power" by "rain-induced attenuation" in this sentence. In summary, as noted in our response to referee #3, we have replaced the statement "Using both simulations and actual link data we have shown that a DSD retrieval on the basis of multiple microwave link variables can be successful and highly accurate, but only when precise high-resolution records of received power are available" with "Using simulated link data we have shown that a DSD retrieval on the basis of multiple microwave link variables can be successful and accurate, but only when precise high-resolution records of rain-induced attenuation are available. This was confirmed when applied on actual link data, where baseline variations prohibited accurate DSD retrievals".

Anonymous Referee #4

Accepted as is.

We thank the referee for his/her positive evaluation of our revised paper.