Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2020-28-AC4, 2020

Atmospheric Measurement Techniques Discussions

Interactive comment on "Atmospheric observations with E-band microwave links – challenges and opportunities" *by* Martin Fencl et al.

Martin Fencl et al.

martin.fencl@cvut.cz

Received and published: 24 May 2020

This is a revised version of our response. Our reactions to six specific comments has not been by accident included into the previous version. We would like to apologize for this mistake and provide here an updated version with complete list of responses.

First of all, we would like to thank the reviewer for a constructive and encouraging review. Bellow are our reactions:

The paper by Fencl et al. addresses a topical and interesting matter, as extends known

Printer-friendly version

Discussion paper

opportunistic precipitation sensing techniques to the more recent E band links. It highlights the new possibilities uncovered by the different frequencies and hardware and focuses on the consequent challenges. The authors give a complete picture of the subject from theory to application, preparing the ground for future studies. The article is therefore certainly valuable and of primary interest to the CML scientific community and AMT readers. The work is well written and the goals defined in the abstract and introduction are all met. The discussion of the main issues is complete and rich, while some redundancy and repetitiveness is found in introductory and methodological sections, combined in certain cases with lack of the detailed quantitative information needed to contextualize some statements. Accordingly, a minor revision is suggested in order to provide the reader with more concise and relevant information in the cases treated in the comments below. The author's answers to previous comments (AC1 and AC2 to SC1 and RC1 resp.) have been taken into consideration.

We will follow specific suggestions of the reviewer to remove identified redundancies and repetitiveness and will provide additional quantitative information where required.

General comments

1. *The fragmentation of the presentation as reported in comment 3. of RC1 is recognized: most of the topics are introduced in Sections 2 and 3 and then corroborated with quantitative data only in section 4 or even 5. Given the different data sets and methods utilized for the various steps of the investigation, the reading results some-times erratic indeed. However, the intentions declared by the authors (AC2) are also well understood. I will then strongly encourage a more widespread use of subsection cross referencing, to help the reader understand without changing the logical structure of the paper. An example of convenient referencing is found e.g. in L362 and 363. This should be replicated diffusely to connect introductory and discussion Sections. It seems to me that multipath*

disturbance instead is not introduced at all before L577 and should be added to Section 2 with some estimate of its magnitude.

Thank you for understanding to our intention to avoid inclusion of Results into Method and Material section. However, to make our presentation clearer we carefully identified redundancies and use more widespread cross-referencing as suggested. In the revised version of the manuscript, we also introduced multi-path disturbance (already in the Section 2.1, where different components of total observed loss are introduced).

2. *Another downside of the chosen presentation layout is the need of re-introducing some aspects generally many times throughout the paper, without going quickly into the necessary detail. A more concise and unitary approach to the problems encountered and the solutions adopted would facilitate a global understanding of the work. I suggest therefore to support the introductory informations, in the first sections already, with quantitative informations and stating author's intentions regarding approximations and further discussions. In that way the reader could expect what to find in the next sections and repetitive recalls to the qualitative introduction would not be needed. Some non-exhaustive examples are reported below and most of the specific comments deal with this same issue. L74 to 78 - though the paragraph's introductory intent is clear, it lacks the detail and clearness about which assumptions are kept and which are discussed, with respect to previous 15-40 GHz approaches. L94 to 101 - It is not clear at this point how the authors will deal with the reported considerations further in the paper.*

Our intention is to avoid inclusion of our original findings in section 1 and section 2. Section 1 provide general introduction with state-of-the art in microwave link rainfall estimation based upon which the goals of this manuscript are defined. Section 2 provides theoretical background enabling reader to follow our original methodology and results. We would like to keep our original methodology and findings clearly separated and thus we want to avoid summary of our

original findings already in the introductory sections. Similarly, we would like to keep our original methodology separated from theoretical background provided by previous works (section 2). Thus, although concept of baseline separation is introduced already in the Section 2 (L74-78) we prefer to explain how we approach this challenge in Section 3 - Material and Methods, specifically on lines L266 – L271. Following the same intention, we prefer not to explain assumptions behind quantifying wet antenna attenuation in this work already at L94 – 101, but again in the Section 3, specifically on lines L273-284. To clarify this intention, we will modify the paragraph describing structure of the manuscript, specifically description of section 2 and 3: "Section 2 of the manuscript summarizes based upon previous works the principles behind retrieving atmospheric variables from CML observations, Section 3 describes the methodology and datasets used in this manuscript for the E-band CML assessment, . . ."

3. *An additional figure showing WAA against link length could be used to illustrate the linear regressions proposed in Eq. 11 and the constant behaviour in dew cases. A sample of how the figure could look is attached.:*

   Agreed. We will show such figure. Details are provided in the specific comment no. 11.

Specific comments (in order of appearance)

1. *L68 - Free space loss (Lbf) is said to be uniquely defined by distance and wavelength. Reporting the formula could be appropriate and helpful for further understanding of the discussion, as the frequency is a key variable for this study (E band)*

   Yes. We will report the formula of free space loss on lines L68-69 of the original manuscript. "Free space loss (L_{bf}) is uniquely defined by the distance (d) between the transmitter and receiver, and by wavelength (λ):

$$L_{bf} = 20log\left(\frac{4\pi d}{\lambda}\right)$$

where L_{bf} is expressed in decibels and distance d and wavelength λ are expressed in the same unit."

2. *L74-78 - The phrase "Attenuation during dry weather is assumed to be a baseline" is apparently in direct contrast with the following "Fluctuations in the baseline during dry weather can be attributed..." if the reader does not know already the different magnitudes involved. Early introduction of orders of magnitude and average behaviours is therefore encouraged.*

   Agreed. We will report typical magnitudes of rainfall and gaseous attenuation in the section 2.1, after description of different components of total observed loss (L74 in the original manuscript).

3. *L101 - "More extensive investigations..." I think this sentence will state the motivation of the author's work, but it could be also interpreted as what still remains unknown after the work's results instead. Please clarify to avoid this ambivalence.*

   The sentence indeed state our motivation. We will try to make it clearer by expressing at the end of the Introduction section, where structure of the manuscript is described, that section 2 provides review of previous work (see response 2 in the general comments).

4. *L131 - Fig. 4 is useful to the contextualisation of this sentence and should be referenced. "Contribute relatively less" is not gaugeable, some more detail may be added.*

   Agreed. We will reference Figure 4 at line 132 of the original manuscript. Nonetheless, we kindly disagree with the second suggestion. We would like to

avoid detailed quantitative description in here. Reader can easily read quantitative information from the figure 1 referenced in this sentence.

5. *L145 and following - The study on the components of N is not justified by following discussion or results and could be omitted as it lacks quantitative information. I think that the qualitative concept of the dependency of k to the various components is already well stressed.*

   Agreed. As suggested, study on the components of N will be omitted. Interested reader can find these details in the cited literature. We will thus remove lines 145-150 of the original manuscript.

6. *L194 - "The periods for evaluating rainfall retrieval and for evaluating the effect of humidity and temperature fluctuations on gaseous attenuation are, therefore, different." The phrase itself is a quite obvious consequence of the previous sentence, while its implications are not. It should either be omitted or some expected implications should also be discussed (or at least some reference to the respective discussion should be made) in terms e.g. of which investigations are precluded by using different time windows.*

   Agreed. The phrase is obvious and we will thus delete it.

7. *L200 and other appearances of "aggregate" - it should be pointed out how the aggregation to different time scales is performed (mean, median, sum, max, other...)*

   We aggregate to different time scales using mean. We will add this information to the corresponding places (L198, L200, and 205 in the original manuscript).

8. *L246 and 247 - The sentence is not clear and should be rephrased and expanded. "dependent" should perhaps be substituted with "depending", commas before and after "therefore" are not necessary and slow the reading. The threshold for Dm is not indicated.*

Thank you, for spotting this typo. This typo apparently led to misunderstanding. D_m as estimated by Eq. 10 is actually the threshold which is used for classifying rainfalls. This threshold (D_m) is dependent on rainfall intensity. We will change the sentence to: 'The approximation (10) is used to calculate threshold for classifying disdrometer records as convective or stratiform. The threshold is dependent on rainfall intensity.

9. *L259 - Visual inspection does not seem like a robust approach to filter the outliers. Some technique should be at least suggested to cope with this kind of artefacts, as the visual approach is clearly not feasible at larger and near real-time scales.*

   Visual check is indeed not a robust approach which could be used in future applications. The automation of quality check is, however, out the scope of this manuscript. Visual identification of artifacts is, in our view, first step towards future automation of this process. Moreover, the correction for artifacts is performed only in a single case. This correction is transparently reported (L260), to ensure reproducibility of the results.

10. *L269 - One-week sized moving window "is sufficiently short": are baseline drifts proven to happen only at longer time scales? Is the same for gaseous attenuation? Could it be that some higher frequency signal is masked by this approach resulting in the weakening of the water vapour detection capabilities?*

    No, as reported on L260-261, also sudden change in baseline occur in the case of CML 3004_3005 and this change was manually corrected. The baseline identification using one-week sized moving window is used only for rainfall retrieval. As reported on L305-306 of the original manuscript, constant baseline is used when analyzing effect of gaseous attenuation and potential for water vapor retrieval. Hardware related artifacts causing slow baseline drift have probably potential to destroy gaseous attenuation signal as discussed in the Discussion section (L581-584 in the original manuscript).

11. *L283 - A reference to Fig. 8 or to the suggested new figure could be added here.*

    Agreed. We will add a reference to the new Figure (created in the revised manuscript according to reviewer's suggestion). The figure is shown at the end of this response (Fig. 2). It depicts the period from 19:00 on 2nd Nov. to 14:00 on 3rd Nov. which is on Figure 7 of the original manuscript indicated as rainy.

12. *L296 and 340 - Since Prague is located at an altitude around 200m (990 hPa), to as-sume the atmospheric pressure of 1013.25 hPa seems either systematically wrong or reported with too high precision (if differences between 990 and 1013 are negligible for the author purposes, then decimals of hPa are even more so). It is therefore suggested to utilize 990 hPa as reference pressure or at least replace the number with a more generic "at sea-level pressure".*

    Agreed. We will replace the number with more generic "sea-level pressure" at L296 and 340.

13. *L323 - Short CMLs are highlighted in some following sections as valuable tools for intense rainfall detection. Here instead the sentence "The performance ..." says that they will be presented only as examples of bad performance. Please clarify.*

    Potential of short CMLs is highlighted in the Discussion section (L556 -562 of the original manuscript) in the context of observing heavy rainfalls associated with high spatial variability by which an assumption about uniform rainfall distribution along a CML path is more likely valid for short CMLs than for long ones. Nevertheless, only light and moderate rainfalls occurred during observation period. Short CMLs are, during these rainfalls relatively more affected by wet antenna attenuation than longer CMLs, as demonstrated e.g. on figure 7 in the original manuscript. The sentence, in the original manuscript actually states that: 'The performance of the short CMLs is shown to demonstrate limitations related to the improper baseline and WAA identification which are more pronounced by shorter

CMLs.' This is in our view, not in contradiction with Discussion section, where shorter CMLs are suggested as valuable tool for detecting heavy rainfalls.

To avoid misunderstanding, we will modify the sentence to: 'The performance of the short CMLs is shown to demonstrate limitations related to the improper baseline and WAA identification which are, especially during light rainfalls, more pronounced by shorter CMLs.'

14. *L347 to 358 - Is there any indication of what could cause the "degraded resolution" on the hardware side? If yes, it would be an interesting topic to read here.*

    In our opinion, the degraded resolution might be related to automatic power control. Nevertheless, this was not tested. We thus prefer to not speculate in this direction.

15. *L365 and Fig. 6 - It should be reminded to the reader that sub-links belonging to one CML are presented in pairs in consecutive order. It should be consequently pointed out that intra-CML correlation creates 2x2 darker squares along the diagonal in the correlation matrix plot.*

    We have decided to change IDs of CMLs in the whole manuscript to better indicate sub-links belonging to the same CML and in general improve clarity of the whole manuscript: each CML will have unique ID (numbers from 1 to 6). IDs of sub-links operating at 73 – 74 GHz and 83 – 84 GHz frequency will then consist of CML ID and suffix "a" resp. "b".

    Regarding intra-CML correlation, we believe, that the first sentence referring to the Figure 6 (L365 of the original manuscript) will then provide sufficient guidance in this respect: "Dry-wet weather classifiers of single sub-links belonging to one CML are strongly correlated (Fig. 6)."

16. *L369 - It is stated that the delay of the rain gauges in detecting rainfall with respect to CMLs "can be attributed to the delay of rain gauge rain detection due to*

the filling of the bucket." Please discuss whether delays and volume losses are compatible to the bucket size.

Thank you for this comment. We will remove last two sentences (L366-367) as they were based on analysis not shown in the manuscript and might have been considered speculative. The section will thus contain only results shown on figure 6 of the original manuscript.

17. *L377 to 380 - Same as L283, the dependency (and independence) of WAA to path length should be presented for rain (and no-rain) occurrences with a specific scatterplot and a linear fit (suggested figure attached).*

    Agreed. Figure 7 will be extended by a scatter plot showing relation between path length and total attenuation together with linear fits indicating effect of WAA. (The figure is shown at the end of this document).

18. *Fig.7 - When comparing signals from CMLs of different path length, specific attenuation (dB/km) should be preferred to pure attenuation (dB). If the aim is to show the different regimes (dependency and independence to path length), then two plots should be shown (dB and dB/km time series), in order to appreciate inter-CML concordance on specific attenuation during rainfall and on pure attenuation during dry periods.*

    The signal shown in the figure 7 is predominantly caused by wet antenna attenuation, which is independent of path length. We therefore prefer to show exclusively total attenuation in this figure.

19. *L405 - "However, it is closer ..." the reported considerations is interesting for an operational use and therefore valuable, but it is poorly proven (only visually). Without a gauge of the goodness of the approximation (or some reference to following consistent results), the ITU fit may as well not be good for either case (convective and stratiform).*

The attenuation-rainfall relation is for theoretical drop size distribution almost perfectly approximated by power-law fits (as reported on L400-401). Thus, distances between presented power-law curves (absolute errors) provide meaningful gauge of goodness. The term “it is closer” on L405 describe distances between the curves. Thus, it is, in our opinion, appropriate. Moreover, reader can easily get information on approximate distances for any rainfall intensity between 0-50 mm/h from the figure 9. In addition, parameters of power-law fits as well as parameters obtained from ITU (ITU-R, 2005) are provided as a part of figure 9. Interested reader can thus easily express exact value of absolute errors for any rainfall intensity, resp. specific attenuation.

20. *L433 - To my understanding, it is the first time here that some speciific deficits in baseline and WAA identification for sub-link 1147 are asserted. It seems quite in contradiction with other parts of the text were the long CML has the best results.*

    The longest CML clearly outperform shorter CMLs in terms of correlation (r = 0.96 resp. 0.97 compared to 0.53 – 0.86 resp. 0.61 – 0.87) to and RMSE (0.39 resp. 0.24 mm h-1compared to 0.64 – 2.18 resp. 0.69 – 1.44 mm h^{-1}), which can be seen in table 5 of the original manuscript. Its markedly better performance is also clearly visible from scatter plots in figure 11 of the original manuscript. The long CML cannot, however, accurately capture very light rainfalls under 1 mm h^{-1}, which represent about 25 % of the total rainfall depth in our case. We will add an information about underestimation of very light rainfalls to the paragraph (L428-L436 of the original manuscript) describing performance of the long CML.

21. *L448, 449 and Fig. 12 - The anti-correlation of the attenuation with temperature is evident from figure 12b and should be highlighted here, as temperature seems to be the dominant component of the signal. Moreover, this appears in direct contradiction with what stated in the first paragraph of Section 4.6, so that may be reformulated differently.*

The negative correlation between attenuation and temperature appears in the figure, because water vapor density is strongly correlated with temperature. As gaseous attenuation is highly correlated to water vapor density, there is also strong (negative) correlation link between gaseous attenuation and temperature. It is, however, not caused by direct dependence, which is almost negligible: See ITU-R, (2019) and Figure 2 of the original manuscript.

22. *L515 - "The similarity in antenna characteristics was not inspected directly." Are the antenna factory features known to the authors? Is this sentence referring to technical specifications of the antennas or to the actual status of the radomes?*

    It will be specified. The sentence refers to hydrophobic properties of antenna radomes as well as actual status of the radomes.

23. *Supplementary material - The ATPC (5th paragraph) is said to be "switched off" but, to mine understanding of Fig. S1, the concept of "saturated" may be more adherent to the case. It seems to me that ATPC can deal only with maximum 7 dB gains on tx, but it keeps working even there, in the sense that the gain remains 7 dB, while "ATPC switched off" is more likely a zero-gain scenario.*

    Yes, the ATPC keeps working in the sense it maintains tx power on the maximal (allowed) level. In the revised version of the supplementary material, we will use the term 'saturated' instead 'switched off'.

Technical observations:

1. *Figure 2 - It is not clear what the coloured bands represent (standard deviation or total spread) and neither is the direction from low to high pressure.*

   The color bands represent total spread. We will clarify this in the figure caption.

2. *L202 and Table 2 - "Height" is used, but maybe "depth" is a more common choice to indicate precipitation amount.*

   Agreed. We will use the term ‘depth’.

3. *Fig. 6 - Raingauge labels differ between image and caption (“wet_” prefix)*

   Thank you for spotting this inconsistency. We will correct it.

4. *Fig. 8 - Since the two plot rows represent different frequency ranges, some labels indicating the two ranges are fostered to be shown to the left of the plot. Otherwise this information should at least appear in the caption with "upper row" and "lower row" indications.*

   We will add to the left two labels indicating frequency ranges.

5. *L413 - I suggest the replacement of "heteroscedastic" with a more generic formulation, e.g. "the spread clearly grows with R and k". Although the adjective is certainly correct for a distribution like the one shown in Fig. 10, its use seems not proper for this context: given its precise statistical meaning and implications, I think it is preferable to run some specific tests of heteroscedasticity before asserting this property.*

   Done.

6. *Fig. 12 - The colours for theoretical and observed attenuations are poorly chosen as they appear very similar (especially light green against light blue), both on paper and on screen.*

   OK, we will adjust the colors in Figure 12 to differentiate better the time series.

Additional references

ITU-R: RECOMMENDATION ITU-R P.676-12 - Attenuation by atmospheric gases and related effects, (online) Available from: https://www.itu.int/dms_pubrec/itu-r/rec/p/R-REC-P.676-12-201908-I!!PDF-E.pdf, 2019.

ITU-R: RECOMMENDATION ITU-R P.838-3 - Specific attenuation model for rain for use in prediction methods, (online) Available from: https://www.itu.int/dms_pubrec/itu-r/rec/p/R-REC-P.838-3-200503-I!!PDF-E.pdf, 2005.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2020-28, 2020.

Printer-friendly version

Discussion paper

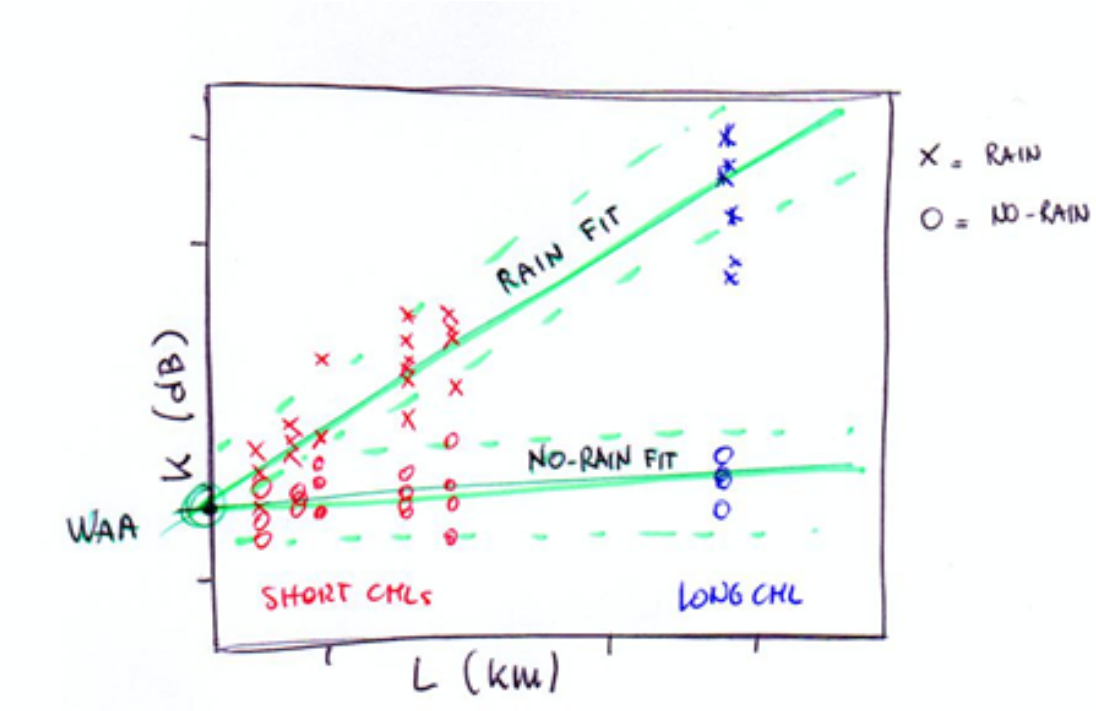

Fig. 1. Scatterplot of attenuation against path length with separated linear for rain and no-rain intervals (reviewer's suggestion).

Printer-friendly version

Discussion paper

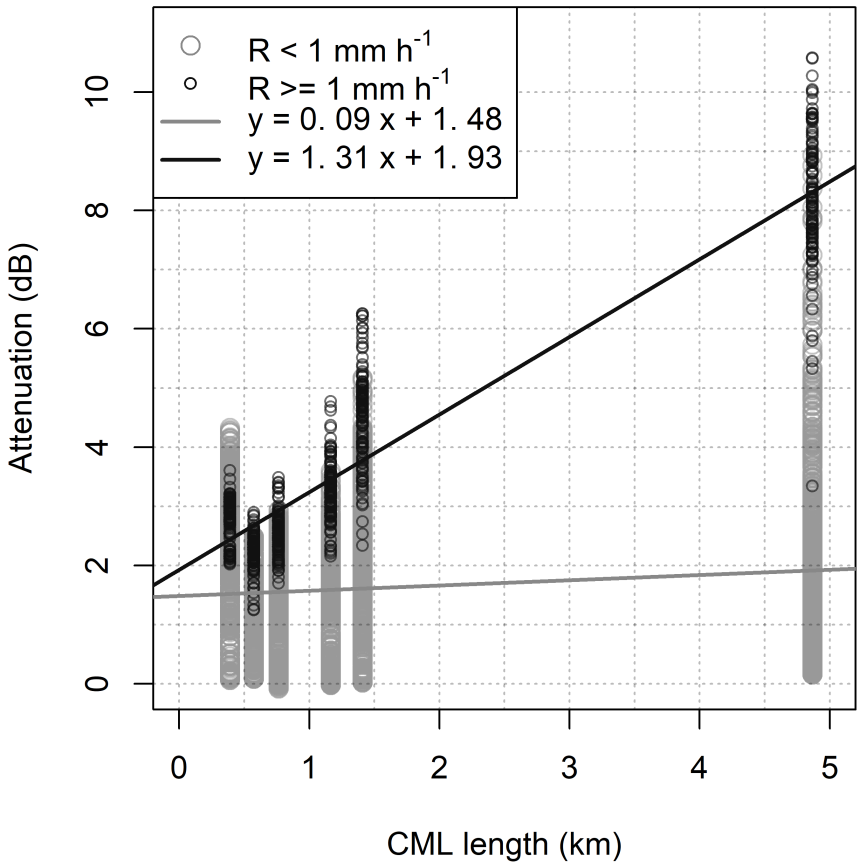

Fig. 2. Scatterplot of attenuation against path length for 83-84 GHz sub-links with separated linear for intervals with moderate rainfall and intervals with very-light rainfall with dry spells.

Printer-friendly version

Discussion paper