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Atmospheric Measurement Techniques Discussions

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

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

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 sometimes 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.

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.

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.

Specific comments (in order of appearance):

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).

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.

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.

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.

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.

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.

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...)

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.

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.

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?

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

L296 and 340 - Since Prague is located at an altitude around 200m (990 hPa), to assume 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 sug-

gested to utilize 990 hPa as reference pressure or at least replace the number with a more generic "at sea-level pressure".

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.

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.

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.

L369 - It is stated that the delay of the raingauges 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.

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).

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.

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).

L433 - To my understanding, it is the first time here that some specific 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.

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.

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?

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.

Technical observations:

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.

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

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

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.

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.

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.

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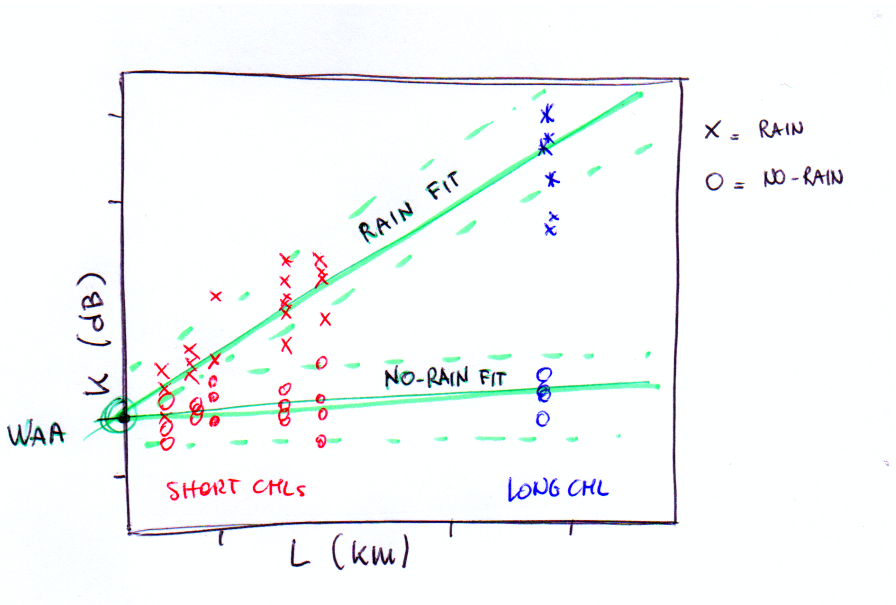

Fig. 1. Scatterplot of attenuation against pathlength with separated linear fits for rain and no-rain intervals.