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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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First of all, we would like to thank the reviewer for his valuable comments and suggestions. Below are our reactions:

Specific comments

1. *Fig.1: in the caption "scattering efficiency" is mentioned. Clearly this is not the same as extinction efficieny. Please clarify.*

   Thank you for spotting this inconsistency. The figure as well as equation 6 de-

   scribe extinction efficiency (Q_{ext}). We will correct this.

2. *Fig.2: I suspect there is something wrong here. I do not see why the water vapor attenuation should have a drop at 60 GHz. Is this affecting results later on???*

   We have checked our results and there is nothing wrong with it. Results are fully consistent with Liebe et al. (1993) and could be cross-checked by independent computer codes available on GitHub, e.g. https://github.com/cchwala/pyMPM.

   The attenuation due to water vapor is in here (as well as later on) defined as the difference between wet-air and dry-air attenuation under the same moist air pressure and temperature. Thus, also effect of water vapor on attenuation due to oxygen is considered as described in ITU-R, (2019), and formerly by Liebe et al. (1993): First, moist air pressure is the sum of dry-air pressure and partial water pressure, thus dry-air pressure decreases during humid conditions (under the assumption of the same moist air pressure). This leads to a decrease in attenuation by molecules of oxygen. Second, partial water vapor pressure influences the width of the oxygen spectral lines (pressure broadening) as it affects the rate of collisions between the molecules (eq. 5 and 6a in ITU-R (2019)). These two effects lead to a decrease in attenuation due to oxygen when water vapor content increases. This decrease can reach about 4 % for conditions with moist-air pressure 1015 hPa, temperature 30° C, and 100 % air humidity. Such decrease has relatively negligible effect for frequencies used in this study (73-74 and 83-84 GHz). Nevertheless, it is not negligible around 60 GHz (as can be seen on Figure 2), where the attenuation due to oxygen dominates over attenuation directly caused by water vapor molecules.

   We will clarify the definition of attenuation due to water vapor in the paragraph describing the Figure 2 (L150-153 in the original manuscript). We will also add an explanation how water vapor influences attenuation due to oxygen. Finally, the reference to ITU-R recommendations (ITU-R, 2016) will be updated to ITU-R (2019).

3. *I find the narrative from 3.3 onwards (including Sect4) very difficult to follow. I would recommend to reshu so that for instance when you talk about Sensitivity of the k-R model to drop size distribution you cover the whole thing (including line 400-425). Same for the other bits (e.g. Quality check, Dry-wet weather classification, Baseline identification, Wet antenna attenuation). At the moment the reader need to jump back and forward because the logical thread is erratic. Also some topic (e.g. gas attenuation should come before rainfall retrieval because of course the effect of gas must be subtracted first!).*

   We thank for the suggestions on modifying the structure of the manuscript. First part of the comment suggests presenting jointly methods and results belonging to the one subtopic. However, at this moment, we prefer to avoid the inclusion of results in the Materials and Methods section. Moreover, the research performed is multifocal, the use of subheadings within the Materials and Methods section, that are mirror-imaged in the Results section, is intended to make it easier for readers of the manuscript to associate which Materials and Methods are used to obtain which results. Furthermore, all the subtopics (e.g. Quality check, Dry-wet weather classification, Baseline identification, Wet antenna attenuation) are part of the same processing chain and influence each other and thus it is in our view reasonable to present them together.

   Regarding the second suggestion (re-order rainfall retrieval and gas attenuation), we agree that effect of gaseous attenuation influences baseline identification and can be presented earlier. We will modify the structure of the Methods, Results, and Discussion section accordingly.

4. *Eq.10 and Fig.4b. Where is this coming from? I have never seen such a relationship between an intensive quantity like RR and* D_M!!!!!

   The parameters of theoretical functions describing drop size distribution (DSD) are scaled to rainfall intensity, as can be seen in Figure 4a. In this particular case, gamma distribution as proposed by Ulbrich (1983) is used. Thus, mass-weighted

diameters (ratio between 4th and 3th DSD moments) need to be also scaled to rainfall intensity, as shown in Figure 4b. The scaling procedure is explained in the paragraph above the eq. 10 (lines 224-226 of the original manuscript) and adequately referenced to the benchmark paper of Ulbrich (1983).

5. *Table 3: you are introducing parameters (epsilon, delta,) that are not defined anywhere.*

   The parameters are taken from Table 2 in Ulbrich (1983). The paper was cited in the Table caption of the original manuscript. We will add details on each parameter to the Table:

   $N_0(m^{-3}cm^{-1-\mu})$ and μ (-) are parameters of semi-empirical gamma distribution function from Ulbrich (1983). $\epsilon(h^{-\delta})$ and δ (-) are scaling parameters of this function as proposed by Ulbrich (1983).

6. *I do not understand the rationale of doing the investigation with the "theoretical DSD" (not clear where they come from). On the other hand I see the point of using disdrometer data but I would recommend to use extensive datasets like available at https://ghrc.nsstc.nasa.gov/home/field-campaigns (and plot density functions instead of plotting scatterplots as in Fig.10). This should also allow to assess uncertainty errors due to DSD variability like done in Tab.4 in a more robust way.*

   Theoretical DSD enables us to show, that results based on observed DSD are consistent with results using widely acknowledged theoretical DSD functions, which in our opinion supports the conclusions and shows that the results are not only site-specific. When describing theoretical DSD, we use gamma distribution function scaled to rainfall intensity as suggested by Ulbrich (1983).

   In our opinion, one year of DSD data from the well-controlled experiment is sufficient to demonstrate the effect of DSD on attenuation-rainfall power-law relation. The dataset was previously used in several papers in high-quality journals (e.g., Schleiss et al., 2013; Wang et al., 2012) and we had the dataset handy. Moreover, the dataset is from temperate climate and thus applicable also for our case study.

7. *The authors mention stratiform vs convective precipitation coefficients. How do they practically envisage to separate stratiform vs convective precipitation?*

   The separation to convective and stratiform rainfalls is performed only in the investigation with theoretical and observed DSD (sections 3.3 and 4.4 of the original manuscript) to demonstrate that attenuation-rainfall relation is sensitive to rainfall type. The classification procedure is described in section 3.3 of the original manuscript.

   The separation of convective and stratiform rainfalls in practical applications is discussed in Discussion section of the original manuscript (L545-548). This section refers to the work of Leth et al. (2019). Unfortunately, detailed elaboration on possible methods for separation of convective and stratiform rainfalls is out of the scope of this manuscript.

8. *Assuming that "rainfall has a uniform distribution over the study area, and that water formation on the surface of antenna radomes is the same for both the short CMLs and the long one" is quite an assumption! This approach is very provisional.*

   We agree that assumptions seem to be strong. Nevertheless, we believe that its appropriateness is, in our case, justified and was carefully discussed in Discussion section of the original manuscript (L511-518). Line of reasoning is briefly repeated below.

   The rainfall spatial variability (in the studied period) is low: The correlation coefficient between 15-min rainfall observations of nearby rain gauges (rg_1 and rg_2) is 0.94–0.96. The correlation between these rain gauges and the more distant rain gauge rg_3 is still over 0.88.

   Wet antenna attenuation (WAA): All the CML units are of the same type and have similar ages (deployed during 2016 and 2017). Furthermore, as discussed on L516-518 of the original manuscript, the procedure is in our case relatively insensitive to differences in antenna properties, because WAA is quantified by comparing short (0.4 – 1.4 km) CMLs by which WAA dominates over path attenuation, to the 4.86 km long CML, which is relatively insensitive to WAA even during light rainfalls. Satisfactory performance of the approach is also demonstrated in the Results section in Figure 7.

   To further strengthen the arguments about the legitimacy of those assumptions, we will add information on the age of the units into section 3.1 Experimental sites and instrumentation.

9. *E-band CMLs are by about one order of magnitude more attenuated by raindrops along their path than older 15–40 GHz devices": this is a very vague (imprecise) statement also given the fact that attenuation at 40 GHz is already 6 times attenuation at 15 GHz!!!! Same for the sentence "Gaseous attenuation at E-band CMLs is detectable, however, it is two orders of magnitude smaller than attenuation due to rainfall" (again quite vague and approximate!)*

   Agreed. We will be more precise in our statements:

   - "E-band CMLs are markedly more attenuated by raindrops along their path than older 15–40 GHz devices, during lighter rainfalls by about 20 times more than 15 GHz and 2 - 3 times more than 40 GHz devices."
   - "Gaseous attenuation at E-band CMLs is detectable, however, it is substantially smaller than attenuation due to rainfall. Fluctuations in specific attenuation caused by water vapor typically not exceed 1 dB km^{-1} in the region of temperate climate. This magnitude is reached by rainfall with intensity around 1 mm h^{-1}."

Additional references

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