

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

Anonymous Referee #1

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The paper investigates challenges and opportunities of the E-band microwave links to derive rainfall. The study mirrors previous studies conducted at lower frequency, it is potentially of great interest but at the moment the paper presents some major issues. 1) Fig.1: in the caption “scattering efficiency” is mentioned. Clearly this is not the same as extinction efficiency. Please clarify.

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???

3) I find the narrative from 3.3 onwards (including Sect4) very difficult to follow. I would recommend to reshuffle so that for instance when you talk about Sensitivity of the k-

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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 needs to jump back and forward because the logical thread is erratic. Also some topics (e.g. gas attenuation should come before rainfall retrieval because of course the effect of gas must be subtracted first!).

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!!!!

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

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.

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

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.

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

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quite vague and approximate!)

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