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Hydrology and Earth System Sciences Discussions

Interactive comment on "Precipitation observation using microwave backhaul links in the alpine and pre-alpine region of Southern Germany" *by* C. Chwala et al.

Anonymous Referee #1

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Evaluation

This manuscript presents the analysis of the rain rate estimated from operational telecommunication microwave link in Germany. Received Signal Levels (RSL) from 5 links as well as measurements from 6 rain gauges and 1 weather radar are used in the present study. First, a new approach (based on the Fast Fourier Transform - FFT) to estimate dry and rainy occurrences from link measurements is proposed. Then the collected link data are processed (estimation of the attenuation baseline, conversion of attenuation into rain rate) and compared to rain gauge and radar observations.

The new spectral approach for the estimation of dry and rainy occurrences is interesting and innovative. It is in my opinion the real contribution of this manuscript, and should hence be the focus of the paper. The possibility to use telecommunication microwave links for rainfall monitoring is not new (and appropriately referenced in the manuscript). The necessary changes and maybe the additional analyses required may be significant (see general and specific comments below). I therefore recommend to send the manuscript back to the authors for major revisions.

General comments

1. As mentioned in the evaluation, I think that the main contribution of this manuscript is the new approach based on FFT to identify dry and rainy periods using link measurements only. The subsequent analyses on the quality of the rain rate estimates should, in my opinion, be refocused to serve the evaluation/validation of the dry/rainy identification method. The current evaluation based on the "final" rain product (the rain rate) has the drawback of mixing all the sources of errors along the processing chain to get the rain rate (e.g., uncertainty in the attenuation baseline, wet-antenna effects, deviations from ITU power law parameters). As mentioned above, the novelty is not in the use of link data to obtain the rain rate, so I would recommend to conduct the same analyses using also some "classical" dry/rainy identification methods (some are listed in Section 6.1). The comparison between the obtained rain rates with rain gauge and radar data will then enable the authors to quantify the improvement of the new proposed identification method with respect to existing approaches.

2. In the current version of the paper, I miss some information/comments about the transferability of the proposed approach to other regions. Is there any specific requirements to be able to run this FFT method? Would it be easy to imple-

ment your “RSL data logger" to other operational networks? These are important aspects concerning the potential of the proposed approach.

3. A maybe less important issue: only the RSL is measured, so the authors implicitly assumed that the transmitted power is constant. From my personal experience, it seems that this is not always true... Could the authors comment on this?

Specific comments

1. P.742, l.22-23: please provide an order of magnitude of this “desired accuracy".
2. P.745, l.1: this sentence may be confusing: if the DSD does not vary in space and time, the rain rate does not vary either...
3. P.745, l.19: is the term “orography" really appropriate here? Maybe topography is more suited (not sure though...).
4. P.746, Section 4.1: nothing is said about the transmitted power (see general comment 3), although it must be supposed constant to derive the attenuation affecting a given link.
5. P.747, Section 4.3: more information about the radar data is necessary: what is the elevation considered? How are filtered the ground clutters (especially in such a mountainous region)? What are the coefficients used for the $Z - R$ power law?
6. P.749, l.18-19: just out of curiosity: is the wet-antenna attenuation always the same in the horizontal and vertical polarizations?
7. P.752, l.8-9: it is not clear to me how these frequency thresholds have been obtained.

8. P.754, l.22-23: a word must be missing, I do not understand this sentence.
9. P.755, l.9-11: in Figure 5, the values of σ are (roughly) between 0 and 1.7. Why using 2.5 on P.753-l.23? In addition, I think it should be more clearly indicated what is necessary to estimate σ (types of data, duration, accuracy,...).
10. Section 7 and Figures 8-9: the correlation coefficient only quantifies the degree of linear co-fluctuation between 2 variables. But 2 variables can be perfectly correlated and deviate one from the other by a large bias (e.g., $y = 2x$). So I would recommend to add a criterion quantifying the possible bias between the rain rate from the different sensors (e.g., ratio of means).