Hydrol. Earth Syst. Sci. Discuss., 9, C300–C303, 2012
www.hydrol-earth-syst-sci-discuss.net/9/C300/2012/

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.

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Received and published: 12 March 2012

Review of "Precipitation observation using microwave backhaul links in the alpine and pre-alpine region of Southern Germany" by C. Chwala, A. Gmeiner, W. Qiu, S. Hipp, D. Nienaber, U. Siart, T. Eibert, M. Pohl, J. Seltmann, J. Fritz, and H. Kunstmann.

General comments

This paper describes a study of rainfall estimation from recorded RSL levels from five commercial microwave links. The emphasis of this paper is on the classification of wet and dry periods, for which the authors propose a new algorithm. The paper is well-written, well-referenced, and shows interesting results. This is the first paper (that I know of) where data loggers were installed by the researchers at the antennas of commercial links, so that the sampling time and the quantization of the logged signal could be controlled. I have some comments, some of which require some additional analyses and explanations. I think that the paper can be published after revisions. Specific comments are given below.

Specific comments

1. When discussing the uncertainties in radar rainfall estimation in general at the bottom of p. 742, you should probably include the effects of the vertical profile of reflectivity (VPR), see e.g. Smith (1986, J. Atmos. Oceanic Technol., 3, 129-141), Joss and Pittini (1991, Meteorol. Atmos. Phys., 47, 61-72), and Hazenberg et al. (2011, Water Resour. Res., 47, W02507).

2. On lines 15-16 of p. 747, it is stated that the RADOLAN $Z - R$ relation is used. Could you give this relation here? And what is the height of the radar measurements above the terrain for the locations of the different links?

3. In Eq. (5) the spectrum is normalized by the mean spectrum occurring in dry weather. It is not clear how this mean dry spectrum ($P_{\text{meandry}}(f)$) is determined. The problem is that you're trying to distinguish between dry and wet periods, and that you somehow use information about which periods are dry in the process.

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Interactive Comment

Please elaborate on this, and explain if additional information about wet/dry periods is needed for this method. If the spectra in dry weather show a $1/f$ behavior, simply multiplying the spectra by f would also solve this problem.

4. An important parameter of the wet/dry classification method is the frequency at which the slow and fast signal variations are separated. This parameter (which corresponds to f_{low2} and f_{high1}, which are close together) is not fitted but chosen based on visual inspection of frequency spectra. I think that because this is such an important parameter, it would be a good idea to optimize it in a manner similar to σ.

5. The same holds for the parameter L. This parameter is currently also chosen based on visual inspection of results, but would also be a good candidate for optimization. If it should be a power of 2, then the parameter $\log(L)/\log(2)$ could be optimized.

6. I had expected a discussion on the space-time structure of rainfall (which may very well be influenced by topography) and its relation to link length and orientation and the employed frequency thresholds. The longer the links, the more averaging occurs, and the longer the typical timescales. This could influence the choice of $f_{\text{low1,2}}$ and $f_{\text{high1,2}}$, which now correspond to timescales of approximately 4 hours (256 minutes), 4 min., 4 min., and 2 min., respectively. I think the authors should devote some attention to this. For example, how well does the assumption hold that all rainfall events have time scales greater than approximately 4 minutes?

7. The order in which things are presented could be improved. For example the example presented in Section 6.3 should probably be presented after Section 6.4, so that the reader does not have to guess on what the value of $\sigma = 2.5$ is based. The choice of the window length could also be moved to the bottom of p. 750, where is was introduced.

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8. If I consider Eq. (9), then I would conclude that for any given $P_{\text{sumdiff}}(t)$, the higher σ, the higher the number of dry hours and the lower the number of wet hours. In other words, the number of wet hours is a monotonically decreasing function of σ and the number of dry hours is a monotonically increasing function of σ. This also means that $N_{\text{link\&gauge=wet}}$ (Eq. (12)) is a monotonically decreasing function of σ and $N_{\text{link\&gauge=dry}}$ (Eq. (13)) is a monotonically increasing function of σ. This in turn means that ε_{wet} (Eq. (12); wet detection error rate) should be a monotonically increasing function of σ and that ε_{dry} (Eq. (13); dry detection error rate) should be a monotonically decreasing function of σ. However, looking at Fig. 5, it can be seen that this is not always the case (ε_{wet} sometimes decreases with σ and ε_{dry} sometimes increases with σ). The reason for this should be explained clearly in the paper.

9. It is shown in Fig. 9 that the comparison is between link and radar is better than that between link and gauge. It would be interesting to see how the values of σ would change if radar data would be used to compute wet and dry errors for those links where radar data are available (hop2-murn1 and hop2-wh0). This may also shed some light on the different behavior of the wet detection error rate of the hop2-murn1 link discussed on lines 15-24 of p. 755.

10. Because the focus of the paper is on wet/dry classification, I think more attention should be devoted to the discussion of the behavior of the wet and dry detection error rates, as these reflect the quality if the algorithm. This discussion should also include radar-based error rates (see also my previous comment).

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 9, 741, 2012.