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Atmospheric Measurement Techniques Discussions

Interactive comment on "Using Markov switching models to infer dry and rainy periods from telecommunication microwave link signals" *by* Z. Wang et al.

Anonymous Referee #1

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General comments

The paper presents relevant information and procedures on how to convert a microwave/mm-wave radio link into a rain rate measurement instrument. I found it very interesting the way in which the authors have been able to increase the rate the attenuation can be read and recorded. The references provide a very good overview of related works. The application of the instrument is put in perspective as something in between point measurements and large volume measurements.

The authors clearly point out the problems involved in the measurement, namely the lack of a stable baseline for identifying the dry from wet periods. The identification of

this base line is the objective of the paper where a State switching model is proposed where different behaviors are to be expected.

Specific comments

I have several remarks on the material presented.

The key issues are discussed in Section 2.2. I think that the Gaussian distribution is not a well-accepted one for rain rate; it is rather the log-normal distribution that is normally accepted, at least for the tail of the distribution.

Also, in the methodology in this section, the assumption is made that the samples used are uncorrelated. This is not the case since the variations during the dry periods are fairly slow, with a period of one day. As for the rainy samples, uncorrelated rain attenuation samples every 4 s are unlikely, in addition to the superposed temperature drifts.

In the same section, I would take some time to present and define the elements in equation (7).

Still in Section 2.2, the authors claim that the other techniques for identify the baseline need to set an empirically derived threshold. However, in Equation (8), a threshold of 1/2 is arbitrarily chosen.

In Section 2.3, the assumption of independence for a multivariate approach, it looks a-priory to be hardly fulfilled. I suspect that all channels are similarly affected by the same thermal drift.

With respect to Figures 2 and 3, what are the numbers in the ordinates? They seem to be very far from the free space loss value plus some small rain-induced loss.

Could the authors quantify the wet antenna effect? I have the impression that the range of attenuation values measured in this very short link (Figures 2 and 3), i.e., not much more than 2 dB, is almost comparable with the wet antenna loss.

In section 4 there is a discussion on errors of type I and II and their rates which seem to be fairly high, especially in the non-stationary case. I wonder, what is their impact in practical application (meteorology, flood control, etc. for example? The statistics of rain, will they be very much off?

Equation (11) is missing some constants in front of the second and third terms on the right-hand side. I believe it is “rho” and then “sqrt(1-rhoˆ2)”.

After equation (11), there is a mention of using transition probabilities, p00, p11 (and p01 , p10). In my experience with a first order Markov model it is not possible to actually reproduce all possible durations of events, wet and dry, short and long.

Technical comments

Suggested text corrections:

P. 414, L. 14: "tend" should be "tends" P. 415, L. 11: add "the" before "dry" P. 417, L. 10: replace "as" with "to" P. 417, L. 26: replace "two" by "to" P. 428. L. 10: replace “than” by “to” P. 248 L. 23: remove “.” after “data”

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 411, 2012.