Atmos. Meas. Tech. Discuss., 6, C800–C803, 2013
www.atmos-meas-tech-discuss.net/6/C800/2013/

Atmospheric
Measurement
Techniques
Discussions

Interactive comment on "Rainfall measurement from opportunistic use of earth-space link in Ku Band" *by* L. Barthès and C. Mallet

Anonymous Referee #2

Received and published: 7 May 2013

Summary and Recommendation: This paper describes how geosynchronous telecommunication satellites can be used to estimate path-averaged rain rates, using a cheap microwave receiver and some signal processing tools. The basic idea is not entirely new but the proposed approach is sufficiently interesting and the results promising enough to be published in AMT. The theoretical formulation of the problem looks safe: it is based on well-established results published by the ITU and builds on several ideas that have been successfully applied to terrestrial telecommunication microwave links. The structure is sufficiently clear and the English is fair (but could be improved). I recommend to accept this paper for publication, provided that the authors address the following list of (minor) issues:

General comments 1: In my view, too much emphasis is put on the (low) cost of the

microwave receiver. The authors do not provide specific numbers but I think it is fair to assume that the real cost (including maintenance and manpower) of such a system is higher than the one of state-of-the art rain gauge networks. Also, if you think about it, the microwave receivers are not necessarily easier to deploy than rain gauges, especially in remote locations and mountainous terrain. The Earth-space links however provide interesting and complementary information about precipitation at the path scale, from the ground surface up to the troposphere (and beyond) that are very valuable for the remote sensing community, especially for people working with ground-based or spaceborne radars. The price is an important argument but it should not be the major motivation in this study. The abstract and the Introduction should be revised in order to clarify this point.

General comment 2: In Section 2.2, the authors make an interesting point by mentioning that the 12 GHz frequency is not optimal and does not allow to accurately retrieve small rain rates. They suggest to focus on higher rain rates instead, which makes more sense from the theoretical point of view but also raises a lot of questions. Indeed, strong rain events are known to exhibit complex spatial and temporal structures (including intermittency, winds, strong temperature gradients and complex vertical evolution of the DSD) that are not accounted for in the retrieval method proposed in Sections$\sim$2 and 4. I think this issue merits some further discussion in Section$\sim$2.2 or in Section$\sim$6.

General comment 3: The authors do not address the problem of mixed precipitation (e.g., hail or wet snow) nor do they develop on the importance of the melting layer in their application. I understand that this might be beyond the scope of the paper but why not mentioning it somewhere in the conclusions or the description of the method?

General comment 4: In Section 4.2, one can point out that the accuracy of the retrieved rain rates strongly depends on the estimated geometric path length L and rain height hR. The rain height obviously depends on a lot of factors, such as the type of precipitation and the local atmospheric conditions. Yet there are very few details about how it is determined in practice. The authors could provide more details about this, rather than

just citing an ITU report.

Specific comment 1: The authors could mention the studies of Kharadly \& Ross (Effect of Wet Antenna Attenuation on Propagation Data Statistics, IEEE TAP, 2001), Crane (Analysis of the Effects of Water on the ACTS Propagation Terminal Antenna, IEEE TAP, 2002), Overeem et al. (Country-wide rainfall maps from cellular communication networks, PNAS, 2013) and Schleiss et al. (Quantification and modeling of wet-antenna attenuation for commercial microwave links, IEEE GRSL 2013).

Specific comment 2: Figure 5 clearly shows that there is a daily cycle in the baseline of the path-integrated attenuation. So why is the baseline interpolated linearly during the rainy periods?

Specific comment 3: The authors should cite the upcoming GPM (Global Precipitation Measurement Mission) by NASA, which will provide remotely sensed precipitation estimates that are not confined within the inter tropical regions (like TRMM).

Specific comment 4: The authors should add an extra term for the wet-antenna attenuation in Equation (5). I know that this term is neglected in the proposed study but it nevertheless contributes to the total attenuation and should appear somewhere.

Specific comment 5: Is there a rough estimate of the uncertainty affecting the emitted power pE in Equation (5)? The transmitted power should be constant in theory but I know that this is hardly ever the case.

Specific comment 6: path-average? path-averaged? path averaged? path-integrated? Please choose one and be more consistent.

Specific comment 7: polarisation vs polarization? polarised vs polarized? Please use the English (and not the American) spelling for AMT papers. Same for kilometre vs kilometer and meter vs metre.

Specific comment 8: Please do not use the expression "drought period" to denote a perfectly normal period without rain. A drought is an abnormally long dry period. Just

write "dry period" or "dry spell".

Specific comment 9: Please do not use the notation x to denote multiplication (e.g., Equations 1-2-3). Use a simple dot or a space.

Specific comment 10: The official abbreviation of minute is min and not mn.

Interactive comment on Atmos. Meas. Tech. Discuss., 6, 2113, 2013.