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Atmospheric Measurement Techniques Discussions

Interactive comment on "Retrieval algorithm for rainfall mapping from microwave links in a cellular communication network" *by* A. Overeem et al.

Anonymous Referee #2

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I was starting to write my review when had the opportunity read that by the anonymous reviewer #1. And actually I agree almost completely with him/her. Therefore, also in my opinion the paper should be rejected, the reasons being those well detailed by reviewer #1 and also some other ones that I discuss below. The only matter of disagreement between reviewer #1 and me stands in the utility of the exploitation of the MWLs signals offered by cellular networks. If considered as opportunity sensors for the remote sensing of precipitation, in my opinion they could be very useful in 1) areas not covered by or very far from weather radars, but densely covered by cellular MWLs, as could be the case of urban conglomerations; 2) areas screened by hills or mountains and therefore not reached (or partially reached) by the antenna beam of a weather radar, as could be the case of little catchments: there, even a limited but continuous information as

that provided by few MWLs could bring important integrative information for alerts, for instance. In fact, it should not be forgotten that a great advantage of cellular MWLs is the continuous signal reception and the consequent very high temporal resolution. This is certainly paid for with a degraded quality of the information due to the low quality of data, but such pieces of information would be almost for free. Certainly, telephone companies are not interested at all in any question of remote sensing (and are not stimulated by governments), but this does not concern the scientific potential of the use of MLWs signals and the related technical issues and algorithms.

Coming to the problems of the paper: its declared objective is to provide the scientific community with a detailed description of a code, that is a "slightly" adapted (adapted to what and why? This is not explained) of their own code used for another publication available on line. However, the paper is much longer than necessary, badly structured and the conclusions section - that is definitely too long and dispersive - includes critical discussions that should have been introduced before in more detail (I refer in particular to that of the non-linearity of the R-k relations). Many issues treated in the paper definitely need to be synthesized, avoiding repetitions and putting them in the framework of a more organized presentation that also requires much more work to be done, as pointed out by reviewer #1 and by my observations below. The authors should focus on a more reasoned description of the algorithm - that should be improved with a significant effort to make it independent of the local situation in the Netherlands - and leave to the references the description of the several problems that can be encountered by using this kind of opportunistic remote sensing networks. In this respect, in addition to (or even instead of) mentioning the too short and somehow unclear 2006 paper by Messer et al. , the authors could refer to H. Messer and O. Semdyk "A New Approach to Precipitation Monitoring: A critical survey of existing technologies and challenges" IEEE Signal Processing Magazine (Volume:32 , Issue: 3) May 2015, that is much more sound from a technical point of view. Still from a reference point of view, in the introduction the authors miss to mention papers that propose a tomographic approach to the same problem:

Cuccoli, F., Facheris, L. and Gori, S.: “Radio base network and tomographic processing for real time estimation of the rainfall rate fields”; Proc. IEEE Geoscience and Remote Sensing Symposium (IGARSS), July 2009, Vol. 3, III-121 - III-124 Cuccoli F., Baldini L., Facheris L., Gori S., Gorgucci E.; “Tomography applied to radiobase network for real time estimation of the rainfall rate fields”; Atmos. Res., vol 119, 62-69 doi:10.1016/j.atmosres.2011.06.024 Cuccoli F., Facheris L., Gori S., Baldini L.: “Retrieving rainfall fields through tomographic processing applied to radio base network signals”; Proc. SPIE Remote Sensing Symposium, Prague, Sept. 2011, Vol. 8174, 81740C -1, 81740C-13.

But I would like to focus on a major point that reviewer #1 has not mentioned: the frequencies used by the NL cellular network and the value of b. The authors write “it is important that the value of the exponent b in Fig. 5 (right) is close to 1, which is the case for a range of frequencies. Here, only links with microwave frequency from 12.5-40.5 GHz are selected. The chosen frequencies can be altered in the script” (incidentally, 40 GHz falls already in the millimetre waves range, so 38.9 GHz is not a “representative microwave frequency” – see page 12). Then, in the conclusions: “the value of the exponent b is close to 1 for the frequencies employed in this study which range from 13-40 GHz. Frequencies between 37 and 40 GHz are denoted by the gray-shaded area in Fig. 5, which contains 81% of the links from the working example. . .”. But (page 14) they also state: “for the link frequencies used in this study (between 13 and 40 GHz) the value of the exponent b is close to 1. Because of this near-linearity of the integrand in eq. (5), the assumption on the distribution, leading to limited errors caused by using the approximation in eq. (6). This was also shown by . . .” I was rather disturbed by this combination of sentences, and have three relevant comments/questions: a) going from 13 to 40 GHz (or from 12.5 to 40.5?!! Please be precise) means moving across three entire bands (the Ku, the K and the Ka band), moving from 2.3 cm down to 7.5 mm wavelength, with significant variations not only of the attenuation caused by rainfall bus also of that due to water vapour and liquid water. But above all: how many frequencies are used by the Dutch telephone companies?

I am pretty sure that the authors are using data from service channels, as can be deduced from Fig. 1, but how many service channels have been activated in the NL? And why do they span such a wide frequency band? The authors do not spend a word on all this while it should be precisely clarified. Apart from that, the statement “Rainfall attenuates the electromagnetic signals transmitted from the circular antenna of one telephone tower to another” and the bubble in Fig. 1 with the notice “estimate rainfall with circular antennas” are simply ridiculous. Rainfall attenuates e.m. signals transmitted from any kind of antenna, and ’circular’ antennas do not exist (choose the right one among parabolic, horn or whatever. . .). b) The exponent b is far from being close to 1 at 13 GHz (also at 40 GHz, anyhow. . .), and consequently the integrand in eq (5) is far from being “near-linear” as pretended in the paper! It is surprising that two of the authors put in evidence this problem in a paper of theirs that is even listed in the references (Lejinse, Uijlenhoet, Stricker: “Microwave link rainfall estimation: effects of link length and frequency, temporal sampling, power resolution, and wet antenna attenuation”, Advances in water resources 31 1881-1493, 2008) and draw opposite conclusions (section 5.1). Since we have perfect linearity in the R-k relation ONLY around 34 GHz, in the proposed approach the use of frequencies far from 34 GHz stands out as a prominent problem as it cannot provide a non-biased average value of R along the MWL. The aforementioned tomographic approach overcomes that, aiming at retrieving the specific attenuation field k(x,y) based on the average k measured along different MWL operating at the same frequency. The rainfall field can then be estimated through the k-R relation that holds at that frequency. c) In the 13-40 GHz band, not only the R-k relations change remarkably as evidenced by Fig. 5, but in general all parameters related to propagation and scattering do. For instance, a MWL operating at 21, 30 or 40 GHz is evidently much more sensitive to rainfall and humidity than one operating at 13 GHz. Therefore, all thresholds (be they empirical or not) used in the algorithm are likely to need an adjustment depending (at least!) on frequency. Not only this is not evidenced at all in the paper, but it even seems that the authors use the same thresholds at all frequencies, which would be absolutely odd and unrealistic.

d) Besides and beyond the issue evidenced in point c): in the conclusions the authors "hope to promote the application of rainfall monitoring using microwave links. . .around the world", but they provide an algorithm with operative thresholds that are strictly dependent on the Dutch climatic situation, without any discussion/suggestion on how such thresholds could be adapted to different conditions. As long as such an effort is not done, one shall easily object that the Netherlands is a relatively small country, abundantly well covered by weather radars and sufficiently flat to remove any important cause of inaccuracy not only in the estimate of precipitation, but also in its classification and tracking. So what is the need there for estimating rainfall through MWLs?

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 8191, 2015.