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Interactive comment on "Retrieval of rainfall fields in urban areas using attenuation measurements from mobile phone networks: a modeling feasibility study" *by* Bahtiyor Zohidov et al.

Anonymous Referee #2

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The paper "Retrieval of rainfall fields in urban areas using attenuation measurements from mobile phone networks: a modelling feasibility study" by Zohidov et al. presents a method which which the feasibility of recostructing spatial rainfall from MWL is assessed. The methodology is as far as I can judge technically correct and it in a comprehensive way allows the understanding of various effects, like MWL link number, spatial rain properties, and storm type, on estimaion quality. The paper is intended for an informed reader and clearly addresses this audience. I think it is suitable for the Special Issue and brings forth a comprehensive analysis methodology that will be of interest to a broad readership. I recommned the publication of this article with some suggestions for improvement.

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Discussion paper

(1) I am aware that there are a number of other studies addressing a similar goal, some of which I have not see referenced and/or they are glossed over in the introduction. It would be very helpful for the reader

(2) The MWL network model is clearly explained in Section 2.1, and as I understand it the parameters a and b for all links (or different GHz bands) are hidden in the model m in equation (3). The identification algorithm described in Section 2.2 then estimates the model parameters in m. The reference in line 24 (page 4) should then be to Equation 3 not 4. Irrespective of the details of the identification algorithm, it would be useful to read what is the outcome of its application, the model m, i.e. parameters a and b. Is this correct?

(3) The iterative application by grid nesting described in Section 3.3 is appealing. I am wondering how does this application will behave in the limit, i.e. in the hypothetical case of very high resolution rainfall data available. Does this depend on the nature of the model m?

(4) The authors define a “pixel density”in Section 4.2 of the MWL network. I find the description of this variable hard to understand. If I am not mistaken Wi is a fraction of the total number of MWLs (i.e. m=256) that is present in a single pixel i, so to me it is a MWL density, not a pixel density. I am starting from the fact that by looking at your equation (11) if you sum up Wi over all pixels i that are covered by a part of the MWL network, then you arrive at 256. This is confirmed by the number of pixels in Fig 4 which is 183 (roughly). Then the meaning of a single pixel having a value W>1 means that it is a cell which contains more than 1 average MWL length. If what I write is correct, I find this a more appealing way to describe why you are choosing certain values of W to define low, moderate and high densities, than the description of medians, etc., in Section 4.2.

(5) From the radar data, ultimately 7 storms were selected for the analysis, in four storm types. Path integrated attenuation was then computed from the rain maps with

literature values for a and b parameters and this attenuation was imagined to be what would have been measured by the MWL. I have to admit it was only at this point in the paper that I realized the authors are not actually using measured attenuation in MWL by telecommunication companies, but computing these from radar. Maybe this fact should be mentioned more prominently earlier (or I have missed it).

(6) In Fig 6 the estimated attenuation shows a strong link frequency effect. This of course is known and therefore expected. Other studies have presented this result. The authors mention on page 10 that they do not account for this effect in the measurement error. I think this needs to be elaborated more, i.e. what is the significance of this fact on the results presented?

(7) Sensitivity to the a priori parameters is presented in Section 5.2. The corresponding results Fig 8 is to me confusing that the y axis does not say this is nL. It is evident that a local neighbourhood is better as well as it seems a low decorrelation distance. I can understand how nL would be related to storm type, but decorrelation distance? Also the absolute magnitude of the best performance measure itself (NS) for the different storm types appears to be very different, and especially poor for the unorganized storm. I do not follow in the description where the reader is supposed to see the effect of rainfall variability. This could be better explained.

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