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Hydrology and Earth System Sciences
Discussions

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 #1

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General comments

The manuscript presents an application of algorithm for reconstructing rainfall intensity fields from rain-induced attenuation of commercial microwave links (CMLs). The algorithm is based on inverse modelling of rainfall-induced attenuation. It is tested on CML network topology of city of Nantes (FR) using numerical experiment where CML attenuation is simulated based on weather radar images. The weather radar rainfall data serve also as reference to which CML constructed rainfall is compared. Although the topic of CML rainfall reconstruction at scales required for urban hydrology is relevant, the manuscript has several shortcomings:

1. The authors do not sufficiently differentiate between their original contribution and methods from other studies. Most importantly, the presented inverse algorithm is adopted from Vignal et al., (2003) where attenuation fields were generated from weather radar data in order to correct for bias caused by this attenuation. The authors, however, do not give credit to this work and refer only to more general inverse modelling work of Tarantola and Valette, (1982). It is not clear, if (and to which extend) the algorithm of Vignal et al., (2003) is modified or if this manuscript presents only new application of the algorithm.

2. One of the major conclusions reached in this study is that it is feasible to use proposed algorithm for rainfall mapping in urban areas. It is, however, not clear, how this "feasibility" is defined, especially what the advancement is in respect to other CML rainfall algorithms reported in the literature. Although authors mention these algorithms in the introduction section they do not discuss their pros and cons, and do not indicate how the presented approach enhance state-of-the-art in the CML rainfall mapping (i.e. where this contribution fills the gap). The results also do not indicate if (or in which cases) the proposed algorithm outperforms the other algorithms, although numerical experiments used in this study represent a suitable playground for comparing different approaches.

3. The authors build on several assumptions (esp. concerning CML errors) when presenting the numerical experiment with virtual rainfall fields. These assumptions are, however, often insufficiently justified (for details see specific comments). Moreover, their influence on results is not discussed.

4. Finally, the presentation of the scientific results is at many places confusing. First, authors are often not specific (and precise) when referring to the literature (see e.g. specific comment no. 1, 3, 9, 20, 38 and few others). Second, methods and results are often mixed, which makes it difficult to reconstruct procedures authors used to reach the results. Finally, inappropriate use of English language at some

places together with inconsistent formatting (magnitudes or units at some places in italic and at other not) makes it difficult to follow the manuscript message. Authors may consider proofreading the manuscript by a native speaker.

Specific comments

1. P2L15: It is stated here, that Ostrometzky et al., (2015) proposed method for estimating all types of precipitation. Please, be more specific, which types of precipitation.

2. P2L16: Please, be more specific and indicate what is "larger dataset" (weeks, months, or years of data?) and "greater number" of CMLs (tens, hundreds, thousands?).

3. P2L17: It is not clear to what "initial findings" is referred to when referring to Overeem et al., (2011) and Rayitsfeld et al., (2012).

4. P2L18: Please, consider changing the phrasing "These promising results must not neglect. . ." as it indicates that works which the authors refer to in the previous paragraphs neglect uncertainties, which is not correct.

5. P2L25: Rahmi et al. (2006) did not use "telecommunication link" for radar correction but specially deployed experimental microwave link.

6. P2L26-P3L12: Please indicate pros and cons of reconstruction techniques reviewed in this paragraph.

7. P3L15-17: It is stated here, that there is not much experience in using CML reconstruction technique in cities well equipped with CMLs. Which studies you have in mind when you refer to "Small number of studies"? Those reviewed in the previous paragraph? And what is exactly specific on reconstructing rainfall in cities well equipped with CMLs? Why not use above reviewed methods?

8. P3L20: When referring to radar images, please, be specific with temporal resolution

(probably five minutes?). Also consider rephrasing the sentence as rainfall data with temporal resolution five minutes are at least in the context of urban rainfall monitoring (and also in the context of capabilities of nowadays local weather radars) not "very high-resolution rainfall data".

9. P3L32-P4L1: It is stated here, that the algorithm rely on works of Tarantola and Valete (1982). It is not clear, if you modify the inverse modelling approach of Tarantola and Valete (1982) or take it as is and apply and test in the new context. Please, be more specific and indicate if you modify it and if yes, how you modify it. Nevertheless, wouldn't be appropriate to refer here to Vignal et al., (2003)?

10. P4L11-12: Does k(j, l) denotes pixel or a function associating pixels with CMLs?

11. P4L17: The vector A is the product of adding of vector m(R) and vector ε, thus all three vectors has to have same length. Does it mean that N and M are the same numbers? What if one pixel is crossed by more links? Please, clarify and describe also more in detail what stands for model m (eq. 2?). Furthermore, please use lowercase for notation of vectors.

12. P4L22: In the following sections different aspects related to reconstruction algorithm are presented. It is, however, not easy to follow the message of the text as some overall description of the algorithm is not provided: e.g. what are the inputs, outputs, parameters, main steps of the algorithm (diagram could be useful), main features, etc.

13. P5L3: Please describe also A and R. These are truth values of attenuation resp. rainfall along the CML path?

14. P5L5: How is the assumption on Gaussian and unbiased residuals justified? This is a strong assumption, please explain.

15. P5L15: Could you indicate how an *a priori* information is determined?

16. P5L23: Could you indicate what is meant with "very high quality data with sufficient quality"? Is it possible to estimate in advance if the problem will be determined (e.g.

from the CML topology)?

17. P5L24: What is meant with “special case”?

18. P5L28: Please rephrase the sentence to make clear what is meant by “. . .resolution adopted to represent a rainfall field complicates to some extent problem resolution”.

19. P6L7: The “HF” abbreviation has not been defined in previous text and, therefore, it is not clear what it stands for.

20. P6L7-11: The works which you refer to study variety of CML error sources. Zinevich et al. (2010) relate the CML errors with their characteristics such as length, frequency or polarization. Leijnse et al. (2010) study effect of drop size distribution and wind speed on uncertainty in CML rainfall estimates. Could you please specify which errors are considered in this case and justify using same alpha parameter for CMLs of different characteristics and also justify the approach when error is determine as proportional to attenuation, i.e. attenuation is multiplied by the alpha parameter? Furthermore, some of the CML uncertainties are not proportional to the attenuation rate (e.g. quantization noise). Could you therefore justify multiplication of attenuation and alpha when estimating standard deviation in eq. 7?

21. P6L21-22, P7L5 and P7L9-10: As shown in table 1, the CML lengths are between few hundred meters and almost 17 km. Therefore, length of CMLs will have great influence on the range used for initialization of *a priori* rainfall. Most probably much larger than parameter n_l.

22. P7L1: Please explain why the eq. (10) is appropriate functional relation for estimating *a priori* covariance matrix CR.

23. P8L5-8: Please consider moving description of table 1 to its caption and adding into table also 95% quantiles discussed in the text. My personal feeling is also that median CML length would be more informative than mean length.

24. P8L13-14: The influence of CML topology on performance of rainfall reconstruction

is very interesting topic which is still not well understood. This includes not only density of network, but also CML lengths, positioning, etc. Focusing more in detail on this aspect might substantially improve scientific significance of the manuscript.

25. P8L15: The aim of described analysis is to classify regions according to density of CML network. Therefore, I find term “pixel density map” confusing, rather referring to arbitrary chosen resolution of the map (e.g. 1 x 1 km^2) than to the CML density. Please consider changing the term to something like “CML density map” or “network density map”.

26. P8L15: It is not clear, what it is meant with “validating network system performance”.

27. P8L21: Please, be more specific in the description of eq. 11. m is the number of links intersecting the pixel I and L is the CML length?

28. P8L25-26: Are the values of W (eq. 11) used to classify study area into “low”, “moderate”, and “high” CML density regions all arbitrary?

29. P8L27: This is unclear to me. How is the 30% threshold related to the density level?

30. P8L29-30: This information might be rather in the legend of the Figure 4.

31. P9L7: The figure 5 does not show each considered event (in total 207 images). Change “for each considered event” to “for four considered event types”.

32. P9L8: In total 34 events are selected for the study. According to which criterion these events were selected? There were probably hundreds of events occurred in the Nantes region in this four year period. Moreover, you state at the end of the paragraph (P9L15) that you use at the end 7 events for the study. Please, explain.

33. P9L18: Please, consider restructuring the chapter. I recommend describing properly how PIA is derived and move results and their discussion into different section(s).

34. P9L24: Computed PIA is quantized to 0.1 dB resolution. How is the quantization 0.1 dB justified? To my experience the quantization of modern CMLs is rather 0.3 dB or even 1 dB. Higher quantization will greatly impact especially short CMLs and CMLs of low frequencies (18 GHz) which are in general relatively insensitive to raindrop path attenuation.

35. P9L27-30: It is stated here that quantifying error sources with precision is out of the scope of this manuscript. Realistic estimation of these errors is, however, crucial when assessing feasibility of CML rainfall reconstruction in numerical experiment with virtual CML measurements. The magnitude of CML errors is influenced by CML characteristics, especially length and frequency. E.g. short CMLs which are potentially most informative when reconstructing rainfall at high spatial resolution have, in general, largest errors (mainly due to additional attenuation caused by antenna wetting). This will definitively strongly influence performance of the algorithm when used on real dataset. The influence of errors (and sensitivity of results on neglecting errors) should be therefore carefully discussed in the manuscript.

36. P9L30: The errors in PIA are considered to be Gaussian distributed with magnitude 5% of the PIA. This is, however, unrealistically low (see e.g. Fenicia et al., 2012). Also the Gaussian distribution is a strong assumption which should be discussed and justified.

37. P10L3: Authors mention at this place “less than 10% average error” due to rainfall spatial variability. It is not clear to me then, why authors decided on P9L30 to estimate total error as 5% of total attenuation (see previous comment).

38. P10L5-6: The references are not correct. The manuscript from Leijnse et al., (2010) indeed investigates effect of rainfall variability on CMLs however the other references not. Zinevich et al. (2010) use findings of Atlas and Ulrich (1977) but do not bring new insight in this problem. There are two papers by Leijnse et. al from 2007 in the reference list, however, none of them is explicitly investigating errors due to DSD

or rainfall spatial variability. Please be more specific and precise when refereeing to the literature and also distinguish between works by same authors in the same year by using suffixes a and b.

39. P10L6-7: It is unclear what the higher percentage refers to? Higher random errors? If yes, you should discuss the influence of higher errors in the section 5 or 6.

40. P10L1-15: It is not clear, what you did. Clear steps how data are processed should be provided, especially as it is referred on L16 to “steps described above”. I would recommend restructuring the whole paragraph.

41. P10L18-19: It is not clear to which events the numbers “113, 26, 34 and 34” refer to. There is also typo, as number 34 is listed two times. Are these maps shown on Figure 5?

42. P10L21-22. CMLs are classified into three length classes. How is the classification useful for “deeper understanding of the rain attenuation variability”? Please be specific, how you work with this classification in the manuscript. I would expect it is used when interpreting results, however I could not find any reference to the classification in the results section.

43. P10L23: The increased attenuation of CMLs with higher frequency and path length is happening from the definition of CML path-attenuation. I think that this should not be presented as a finding.

44. P10L25: It is unclear to me how can analysis performed on virtual CML dataset where CML measurements are simulated “reveal close-to-reality type of measurement errors”.

45. P10L27: You should be explicit when reporting which errors were considered in the study. The literature to which is referred to report many sources of errors and it is not discussed above, if these errors are considered (e.g. error due to wind reported by Leijnse et al. 2010) and how they are simulated.

46. P10L27-30: It is unclear how the errors were analysed. Please be more specific.

47. P11L1: It is claimed in here, that the sensitivity analysis is performed in this chapter, however, optimal parameter combination resulting in highest NSE is presented at the end (P12L8-12 and L14-17). Optimization of model parameters is not a sensitivity analysis. It is also not clear why analysis is performed for parameters n_l and D and not for some others.

48. P11L13-17: I recommend to move the description of the rainfall retrieval algorithm to the method section, where the algorithm is first discussed but not described.

49. P11L21: Eq. at this line should be numbered as 12.

50. P12L23: Evaluation section is divided into two subsections: Evaluation principle and Evaluation results. First, subsection is really describing performance evaluation, however second subsection mixes up presentation of results with describing another performance evaluation procedures. This is confusing.

51. P12L24-L25: The first point of the evaluation procedure is not clear to me, what is exactly meant with "efficiency curve"?

52. P13L8: It is stated here that "spatial variability for all rainfall types has been far better captured". Please specify, "far better captured" than what?

53. P13L12: Where exactly it has been shown that local initialization "significantly improve retrieval algorithm performance"?

54. P14L16-17: It is claimed here that it has been demonstrated that *a priori* knowledge heavily influence the accuracy of retrieval. To my opinion the figure 8 shows rather the opposite, especially for shower, unorganized and organized storm.

55. Figure 7: This figure requires more detailed description. What does parameter k stand for? What is the step by which parameters were changed (dn_l resp. dD)?

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