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Interactive comment on "Areal rainfall estimation using moving cars – computer experiments including hydrological modeling" *by* E. Rabiei et al.

Anonymous Referee #2

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Review of the manuscript HESS-2016-17: "areal rainfall estimation using moving cars - computer experiments including hydrological modeling" by E. Rabiei, U. Haberlandt, M. Sester, D. Fitzner, M. Waliner

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Summary

This manuscript presents a simulation study to evaluate the potential benefit of using information derived from sensors on-board moving cars to improve the estimation of areal rain rate over small (sub-)catchments for hydrological modeling. Because of the difficult access and limited number of actual data from moving cars (denoted RC), a simulation approach is employed, assuming car density and movement along main and secondary roads within 3 catchments in southern Germany. The reference areal rainfall is derived from an operational weather radar, in order to have realistic spatial patterns. The contribution of RCs is analyzed by comparing the quality of the retrieved areal rainfall and simulated discharge over the three catchments with results obtained

using rain rate from rain gauges (from 1 in the vicinity to 2 within the catchment) only. The added-value of using RCs (at varying densities) is demonstrated in terms of areal rainfall and discharge simulation in situation where the rainfall may vary significantly within the catchment.

Recommendation

The topic is original and relevant to the hydrological community focusing on small scale catchments. The task is difficult because many influencing factors must be realistically simulated in order to obtain meaningful results. I think the authors do a good job to this respect, although some of the assumptions or simplifications should be more clearly mentioned and better explained. The question of the transferability of the obtained results and conclusions remains open and should be better tackled.

Overall, there are a few issues that need to be properly addressed in order to have a manuscript acceptable for publication in HESS. I provide a list of comments and questions below.

General comments

1. Rainfall is a complex process having many facets. What this work is about is rainfall intensity or rain rate estimation, but there are other aspects of rainfall that may be relevant to different communities (e.g. microphysics). I hence suggest that rain rate is used instead of rainfall throughout the manuscript when this is what is of interested. Starting in the title...
2. Concerning performance assessment for areal rain rate, an important aspect of rainfall that is not mentioned in the manuscript is intermittency. In particular when

working at a temporal resolution of 5 min, there will be a significant proportion of the catchment where there is no rain. The employed evaluation criteria do not take this into account. I suggest to evaluate the occurrence and the quantity separately.

3. Still concerning rainfall, using ordinary kriging requires some important assumptions, not mentioned: the data must be Gaussian for the optimality and rain rate is far from Gaussian; the random process of interest must be stationary and this is likely not the case in the mountainous catchment (the influence of topography is mentioned in section 4 but not the issue with ordinary kriging); depending on the employed variogram model, negative values can be generated, if it is the case the way they are treated should be explained.
4. I did not understand if the areal rain rate and the discharge values were continuously estimated over the 5 years or if the analysis was on an event basis. In particular in the results displayed in Figure 6, 7 and 8. A point to clarify as if a continuous approach is employed, there will be mostly no rain over the considered catchments (in the order of 10-15% of the time steps are rainy I imagine).
5. The results presented and analyzed in this work are specific to the considered catchments. Therefore, the question of the transferability or generalization of the conclusions can be raised. I recommend the authors to clarify which conclusions may hold for other regions and which ones may not, with appropriate explanations of course.

Specific comments

1. P.2, l.15: maybe the spaceborne radar of the GPM mission could be mentioned, as there are active remote sensing instruments in orbit.

2. P.2 and 3: the scientific questions 1 and 2 read quite similar to me, the difference should be clarified in my opinion.

3. P.4, section 2.1: it was not clear to me while reading this section if the rain gauge data were also extracted from the radar image. This is clarified in the conclusions, but I think it should be done earlier.

4. P.5, item b: rain rate will influence the speed of cars via visibility. It becomes very difficult to drive when the rain rate is above 40-50 mm/h...

5. P.5, l.14-15: I do not understand why the day-night cycle does not influence the estimation of rain rate by RCs, as much less cars are to be expected during night. This point should be clarified.

6. P.5, Eq.3: what is the value used for r?

7. P.6, l. 15-19: what do these sensors exactly measure? What is the signal coming out of those? Not essential, but out of curiosity...

8. P.7, l.5: it should be mentioned that the parameters of the linear regression on the log are optimal in the log space but not in the (original) linear space, giving more weight to small R values.

9. The normalization proposed in Eq.8 can be called "climatological variogram" (*Lebel and Bastin*, 1985).

10. P.9, Eq.11-13: RMSE is a dimensional quantity, so its units should be provided. More generally, the units (also for dimensional quantity like Pbias - in %?) are missing in many part of the text, this should be corrected.

11. P.10, l.10: from which data is the mean annual rainfall derived?

12. P.11, l.15-16: the values of a and b should also be given here, not only in Fig. 5.

13. P.13, l.10: the goodness of fit of the variogram model should be provided to give the reader a sense of the accuracy of the fit.

14. P.14, l.23-24: as mentioned in the general comments, this sentence clearly refers to non-stationarities, which hence question the use/applicability of ordinary kriging.

15. P.15, l.17: it is a matter of detail, but it cannot be concluded from the data at hand that the discharge will be dominated by these 2 sub-catchments, but only assumed. Other factors than rain rate may influence discharge amount (geomorphology, karst,...).

16. P.15, l.24: RMSE is an absolute value so it cannot be compared between the three catchments.

17. P.18, l.1-2: I appreciate the tentative to generalize the results, but these statements are very speculative. This comment is in the line with item 5 in the general comments.

18. P.18, l.21-22: I do not understand this sentence.

19. P. 19, l.1: this refers to the positive skewness of the distribution of rain rate, that could be mentioned here.

20. P.20, l.29: a reference to the more recent paper by *Berne et al.* (2004) on the same topic could be added here.

21. Tables 2-6 and Figures 6-8: units should be provided.

22. Fig.1: the lowest altitude in the color bar is negative, it should be to 0 (or more) I guess for this region of Germany.

23. Fig.5: I am surprised by the identical correlation coefficient in both panels...

24. Figures 7-8: when dealing with differences for which 0 matters, I suggest to use another color bar (blue for negative, red for positive, white for 0) which would ease the identification of the global pattern of under- or over-estimation.

References

Berne, A., G. Delrieu, J.-D. Creutin, and C. Obled (2004), Temporal and spatial resolution of rainfall measurements required for urban hydrology, *J. Hydrol.*, *299*(3-4), 166–179.

Lebel, T., and G. Bastin (1985), Variogram identification by the mean-square interpolation error method with application to hydrologic fields, *J. Hydrol.*, *77*(1-4), 31–56.