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Discussions

Interactive comment on "Geostatistical upscaling of rain gauge data to support uncertainty analysis of lumped urban hydrological models" *by* Manoranjan Muthusamy et al.

Anonymous Referee #1

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General comments

The goal of the paper is the analysis of the uncertainty of the areal interpolation of precipitation data from a dense gauge network with a high temporal resolution. The topic itself is relevant and currently discussed, in particular, in the context of radar measurements. From this point of view it is very interesting to analyze such a very dense gauge network with respect to the influence of the spatial variability/measurement errors on the areal interpolation. The paper is well structured and the methods used are (in most cases) clearly presented using a step wise explanation. However, a major problem of the study is the sample size of only 13 events (of 2 years). As these events are highly variable as shown in table 1, the uncertainty of the entire study is rather high. Although

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Discussion paper

there is a pooling method presented that tries to overcome this issue, the underlying assumption might still be problematic (see comment [5: 5]). Furthermore, many descriptive results are presented, but their consequences are rarely discussed. There are also a lot of trivial results in the paper. It should become much clearer, what is a logical consequence of the precipitation structure and what is an actually new result of this study. For example, temporal averaging will always reduce precipitation peaks. Finally, the language could be more precise because several sentences are too fuzzy.

Specific comments

[Page: Line]

[2: 19] "Since rainfall can vary over space significantly, any method for scaling up the point rainfall measurements adds uncertainty on top of existing measurement error." The measurement error is not explained so far. It should be introduced as it is a major aspect in the rest of the paper.

[Fig 5] For a better comparison, the classes of the histogram should be the same as the classes used later for the variogram.

[2: 32] "...not always..." Rainfall intensity values are almost never normally distributed

[3: 32] "During the dynamic calibration..." How did you identify the volume error per tip using a dynamic calibration? The entire section is not clear: "Every long set of data...if the differences..." What is the long set and what kind of differences?

[3: 8] "... to obtain a more normally ..." After the transformation they are perfectly normally distributed.

[4: 14]: How are the events defined? What are the criteria of the end of the event; if all stations show zero precipitation? Is there a minimum separation time of two events? Or will a few minutes without rainfall already separate the events? Why does the event need to be at least 20 min?

[5: 5] “The underlying assumption. . .“ This is your major assumption for the pooling based on the trade-off between independence and number of available time instants for the three defined classes. However, you did not show any analysis to validate the independence assumption. Instead, literature is cited, which shows the dependence of the spatial correlation on the intensity. How can you be sure that the influence is small enough to be disregarded, and that the pooling procedure does not mess up your further analysis?

[5: 11] “As expected, with increasing temporal averaging. . .” This is not only expected, this is obvious for an aggregation process of rainfall.

[5: 12-13] This is also not surprising due to the skewed intensity distribution of precipitation. It should become clearer in this part [5:11-13], that the results are just natural characteristics of precipitation.

[5: 14] “. . .only eight. . .” Where does this number come from? In figure 5 these are way more than eight for the 30 min average.

[6: 26] “It is negligible small. . .” Isn’t that contradicting to the later parts where the uncertainty of the tipping bucket error is analyzed (for example in [12:15]).

[6:29-7:22] In the entire chapter it becomes not clear what kind of stochastic simulation was performed (conditional/unconditional) and which method was used to obtain the 500 simulation results.

[7: 10] Kriging would be possible, if the back-transformation of the individual points was performed before the averaging. (No block kriging, but ordinary kriging of single points (25 x 25m grid)

[8: 1]”. . .is spatial aggregation of each and every simulation. . .” Should it be of “each time step”?

[8: 16] How can you be sure that this effect is caused by measurement errors? Couldn’t the nugget effect be also caused by the pooling technique, that is, by mixing different time steps; or by a high variability in the natural precipitation process? (see Remark [9: 7-21])

[9: 2] Is there a reason why the content of Habib et al. is mentioned explicitly and not of Villarini et al?

[9: 7-21] The important point of the interpretation of the variograms is not clearly stated. The variograms actually show, that for short time periods < 5 min (except for high intensities), there is almost no spatial correlation, that is the field is just random. If the nugget (explained here as tipping bucket error) is almost as high as the sill, there are two options. First, there is just no spatial correlation at the regarded distance, or the spatial correlation of the field cannot be detected by the tipping buckets because of the measurement error. There is also a very weak correlation (even for high aggregations) for the intensities smaller 5.0 mm/h. How can you be sure, that the nugget comes from the tipping bucket error, and does not represent a very high spatial variability of the natural precipitation at very short distances?

[10: 16] “Here it can be noted. . .” As the precipitation intensities are never uniformly distributed, this effect is a logic consequence.

[10: 30] This chapter should be rewritten, could be shortened and included in the next one. As explained in the last sentence of the paragraph, it is difficult to compare the standard deviation of different absolute values. Figure 10 is rather useless, as the intervals (uncertainty) cannot be read. A table including the standard deviations in addition with the CVs for the single events could help.

[11: 12-26] What is the actual goal of that chapter? Has there any kind of significance testing be performed? As there is one very large CV value <10 mm (2 min) out of six, the comparison between the means might be biased. If this value was an outlier, would the result still be that considerable? There seem to be a tendency, but the sample size is very small and thus, there is a lot of uncertainty in this result.

[11: 22] “This is fairly high. . .” How did you judge that? 25% to -15 % of the peak runoff is your reference. But what is the expected influence of the uncertainty of the rainfall when estimating the runoff from the precipitation? This is an important question for the judgement as one should now the necessary accuracy of the input precipitation and not the total uncertainty of the runoff.

[11: 26] “Hence a better trade-off . . .” What does this actually mean? How could this be achieved? There is a link missing between the shown problems and the actual application. Later, in the conclusion, it becomes clearer, but in this chapter this sentence is kind of fuzzy.

[12: 6-10] It should be mentioned, that the result of “peak” intensities are explained here.

[12: 24] “. . . radar measurements . . . would be much higher. . .” Please, give some references here.

[12: 27] You did not show explicitly the advantage of the paired rain gauges. Unless the improvements are shown in the main chapters, they should not be part of the conclusions.

Technical corrections

[2: 7] . . .where time series of areal . . . “are” needed.

[2: 27] . . . [8,9] . . . citations missing

[4: 9] “That” is because. . .

[8:5/8] Index Error: px should be pi

[9: 21] . . . “look” similar. . .

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