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Hydrology and Earth System Sciences Discussions

Interactive comment on "Spatial interpolation of daily rainfall at catchment scale: a case study of the Ourthe and Ambleve catchments, Belgium" *by* S. Ly et al.

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In this manuscript the performances of several geostatistical and deterministic spatial interpolation techniques for rainfall are examined. Results are compared, and an assessment is made of the effect of the density of available rain gauges on interpolation results. I think that such analyses can be useful for hydrologists working with sparse rain gauge data. However, I feel that the paper needs to be thoroughly revised before it can be published. Specific comments on the manuscript are listed below.

Major comments:

- The English of the paper should be improved.

- The introduction should be clarified. It should be made clear how this paper contributes to our understanding of the use of geostatistical methods for rainfall interpolation, and what sets it apart from other work that has been done in this field. I suggest that the authors move the discussion of findings in the literature in Section 4 to the introduction. In this way it can be made clear what results have already been presented, and what the contribution of this paper is. The second question that the authors state they will attempt to answer in the introduction ("With the raingage in which position?", p. 7387, lines 9-10) is not tackled in the remainder of the paper. This should also be clarified.

- Using many different variogram models to estimate the optimal variogram will in principle yield better results than when only one type of variogram model is used. However, it also makes the interpretation of the results presented in this study more difficult. It would be interesting to know how the optimal variogram models and their parameters change in time. Is there a single variogram model that is often the optimal one, or are they all used equally frequently? And how large is the improvement over using only, say, the spherical model? While these are all interesting questions, I feel that the paper would gain much in clarity if only one variogram model were to be used, especially given the main objective of the paper. Looking at the example of Fig. 2, the spread of the points around the different fitted variograms is much larger than the difference among the variogram models. This leads me to believe that using these seven different variogram models will not greatly improve results.

- Zero rainfall is not mentioned. This deserves some attention, as it is one of the points that make rainfall modelling and interpolation difficult.

- On p. 7393, lines 5-6, it is stated that "kriging can lead to negative estimates. Thus, the variogram model was changed to another one until the kriging estimates were all positives." The kriging estimates can only become negative for some types of kriging (for example, it is not possible with ordinary kriging, where all values are between the minimum and maximum data values). And it is entirely possible that all variogram models will yield negative rainfall at some point in space for for instance kriging with external drift. Was the variogram model changed if one of the kriging methods yielded negative results somewhere, or was the model only changed for the kriging methods that yield negative values? And what happens if all variogram models yield negative results with a particular kriging method? The "C+10000" statement in Fig. 1 does not help me understand how this is accomplished.

- I find Figs 5 and 6 impossible to interpret. It would be much better if the DEM would not be displayed in these figures (it can be seen in Fig. 3), and that the rainfall fields would be presented as color maps (such as the ones for the Thiessen interpolation). - Results for which interpolation technique are presented in Fig. 7?

- The fact that the Robertville (misspelled as "Rotbertville" on p. 7400, line 17) rain gauge produces such a large RMSE in Fig. 7 compared to the other gauges is striking. Can this be explained in some way? What can be concluded from this regarding the robustness of RMSE of 7 gauges as a measure of the quality of the interpolation technique? As stated in the conoclusions (p. 7404, lines 11-14), a fuller assessment of the strengths and weaknesses can be given using a more thorough cross-validation. Given the fact that this is central to this paper, I think that the authors should consider including such analyses in this paper.

Minor comments:

- In the abstract, what is meant by "the majority of existing geostatistical algorithms are available only for single-moment data", and how does this relate to the rest of the paper? There are ar least two open-source packages (gstat and geoR; both based on R) that offer a wide range of geostatistical methods.

- On p. 7387, lines 1-2, it is stated that "a daily time step is optimal for an understanding of the soil-plant-water relationship and long-term catchment management simulation".

I don't think that this statement is true in general. It may hold for certain aspects of the soil-plant-water relationships and catchment management simulation on certain spatial and temporal scales, but it is certainly not generally true. I therefore strongly suggest removing this statement, or providing ample evidence that supports it.

- In Eq. (1) (p. 7388), it should be made clear that λ_i is a function of g.

- On p. 7390, it should be clearly stated that statistical homogeneity is assumed.

- On p. 7390, lines 8-10, the authors mention that the distances are binned. How large are these bins? And does the bin size depend on the number of gauges used?

- The distance between two points h is a vector in this paper. For simplicity, the authors could consider using a scalar instead, because they use the assumptions of statistical stationarity and isotropy.

- The logarithmic variogram is one I have never heard of. This may be because it is minus infinity close to $h = 0$, which is impossible to interpret in terms of variances. I therefore strongly suggest removing this variogram model from the analyses.

- For the power model, why is the parameter θ_2 constrained to be smaller than 2?

- For the spherical and penta-spherical models, the last two conditions overlap. The last condition should be $h > \theta_2$, and not $h \neq 0$.

- On p. 7392, lines 5-7, it is stated that the most common method for fitting semivariogram models is by eye. Can the authors provide some references in which this has indeed been done? In most of the work that I'm aware of automatic fitting has been used.

- On p. 7397, line 4, it should be mentioned that Pearson's coefficient is computed from the correlation between rainfall amount and elevation.

- In Section 3.1, the authors could consider using a table to summarize the annual mean rainfall at the highest and lowest points of, and the average over the catchmant.

Other values that could be added to this would be the minimum and maximum values over the catchment (which should be similar for e.g. inverse distance weighting and ordinary kriging).

- In the analysis effect of the number of gauges, the authors could include a discussion of the results presented by Berne et al. (2004, J. Hydrol., 299, 166-179).

- Are the points in Fig. 8 the means of the seven gauges? and are the error bars plus and minus the standard deviation of these seven gauges?

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