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Hydrology and Earth System Sciences Discussions

Interactive comment on "Mapping rainfall erosivity at a regional scale: a comparison of interpolation methods in the Ebro Basin (NE Spain)" *by* M. Angulo-Martínez et al.

Anonymous Referee #1

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General comments

The topic is significant and of high international relevance. Erosion poses a major environmental thread considering population growth, climate change and with that the increasing need for sustainable land use. Mapping of erosion risk can help to set up measures to prevent erosion. The authors compare different methods for the interpolation of the RUSLE R factor and the average erosivity index EI30 for the Ebro river basin. They are fortunate to have access to a dense network of 112 recording rainfall stations with observed data of 15 min temporal resolution over a period of 10 years.

The methods and ideas are not new. However, application and comparison of the

methods using high resolution rainfall data for the Ebro region has a specific novelty. The presentation is quite clear and well structured. The description and discussion of the scientific methods, especially the geostatistical techniques, is not sufficient. For instance no information about variograms, anisotropy, used co-variables for co-kriging, mean for simple kriging etc. are given (see specific comments). The conclusions are not fully supported by the results. The different validation criteria noticeably favour different interpolation approaches. This is partly surprising and should be discussed more in detail. It becomes neither clear from the results that the mixed methods are the best ones, as the authors conclude, nor which method should be applied for mapping in the end. Using longer time series, as suggested by the authors, will not reduce the (interpolation) uncertainty. Instead it would be a good idea to quantify the local uncertainty e.g. by Gaussian or indicator kriging approaches (Goovaerts, 2001). Also, a seasonal differentiation in the mapping of erosivity considering the different climate conditions would probably narrow the uncertainty and allow more specific measures regarding land use management. Overall, the manuscript has a good potential but still requires major revision to reach a quality for publication in HESS. More details are given below.

Specific comments

1. Abstract: The conclusions need to be revised according to the results. Where is it proven for instance, that the spatial pattern are correctly represented by the methods?

2. Sect. 2.3: Discuss the complementary character of R and EI30. Why are both criteria used/ required?

3. Sect. 2.4.2: Briefly describe the theory of the geostatistical approaches, at least the main characteristics, assumptions etc. of the used methods in comparison.

4. Sect. 2.4.2: How are the variograms inferred? Which variables are used for co-kriging? How is the mean for simple kriging estimated?

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5. Sect. 2.4.2: The application of regression residuals for interpolation in kriging implies methodological inconsistencies. Residuals from the regression theoretically need to be independent, but for kriging they are assumed to be dependent; i.e. they are related to the distance between the station locations in order to estimate a variogram. This problem has usually only little practical relevance but should at least be mentioned. There are also workarounds for that problem (e.g. Neuman and Jacobson, 1984).

6. Sect. 2.5: There are too many validation criteria involved. It is difficult for the reader to judge the results. I would suggest reducing the number of criteria to some most significant but complementary ones (e.g. bias, mean absolute error, coefficient of determination and variance conservation).

7. Figs. 2 and 3: Discuss the differences between the spatial pattern of R and EI30. This issue is related to comment 2. I would assume it has something to do with the frequency of rainfall events, which is considered in R but not for EI30.

8. I would recommend thinking about carrying out the analysis not only for the whole year but also for different seasons, which would consider the different climate conditions e.g. prevailing convective and frontal rainfall events. The results would also benefit a better land use management which depends on the seasons.

9. Before interpolation a structural analysis should be carried out based on variograms. The results would reveal the spatial characteristics of the two target criteria and explain also part of the uncertainty of the results. This includes also a discussion of the anisotropy, especially since this feature is used for kriging with anisotropy but not discussed.

10. Some more information about the application of kriging would be useful e.g. how many neighbours are included, what search radius is applied, etc. This would also help to assess the results e.g. like smoothness of the maps.

11. Tables 5 and 6: The performance measures (e.g. looking only at R2, MAE and D)

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provide a significant different ranking of the interpolation methods. This is unusual in this extent and should be double-checked and discussed (consider also comment 6).

12. Tables 5 and 6: I cannot see from the validation criteria, that the mixed models (e.g. the last two methods in both tables) are outperforming the other ones as concluded by the authors. For instance in the interpolation of R (Tab. 5) the last two mixed methods are never best or second best according the criteria R2, MAE and D?

13. The paper would much benefit from a quantification of uncertainty for R and EI30 e.g. utilising the estimation variance from kriging (requires Gaussian assumption) or by using an indicator approach (Goovaerts, 2001).

14. Figures 4 and 5: It should be indicated on the figures or in the text of the legend which axis represents predicted and which represents observed values.

References

Goovaerts, P., 2001. Geostatistical modelling of uncertainty in soil science. Geoderma, 103: 3-26.

Neuman, S.P. and Jacobson, E.A., 1984. Analysis of nonintrinsic spatial variability by residual kriging with application to regional groundwater levels. Mathematical Geology, 16(5): 499-521.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 417, 2009.