

Interactive comment on “Modelling dominant runoff production processes at the micro-scale – a GIS-based and a statistical approach” by C. Müller et al.

Anonymous Referee #4

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In this paper, two algorithms or procedures are proposed to attribute various Dominant Runoff Processes (DRPs) to subareas of the Zemmer basin in Germany. The results of these procedures were compared with a DRP reference map, developed by Schobel (2005; Figure 3 of the manuscript). Authors report that both approaches, one GIS based and the other statistical, provided satisfactory results. From the authors' conclusions, I get the impression that they cannot really choose which of the two approaches they will use for mapping meso-scale runoff processes in the Rhineland Palatinate and the Grand Duchy of Luxembourg area. At any rate, permeability/impermeability of the subsurface, used as a kind of boolean operator, and topography of the area, mainly defined by the variables SLOPE, LS, and S (see Table 5) seem most important, so I

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assume authors will use these parameters, either in an upscaled GIS approach or in an upscaled statistical approach.

I have a few major comments and a list of minor comments.

Major comments:

Half a manuscript.

In my opinion, all this manuscript does is comparing the results of two simple procedures with a map, prepared by one of the authors in 2005. Is this the way to report a hydrological study? Or, more in general: Is this the way scientific results should be reported? I am not really in the position to judge scientific reporting as a whole, but I sure am able to say something about reporting hydrological studies, and I do not think that this paper has all the ingredients for a good paper (yet). The missing ingredients are: 1. Rainfall and runoff data, measured at various locations in the micro-catchment, 2. The translation of the GIS and statistical approaches into runoff, 3. The comparison of measured and modeled runoff, and 4. Some form of sensitivity analysis.

This plus what is already described in the present manuscript in a condensed form in one paper. Then I would call this a manuscript with a head and a tail. The effect of reading a paper that is only halfway of becoming one, is that the hydrologic information is very thinly spread, and the paper as such becomes extremely boring. All I get to read is stuff on GIS and statistical procedures. Not really an exciting experience, I must say. A good example of the lack of news in the manuscript is hidden in the text on page 1686, line 7. Here authors state that 'The steepness of the slopes influenced the classification most. This was not astounding since the mapping method of Scherrer and Naef (2003) separated the different DRPs by taking slope into account'. I think this conclusion is completely trivial, since the statistical approach was entirely based on a DEM of the area. Another very good example of the lack of news in the manuscript is the description of part of table 6 (see page 1687, line 5-16): a boring repetition of falsely or correctly classified areas. Not only because the authors do not know how to

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report, but mainly because there is nothing to report. So the result is an extremely dull paper, and I am quite sure that, except for the authors themselves, nobody will ever cite it in its present form.

Quality of the English.

Another part of my bad impression of the manuscript is based on the low quality of the English. It is quite clear you translated the paper yourself, and this not only creates confusion (I have included a number of examples in the Minor Comments section), but it also reduces the value of the message you would like to get across.

The word 'Modelling'.

Throughout the manuscript the words 'model'; and 'modelling' are used, mainly for the GIS approach and the statistical approach. However, I find these two models merely approaches or simple mapping techniques, comparable with vulnerability mapping techniques, like the DRASTIC approach or any other approach of this kind. Once flows and volumes are added, then the result is a kind of parametric model, which deals with conceptual processes assigned to certain areas, because these processes cannot be properly encompassed by existing numerical codes. Still far from being a physically based model description of the area, but, nevertheless, only then I would call your approach a model.

Figures and Tables.

Table 5 can be left out, because the numbers are meaningless for the reader. Table 6: upper matrix is in ha, while lower three are in percentages. Very confusing. Fig. 5a: Legend is not very appealing. Axes labels are hard to read. Please revise.

My conclusions.

In my opinion, authors are halfway, and in a revised version of the manuscript, I would like to have included the ingredients I mentioned above. Furthermore, use a native English person to edit the manuscript before re-submitting, and use the word 'model'

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with due care.

Minor comments:

P. 1680: L7: derivates should be (SB) derivatives. Please adjust throughout the text. L27: lie SB lies.

P. 1681: L2: processes compacted SB processes, compacted. L3: and further surface gleying? What is this? L8: Two model approaches SB Two approaches. Please delete the word model, when discussing these two approaches. L12: holding SB containing. L13: (see Fig. 1)SB(Fig. 1). Please omit the 'see' in all other Figure and Table announcements in the text. L23: Figure 2a schematically depicts this approach SB (Fig. 2).

P. 1682: L5: As a second step SB Secondly. To be followed by Thirdly on L14. L8: infiltration permeability doesn't exist. Should be either 'infiltration capacity', 'unsaturated permeability' or permeability. L28: For each DRP unit, which were mapped SB For each DRP unit mapped.

P 1683: L13: paket SB package. L26: This data is SB These data are. L29: What are soil sealing features? No water can enter? Please explain. L29: will be analyzed SB were analyzed.

P. 1684: L3: the partial F-values range between 2.71 and 3.84. I don't know what the F-value is, and neither do I understand why these should have the value 2.71 or 3.84. Please explain. L14-16: can be omitted. Just refer to the figures in the text. L18: was a SB was.

P. 1685: L12: Impartial fro the above-presented differences in both maps SB Apart from the differences in the two maps. (There are not more than 2 maps in the entire manuscript) L12-17: why are these distributions of dominant runoff process areas so typical for low mountain ranges? Please explain. A few good references might help. L19-22: Should be 'Approach 2 gave 2 outcomes: Eigenvalues and canonical correla-

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tions for two sets of discriminant functions (Table 4) and canonical function coefficients of the said discriminant functions (Table 5)'. Makes it a bit more clear I think.

P. 1686: L16: The geological structure of the Zemmer study area attributed this. What is it exactly that could be ascribed to the geological structure? And do you mean geology of the area or are there specific structural features, that I am unaware of?

P. 1686, L19 - P. 1687, L16: Here, I stopped looking at the English of the manuscript. Really, you desperately need a native English editor to look at your manuscript.

P. 1687: L23: two domination runoff processes SB two dominant runoff processes.

P. 1688: L3: a respect to SB respect to. L. 19: permeability mainly SB permeability was mainly or permeability could mainly be.

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