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Hydrology and Earth System Sciences Discussions

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

C. Müller et al.

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We would like to thank the anonymous referee #4 for his suggestions.

Major comments:

Review Comment: 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.

Author Comment: Since it was not the intention of the authors to model rainfall runoff relationships the availability of rainfall and runoff data was for this study no issue (see also general authors comment). The aim of this study is to develop two approaches to identify dominant runoff processes (DRPs) at the micro-scale with a view to regionalization for the meso-scale. The approaches are based on studies of Scherrer and Naef (2003) and Scherrer (2006) and can be understood as a soil function interpretation, concerning the type of runoff that will be generated on that kind of soil during long-lasting rainfall events. However, the application of these studies in the field is very time consuming, very expensive and they need very detailed information about the soil distribution and the properties of the soils (see also general authors comment). Schmocker-Fackel et al. (2007) simplified the complex decision scheme of Scherrer and Naef (2003), but still required data from detailed soil maps (1:5000). However, in most areas, even in Western Europe, this kind of data is very often not available. Therefore it can be useful to develop methods, which can determine DRPs that do not require such a heavy data load. The availability of the reference map (Schobel 2005) of the Zemmer basin established with the methodology of Scherrer and Naef (2003) offers the opportunity to develop more simplified approaches to determine DRPs and check the results of the approaches. The authors think its legal (even necessary) to use the map of Schobel (2005) as reference, because this DRP map is one of the few existing maps contrived using the original method of Scherrer and Naef (2003). The map is of first class quality due to the immense field data accounted (constructed on the basis of 728 soil drilling points and 15 soil profiles in a field campaign during several weeks). The same reference map would have been obtained by any other operator using the underlying method of Scherrer and Naef (2003).

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Interactive Comment

Review Comment: 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.

Author Comment: This part of the manuscript will be adapted since what is presented is partly false. Scherrer (2006) used 31 parameters of which slope is only one; but for impermeable substratum slope is the most important parameter. Moreover, for the permeable substratum, the topographical-index is the most important parameter. These results, defined by discriminant analyses (approach 2), will be implemented in the revised manuscript.

Review Comment: 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.

Author comment: This part of the manuscript will be adapted and presented in a more compact way. Nevertheless, the authors think that a detailed identification of the runoff types where the methods applied are successful or where they fail is crucial for the differentiation which of the DRPs. This is in the opinion of the authors important for further development of the approaches.

Review Comment: 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.

Author comment: A native English speaker will edit the text of the revised manuscript.

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Interactive Comment

Review Comment: 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.

Author comment: About the use of the words model and modelling, please see comments above and general author comment.

Review Comment: 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.

Author comment: The numbers in table 5 represent the values of the functions of the statistical approach and are in our opinion necessary information to reproduce the results of the study. Concerning table 6, we indented to give absolute values (ha) and provide not only percentages. The tables will be adapted in the revised manuscript. Figure 5a will also be revised.

Minor comments: Author comment: All minor comments are justified and will be implemented in the manuscript. Explanations will be provided where they are demanded. The part from P 1686, L19 - P 1687, L16 will be rewritten.

Review Comment: 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.

Author Comment: Soil sealing features are for example streets, parking lots etc. In the

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manuscript the authors will replace this by built up area.

Review Comment: P. 1685: 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.

Author Comment: The part typical low mountain range will be removed from the text.

Review Comment: 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?

Author Comment: This sentence refers to the previous one. However, in the manuscript this is not clear and the sentence will be adapted accordingly.

References

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 5, 1677, 2008.