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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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The authors would like to thank the anonymous referee #5 for the comments.

General comments

Review Comment: This objective is of international interest for understanding and modeling runoff genesis on un-gauged watersheds. However, the paper presents only technical results (GIS application, statistics interpretation) on only one case. There is no validation of the approach neither on other watersheds, neither using hydro-meteorological data. It is not obvious if the results can be generalized and applied on other study cases.

Author Comment: The objective of this study was to simplify the original method of Scherrer and Naef (2003) which needs very heavy data load for its application (see also general author comment). Furthermore, a validation of the new approaches in other basins is complicated, since very few basins exist in which the original method of Scherrer and Naef (2003) was applied. Therefore, the application of the new approaches in a new basin with similar properties offers the opportunity to detect methodological errors in the new approaches more clearly. In the revised manuscript an extra basin is introduced, in which the two approaches will be applied. The results of the approaches in the new basin will be compared with existing results of the method of Scherrer and Naef (2003) in this new basin and with the already obtained results of the Zemmer basin. This provides the opportunity to reflect on the ability of the approaches to be generalized. The results of the original method of Scherrer and Naef (2003) (i.e. maps with dominant runoff processes) has no direct relation with quantitative aspect of hydrographs. Moreover, these maps show a soil functional characterisation. Therefore the use of hydrographs or hydro-meteorological data is not suited to validate neither the results of the original method of Scherrer and Naef (2003) nor the results of the developed approaches.

Major comments

Review Comment: 1. The objective of the paper: The introduction ends (page 1680, lines 4-9) by presenting the objectives of the paper to develop two different models for regionalization of different runoff processes. The abstract starts also by presenting the methodology used and did not states why we do this work. But, why to develop different models, and how these models will be used in practice?

Author Comment: The approaches will be used to identify dominant runoff processes in micro- and meso-scale basin, where the information is lacking to apply the original method of Scherrer and Naef (2003) (see also general author comment). The developed approaches are a geo-statistical model and a statistical model. One of the objectives should be to find out which approach is suited better to reflect the results of

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the original method in micro-scale and meso-scale basins. The previously mentioned new basin will be used to test this as well.

Review Comment: Once the main hydrological processes identified on each zone, how to model hydrological processes? For what category of spatially distributed models the resulting maps of the study (Figures 3 and 4) will be used? And how to use these maps in practice, and for what range of application cases: is it for flood modeling at the event scale for example?

Author Comment: The results of the developed approaches in this study are maps, which identify dominant runoff processes at the upper micro-scale. These maps represent a spatial distribution of the hydrological behaviour of the soil during intense rainfall events. With such maps, areas relevant for the formation of floods can be identified (Schmocker-Fackel et al. 2007). The approaches are not suited for use in hydrological computer models yet. This is object of further study. Concerning the usefulness of the DRP-maps, Schmocker-Fackel et al. (2007) argued that such maps could help to improve rainfall runoff modelling of flood events. In addition, they might be used to predict potential risk areas like for pesticide loss or soil erosion.

Review Comment: Second, why two models using different approaches, and does the objective is to select one of the two approaches proposed? If yes, which one (the paper does not give any response)? If no, how to proceed in practice for regionalization, and what approach should be used?

Author Comment: The authors agree with the comment of the referee. See also answer above.

Review Comment: 2. The methodology used: The methodology compares the results of the two different approaches to a reference model. But, what is the accuracy of the reference model?

Author Comment: The reference DRP map of Schobel (2005) was obtained by the

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literal application of the method of Scherrer and Naef (2003). Any other experienced operator using the underlying method of Scherrer and Naef (2003) will obtain the same reference map. The reference DRP map of Schobel (2005) 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) and therefore assuredly consisting of observed soil-hydrological data. This will be stated more clearly in the revised manuscript.

Review Comment: Moreover, the discriminant analysis was done on the basis of the observations obtained on the reference map. Consequently, it is normal to find in the results of the statistic approach (the slopes as a major factor for example) the input of the reference map established on the basis of slope classification of the method of Scherer and Naef (2005). Please also give a map illustrating the results of the application of the statistic approach (similar to Figure 4 for the GIS-approach).

Author Comment: The input map was not only generated with morphographic characteristics, but also using a large number of soil variables (see above). The intention is to elucidate also the weight of the morphographic pattern on the differentiation of the soil functional groups. This will be stated more clearly in the revised manuscript. In the revised version of the manuscript a map of approach 2 will be included.

Review Comment: 3. Domain and limit of application of the two approaches: The paper is based on only a comparison between the two approaches and a reference map on an experimental site. There was no validation on other watersheds, using new cartographic data not already used in the phase of analysis of the two approaches. Can results of Tables 5 and 6, and Figure 6 be generalized? It is not obvious that the two approaches will give good results on other basins. The two approaches needs to be validated on other basins, and by discussing the domain and limit of application of each approach: Does the domain of application of the two approaches limited only to the Rhineland Palatinate and Luxembourg regions as stated by the authors? Can the approaches be used in other hydro-meteorological conditions?

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Author Comment: In the revised manuscript the authors will add a new basin (as mentioned above), with which it is possible to address the domain and limit of application of each approach. Since the application of the new approaches takes place in a new basin with similar properties (see also above), the domain of application of the two approaches is limited to that part of the Rhineland Palatinate only. The original method of Scherrer and Naef (2003) incorporates climatic and physiographic characteristics. Results of the application of the original method reflect therefore changes in climate and physiographic characteristics. It has been applied successfully in Switzerland (Schmocker-Fackel et al. 2007, Scherrer and Naef 2003). The application of the approaches in other hydro-meteorological conditions is object of further study. In this study the second study area was introduced to detect methodological errors (see also above). This will be stated in the revised version of the manuscript.

Review Comment: What is the spatial scale of application? When to use one approach instead of another? What about comparing approach 1 to approach 2?

Author Comment: See answer above.

Review Comment: 4. Hydrological processes: A large part of the paper concerns the interpretation of technical results, such as crossing maps in a GIS or analyzing statistical procedure results (i.e. Table 5), or comparing areas between the reference and the modeled maps (Table 6, Figures 3 and 4). However, the paper lacks of discussion on hydrologic processes: What are the main hydrologic processes on the studied watershed and how to model these processes? Do hydrological processes remain the same on a given pixel during the whole year? What about the spatio-temporal variability of these processes function of rainfall intensity, water table level, initial conditions of soil moisture? The methodology developed by Scherrer and Naef (2003) is considered as a reference, what is the accuracy of this approach and what is the domain and limits of application of this approach?

Author Comment: The discussion on soil-hydrological processes which are obtained

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through the reference map (Schobel 2005) will be improved in the revised manuscript. The approach developed by Scherrer and Naef (2003) was based on large number of field- and sprinkling experiments (Faeh 1997, Scherrer 1997, Weiler and Naef 2003). Scherrer and Naef (2003) indicate the limitations of their approach as follows: Rains of low intensity infiltrate into the soil predominantly by matrix flow and the scheme presented does not apply to such conditions. The matrix-macropore system probably only becomes active during rainfall of higher intensities (Faeh 1997). Field experiments emphasized the important role of the nature of the surface-topsoil interface for infiltration and runoff formation. As this interface is more complex on arable land (soil compaction, plough pans, surface sealing effects, etc.) and in forests than on grassland, special decision schemes are required for these other land-use types (Scherrer and Naef 2003). This will be cited in the revised manuscript.

Review Comment: 5. Modeling dominant runoff production processes: As stated above, the paper does not give a clear response to the objective announced in the title. The word modelling must de defined clearly, because the model used in the applications refers mainly to crossing maps technique and statistic analysis.

Author Comment: To avoid misunderstandings concerning the word modelling the authors will delete it from the title and use it throughout the manuscript with the utmost care.

Review Comment: There are no applications to calculate the runoff flow production at the local scale. The paper needs to be strengthen by showing an application case of the methodology presented to simulate runoff production at the local scale for various hydrological processes. This needs the use of hydrometeorological data (e.g. rainfall-runoff, water table level) in order to show a concrete application case of modeling, and in order validate the approach on measured data. A sensitivity analysis must be done and discussed in order to establish what are the main parameters in modeling runoff processes.

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Author Comment: Since the objective of the study was not modelling of rainfall runoff relationships, but about developing approaches, which identify dominant runoff processes. However, this was not clearly defined in the manuscript. See comments above and also general author comment.

Specific comments

Review Comment: DEM: Please indicate the accuracy of the DEM used. How slopes were calculated on each pixel and how a mean slope was calculated on hillslopes or on a group of pixels (for example page 1682, line 1 and in the whole paper)? What is the accuracy of slope calculation on flat areas?

Author Comment: The DEM used is the main DEM available from the government of the Rhineland-Palatinate with a grid resolution of 20 x 20 m. The authors will include in the revised manuscript more accurately how the derivates from the DEM were calculated. As mentioned in the manuscript, the authors used GRASS-GIS 6.3.0. The commands were: r.slope.aspect for slope calculation, the curvatures and the partial derivates. It uses a neighbourhood of 3 x 3 cells.

Review Comment: There are a lot of references in German (for example the paper of Schobel (2005) is cited 11 times). In order to help the reader, a short synthesis of the main results of the papers in German should be given in the main text.

Author Comment: The authors agree with the comment of the referee and will include short synthesis of the main results of the papers.

Review Comment: Page 1678, line 4: Please indicate the range of areas for micro-scale.

Author Comment: The range of the micro-scale is given in the introduction (Page 1678, line 4). The authors feel no need to include this in the abstract as well.

Review Comment: Page 1682, line 1: The reference (PBS, 2006) is not given in the reference list. Please explain also the abbreviation PBS.

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Author Comment: PBS means in German Prozessbeurteilungsschema. It is the original decision tree for field campaigns to determine DRP according to Scherrer and Naef (2003). The reference is Scherrer (2006). It will be adapted in the revised manuscript.

Review Comment: Page 1684, Lines 2-4. Please explain the origin of the F-values 2.71 and 3.84?

Author Comment: The F-values applied were the ones proposed by the SPSS program package, which ensures a substantial variable reduction taking into account the significance of the variable to derive the discriminant functions. This will be mentioned in the revised manuscript.

Review Comment: Page 1681, Line 16: The references of Müller (2007) and Müller (2008) do not exist in the reference list. May be Müller (2007) should be replaced by Müller et al. (2007).

Author Comment: The authors agree with the referee comment and will change the references in the revised manuscript.

Review Comment: The reader cannot easily interpret the numbers given in Table 5. The values given needs either to be further discussed or synthesized.

Author Comment: The table reflects the function coefficients. It is mainly thought to complete the documentation, as these are the factors of the linear models. Nevertheless, the authors will include (in combination with the data description as reflected in fig. 5a and 5b) a further discussion on the tables.

Review Comment: I do not understand the meaning of the different colors on Figures 2a and 2b.

Author Comment: Scheme colours should reflect different input and output data levels (field data, synthetic data, model building etc.). They will be homogenised for both schemes.

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Review Comment: What represents PBS on Figure 2a? What slope is used on Figure 2a, is it the local pixel slope, or a mean slope on a group of pixels?

Author Comment: About the meaning of PBS see above. Concerning the slope, the authors used the local pixel slope of two neighbouring pixels.

Review Comment: The legends of Figures 5a and 5b are not clear. The characters on the x-axes and y-axes on Figure 5a are too small. What represents the minimum and maximum values of each function.

Author Comment: The figures will be adapted for a better readability. In addition, the interpretation will be enhanced including the numerical meaning of the values

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