**Reviewer 2**

We appreciate the positive and constructive criticism and we look forward for further clarifications in case we have not sufficiently responded.

**General comments**

This study presents a novel approach for parameter identification in a simplified process-based rainfall-runoff model for river basins that are dominated by subsurface runoff generation processes. Subsurface flow parameters in a grid-based model structure are optimized in a way that the climatology-based simulated pattern of saturated cells matches the stream network of a topographic map of the river basin. This is an appealing approach for parameter estimation in ungauged basins because it is based on widely available input data.

The paper is well and concisely written and clearly structured. The authors honestly list and discuss the deficiencies of their results in simulating the hydrograph and water balance components in view of, e.g., model simplifications. However, the paper is not convincing with regard to its main issue, i.e., properly setting the ground for the method of subsurface parameter estimation by simulating stream networks. Conclusions such as 'the underlying assumption that the development and initiation of stream networks is controlled by the properties of the subsurface could be plausibly confirmed : : :' (page 862, line 5) or 'stream network modeling seems to be a good alternative approach for ungauged basins' (page 862, line 27) cannot be supported by the analyses presented in the manuscript.

*Answer: Both reviewers have expressed reservations according the ability of the presented approach to handle the ungauged basin problem. We can relate to that to some degree and have also mentioned some problems and deficiencies in our discussion. However, we think it is important to present also results which are not perfect and to provide approaches that differ from conventional regionalization methods or parameter optimization methods with limited data availability.*

*In this paper we wanted to test and present an approach, which could contribute to handle the ungauged basin problem. The setup of this test might lead to the impression that our sole goal was to establish a new calibration method for ungauged basins. Certainly the focus of the paper is on gaining information about the parameter space but it was never our intention to present a stand-alone calibration approach. Rather, our approach should "guide hydrological modeling in ungauged basins" to receive additional information about the catchment, in particular the subsurface, and the model. This could be combined with other techniques to handle some problems in ungauged basins (e.g. to evaluate a specific model setup). After we received the reviews and re-read the discussion we have to admit that the paper lacks some information about our motivations and goals and is written in a way which could lead to misunderstandings. In the revised manuscript we will extent these parts (especially introduction & conclusion) to make clear what we want to achieve.*

**Comment 1**

The selection of optimum parameter values is steered by comparing simulated with mapped stream networks. Mapped stream networks are based on a topographic map with a scale of 1:200 000. This is a rather coarse scale where the stream network will have undergone a considerable process of generalization. What really is the information content of such a map in terms of the stream network? Do the authors believe that this stream network is appropriate for the process-based approach they take, i.e., can it be used as a proxy for the initiation points of a channel network where the groundwater table intersects the terrain surface? I assume that finer scale maps are required for that purpose.

*Answer: This was also a point we first were concerned about. Thus we have compared the mapped stream network of 1:200 000 map with the 1:25000 scale map. There were some minor differences in the headwaters, however, after rasterizing to the model resolution the effect is hardly visible. The reason for this minor difference is that the selected 1:200000 maps are based on smaller scale maps and only the small scale features (bends etc) were removed. However, if we would use a finer grid resolution for the model, this could make a difference.*

**Comment 2**

When introducing the new method on subsurface parameter identification based on stream networks, it is mandatory within a proper evaluation to give insight how different parameter values impact on the simulated channel network. The authors only give the 'optimum' channel network for the study areas. However, they but do not explore its sensitivity on parameters (i.e., does it change markedly or only to minor extent when changing the parameter values), nor do they discuss from a more process-based perspective success and failures of the simulated stream network in comparison to the reference map (not just by showing the comparative statistics). For example, the simulated network does not only consist of linear stream features but also of larger wet patches. The approach is interesting in general, but I'd like to learn something on the patterns of stream networks and how they depend on the model approach and catchment characteristics.

*Answer: This is again a very interesting point. Especially if we want to shift the focus more on the general benefit of stream network modeling and away from a stand-alone calibration method. However, a complete analysis for all catchments and all parameter combinations would be very time consuming, because it would be necessary to rerun all simulations. Thus we would suggest to select some interesting parameter sets for specific catchments and show this by mapping the probability of occurrence of stream cells consider the sensitivity of parameters.*

**Comment 3**

Finally, as the ultimate goal of the approach is rainfall-runoff modeling in ungauged basins, the question is how uncertainties in estimating model parameters based on stream networks map into uncertainties of simulated hydrographs / water balance components. The authors show that some parameters can be identified with more or less clearly defined optima. What, however, is the resulting range of hydrographs for the given model? In combination with my point 2) above, how do ranges of optimized stream network patterns, i.e, their respective parameter sets, translate into runoff simulations? For example what are 'significant differences between the individual parameter sets' (page 859, line 14) and how do they express both in terms of simulated channel networks as well as in terms of river discharge?

*Answer: Of course, it would be interesting to do a complete uncertainty analysis with respect to runoff simulations. However, this paper shouldn't present a stand-alone calibration approach. Rather, the benefit of stream network simulation for the ungauged basin problem should be investigated (see general comment). Doing a complete uncertainty analysis would raise other problems and question (e.g. what are behavioral and what are non-behavioral simulations when using Kappa statistics as objective function), which would go beyond the scope of the paper. Thus we suggest for revision to look more into the processes of stream network simulation (as suggested in comment 2) than doing a complete uncertainty analysis.*

**Minor comments:**

1) The abstract does not give a clue on what is meant with ‘explicit simulation of stream networks’.
2) page 854, line 18: ‘Both derived from an error matrix’. Not a full sentence.
3) page 854, line 18: ‘discrete multivariate technique’: Not clear to which part this term refers (map comparison methods? Kappa statistics?)
4) page 854, line 25: ‘the initiation of stream networks is controlled by seasonal variations’. This is one factor, but isn’t also a function of the weather conditions, e.g., extended rainy periods, that may cause deviations from a seasonal pattern? The authors may comment on the antecedent conditions when presenting the spring and latesummer stream patterns.
5) The different extents of stream networks and the reference map can hardly be distinguished in Figure 3.
6) page 860, line 18: ‘the simulated recessions are too small. . .’ What does this mean?
7) Comparison of efficiency values with other studies in ungauged basins: are all values based on discharge time series with daily resolution?

*Answer: We appreciate the editorial notes. All notes will be included and the abstract rewritten. The efficiencies of the other studies are all based on daily discharge values.*