

Reviewer 1

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

General comments

In the paper, the authors report a method for the simulation of a stream network using a hydrological model driven by long term average climate inputs. The authors implement the approach for the estimation of the parameters of the hydrological model by comparing simulated stream networks with digital ones. They implement the parameters thus estimated for the simulation of runoff. I generally find their attempt interesting. However, I find some important elements, especially description of the model structure and its modification rather sketchy. Furthermore, while the objective of the work is introducing a methodology for handling the ungauged basin problem, I feel that many of the simplifications and assumptions implemented in the work have serious implications on the applicability of the methodology in achieving the intended objective.

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

It is only mentioned that flow in the unsaturated zone is simplified and no details are given about the simplification. How is it represented in the model and what is the implication of its representation to the runoff generation process? Is precipitation /snow melt directly added to the saturated soil zone? If that is so, how can a model in which the overland flow process is missing be potentially useful for simulation of runoff?

Answer: It is nearly exactly as the reviewer has assumed. Precipitation/snow melt is directly added to the saturated zone. We will extend the description of the model to make it clearer. But overland flow can be generated from saturated zones (saturated overland flow), which is very typical for humid catchments to have a runoff response by subsurface flow and saturation overland flow., We think that this is still an adequate representation to simulate discharge in humid catchments, which are mostly covered by forest and Hortonian overland flow seems to contribute only little. We also address this point in our discussion (P860, L.9).

Comment 2

In sections 4 and 5, the authors have highlighted on many of the limitations of the model structure and to some extent on the implemented methodology as well. It is also mentioned in section 2.1 that only processes affecting the initiation of stream networks are included in the model structure. Given such limitations, how plausibly can the model in its present structure as well as the parameter estimation technique be implemented for achieving the intended objective of the work, i.e., handling the ungauged basin problem?

Answer: We are aware of the limitations of the model as well as of the methodology. We do not think that we have found the holy grail to solve the ungauged basin problem. Nonetheless the presented approach can contribute to handle the problem. See also P.862, L.14 – P.863, L.6. where we try to balance the results and the mentioned limitations. However, we agree, that the paper lacks some explanations (see general comment).

Comment 3

P 854, L14-16: It is not clear how the ‘adequacy’ of the goodness of fit measure implemented in the work was judged. What are the other methods that were tested and how was the evaluation performed? Also, the Kappa goodness of fit statistic is not described adequately. It is used to evaluate the model performance and therefore enough details should be presented to enable readers who are not familiar with it understand how it works.

Answer: We agree that this section is rather sketchy. We will rewrite the section and deliver more information, in particular on the Kappa statistics (which we omitted since we thought this is generally known)

Comment 4

Page 857, lines 13-14: Is that not an obvious consequence of the period over which the parameters were estimated?

Answer: Yes it is. We appreciate the indication and will rewrite the section.

Comment 5

P 859, first paragraph: It is indicated how the stream network is simulated by the model and that the approach can not actually simulate the complete network. Does not this then put the viability of the method in question? If it is not able to simulate the network, why try to compare the simulation with the actual network and go as far as exploiting it to estimate model parameters?

Answer: In this section we try to explain why the Kappa values are rather low. This is due to a combination of the model structure which can not actually simulate every cell of the stream network and of the nature of the Kappa calculation. We hope that this will become clearer when we deliver more information about the calculation of the Kappa statistics.

Despite the low Kappa values, it seems that the main features of the network can be represented (see figure 3). Thus, we think that we still can use the method to gain information about the parameter. Especially if it is less important to reach absolute high Kappa values than to detect significant differences between the individual parameter sets. However, if we go to bigger catchments it would be necessary to include a stream flow routing.

Consider this point from a process standpoint, it is obvious that subsurface flow to a stream is very variable (see for example recent publications by McGlynn’s group) and only some proportion of the stream network actually receives water, which is simulated by the model. The cells in between are mapped as streams, since water flows downstreams, but this is not

simulated by the model. Therefore, the mapped streams are usually larger than the simulated ones.

Comment 6

P 859, L 9-15: Not sure if I have got what the intended message is. It would also have been good if sensitivity of the model performance in terms of simulating the runoff with respect to those parameters estimated by the approach had been investigated.

Answer: This again is related to the calculation of the Kappa statistics. We hope that this we will become also clearer when we deliver more information on the calculation of the Kappa statistics.

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). Thus we suggest for the revision to look more into the processes of stream network simulation (as suggested by reviewer 2) than doing a complete uncertainty analysis where additional methodological issues would occur (what are behavioral and what are non-behavioral simulations when using Kappa statistics as objective function). In addition, a complete uncertainty analysis would remove the focus away from the main idea of the paper.

Minor comments

Figures 3 and 4 are not legible. Would be good to redraw them with better contrast between the different lines

P 848, L 19: remove the 'to' at the beginning of the line

P849, L15 and P850, L20: '...representative of...'

P851, L23: replace 'than' by 'then'

P852, L5: '...capable of calculating...'

P856, L21: replace 'respectively' by 'or' or reword the whole statement differently.

P856, L26: replace the comma before 'however' with a period.

P857, L24: suggest inserting a comma and 'which is...' after 'overestimated'.

P858, L1: 'satisfactorily' instead of 'satisfying'.

Answer: We appreciate the editorial notes. All notes will be included.