

***Interactive comment on* “Explicit simulations of stream networks to guide hydrological modelling in ungauged basins” by S. Stoll and M. Weiler**

Anonymous Referee #1

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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.

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Specific comments

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?

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?

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.

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

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?

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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.

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'.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 847, 2010.

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