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Hydrology and Earth System Sciences Discussions

HESSD

Interactive comment

Interactive comment on "On the potential of variational calibration for a fully distributed hydrological model: application on a Mediterranean catchment" *by* Maxime Jay-Allemand et al.

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We would like to thank the Reviewer for careful reading. We gratefully consider all Reviewer's comments and suggestions (see below in italic).

Major comments :

1.The methodology is quite well described but lacks for deeper evaluation. The results

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section is only one page and the discussion is reduced to less than 20 lines. I am sure that the study could benefit a lot from a more comprehensive analysis of this new methodology. For instance, some hydrographs could be included to show how the model behaves at the different stations depending on whether they are used or not in the calibration process. Also, do some stations always show good/bad performances? It lacks some time series to get an idea of interannual and spatial variability, flood severity (Qmax/Qmin).

These remarks are consistent with the remarks by Reviewers 1 and 2. Thus, we extend the analysis and the discussion. The extensions in progress are summarized below. 1- Previously, both calibration and validation have been performed using the same criterion (Nash-Sutcliffe (NSE) norm). Now, for validation we use both NSE and another criterion called KGE. This should provide a more reliable evaluation, because this criteria is different from the one used for calibration. 2- We analyze the results by periods and by stations as suggested by all Reviewers. 3- We focus on particular flood events and evaluate the model performance at stations using hydrograph and other event based criteria such as the temporal position of the modeled and observed flood peak, its intensity, etc. 4- More details are provided on the climatic properties of the Gardon watershed, such as the discharges quantiles at the gauge stations, the annual and seasonal average precipitation, the hourly rain intensities quantiles for different locations, etc. That helps to characterize the chosen events and to enhance the discussion.

2. Mathematical notations should be carefully revised as they are not always defined and sometimes not consistent (see following comments). This is very confusing and does not help the reader to fully understand how the method works.

We tried to combine classical notations from two different fields: hydrology and data assimilation. The notations are revised according to the Reviewer's comments.

Minor comments :

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3. P2L20. Do the authors know if there is any effort in the community to relate parameters of lumped or conceptual models to physical characteristics (e.g. average slope of the basin, concentration time...)? This could have important implication for the extension to ungauged basins.

Many attempts have been made to relate the parameters of conceptual lumped models to the physical properties of watersheds. For example, one may cite [Lobliegeois Florent, IRSTEA, PhD Thesis, 2014]. In this work, the relationship between the parameters of the conceptual lumped model GR5H and catchment indicators are sought. It has been concluded that no obvious relationship exist for the capacities of production and transfer reservoirs. However, the routing scheme parameters are correlated with catchment properties such as slopes, surfaces and hydraulics distances. One can find some other publications on this subject, such as [Johansson, Barbro. "The relationship between catchment characteristics and the parameters of a conceptual runoff model: a study in the south of Sweden." IAHS Publications-Series of Proceedings and Reports-Intern Assoc Hydrological Sciences 221 (1994): 475-482.], [Post, David A., and Anthony J. Jakeman. "Predicting the daily streamflow of ungauged catchments in SE Australia by regionalising the parameters of a lumped conceptual rainfall-runoff model." Ecological Modelling 123.2-3 (1999): 91-104.], [Seibert, Jan. "Regionalisation of parameters for a conceptual rainfall-runoff model." Agricultural and forest meteorology 98 (1999): 279-293.], [Wagener, Thorsten, and Howard S. Wheater. "Parameter estimation and regionalization for continuous rainfall-runoff models including uncertainty." Journal of hydrology 320.1-2 (2006): 132-154.]. The common approach to solve this issue is called 'regionalization'. This approach is based either on relationship between parameters and catchment indicators, or on the spatial proximity of the catchments. One mat cite [Oudin, Ludovic, et al. "Spatial proximity, physical similarity, regression and ungaged catchments: A comparison of regionalization approaches based on 913 French catchments." Water Resources Research 44.3 (2008).], [Odry, Jean, and Patrick Arnaud. "Regionalisation of a distributed method for flood quantiles estimation: Revaluation of local calibration hypothesis to enhance the spatial structure

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of the optimised parameter." EGU General Assembly Conference Abstracts. Vol. 18. 2016.], [Odry, Jean, and Patrick Arnaud. "Spatial disaggregation of a nationwide flood frequency analysis method." EGU General Assembly Conference Abstracts. Vol. 20. 2018.]. Unfortunately, the results presented in these papers are not generic, i.e. are relevant only to the models considered in these papers.

4. P4L25. Is there any name or reference for the radar precipitation estimates provided by Météo-France?

We use “ANTILOPE” radar rainfall product provided by Météo-France. This product corresponds to the re-analysis of the direct rainfall radar estimation with in-situ observations. This is to be mentioned in the revised version.

5. P6. It appears to me that q (from transfer function, Eq. (8)) represents the runoff, while Q (from the routing model) represents the discharge in rivers. It is not clear in the text (L7 and L19).

6. P6L12. “Assuming Pn is the impulse function”: isn’t it Pr?

We define notations more carefully.

7. P6L22. Tau_i represents the runoff (or discharge? See previous comment) delay from node i-1 to node i. In a drainage network, a particular node i may have multiple direct upstream nodes i-1. Does Eq. (9) mean that all nodes i-1 flowing into node i share the same velocity and distance to node i? Rather, I would have defined v_i and d_i as the velocity in node i and the distance between node I and node i+1. Please correct me if I am wrong.

You are correct.

8. P7L20. What does “without the initial shock” mean?

We should probably better explain this in the revised version. In Lag and Route method, the elementary discharge q(t) is a discontinuous function which is zero for t<t_0+delay.

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It means the runoff from cell i arrives to cell i+1 as a 'shock'. If the Gaussian function is used, there is no discontinuity, i.e. runoff from cell i arrives to cell i+1 progressively. This scheme is more suitable for cell-to-cell implementation as it is more stable for direct modeling and it is absolutely necessary condition for the differentiability of the forward operator.

9. P7L26. Identifiability is not only due to scarcity of observations in space. It may also comes from the model structure (concurrent parameters) and from processes (concurrent variables).

This is true. We will mention this.

10.P16L5. What justifies the given bound values? Would the results be better without these bounds? In my opinion, it is better not to constrain parameter values except in case of numerical constraints. This is especially true for a conceptual model for which parameter values have no interpretable physical meaning.

The need for lower bounds is evident: neither reservoir capacities or velocity may be negative. Concerning the upper bounds, these are defined to preserve the model dynamics. For example, we choose 5 m/s as the velocity upper bound since above this value the system delay wont decrease significantly. For the production and transfer stores we set the upper bound to 1000 mm since higher values do not drastically change the model dynamics (reservoir remains close to steady state). In practice, if the upper bounds are very high (which is equivalent to the absence of such bounds), the calibration results are indeed better, however the validation results are noticeably worse.

12. P23Figure6. Parameter values are very different whether P1 or P2 is considered as the calibration period. What are the implications on the stability of the calibration (similar performances with P1 or P2)?

This question is not clear. If the Reviewer means 'What are the implications on the

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stability of the validation' then the answer should be clear if the model performance is accessed as described in answers to major remark 1.

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