Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2019-331-AC2, 2020

Hydrology and Earth System Sciences Discussions
Open Access

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.

Maxime Jay-Allemand et al.

maxime.jay-allemand@irstea.fr

Received and published: 15 January 2020

Answer RC2, Received and published: 10 December 2019

We would like to thank the Reviewer for the careful reading. We gratefully consider all Reviewer's comments and suggestions.

1. The abstract should be more precise and synthetic, both on the context and on the results of the study. 2. P1 L5 Replace "accounting for spatial variability of the rainfall and the catchment properties, based on the radar rainfall observation inputs"

Printer-friendly version

Discussion paper

by "accounting for spatial variability of the catchment properties and the rainfall, based on the radar rainfall observation inputs". 3. P1 L15 "scalable" has not been defined yet.

This remark is consistent with the remark by Reviewer 1. We agree that the abstract has to be be rewritten more precisely. It must emphasize the objectives, the novelty and the results of this study. Scalable algorithm is the one able to maintain the same efficiency when the workload grows. The definition is added.

4. P3 L12 and P17 L9-10 The calibration of a distributed hydrological model using variational methods including the adjoint model has already been tested, at least several years ago in two PhD Thesis using the MARINE model (References below). I think a thorough and well-documented review of the state of the art research in this topic is needed to emphasize the novelty of the present study. This remark is consistent with the remark by Reviewer 1. That is true, some papers on this issue had been overlooked. The bibliography is completed by the references suggested and some others. The similarities and differences with our present work are underlined. These include: use of a fully conceptual model, calibration using a very long assimilation window (continuous modeling over several consecutive years, rather than for one particular event) over large spatial areas (for example, metropolitan France in the framework of the vigicrue-flash service), which requires a memory efficient and fast code.

6. P6 L20 "This model is build on top of a digital elevation model, which defines the runoff directions between the routing nodes. How the runoff directions are defined? How many runoff directions are allowed for each cell: 4 or 8? Is there a fill-sink algorithm?"

The runoff directions are defined according to a digital elevation model with 1 sq.km. resolution using the runoff forcing over hydrographic network. Runoff direction are chosen according to the maximum negative slope involving 8 directions. The modeled catchment (Gardon) cumulated surface map have also been carefully examined to ensure that every cell is connected to a downstream cell.

7. P8 figure 1 The model doesn't include any representation of the surface flow? It is quite surprising for flash flood simulation. Even if the authors mentioned that for the Gardon catchment "the water circulation appends mainly underground" (P11 L20), this is likely not to be the case for all the catchments prone to flash flood. Moreover, even if the dominant process in the generation of runoff is not the Hortonian one, surface runoff can also be generated by soil saturation. Can the authors comment on this?

The current model is very simple. The integral flow including surface, subsurface and deep underground, is simulated by the production and transfer reservoirs at the cell level, and by routing scheme at the inter-cell level. At the cell scale, the rainfall-runoff model contains only two operators (the production and the a transfer stores). These operators are conceptual with no obvious physical meaning. Currently, the model conceptually describes the global hydrological process, rather than its physically meaningful components. The nonlinear production and transfer stores inherited from the GR model family are known to represent well the integral hydrological processes and have been used by many authors (Perrin et al., 2003, Mouelhi et al., 2006...). We agree that for more accurate simulation more operators can be used, however it is not warranted that the more complex system can be eventually calibrated. Answering this question is not the purpose of this study. The cell-to-cell routing scheme suggested is also very simple, with just one parameter (the velocity). The estimated velocity may not be relevant for the full range of discharges. Finally, the parameter estimates depend on the calibration criteria (the NSE criteria promotes high flows) and our model simulates only the global and dominant process.

8. P9 L4 It is quite confusing to use the same letter P both for precipitation and for the parameter vector even if one is in capital letters and the other not.

We take this remark into account.

9. P10 equation (20) the "T" is not in the right place, I assume it should be the limit of

the integral.

We take this remark into account.

10. P12 L6 "A model warm-up of one year long is performed before starting the simulations." Did you test the impact of the duration of the warm-up period on the simulation results? Is the initial state of both reservoirs completely "forgotten" after one year of simulation?

Warm-up period is required for the system to approach the steady-state, which helps to remove the impact of the initial condition on the calibration results. We assume that one year is enough, according to recommendations by [Claude Michel et al., Technical report, Cemagref, 1989]. Moreover, the Mediterranean catchments are subjected to long drought periods during the summer which resets the reservoir (stores) states.

11. P13 L10-13 From a chronological point of view, it would be more relevant to calibrate on P2 and validate on P1 for forecasting purpose. Indeed, if there is a trend in the data, you will miss it when calibrating on P1 and validating on P2. Did you see any impact on your results?

Both have been performed, i.e calibrating on P1, validating on P2 ; calibrating on P2, validating on P1. We may add an extended analysis of both calibration-validation experiments by periods and by stations.

12. P14 L15 "One can see that the model spatial predictive performance is also better if the distributed calibration (red) is used, with one exception" on fig. 4 right. There is also one exception on fig. 3 left (rank 8). Can the authors comment on these exceptions: any reasons? Maybe related to the catchment or the period? For the same reasons, it would be interesting to mention in the fig. 3 and 4 not the rank which is obvious but the catchment and the calibration period in order to see if there are any correlations between the performances and the calibration sets (also see previous comment on that aspect)

This remark is consistent with the remark by Reviewer 1. The chosen presentation style could be a good choice for multiple catchments, but we agree that for a single catchment one should add the results station by station for both periods. We discuss the station by station performance and analyze the climatic condition and the catchment properties in a revised version.

13. P14 L16 "This depends, however, on the spatial variability of the test signal (rainfall)." It would have been interesting to correlate the performances with the spatial variability of the rainfall, for instance using Zoccatelli et al. (2011) indices. This could also have helped justifying the analysis P14 L25 "This effect can be attributed to quite a different rainfall pattern over the reference periods."

Particular events to be studied at the validation stations (hydrographs will be added, other criteria will be computed). Moreover it is interesting to verify if the calibration results are dominated by one particular event. For that, we compute a sliding NSE criteria over the full calibration period.

14. P14 L28 "calibrating the model independently for different hydrological regimes" or maybe calibrating the model using a data set including the different hydrological regimes?

No, we mean exactly "calibrating the model independently for different hydrological regimes". The idea comes from the approach known as 'pooled analysis' in statistics.

15. P16 Table 2 The parameter ct is presented as the capacity of the transfer reservoir (P4 L32) why isn't it in mm as cp?

Ct is scaled with respect to the simulation time-step to make it time-step independent. We recently revert this change, since it is more natural to have this quantity in millimeters. This has no impact on results. However the model user have to be aware that in the latter case the parameter values has to be re-scaled if a different time-step is used.

16. P16 Table 2 and P17 Table 3 are never mentioned in the text.

Printer-friendly version

Discussion paper

Yes, it is a mistake.

17. P16 L3-5 "This poor hydraulic behavior can be partially compensated by the others model parameters during the calibration process, which may explain the extreme (equal to the upper bound) parameter values." I assume this refers to the value of 5 m/s for v in Table 2 and 3? What about 1000 mm for cp, is it the upper bound too? What does it mean for a physical point of view if you need a biggest production reservoir?

At the first step, certain bounds are defined for the uniform calibration 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 states will remain almost constant in time). Notice that the production store is a non-conservative operator that stores and evaporates a fraction of the rain (this rain is finally lost). In the revised version we extend the bound of the production store for the uniform calibration up to 2000 mm (optimal uniform value is around 1300 mm), however the velocity upper bound remains the same for the uniform calibration. Next we define the optimal bounds for the distributed calibration. We compute a box around the optimal values for each parameter. All experiments have been updated according these new bounds. Physically, the production reservoir models the rainfall storage and the evaporation on the soil surface. The stored rainfall quantity depends on the water saturation of the soil surface. A biggest capacity of the production reservoir means a biggest storage capacity of the soil surface. At each time-step, the stored rainfall depends on the current state of the production reservoir. However, the dynamic of a big production reservoir is alleviated with lower states variations in time: a big production reservoir likely operates in steady-state and stores a constant fraction of the incoming rainfall in time.

18. P16 L11 "hit" instead of "heat"?

It is ’hit’.

19. It would have been interesting to study the correlation of the value of the calibrated parameters ct and cp with the actual soil properties: storage capacities but also soil texture and soil depths. Of course GRD is a conceptual model but the calibration of the local routing velocity v clearly shows a distinct behavior of the drainage network. Is it the same for the 2 others parameters? Can the analysis of the correlation between ct, cp and the actual soil properties be instructive to improve the model structure or the calibration methodology? I think that could be one of the interesting contributions of the study.

We think that velocity is a “physical” parameter and its estimated value is relevant to the hydraulic properties of the catchment. The calibration shows that the velocity changes mostly (with respect to the background) in the main drains where discharge is higher. For other parameters, calibration results associated to different assimilation periods are quite different (though some similarities exist at the northern and southern parts of the catchment). It means that these estimated parameters depend on the rain patterns rather than on the soil catchment properties. However, these could be different for other catchments, longer assimilation periods, etc. We agree that the correlation between the catchment geomorphology (if known) and the estimated capacities could be instructive to improve the model structure, and this could be a subject for future research.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2019-331, 2019.