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Interactive Comment

Interactive comment on "Calibration and sequential updating of a coupled hydrologic-hydraulic model using remote sensing-derived water stages" *by* M. Montanari et al.

M. Montanari et al.

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We would like to thank the anonymous Referee #2 for his helpful and constructive comments. They provide a good opportunity to better explain the rationale behind our study. In the following pages we try to answer the main issues raised by the referee.

Major issues

In order to answer to the main concern of Referee #2, we agree that the data set that is used in the present case study does not exactly correspond to what you would expect to have in an ungauged or poorly gauged catchment. When we refer to “ungauged catchments” it is our objective to provide reasonable estimates of discharge and flood extent with a limited amount of data at hand. Our question would be as follows: assuming there is a need to provide prediction of flood extent in an ungauged catchment, what minimum amount of data do we need to collect in order to come up with useful and reliable predictions? To this we would answer the following: 1) in our case study, a continuously recording raingauge is considered as a prerequisite (an alternative would be to rely on the outputs of atmospheric models to provide the forcings – with the associated uncertainty expected to be very high); 2) a constant value of base flow (specific discharge recorded at another streamgauge within the region, alternatively an average mean winter discharge of a sample or gauges within the region - see comment below); 3) the geometry of the river channel and the associated floodplain (although LiDAR techniques are nowadays a useful tool to acquire fine topographic and bathymetric information without intensive field work; here the alternative would be the use of a global remote sensing-derived DEM such as the SRTM (cf. Schumann et al., 2008)); 4) a single flood event that is recorded via streamgauges in order to provide the data that allows calibrating both the hydrologic and the hydrodynamic components of a flood forecasting system; 5) remote sensing observations of floods in order to monitor effective rainfall amounts, thereby providing a sequential update of the stormflow coefficient of the hydrologic model. In other words, once the sequence of models is calibrated, only precipitation (via raingauges), a constant value of base flow (via streamgauges located outside the area of interest, i.e. the floodplain) and flood extent (via remote sensing observations) are needed to provide flood forecasts. Thus, we argue that our approach can be applied to a poorly instrumented catchment where no streamgauges are permanently installed and rainfall is the unique information that is continuously recorded. This was the main argument for presenting our methodology as a new tool in the framework of the PUB initiative. Nevertheless, we admit that the link is disputable since PUB refers to prediction of streamflow, which is not based on the availability of measured data and thus precludes local tuning or

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calibrations (PUB Sciences and Implementation Plan, 2003). Thereby, we fully understand the comment of Referee #2 and we suggest not to mention the PUB initiative in the final text version.

The answer to the second remark of Referee #2 is closely related to the previous one. There is no doubt that, in presence of permanent stream gauges at the outlet of the basin, the assimilation of discharge data would be preferable with respect to a hydrologic-hydraulic model coupling and that it would provide better results, but it does not fall within the working hypothesis of this study. As a matter of fact, we suppose that in our test area the discharge data are available only for a single well-documented flood event. We demonstrate that once we measured the discharge at the upstream boundary of the hydraulic model for one flood event and used the corresponding data to calibrate both the hydrologic and the hydraulic model components, we do not need this information anymore and the approach can be applied to any other flood event that is observed with SAR imagery. Hence, this approach must not be viewed as an alternative to the use of discharge data, but rather as a helpful methodology in case of a lack of discharge time series. Probably the doubts of the referee come from Sec. 5.3 where the actual stormflow coefficient, calculated using discharge data recorded during the case study flood event, is used to validate the model results. We will highlight this aspect in the final text version.

The third major remark of Referee #2 concerns the use of constant base flow. We agree with the reviewer that the base flow is very variable during hydrograph peaks and we may have inappropriately used this term. As a matter of fact, there is no specific groundwater reservoir in our rainfall-runoff model, but rather a pre-event component (supposed to be constant) and an event component. The latter represents the amount of runoff that is generated during the storm event. The water volume that is obtained by merging the flood extent information extracted from remote sensing observations with the terrain geometry includes pre-event and event water, thereby allowing the correction of the total runoff simulated by the model (total runoff = pre-event runoff +

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event runoff) via a sequential updating of the stormflow coefficient (total runoff = pre-event runoff + c * total precipitation). Further clarifications will be added in the final text and the term “base flow” will be replaced by the term “pre-event flow”.

The last major concern of Referee #2 is about the use of a floating point variable as number of reservoirs. In our study we do not mean to represent the basin as a physical cascade of reservoirs, but rather to calculate the discharge at the outlet through the equations that are the basis of the Nash cascade. For this reason we decided to assign a floating value to the number of reservoirs. This issue will be clarified together with the detailed description of the Nash cascade in the final text version.

Minor issues

We will fully address all the minor issues in the final response. Here we would like to shed a light on those we believe to be important for immediate comment.

Referee #2 states that “maximizing the Nash-Sutcliffe efficiency is the same as minimizing the RMSE” and the use of the same performance criterion for both hydrologic and hydraulic model calibration would be more consistent. We totally agree but we would like to explain our choice. We decided to apply the Nash-Sutcliffe criterion to assess the hydrologic model performance because it is widely used among the hydrologist community, thus its value provides a straightforward assessment of the model skill. Moreover, this criterion is well suitable for a flood hydrograph evaluation, since, in this case, it gives more weight to the highest values of discharge in the performance calculation. Although we agree that the consistency of our evaluation procedure is a moot point, we believe that modifying it now will generate a very large amount of calculations and will not impact the results and the value of the presented methodology.

We completely agree with Referee #2 about the causes generating equifinality. It is due not to correlated model parameters, but to a compensation phenomenon between parameters, which lead to multiple parameter combinations giving similar results.

The local evaluation helps to understand the reasons why it is not possible to detect a best value of stormflow coefficient. From a visual analysis of the plots represented in Fig.7a and b, two peculiarities are observable. 1) For almost all the cross sections, the range of stormflow coefficients giving simulated water levels that are within the uncertainty interval of SAR-derived water level estimates is quite wide. This is a consequence of the fact that the spread of the water lines simulated using different values of stormflow coefficient has the same order of magnitude than the uncertainty interval that is related to the estimation of the water levels from the SAR images. 2) Comparing the ranges of stormflow coefficients corresponding to different cross sections, the intervals do not overlap over the entire reach whereas they should unless major inflows were not considered. As a matter of fact these results might appear incoherent because in theory there should be a single value of stormflow coefficient. These incoherencies are probably due to local errors and uncertainties in the model and/or in the dataset.

References:

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