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Hydrology and Earth System Sciences Discussions

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

Anonymous Referee #2

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The concept that this paper introduces, using remotely sensed water stages to update the state of a hydrologic model, is interesting and has, to my knowledge, not yet been published. The results seem to be promising. However, at this point, there are a number of issues that need to be resolved before this paper can be published.

Firstly, on page 3215 the authors suggest that their approach can be used in the framework of PUB. I completely disagree with this. If one looks at what is need to apply this approach (as the authors explain: 3-D geometry of the floodplain, hydraulic pa-

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Interactive Discussion

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rameters, …, plus discharge data to calibrate the hydraulic model!), this is an approach that cannot be applied in an ungauged or poorly gauged catchment at all! The authors need to justify their PUB-statement much stronger or remove it.

This brings us to my second major remark. The authors rightfully discuss the possible use of remote sensing for hydrologic model updating. But then (on page 3217) they immediately discuss their hydrologic-hydraulic model approach. One issue that has completely been left out here, while it is very relevant, is the use of discharge data to update the state of hydrologic models. This is important because for this approach they need discharge data to apply and/or calibrate/validate their hydraulic and hydrologic models. Thus, in a test area where there are no discharge data available, the approach cannot be applied. Which means that, if discharge data are available, they might just as well use those data for assimilation into their hydrologic model. For a hydrologist, this makes more sense than using this coupled hydrologic-hydraulic system: it is much easier to do, discharge data are available more frequently than remote sensing images, much less hydraulic data are needed,… The approach suggested by the authors is not easy to apply for a hydrologist! In the last five years or so, a couple of papers have been published on the use of discharge data to update the state of hydrologic models. What needs to be done in this section of the paper is to include a discussion on the use of discharge data to update the state of a hydrologic model, and a strong justification of why the approach in this paper is better than using discharge alone. This is something that really needs to be done before the paper can be published.

We also need a better justification of the use of constant base flow. In north western Europe, base flow is a large component of the stream flow, and it is also very variable during hydrograph peaks. This approach assumes it is not. This may actually be one reason why the modeled and remotely sensed floodplains are not matching. Please provide a stronger justification of the constant baseflow approach.

And also, on page 3229: the number of reservoirs can be a floating point variable? That means that, for example, 3.2175 reservoirs are used to model the surface runoff.

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That is physically not very realistic. Please provide a thorough justification for this or correct.

Some minor issues are::

After equation 1, please add units to the explanation of the variables.

Page 3221, please explain the three-parameter stormflow coefficient model.

Page 3223: please explain what “normal depth” is.

Page 3226 and afterwards: maximizing the Nash-Sutcliffe efficiency is the same as minimizing the RMSE. This will thus not impact the results, but why was, in the calibration procedure, not the RMSE used in the calibration of the hydrologic model, similar as the calibration of the hydraulic model. This would be more consistent.

Page 3227: please add units to the explanations of Qobs and Qsim. Also, the Nash criterion does NOT represent the percentage of the observed discharge variance explained by the model. That would be the determination coefficient multiplied by 100. The Nash criterion describes how a model performed as compared to a knowledgeless model, that only reproduces the mean of the observed streamflow. A positive number indicates it does better (1 is a perfect fit), a negative number indicates that it does worse. Also, after equation 5, please add units to the variables.

Page 3228, line 11: part 4.2 should be section 4.2.

Same page: equifinality is not in the first place caused by correlated parameters (certainly not for the models used in this study), but by nonlinear model structures, which cause multiple parameter combinations to lead to similar results.

The discussion in the second paragraph of page 3232 is unclear, especially from line 19 on. This needs a better explanation.

Page 3233: why would the study be controversial? Right after this sentence the authors state that the data set used in the calibration is not unrelated to the model output.

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Figures: please modify “dotty plot of” into “relationship between” or something similar.

Figure 5: please replace “ground” by “Surface elevation” or so.

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