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Hydrology and Earth System Sciences Discussions

Interactive comment on "Assimilating SAR-derived water level data into a hydraulic model: a case study" *by* L. Giustarini et al.

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First we would like to thank G. Di Baldassarre for his helpful review and for his recommendations and suggestions.

In the following we will address the specific comments outlined in the review.

1. The period to which the measurements of water level and discharge refer is rather long (1996-2010) and indeed during that time the geometry of the river might have changed. The geometry of the river described by the 144 channel cross sections refers to the year 2001. In this year topographic surveys were carried out and a LiDAR scan of the surrounding floodplain took place. It is important to note that the SAR-observed

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flooding event occurred in January 2003. All the cross sections with available measurements of simultaneous water level and discharge values were analyzed, comparing the cross section of the hydraulic model with the cross sections observed during each discharge measurement campaign. No significant differences were found, arguably due to the fact that the considered cross sections are located at bridges where the bed is generally stabilized. There is no evidence that points towards significant changes in riverbed geometry. Hence we adopted in this case study the assumption of temporally stable river geometry. With respect to this point a discussion will be added to the revised version of the manuscript. Concerning the comment on floodplain contribution, for the Alzette River and for the considered flood event, previous studies have shown that the floodplain does not play a significant role in the flood hydraulics (Hostache et al., 2009; Montanari et al., 2009)

2. In the considered case study, we first tested the assimilation of water levels into a model with the same Manning's values as in Montanari et al. (2009): one value for the channel and one for the floodplain. However, afterwards we decided to make the best possible use of the available data to reduce potential sources of errors originating from the model: structure errors, like 1D flow approximation and errors in the geometry, and parameter errors, like Manning's roughness value. We agree that our calibration approach with 4 different Manning values could suffer from equifinality but we considered it as an efficient way to reach a better local fit between simulation results and stream gauge measurements. As a matter of fact, an effort will be done to better clarify and support the calibration procedure in the new version. This point is also addressed in the answer to D. Yamazaki's review.

3. As already mentioned before, for this specific flood event the contribution of the floodplain is not particularly significant. Therefore, the use of the Manning's coefficient derived by Montanari et al. (2009) does not particularly influence the results. A discussion on this topic will be developed in the resubmitted version.

4. Thanks for the idea to extend the application to larger rivers and to test the appli-

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cability of the assimilation scheme to other case studies. In particular, we share G. Di Baldassarre's opinion that the proposed assimilation scheme has the highest potential for model improvements in large river systems that are poorly gauged. This idea will be mentioned in the new version, including a reference to Schumann et al. (2010). Hence it will be interesting to test the approach with freely and globally available space-borne data in order to check the performance of the proposed assimilation method in different regions and larger basins. In particular, we hypothesize that in larger river systems the dominating effect of the boundary condition is reduced and this would indeed favor more persistent model improvements through data assimilation procedures.

5. Thanks for the suggestion about the references, which will be added to the introduction to make it more complete.

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