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Interactive Comment

Interactive comment on "Multi-objective automatic calibration of hydrodynamic models utilizing inundation maps and gauge data" *by* N. V. Dung et al.

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The authors present a multi-objective automated calibration procedure for a 1D flood model for a large area of the Mekong delta using gauged level records and ENVISAT SAR imagery. The calibration process is controlled by NSGAII and maximises two widely used objective functions: the NS efficiency and the F spatial performance measure for flooded area overlay.

The paper is well written and follows a nice and easy to read structure. In general I think the paper is a nice contribution and should be publishable after addressing some major comments. My main concern regards the authors' choice of the second objective function (see point 8) below):

1) In the abstract, the authors claim that 'calibration of hydrodynamic models is still underdeveloped' – to avoid confusion and misleading readers, I suggest the authors elaborate this statement, probably emphasizing that they mean multi-objective or Monte Carlo based calibration as is often performed with hydrological models

2) Please check reference Schumann et al., 2009b in line 25 on p. 9179. Should this be Schumann et al. 2009a?

3) L11P9180: How are these flood maps obtained? The authors should briefly describe which sort of algorithms were used

4) L18P9180: How large is the model domain? More detail about why a 1D model is the only feasible option should be given. For example, what about a simple, relatively coarse 2D model? Is it because of the complex micro-topography on the floodplain that the authors preferred a quasi 2D model built from a 1D structure?

5) Section 4.1: is 0.05 as an upper bound a reasonable value given the likely degree of compensation that might be needed to account for errors in the geometry and boundary conditions over this large model?

6) Section 4.3.1: the NS objective function performance depends largely on the quality of the model boundary conditions; can the authors comment on this effect?

7) Section 4.3.2: the F measure (F2 in the paper) is known to be biased towards large inundation extent (the performance increases with larger inundation and thus may lead to unidentifiable parameter spaces). Although no alternative for a spatial performance measure has so far been proposed in the literature it is worth noting this.

8) An alternative way to assess a (1D) model with a satellite image – and in this study this would have facilitated the assessment as there would have been no need to transform from a 1D output to a pseudo 2D (P9188) – would have been to take the width

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of the inundation at each cross section and then measure the inundated width at that same location on the SAR images thereby allowing a simple RMSE distance to be computed that is less biased and also less affected by a possible geopositioning error of the images. The use of F here (F2) is my main concern. The authors show in Table 5 that according to F1 (NS) their model calibrates well, so by trying to maximise F2 (the F measure) the authors may have introduced errors other than the model structural error given that F prefers larger inundations (which illustrates itself by lowering the dike elevations which increased F2). The final calibration shown in Table 6 demonstrates that trying to reach a maximization for both measures very much degrades the performance of the water levels in the channel (from 0.93 to 0.759), which is the actual capability of the calibrated 1D model.

Would the authors have gotten to the same conclusion about model structural errors by using a less biased inundation width RMSE???

9) P9187-9188: why this 'subjective' uncertainty introduction of modelled flooded cells in 2D? Why not simply use the DEM to create a 2D information on the number of inundated cells using the water level modelled in the channel, i.e. using a simple interpolation approach?

10) Section 5: the authors hardly mention the quality of the processed SAR data as a limitation in their calibration procedure

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 9173, 2010.