

Interactive comment on “Variational assimilation of remotely sensed flood extents using a two-dimensional flood model” by X. Lai et al.

Anonymous Referee #2

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The manuscript aims at assimilating a flood extent image into a 2D shallow water model. The data assimilation method used is based on the adjoint method (4D-var method). The subject is currently relevant. The authors write they "explore a novel way of utilizing" the satellite image by defining a "new cost function" which is wrong. After an attentive reading of the manuscript and the articles Lai et al JoH 2009, Hostache et al JoH 2010 (articles cited), it appears that the present manuscript reproduces exactly the same method, the same equations, the same numerical schemes (the redaction is the same), and very probably the same software than these two articles. Despite what the authors claim, they do not assimilate the flood extent but the water depth for each mesh cell. Therefore, the authors use the same kind of data as in the articles mentioned above but in an unrealistic way, see Lai et al 2009 (e.g. p 8), Hostache

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et al 2010. No uncertainty estimation (deriving from the DEM and the image accuracy) is presented in this manuscript despite its crucial aspect. When reproducing the two articles mentioned above, the authors should have read that the present naive approach is not reliable. Indeed, as shown by some contributions (yet cited in the present manuscript), the image must be analyzed before extracting reliable water depth values. Typically, some areas must be excluded (eg vegetation) and an uncertainty reduction method have to be applied, see e.g. Raclot 2006, Schumann et al 2008, Schumann et al 2009 .

In short, the "new cost function" proposed is only a different formulation of measuring the misfit between the water depth computed and "those observed" for all cells, hence not realistic . The data accuracy is not addressed here. The authors should (re)read the original papers they reproduce in view to understand this key issue and how to try to circumvent it.

In other respects, it is well-known (see e.g. Toro's book 2001, cited) that the HLLC finite volume solver the authors use requires a cut-off at wet-dry front. Then the flood dynamics, hence the water extension at image time, depends greatly on this numerical regularization. This crucial problem is not mentioned here. It is a well-known and difficult numerical problem which prevents to directly assimilate the flood extent using the present 2D shallow-water numerical model (even if it was a perfect observation).

In summary, the "novel way of utilizing" the satellite image presented is in fact an unreliable naive approach, demonstrating the misunderstanding of the authors of the crucial issues and a misunderstanding of the numerical model features used.

The only new aspect of the present study is the application of the (unreliable) method presented here to a new (?) real data set (Section 5). But, again, the authors demonstrate their misunderstanding since they use the (heavy) adjoint method to identify one (1) parameter (the roughness coefficient supposed to be uniform). Assuming, the model and observations reliable, the authors could identify this scalar value by running

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direct models only, see Figure 9.

In summary, the authors demonstrated they did not understand the key issues of the problem addressed nor the key points presented in the articles cited (in addition to reproduce very closely existing redaction and using the corresponding numerical model without understanding its advantages and limitations). This manuscript does not deserve to be published in any scientific research journal.

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