

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

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Comments from R. Hostache (Referee)

This paper proposes an interesting method for the direct 4D-Var assimilation of flood extents derived from Earth Observation (EO). The aim is really innovative and the applications are of great interest as EO-derived flood extents can be produced for many flood events in many part of the world. In my opinion this paper is worth too be published especially because of its interesting applications and because of its scientific qualities. I think nevertheless that some improvements are necessary before publication. The paper is well structured and the presentation of the results is fair. I think nevertheless that the English should be further polished and slightly improved some-

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times.

[Re: Thanks for your comments and time for this manuscript.](#)

The introduction is pertinent and rather well written. For author's information, there is now a new article from our group related to the assimilation of actual SAR derived water levels into a hydrodynamic model (in relation to the citation Matgen et al. 2010): Giustarini, L., Matgen, P., Hostache, R., Montanari, M., Plaza, D., Pauwels, V. R. N., De Lannoy, G. J. M., De Keyser, R., Pfister, L. Hoffmann, L., Savenije, H. H. G., 2011. Assimilating SAR-derived water level data into a hydraulic model: a case study. *Hydrol. Earth Syst. Sci.* 15, 2349–2365.

[Re: We will read and insert this reference.](#)

The methodology is relevant and mature in my opinion although I have some few concerns about the explanation given for the cost function. In my opinion, this part should be better explained and re-written in a clearer way. I found some paragraphs from pages 6934 and 6935 (end of section 3) a few confusing but maybe I missed or misunderstood something. First of all, the authors should motivate better the cost function formula. Especially one question that arises for me is: Is it mandatory to take account of the water depth  $h$  in the cost function? If not the cost function could be the deviation between the observed and the simulated flood extents:  $J = .5(A - A_{obs})^2$ . But maybe I'm wrong. Could authors please comment on this?

[Re: Yes. It is mandatory component for assimilating flood information included in flood extent data from our numerical experiments because the introduction of water depth can link the state variable with cost function using L2 norm in the framework of standard 4D-Var. If just using  \$J = .5\(A - A\_{obs}\)^2\$ , the adjoint model in variation method can not be driven. The optimization algorithm will not run.](#)

My other concerns are about the formulas for  $J1$  and  $J2$ . For  $J1$ , authors assumes that  $h_{obs}=0$  (Could also authors explain what "essentially hc" means). This is a technical

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solution for estimating  $J1$  and I have no problem with this. However, to my understanding, this assumption would lead to the following formula:  $J1 = .5h^2$  if  $h_{obs} = 0$  or  $J1 = .5(h - w * hc)^2$  if  $h_{obs} = hc$ . The formula proposed in the article for  $J1$  corresponds for me to the following assumption:  $h_{obs} = h$ . Another concern is about the proposed formula for  $J1$ . To my understanding the latter implies that  $J$  is the more penalized by cells for which the water depth is high (and of course  $w < 0$ ). Could the authors please clarify and argue on these points?

Re: After careful examination, we found there is a little bit confusing in our text of this version, which may lead to this concern. The weight  $w$  does not represent the certainty of observed water depth, but the certainty of a cell being wet deriving from observations. So this weight should be used to constraining the discrepancy of predicted and observed water depth (we do not state this in our text, we will revise them). Therefore, we can obtain  $J1 = 0.5(1 - w)^2(h - h_{obs})^2$ , in which  $(1 - w)^2$  is considered as the weight representing confidence of observed wet-dry status. When both predicted and observed cell statuses are wet ( $w = 1$ ),  $J1 = 0$ , that is equivalent with the assumption of  $h_{obs} = h$ . When the predicted cell status is wet but the observed cell status is uncertain of being wet or dry ( $0 \leq w < 1$ ), then  $J1 = 0.5(1 - w)^2h^2$  if  $h_{obs} = 0$  was assumed. According to our definition,  $J1$  is more penalized for those cells with low certainty of being wet, when the predicted cell status is wet (i.e. in  $\Omega1$ ).

For  $J2$ , authors assumes that  $h_{obs} = 2 * h$ . This is a technical solution for estimating  $J2$  and I have again no problem with this. However, to my understanding, this assumption would lead to the following formula:  $J2 = .5 * w^2 * (2h)^2$ . The formula proposed in the article for  $J2$  corresponds for me to the following assumption:  $h_{obs} = h$ . Another concern is about the proposed formula for  $J2$ . For every cell with simulated depth strictly equal to 0 ( $h = 0$ ),  $w^2 * h^2$  is equal to zero whatever the observation is. Is that not a problem as it would mean that if only few pixels have depth in-between 0 (excluding 0) and  $hc$  more or less only model overprediction penalizes  $J$ ? Could the authors please clarify and argue on these points?

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Re: We understood the reviewer's comments that water depth ( $< h_c$ ) in  $\Omega_2$  is considered to be "zero" when deriving flood extent map. But we still use the real water depth in our cost function (because water depth ranging from 0 to  $h_c$  is meaningful in computation). For  $J_2$  in  $\Omega_2$ , we have  $J_2 = 0.5w^2(h - h_{obs})^2$ , in which  $w^2$  is weight coefficient. If both predicted and observed cell statuses are dry ( $w = 0$ ), then  $J_2 = 0$ . For those areas covered by the remotely sensed flood extent in  $\Omega_2$  ( $0 < w < 1$ ), we obtain  $J_2 = 0.5w^2(-h)^2$  if we set  $h_{obs} = 2h$ . For this definition, we can find that  $J_2$  is more penalized for those cells with high certainty of being wet.

It is true that only few pixels (cells) have depths ranging from 0 and  $h_c$  (excluding 0) when the predicted status is close to the observation. In fact, there is also few active cells for computing  $J_1$  (those cells for which the predicted wet-dry statuses are wet, but the observed statuses are possible dry,  $0 < w < 1$ ).

However, it is not a problem for our assimilation. When flood extent is over-predicted,  $J_2$  is more penalized; but when flood extent is under-predicted,  $J_1$  is more penalized. After  $J$  is minimized, we reach a compromise between over- and under- prediction.

In the formula of  $J$ , could you explain what is exactly  $\alpha$ ? I do not understand why velocity suddenly appears?

Re:  $\alpha$  is the scaling parameter (weight coefficient) that weights different kinds of cost functions. It can be considered as a normalized parameter. When other types of observations (e.g. water depth or flow velocity data) are assimilated together, the component of cost function for flood extent observations should be properly scaled in Eq. (9) to respect an initial balance between different components of cost function. The  $\alpha$  in Eq. (9) is superfluous for this manuscript. We will drop  $\alpha$  and remove the relative words.

Could you explain as well how the cost function is computed when you assimilate punctual water depth hydrographs?

Re: For time-series data of water depth, we can also add another item for their assimilation,  $J_3 = \sum 0.5((h(t) - h_{obs}(t))^2$  into the total cost function  $J$ , say,  $J =$

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$$\alpha * (J1 + J2) + J3.$$

The result and discussion part is pertinent and rather well written. Numbering of figures (fig. 8 and 9 instead of 6 and 7) might be revised. The conclusion is good.

Please find below some other comments:

P6924 I21: eliminating errors is rather impossible in my opinion.

P6926 I6-10: Please split the sentence into two.

P6934 I12-15: Is the formulation “as how to” as used in the paper correct in English?

P6939 I14: If I am correct “set to” might be better than “set by”

P6942 till the end: there are incorrect reference numberings (figure 6/7 instead of 8/9). Could you please check?

P6943 I16-20: Misclassification can also occur. Could you please mention it

P6943 I23: I believe that there is a difference between a visual interpretation and a demonstration. Could you please rephrase the sentence?

Table 1 and 2: could you please use the same way of calling series in the two table: Either series A,B: : : or N, Qin: : :

[Re: Thanks for these detailed comments. We will revise or correct all above language problems.](#)

Figure 3: There are 5 time steps and 6 subfigures for each experiment. This is confusing.

[Re: The first sub-figure is for the prediction using guessed Manning’s roughness coefficients. The other five sub-figures are the results after assimilating the observations of Group A, B, C, D and E. We rephrased the figure caption as: “Comparison of the predicted and “true” flood extents at t = 1, 2, 3, 4 and 5 s for different simulations using guessed Manning’s n and by assimilating the observations of Group A, B, C, D and E.”](#)

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 6923, 2014.

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