

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

**R. Hostache (Referee)**

hostache@lippmann.lu

Received and published: 24 July 2014

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 sometimes.

C2674

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.

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? My other concerns are about the formulas for  $J_1$  and  $J_2$ .

For  $J_1$ , authors assumes that  $h_{obs} = 0$  (Could also authors explain what “essentially  $h_c$ ” means). This is a technical solution for estimating  $J_1$  and I have no problem with this. However, to my understanding, this assumption would lead to the following formula:  $J_1 = .5h^2$  if  $h_{obs} = 0$  or  $J_1 = .5(h - w * h_c)^2$  if  $h_{obs} = h_c$ . The formula proposed in the article for  $J_1$  corresponds for me to the following assumption:  $h_{obs} = h$ . Another concern is about the proposed formula for  $J_1$ . 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?

For  $J_2$ , authors assumes that  $h_{obs} = 2 * h$ . This is a technical solution for estimating  $J_2$  and I have again no problem with this. However, to my understanding, this assumption

C2675

would lead to the following formula:  $J_2 = .5 * w^2 * (2h)^2$ . The formula proposed in the article for  $J_2$  corresponds for me to the following assumption:  $h_{obs} = h$ . Another concern is about the proposed formula for  $J_2$ . 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  $h_c$  more or less only model overprediction penalizes  $J$ ? Could the authors please clarify and argue on these points?

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

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

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:

C2676

Either series A,B... or N, Qin...

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

---

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

C2677