Hydrol. Earth Syst. Sci. Discuss., 10, C4851–C4854, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C4851/2013/

Hydrology and Earth System Sciences
Discussions

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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Received and published: 11 September 2013

Versions of this paper have been reviewed before and not being an expert in 4D var assimilation techniques but rather remote sensing and hydraulic modeling, I'll focus my comments mostly on these two aspects. Also, comments on assimilation have been given by previous reviewers. As far as I can tell my previous concerns have not been really addressed in this submission to HESS (unless I missed it), so I will put the same ones again and expand a little to clarify my main concerns:

Review of the paper by X. Lai et al. 'Assimilation of remotely sensed flood extents using a two-dimensional flood model'

I think this paper treats a very interesting and timely topic, however many questions (re-

garding both assimilation and results) remain unanswered and I think further revisions are needed. I've listed my main concerns below:

- When the authors refer to water level from remote sensing, I believe they mean mostly indirect retrieval. In other words the focus is on the DEM and the inherent accuracy which of course impacts the results greatly since a remotely sensed flood edge is first retrieved and then fused with a DEM. I totally agree with the authors that in such a case, using only uncertain flood extent may be preferred. However, the situation completely changes and is turned around when water levels can be directly observed, such as from altimeter (laser and radar), the resolution of which is comparable with the MODIS image used by the authors. In this case, it might be much better to use direct water levels from remote sensing and as such their statements throughout the paper that water level accuracy is low and lower errors are achieved from assimilating extent, should be rephrased or removed.

- The variational methods are actually governed pretty much by 1) the successful derivation of 'uncertainties' in the observations and 2) the associated weight as clearly shown in their equations. I can see that this could be a preferred choice over other types of assimilation in the presence of uncertain data (points). However, it is bot clear to me what happens if many data points have extremely high uncertainty as is easily imaginable at flood boundaries as shown in their paper. Would the assimilation scheme not 'prevent' the model from flooding accurately in places where the data were wrong but flooding would have taken place. In other words, where there is a lot of emerging vegetation creating a high uncertainty situation at flood boundaries, what would happen to a simulation that attempts to accurately predict in those places.

- As I understand the authors, flood extent-related information was assimilated (also I do not necessarily agree with this statement in the paper, please see next comment). Maybe I missed something, but why not assimilate flooded area? I'm asking this question from a model performance point of view. A model might get extent right since this is usually governed pretty much by large topographic constraints but might fail to

predict inundated area patterns correctly because there are often micro-topographic features governing floodplain flow paths around small 'islands' or other features inside the inundated floodplain area that should remain dry but are flooded incorrectly by the model. Or maybe the authors mean 'area' and I completely misunderstood, in which case the term extent need to be replaced by area. If area was used, meaning every pixel present in the MODIS observation has a weight, how did the authors deal with the fact that many completely wet pixels inside the area of flooding (w = 1) are also very easily predicted by any model parameterization? This means that many of the weights of 1 do not help in constraining the parameter space during assimilation at all. However, I think the authors use extent information only and thus the next comment is of major concern

- The authors claim they assimilate flood extent but when reading the text I strongly disagree. To me, and this was stated in previous reviews of this paper, the authors use depth at extent, rather than the extent information itself. Assimilating extent, in my view, would be to assimilate location (x, y coordinates at flood edge for instance) and not flood depth at extent. Moreover, the assumption that 'Herein it is assumed that the observed and predicted water depths are the same in those overlapping areas between the predicted and observed extents. In those non-overlapping regions, different assumptions have to be made, de- pending on the specific location under consideration' (P 11194) is very unlikely to be correct, given my first comment related to dependencies of DEM accuracy. In this case the authors relate flood extent from both model and observations to modelled depths where both extents overlay and argue that this is must then be the observed depth. Am I correct in formulating it this way? If this is true then why assimilate at all since you argue that your model gives you the correct depth when extents agree, just a simple calibration on extent would be enough. I do not see the benefit of assimilation in this case. But please correct me if I'm wrong. I think the real challenge would be to assimilate observed water surface elevation (from altimetry or post flood field surveys) or water depth (if this existed) because micro-topographic structures (often not present in DEMs and therefore missed in models)

prevent extents from expanding but cause water depths (or stage) to rise. In your case, depth at extent from models and hence observations (since in your case the latter is made equal to the former by your assumption mentioned above) would nearly always be zero if extents overlap. This is in my opinion not a realistic formulation of the cost functional.

- It is not clear to me how the authors can argue that the results are improved by showing very physically meaningless roughness values for the floodplain after assimilation?? This is the illustration of 'getting the right answer for the wrong reason' I feel. I think this highlights only that either the model built or the observation data are quite bad. I assume the model built is fine and that it is really the data that is problematic here and forcing the model to use an extremely high roughness value to 'reach' the observed 'uncertain' extent? Is the MODIS largely overestimating the flood extent or is it due to the 'uncertainty' estimation?

- In some places, the language needs to be revised.

I feel the comments above need to be addressed thoroughly before this paper is acceptable for publication.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 11185, 2013.