Comment on hess-2021-125

Anonymous Referee #1

Referee comment on "Simultaneous assimilation of water levels from river gauges and satellite flood maps for near-real time flood mapping" by Antonio Annis et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-125-RC1, 2021

General comments:

This manuscript shows a case study with assimilation of real observations of satellite-derived flooded areas and local gauge observations to keep a flood forecast model on track. The paper is well written and the topic is relevant for HESS.

The authors use the (perturbed-observations) EnKF to update the state of a 2D hydraulic model, coupled to a hydrologic model, which provides the inflows as input the the hydraulic model domain. The hydrologic component and global model setup is fine from this reviewer's point of view, and the authors have done a good effort throughout the study.

However, after reading the manuscript, one is left with the impression that on the assimilation approach the authors have come with a "preconceived" plan without seriously considering standard assimilation possibilities that are already out there. It is my belief that studies that defend alternatives methods should always consider existing approaches for benchmarking. Otherwise, there is the risk that we are left with a bunch of alternatives and without enough information to decide whether it is worth (or better) to try one or another. Also, one is left with the impression that the overall results in this study could be rather different using alternative assimilation approaches. This does not invalidate the study at all, but it should be augmented with results from applying (assimilation) techniques that are already on the table.

The joint assimilation of observations from satellite and local river gauge time-series is of clear interest in this context, and it is natural that flood forecast feeds on as many observation types as possible. However, despite the effort put by the authors, the above issue plus a scarcity in the section for results is, in my opinion, a missing opportunity.

In the introduction, it would have been also nice also to refer, somehow, to the advances in Numerical Weather Prediction (NWP) to frame the context of the early warning systems.

It is not easy to select a starting point as best reference on assimilation in this field along the chain of available papers, ranging from general methodological assimilation papers (outside from hydrology) to more applied ones in this specific field of flood forecast. Perhaps, on the assimilation of real satellite-based flood extent observations with a 2D

model stretching over a number of rivers (some main rivers plus some tributaries), and using an upstream coupled hydrologic model for the inflow timeseries (as well as estimating online model parameters, as this study), the authors should look back to García-Pintado et al. (2015). From there, they could also further look into citing articles to go forward in time towards more recent studies.

For a more general context about flood forecast considering assimilation, it is also advisable to read the review by Grimaldi et al. (2016). Although this paper is cited here, the manuscript indicates the authors have not actually gone through the paper details.

Last but not least, it is surprising that despite the effort put into this study the authors do not show any spatial graphical output (despite they use a 2D model) for diagnosis of the assimilation. They could well show maps with, for example, assimilation increments at specific times, covariances, etc. These kind of 2D plots are very helpful to try to understand what is under the hood in the assimilation. This greatly eases the understanding and evaluation of what is working well or not.

Specific comments:

L45. Some of these studies do use a 2D hydrodynamic model. In summary:

Andreadis et al., 2007 : 2D

Matgen et al., 2007: 1D

Neal et al., 2009; 1D

Hostache et al., 2010: 2D

Matgen et al., 2010: 1D

Giustarini et al., 2011: 1D

García-Pintado et al., 2013: 2D

Mason et al., 2012: no DA but aimed to 2D

Andreadis and Schumann, 2014: 2D

What seems to indicate that the authors have actually not read these papers.

L57: Clarify here in which sense is the DA framework novel? Specifically, the EnKF has now a long history.

L77: The regular grid and simple IO formats do not make the model more "suitable" to DA than using an unstructured mesh or more complex (e.g. hierarchical) IO formats. This just allows for a simpler code.

L94 "assess maximum flood energy gradients". How is this relevant? How is energy coupled with the 2D flood model or used here? It is also unclear if the floodplain computational domain evolves with time along with model integrations or is preset, based on GFPLAIN.

L114: No uncertainty is taken into account in the rainfall input. It is worth to a) discuss briefly the errors in rain gauge data [e.g. the possibility of generating quantitative precipitation ensembles via Sequential Gaussian Simulations, etc.] and how the

uncertainty is propagated downstream in the forecasting chain, and b] some reference to coupling with [possibly ensemble] NWPs.

Section 2.2

L140: Satellite and river gauges give observations as water levels. It appear as more natural to use the forward operator H to map the model state into the the observation space.

Section 2.2.2

L155: "significant". Please reserve the term "significant" for its statistical meaning in manuscripts. This is just a threshold. Use simply "high".

Overall, I'm sorry to be so negative here. Why, not just use covariances for the simultaneous assimilation of several observations. This whole section [specifically weight the observation values based on inverse distance weight] goes against the whole spirit of the EnKF. If the authors believe their approach is better than using covariances to control the updating steps (as standard), they should at least put this into context to defend their approach.

The authors should look at approaches that have been deeply studied (as localisation and ensemble inflation), and refer to them. There is plenty of bibliography (mostly on the NWP field) on this. It is even better if they can actually implement an EnKF with localisation (and, possibly, inflation). Then use this as benchmark to evaluate if their approach actually makes things better/worse/different...

Further, to stop the model each time a new observation arrives, as it is described, seems rather ineffective regarding the assimilation of the river gauge continuous time-series. The authors could look at asynchronous assimilation approaches, so it is not strictly needed to stop the model as frequently.

Section 2.2.3

L205: provide some citation (i.e. to George Matheron) for Kriging.

Section 2.2.4 Model errors

L222 The Q^true should be Q^observed. Also, the inflow error is simulated as as white noise. This is hardly realistic and, beyond this, a more realistic noise should actually improve the assimilation. I believe this partly explains the lack of success shown by Figure 8.

Section 2.2.5

L242 2.2.6 should be 2.2.5.1

L248 Again, replace "true" by "observed". And, again, hard to believe the lack of temporal correlation in these errors.

L252 2.2.7 should be 2.2.5.2

L268. Which GIS algorithm?

L288 agricoltural -> agricultural

Figure 5: An issue with this plot is that from the manuscript it is unclear how often are the observations, and as this seems to be both an "observed" location whose information is assimilated into the model and a location with observations against which the model forecast is evaluated, it is difficult (if possible) to disentangle the forecast skill here, or how much of the assimilated run line comes from the gauge observations themselves.

Figure 8 & Section 4.2: The lack of persistence in the benefit in assimilation the satellite observation seems directly related to the white noise mentioned above. Consider time autocorrelated inflow errors.

REFERENCES

García-Pintado, J., Mason D.C, Dance S.L, Cloke H.L, Neal J.C, Freer J., Bates P.D. Satellite-supported flood forecasting in river networks: A real case study, Journal of Hydrology, 523, 706-724. DOI: 10.1016/j.jhydrol.2015.01.084.

Grimaldi, S., Li, Y., Pauwels, V. R., and Walker, J. P.: Remote sensing-derived water extent and level to constrain hydraulic flood forecasting models: Opportunities and challenges, Surveys in Geophysics, 37, 977–1034, 2016.