Interactive comment on "Assimilation of wide-swath altimetry observations to correct large-scale river routing model parameters" *by* Charlotte M. Emery et al.

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Emery et al. propose a new data assimilation scheme (AEnKF), to be used for assimilation of wide-swath altimeter information from the upcoming Surface Water and Ocean Topography (SWOT) mission. Given the large-scale nature and long-time scale revisit times, using an asynchronous scheme, that to the best degree possible utilizes time and space varying availability of information seems like a logical and useful choice, compared to more synchronous approaches such as classical EnKF. I consider this to be a useful contribution to HESS and very much in scope, and useful as a preparation for using SWOT in large-scale hydraulic simulations and forecasts.

I do have a number of comments that lead to my verdict that this paper requires major revisions.

My largest comments are a) the choice to update Manning's n (rather than a state); and related to this, b) the choice to only evaluate the assimilation performance on the basis of water levels (or depth). In most applications, the user will require a good estimate of the river flow (besides water levels), because river flow (a volume in time) controls availability of water for some process that is to be predicted by the model used, not just the water level. A hydrology-hydraulic model-cascade could for instance be used to provide inputs to water allocation predictions for the forthcoming weeks/months (requiring an amount of flow over a given time span), or an upstream boundary condition for a flood simulation of a downstream river stretch. For all such applications, an accurate volume per unit of time is required, not just a stage (except for a flood simulation in a steep area, where floodplain storage is negligible to event accumulated flow, but these are generally small streams, definitely not comparable with the size of the Amazon and its tributaries). I consider this a large (and unnecessary) weakness in the approach, with the additional risk that the Manning roughness will change to physically highly illogical values (which it in fact does in this study!), because e.g. either the water balance of the underlying hydrological simulations of ISBA are biased, or the channel dimensions are poorly defined. To become useful for typical applications, data assimilation should be as much as possible aimed at correcting the amount of water in channel sections, so that predictions after state updating can be made useful and reliable. This is now not proven. I find it a pity that the authors decided to apply this AEnKF on parameters with (As far as I can find in the text) the sole reason being that other authors already used it for state estimation experiments. This makes the study purely theoretical, as I don't really see how the experiments would ever be applied in a real-world case. The authors should at least show river flow as an additional benchmark variable and show how the 3 experiments affect the accuracy of river flow and discuss this result. My logical feeling is that discharge will be quite heavily impacted especially in experiment 3 where a bias is introduced.

Second point: I don't fully understand the zonal approach to updating Manning's n. To me it would make more sense to use an upstream-downstream relation in manning coefficients (e.g. update manning coefficient at location, as well as upstream and downstream) which could easily introduce a logical covariance between n values (rather than assuming everything to be independent). In fact, the full zonal approach with areas that may have very little relationship to each other suggests, that there is a 100% covariance across the zone. Why was this selected in this way (or would things become overcomplicated if done in a different way)? Consider discussing this in the last sections of the paper.

The English writing and sentence constructions are not everywhere up to standards. Please make sure the manuscript is reviewed by a (near-)native English person.

Detailed comments are listed below:

Introduction: there are many "however"s in the text. Some or many of these can be removed.

P. 3, l. 32 "at a coarser scale", please just describe the scale.

p. 4. l. 13: "gravitational drainage", do you mean groundwater outflow?

p. 4. l. 20. Replace "empties" by "spills"

p. 4, l. 29, Replace "fixes" by "results in"

p. 5 l. 2 (p.5): "…values between 0. 75 and 1 for smaller and mountainous tributaries…" I guess you mean 0.075 and 0.1 s m-1/3. The values you mention are ridiculously high!

p. 5. Eq. 1. Why is SOmax not simply 1 as it is only a way to scale values?

p. 5, l. 10 replace "as the cells approaches" for "towards"

p. 5, l 11. V(t) is not the surface flow, but the average cross-sectional flow velocity.

p. 5, eq. 3. S is not defined

p. 6, l. 3. "forcings are considered perfect". This is my point above. They never are and the assimilation should work to correct these forcings. In the case of ISBA this concerns errors in the water balance, and in CTRIP errors in the transport of mass and momentum through the channel network.

p. 6, l. 26. "white noise", is this a reasonable assumption? And if reality is different, how would it affect your results? Discuss this in Section 7.

p. 11, eq 17 and 18. Are these the selected zonal Manning;s n values? These are unrealistically high. Why are these selected in such a strange domain?

p. 11, 19. Describe briefly what your expected results are (i.e. why these experiments) on both water levels and flows!

Section 5.1. Describe what you I hydrological sense expect from the spinup time experiment. You can relate the expected required spinup to the time of concentration of the considered basin.

p. 13. L. 9, you only show spatial average results. Why not spatial patterns? That may reveal the locations where things go right / wrong.

p. 15, l. 16-28. I find this paragraph not very clear, it is not clear why the results behave so differently from zone 1,2,3. I was wondering if there is not simply too little water in the system to get correct results in this part of the domain? If you only change manning's n, you can never introduce new water or take water out of the system (see main comment).

p. 17, l. 20. Around this part you should definitely discuss the state updating versus parameter updating, and the water storage errors that you can never resolve with parameter updating.

p. 17, l. 31. I am very curious what kind of exceptional hydrological event you mean

here.

Some of the figures have too small fonts, please make the figures readible throughout the text.