EGUsphere, referee comment RC4
https://doi.org/10.5194/egusphere-2022-412-RC4, 2022

Comment on egusphere-2022-412

Anonymous Referee #3

Referee comment on "Assimilation of transformed water surface elevation to improve river discharge estimation in a continental-scale river" by Menaka Revel et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-412-RC4, 2022

This manuscript explores different strategies to assimilate water surface elevation (WSE) derived from satellite altimetry into the CaMa-Flood global hydrodynamic model. During the recent years, a large number of studies have demonstrated the potential benefits of assimilating WSE for various purposes, such as parameter estimation or river dynamics modeling improvement. One of the main difficulties relies on the fact that WSE from altimetry provides the water elevation based on a reference (geoid) that can differ from the model reference (because of DEM errors for example) and the induced bias may highly degrade the assimilation performances. An alternative consists in considering WSE anomalies instead of absolute values. Another one, introduced in this study, also normalizes WSE anomalies to account for possible errors in the signal amplitude. Here the authors explore those three strategies and their respective performances in reproducing river discharge over the Amazon Basin. This scientific question is highly relevant and the study is well conducted, which makes it worth publishing, especially in the context of the forthcoming SWOT mission.

Overall, the manuscript is well written and well organized. The methodology is clearly stated and the results are quite convincing, although the latter should be revisited to correct some mistakes and clarify some points, as explained thereafter.

Major remarks

1. The quality of figures 4 to 6 is quite bad and colors are hard to identify (in the maps and in the time series). Also the time series subplots do not correspond to the description in the results section (3.1.1, 3.1.2 and 3.1.3). Hence it is not possible understand, confirm

or refute the description of these plots, as well as the conclusions drawn (L309-323, L332-342, L351-362).

2. It is not clear to me which variables are included in the state vector $\boldsymbol{x}$. I understand from L509 that the prognostic variable of CaMa-Flood is water storage. On the other hand, it is stated (L188) that the state vector includes river discharge, WSE, flooded area, flood height and storage. Could you clarify this point? Also, in the latter case, shouldn't the observation operator $\boldsymbol{H}$ contain only zeros except for the column corresponding to WSE? Moreover, are all state variables (river discharge, WSE, etc.) converted to anomaly and normalized values as written in L193?

3. More importantly, I think that the analysis of DA performances when runoff forcing or bathymetry are biased (section 4.2) requires a bit more explanations.

- Model simulations are affected by biases (errors in absolute values) and by errors in dynamics. Decreasing the river bathymetry (by lowering the river bottom elevation) would lower the absolute WSE without impacting the flow dynamics, except if bank overflow occurs (flooding). If there is no flooding, I would have expected large impacts on the direct DA performances but no impact on anomaly and normalized value DA. Given that, do the results of Fig. 11b mean that the degradation of those two experiments is due to bank overflow? In addition, what can explain the very poor performances of normalized value DA method? Maybe some example time series could help better understand these results, as done for the previous experiments (perhaps as a supplement).

- Is there any possible explanation of the better normalized value DA performances with runoff bias and bathymetry error compared to performances with only bathymetry errors (panels b and d)?

- Finally, considering only one runoff (HTESSEL) to generate the ensemble reduces the dynamics variations between the members. Could this be a reason of the poor DA performances, especially with the normalized value DA method?

Each of the three DA methods can outperform the other two depending on the configuration, making the choice of the DA method quite difficult for further studies. I think providing more insight in these experiments might help readers better understand the pros and cons of each method.

Minor remarks

Fig. 1. In panel b, upper left square, it should be “Altimetry Auxillary Data”.

L134-137. “VSs with considerable variation in mean WSE compared to the MERIT Hydro (Yamazaki et al., 2017, 2019) elevation (expressed as riverbank height) were filtered through comparison of mean observations and riverbank heights.” What could be the cause(s) of such errors? Maybe the answer is given in L506-508.

L157. Is the river width from remote sensing available for every reaches of the river network? If not, how is it determined?

Fig. 2. I would suggest to add a legend in the time series, and maybe add error bars in observed WSE (from HydroWeb).

Eq. (2). Since $\boldsymbol{H}$ is linear, maybe it is better to write $\boldsymbol{H}\boldsymbol{x}_{\boldsymbol{k}}$ instead of $\boldsymbol{H}(\boldsymbol{x}_{\boldsymbol{k}})$.

L288. Sharpness and reliability are not defined.

L285. Nash and Sutcliffe (1970) and Kling and Gupta (2009) are cited several times, which is not necessary.

L313. It is written (but I cannot verify it) that the 95 % ensemble spread is improved until mid-2010, when the ENVISAT satellite was available. But this satellite is supposed to be available until 2012 (Tab. 1). Also, could you explain what an improvement in ensemble spread is? Is it a reduction of the spread?

L321. Considering numbers in L300-302, I would not say that “direct DA generally improved flow dynamic simulation to a moderate extent”: concerning river discharge, 8 % of gauges show an improvement while 43 % show a degradation.

L326. What could be the impact of choosing different time periods for observed and simulated WSE when computing the long term mean (and std)? For example if a multi-year drought is accounted for in one period and not in the other.

L336. It should be “improvements in r and ISS” not “in Dr and rISS”.

L348. Considering the quality of the figure and the color range, decreases in discharge correlation is not that evident in the Amazon mainstem.

L365. It is stated that the assimilation has very little influence outside the area of satellite observations. First, does the satellite coverage area correspond to the reaches downstream any VS? Second, shouldn't the localization method used here allow to correct river discharge upstream VS?

Fig. 7. In the caption of a, it should be "probability distribution" instead of "cumulative distribution". Also, it could be helpful to plot the vertical line at 0. Same remark for Fig. S5.

L385. "A large reduction in sharpness was observed in the direct assimilation experiment (Exp 1), mainly because the assimilation was conducted directly." I do not see the link here, could you expand a bit more?

L408. For river discharge, sharpness is also considered (L415).

L530. Indeed, a huge potential from SWOT is expected in this kind of study. But how to deal with the need to compute long term mean and std for the derivation of anomalies and normalized values?

L536. Water height in the river is approximately 50 % lower, not WSE.

Fig. 12. The range of the y-axis could be reduced.

Minor remarks in supplementary material

Fig. S1. Square and circle are inverted in the legend.

L45. It should be “Exp 2a and Exp3a”.

L57. What is Exp 3b?

Fig. S7. It is hard to see the effect of DA on low flows. Maybe consider a log-log scatter plot?