EGUsphere, author comment AC4
https://doi.org/10.5194/egusphere-2022-412-AC4, 2022

Comment on egusphere-2022-412

Menaka Revel et al.

Author 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-AC4, 2022

Referee #3

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.

Reply:

We would like to convey our acknowledgment to referee #3 for his informative comments. We will address all the comments in the revised text, and responses are provided below.

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).

Reply:

We would be grateful to referee #3 for raising this issue. When creating these figures, a mistake occurred. Therefore, we will revise figures 4-6 to correspond to the descriptions in sections 3.1.1, 3.1.2, and 3.1.3.

2. It is not clear to me which variables are included in the state vector 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 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?

Reply:

We would like to express our gratitude to referee #3 for the insightful comment. We found that the using same H (observational operator) in both equations 2 and 3 is confusing. Equation 1 is about the CaMa-Flood model time evaluation and equation 2 is the conceptual relationship of CaMa-Flood state variables with the observations. So, the x (simple x) vector consists of CaMa-Flood state variables. Equation 3 is the analysis equation for data assimilation, there we used WSE only for data assimilation. Therefore, we think H in equation 3 is a subset of H in equation 2. Hence, we will revise the H in equation 2 to be (curly H) and will revise the text to explain that we used only WSE in the LETKF analysis equation.

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).

Reply:

We would like to thank referee #3 for the valuable comment and suggestion. In these experiments, we try to assess the performance of DA methods in simplified error conditions in forcing and parameters of the model. We agree with referee #3 that including corrupted bathymetry will lower the absolute WSE. Hence, it affects WSE assimilation in direct DA.

But in the cases of anomaly and normalized value DA, the degradation of discharge accuracy is mainly due to the errors in the statistics (i.e., mean and standard deviation). These statistics were computed using long-term open-loop simulations. When the river bathymetry is corrupted the mean open-loop WSE will also be biased, and the standard deviation will be different. Hence the assimilation result will also be inaccurate.

- 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)?

Reply:

We would like to express our gratitude to referee #3 for raising this question. With current experiments, the statistics of the normalized value DA method should be much lower because of the lowering of bathymetry as well as reducing the runoff. Hence, we need to investigate the experimental setting again whether there is an error in preparing Figure 11. We will re-check Figure 11 and add some explanation about such a trend in these experiments.

- 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?

Reply:

We would like to express our sincere thanks to referee #3. We don't think the poor performance of the normalized value DA method in corrupted bathymetry or runoff error cases is due to low variation in perturbations. It is mostly due to the bias and differences in statics.

4. 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.

Reply:

We would like to appreciate referee #3 for the great comment. We will add some more recommendations and improve the current description to enhance these ideas.

Minor remarks

1. In panel b, upper left square, it should be "Altimetry Auxillary Data".

Reply: Thank you for your recognition of the error. We will correct the text accordingly.

2. 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.

Reply: We would like to thank referee #3 for raising this question. From our analysis, we found that most erroneous VSs are in narrower rivers at high elevations. There can be several reasons for these, 1. Non-nadir direction observations, 2. Errors in post-processing of VS (e.g., geoid conversion), etc.

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

Reply: We thank referee #3 for the question. Remote sensing river widths were used in the river reaches with river width > 300m (Yamazaki et al., 2014). A power law relationship of the average river discharge was used to estimate the river width for the other smaller river reaches. We will revise the text to reflect these ideas.

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

Reply: Thank you very much for the suggestion. We will revise Fig 2 according to referee #3’s suggestion.

5. Eg. (2). Since H is linear, maybe it is better to write Hxk instead of H(xk).

Reply: Thank you for the suggestion. Here H is more of an operator for converting simulated variables to observable variables. Not all the simulated variables are possible to observe. So, we would like to keep the equation as it is, but we will use two symbols for equations 2 and 3 because H in equation 3 is a subset of H in equation 2.

6. Sharpness and reliability are not defined.

Reply: Thank you for the suggestion. We will define sharpness and reliability in the methodology section.

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

Reply: Thank you. We will remove those citations.

8. 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?

Reply: We appreciate referee #3 for raising this issue. ENVISAT nominal period is 2002-2010 after 2010 ENVISAT track was changed. Therefore, ENVISAT data after 2010 was not used in HydroWeb. Hence, we will revise Table 1, we will add only the nominal periods used in HydroWeb for each satellite.

An improvement in ensemble spread is referred to as the reduction of spread or reduction of sharpness. We think "improvement in ensemble spread" is better be revised as an improvement in sharpness. Hence, we will revise the manuscript according to referee #3's comments

9. 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.

Reply: We would like to express our gratitude to referee #3 for pointing out this. It is evident that many gauges degraded their accuracy of river discharge by the direct DA method. However, some gauges such as Santo Antonio Do Ica and Sao Paulo de Olivenca improved their discharge accuracy. Hence, we will revise the text according to the comment of referee #3 to reflect that the river discharge estimated of some gauges show improvement.

10. 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.

Reply: We would like to thank referee #3 for the question. There can be not much effect of using different time periods for statistics for simulation and observations if the statistics are representative statistics of the long-term values. But if the statics are largely different from the observed statistics as shown in the biased runoff experiment case, the accuracy of estimated discharge by the anomaly and normalized value DA can be hampered. We checked sensitivity on the simulation WSE statistics where we found that the statistics calculated using simulation of 5 years or more are reasonably reproduced river discharges.

11. It should be "improvements in r and ISS" not "in Dr and rISS".

Reply: Thanking referee #3, we will revise the text.

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

Reply: We would like to thank referee #3 for the comment. We will improve the quality of the figures in the revised manuscript. We agree with referee #3 that the correlation in the Amazon mainstream is quite good. This may be because model parameters (e.g., bank full height, river bathymetry, etc.) are still very good on these river reaches.

13. 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?

Reply: We would like to express our gratitude to referee #3 for the great question. Here, we define the satellite coverage area as the river reaches located downstream of the most upstream VS in each tributary (green circles in Figure S1 indicate GRDC locations that were in the satellite coverage area). Hence, the satellite coverage areas cover downstream of all the VSs. Although using the localization method, the WSE can be updated without using any local observation. But in the upper reaches (narrow rivers with small catchment areas), the size of the local patches is small (Revel et al., 2019). Hence, assimilation efficiency can be lower in these upper reaches.

14. 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.

Reply: We would like to thank referee #3 for the suggestion. We will revise the caption of Fig. 7 and Fig S5 according to referee #3's suggestion.

15. "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?

Reply: Thank you very much for asking for calcification. Sharpness reduction means the reduction of the ensemble spread. When the assimilation was performed in real values the ensemble spread become smaller than it was performed in anomaly or normalized values. We will revise the text to reflect this meaning in the revised manuscript.

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

Reply: Thank you very much. We indeed calculated sharpness for river discharge as well even though we did not include it in Fig 8. The median sharpness values were shown in Table 3.

17. 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?

Reply: We would like to thank referee #3. In this study, we did not assess the possibilities of using anomaly or normalized DA methods for the real-time forecast or usage with short-term observation records. However, a representative estimate for SWOT observations can provide reasonable discharge estimates. We will assess the sensitivity of the observational statistics which can be used for the real-time/short-term forecast in our future studies.

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

Reply: Thank you very much for pointing out this. We will revise the text accordingly.

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

Reply: Thank you very much. We will revise Fig 12.

Minor remarks in supplementary material

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

Reply: Thank you very much. We will correct it.

2. It should be "Exp 2a and Exp3a".

Reply: Thank you for pointing this out. We will revise it.

3. What is Exp 3b?

Reply: We would like to thank referee #3. Exp 3b should be removed. We will revise it.

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

Reply: Thank you very much for the comment. We will revise Fig. S7 accordingly.

Reference:

- Revel, Ikeshima, Yamazaki and Kanae: A Physically Based Empirical Localization Method for Assimilating Synthetic SWOT Observations of a Continental-Scale River: A Case Study in the Congo Basin, Water, 11(4), 829, doi:10.3390/w11040829, 2019.
- Yamazaki, D., O'Loughlin, F., Trigg, M. A., Miller, Z. F., Pavelsky, T. M. and Bates, P. D.: Development of the Global Width Database for Large Rivers, Water Resour. Res., 50(4), 3467–3480, doi:10.1002/2013WR014664, 2014.