EGUsphere, author comment AC2
https://doi.org/10.5194/egusphere-2022-412-AC2, 2022

Reply on RC2

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-AC2, 2022

Referee #2

The authors investigated the assimilation of satellite altimetry to improve discharge. The article is excellent. The authors presented 3 types of altimetry assimilation, which are 1. direct, 2. anomaly, and 3. normalized assimilation, and concluded that to improve discharge it is better to just assimilate the water surface dynamics (3.) given that the model is relatively accurate. If the model is completely corrupted, the authors concluded that anomaly and/or direct assimilation are more effective.

Reply:

We would like to express our gratitude to referee #2 for his thoughtful comments. We will address all the comments in the revised manuscript and detailed responses to the comments are given below.

1. My main complaint about this manuscript is in section 3.1 (specifically 3.1.1, 3.1.2, and 3.1.3) where the figures (fig 4, 5 and 6) showed hydrographs that were different from the locations discussed in the text. Besides that, I have just some small comments that are specified below:

Reply:

We would like to thank referee #2 for identifying this issue. We found that some panels of figures 4, 5, and 6 are not matching with the description. Therefore, we will revise figures 4, 5, and 6 to be compatible with the description in Sections 3.1.1, 3.1.2, and 3.1.3.

2. Line 40. I think it is better to change GHM definition only to Global Hydrodynamic Models instead of Hydrological due to some features the authors discuss further such as "runoff as a forcing factor", "discretized river", "surface water dynamics", etc.. Line 77 and 519 could be GHM instead of global hydrodynamic models.

Reply:

We would like to express our gratitude to referee #2 for his valuable suggestion. We agree with referee #2 that our introduction should be more focused on global hydrodynamic models. We will revise the manuscript according to the suggestion.

3. Line 52. The authors could also mention Laser altimetry. The ICESat missions are also very used in academic research. Maybe instead of a radar pulse, can be a radar/light pulse or even an electromagnetic pulse.

Reply: We would like to thank referee #2 for suggesting introducing laser missions. So, we will revise the text in the revised manuscript to address the referee's comments.

4. As an alternative to the semi-variogram analysis to determine the spatial dependency weights, the authors could have used "backwater lengths in rivers" studied by Samuel (1989). It can give an idea of which river reaches are affected by WSE variations at the VS locations. It would be a good idea to compare both approaches in future studies (not now).

Samuels, P. G. (1989). Backwater lengths in rivers. Proceedings of the Institution of Civil Engineers, 87(4), 571–582. https://doi.org/10.1680/iicep.1989.3779

Reply: We would like to express our gratitude to referee #2 for the suggestion. We will consider the suggestion in our future studies.

5. Line 156. using a power law dependent on what? Width? Upstream drainage area? Is it the same power law parameters for the whole basin?

Reply: We appreciate referee #2 for raising this question. The power law depends on a prior annual average river discharge and uses a single set of parameters (i.e., a and b) for the whole basin (a=0.1, b=0.5). The power law is shown in Equation 1 in the supplementary document. We will revise the text according to this comment.

6. Line 199. Something went wrong with the font size of some words. Line 351. Line 438. Line 637.

Reply: We would like to thank referee #2 for identifying those mistakes. We will correct all those kinds of font errors.

7. Line 229. So, the mean and standard deviation were calculated based on the open loop simulation?

Reply: Yes, the mean and standard deviation were calculated based on the long-term open loop simulation (2000-2014).

8. Line 231 to 239. Some of your readers might be unfamiliar with the Amazon Basin. It would be interesting to write a short and objective section about this basin, presenting a DEM map at least (a mean Precipitation map would be nice too).

Reply: We like to thank referee #2 for the great suggestion. We will be happy to include a description and a figure of the Amazon basin but considering the length of and the number of figures in the manuscript, we will add them to the supplementary material.

9. Line 275. How do you measure the relative sharpness and the difference in reliability? Line 383 should be here.

Reply: We believe Line 279-283 describes the same ideas as Line 383. We will modify the text to enhance the understanding of the Interval Skill Score (ISS).

10. Several wrong references in section 3.1.

Line 309. The authors said that the Santos Dumont gauge is in the Purus River, but in

Figure 4 it says Jurua River.

Line 332. "Figure 5c–e displays hydrographs of the Jurua (Gaviao), Amazon (Manacapuru), and Negro (Serrinha) rivers" but in Figure 5 it says Manicore on the Madeira River, Aruma on the Purus River, and Sao Felipe on the Negro River.

Line 351. "The lower panels of Figure 6 illustrate flow dynamics along the Amazon mainstem (Sao Paulo De Olivenca; Figure 6c) and Japura (Vila Bittencourt; Figure 6d) and Negro (Curicuriari; Figure 6e) rivers." but in Figure 6 it is written "Hydrographs recorded at Humaita on the Madeira River, Santos Dumont on the Jurua River, and Canutama on the Purus River are presented on panels c, d, and e, respectively."

Reply: We would be grateful to the referee for identifying the mistake. We revised Figures 4, 5, and 6 corresponding to the description of sections 3.1.1., 3.1.2., and 3.1.3.

11. Line 321. Saying that the "direct DA generally improved flow dynamics" is very optimistic. Based on these results, I'd probably say that the direct DA maintained or even degraded the general performance, at least for discharge.

Reply: We would be thankful to the referee for the detailed comments on the text. We agree with the referee that the Direct DA method somewhat degraded the performance of discharge estimations in some locations. But the discharge was improved in some gauge locations in the Amazon basin such as Santo Antonio Do Ica and Sao Paulo de Olivenca. Therefore, we will revise the text to deliver the ideas suggested by referee #2.

12. Line 340. Once again, I think it is an optimistic conclusion. In the last sentence, the authors just said: "although NSE and ISS values worsened slightly." So how can the authors say afterward that "discharge estimates improved moderately"? I don't think that improvements in the correlation coefficient are enough for such a statement given that the NSE has become worst. But I reckon that seasonality got better as correlation got higher. Maybe the authors should clarify what they try to achieve with DA assimilation.

Reply: We would like to express our gratitude to referee #2. Our goal is to improve the overall performance of the river discharge hence higher NSE the better discharge estimated would be. But we try to look at the positives of each method and come to a broader conclusion as none of the methods performed perfectly in all the scenarios. Ideally, it is better if the direct DA performs better than others as assimilation will not depend on prior statistics of the open loop simulation. We basically discuss the median performance in the discussion of NSE and ISS here. But there is a large variation in the statistics as shown in figure 7, table 2 and figure S2. A reasonable number of gauges improved their performance by anomaly DA. An overall moderate number of gauges improved their discharge estimations. Therefore, we will modify the text to highlight the percentage of improved gauges.

13. Figure S2 should be in the main manuscript. It could be together with Figure 7 as 7c, 7d, and 7e. Figures 7a and 7b don't need to be so large.

Reply: We would like to appreciate the suggestion by referee #2. We will include all or some of the panels of Figure S2 in Figure 7.

14. Line 427. "However, the direct DA experiments efficiently improved sharpness, thereby increasing confidence in the assimilated river discharge." I would say "FALSELY

increasing confidence" as the authors just observed that the reliability drops more than 50% for direct DA experiments. What is the point of being narrower if the observation falls out of the confidence interval? I think the authors should be careful with that.

Reply: We would like to thank referee #2 for pointing out this. We agree with the referee that "falsely" increasing the confidence will not be beneficial. In this sentence, we mean that when data assimilation is performed in direct values, the spread of final assimilated values will be narrower than in other methods because the assimilation was performed in direct DA. But not in anomaly or normalized value DA. Of course, the reliability should be higher. We will revise the sentence to convey the idea more clearly.

15. On tables 3 and 4 it would be nice to see the Open Loop and the CaMa VIC BC performances for comparison.

Reply: Thank you very much for the nice suggestion. We will include the median statistics of the Open Loop and the CaMa VIC BC in tables 3 and 4 even though CaMa VIC BC cannot be assessed for sharpness.

16. Line 541. HTESSEL not HTEESSEL.

Reply: Thank you for recognizing the mistake. We modified all the instances which have HTESSEL in the revised manuscript.

17. Line 624. Which experiment is that? The one in section 4.2.? Or the one with VIC BC (section 3.3)?

Reply: Thank you for recognizing the mistake. It should be VIC BC. We will correct the text in the revised manuscript.