

***We would like to thank Anonymous Referee # 1 for reviewing this manuscript. The following response aims to address the comments provided.***

Referee # 1: Credit to previous works, including those by Getirana (!!), is incomplete

***Reply: This manuscript was initially written to be published as a letter, which explains the short description of current studies in the same field. However, we finally decided to submit it as a regular paper. A better contextualization will be provided in the revised paper.***

Referee # 1: The so-called Relative Error criteria. Maybe it is a typo error but as it is given in Eq 5, this criterion is rather meaningless. Indeed, it varies from  $-\infty$  to  $+\infty$ , and is zero simply when the two signals (x and y) have the same mean. Which is absolutely not a criterion of good fit. Surprisingly, the formula given for RE in the present study is completely different from that used in Getirana et al., J. of Hydrology, 2011 (Eq 8)

***Reply: This robust formulation indicates whether averaged estimates under or overestimate observations and we think it is complementary to the other two coefficients used in this study (NS and NRMSE).***

Referee # 1: The computation of the Z parameter. The method used in the present study is very similar to that presented in Leon et al., which is not acknowledged in the manuscript. Besides, it is very difficult to admit that changing the z value by tens of meters until unrealistic values does not change the value of  $R^2$ , when it changes that much for realistic values. I think that it is not possible to consider z values so far from reality as a good result, as provided in Figure 4 (z is negative in first exp shown....). Also, the method failed to provide a z value for a half of the series. Anyhow, if true, this suggests that  $R^2$  is not a good criterion and another criterion must be sought. It seems that the authors used sat series on the Rio Negro, same as it was done in Leon et al. 2006 or in previous studies by A. Getirana himself. In all these studies, the method is said to work well. Authors must compare their results with all these studies and explain why their results are so poor compared to these previous studies. This also holds for the depth values, that could be compared with known values.

***Reply: Leon et al. and Getirana et al. will be referenced. Other criteria such as RMSE, NRMSE, NS and  $WR^2$  have been evaluated.  $WR^2$  was the only one providing more VS with realistic z values, but no significant improvement in discharge estimates was noticed. A discussion about the effects of using different criteria will be provided in the revised version of the manuscript.***

***The negative value in figure 4 is explained by the fact that the surface water level at VS 8 is near sea level. A comparison against previous works will be presented in the revised version of the manuscript.***

***The poorer results are explained by the fact that the models were not calibrated. It is clear that a fully calibrated hydrological model works better than a physically-based LSM. But that is not the point in this study. As explained in the paper, the objective of this study is to evaluate the potential of estimating discharge from large altimetry datasets and global models and datasets. This point will be better discussed in the revised version of the manuscript.***

Referee # 1: In my opinion, the study should include comparison with in-situ data, in order that both the part played by sat altimetry on the one hand, and by discharge modeling on the other hand can be evaluated separately, before both dataset are put together. Such a comparison should include a comparison of the model discharges with gauge discharges, a comparison of the altimetry heights within the gauge readings, and a comparison of the rating curves computed in this study with the in-situ one.

This paper is not the first one to show that rating curves could be determined using model outputs of discharge and sat altimetry. Its interest must be found elsewhere. So, the computation of rating curve is interesting if the coef are published, in order that other investigators can re-use them or compare them with their own findings. I suggest that the coefficients are given in a table, maybe in an appendix. Other possibility could be to mention at the end of the paper that they are provided on request. Therefore, I suggest a major review.

***Reply: Simulated discharges are already evaluated in Getirana et al. (2012) and radar altimetry are compared against water levels in da Silva et al. (2012) and in the present manuscript. A comparison of rating curves can be performed where data are available.***

***In our opinion, a table with hundreds of coefficients would not be as useful as electronic access. However, it is a good suggestion to include a comment regarding the availability of these curves. Yes, these datasets are available upon request. In fact, these coefficients are currently being used by another research group in collaboration with the first author. A sentence will be added in the revised manuscript encouraging those interested in the dataset to contact the first author.***

***The goal of this study is not to present a new technique, improve an existing one or to obtain perfect discharge estimates. Its main objective is to evaluate the potential of applying this well-known robust technique at a large scale using global scale models and datasets. As a first attempt, the entire Amazon basin was chosen as study area. This should be more emphasized in both the abstract and introduction and a revised version will highlight these objectives.***

Referee # 1: Minor comments : - At the end of the reading, it was not clear for me what were the reasons for data pairs to perform well or bad. A sentence such as "accuracy is highly sensitive to the quality of the input data" bears no information. It must be reworded. Similarly, naive sentences such as "the calibrated rating curve parameters may not be reflective of the actual channel hydraulics" (§5, line 25-26) should avoided. The parameters do reflect the channel hydraulics, even if difficult to interpret.

***Reply: All mentioned sentences will be rephrased in the revised manuscript.***

Referee # 1: Figures: - in the version that I got/downloaded, figures are so small that it was almost impossible to see in detail. In particular, texts strings within the figures are really un-readable. Authors must redraw the figures. For figure 4, I suggest that the formulas are put in the legend, or listed in a table instead of being written inside the figures.

***Reply: Figures will be reformatted in the revised manuscript.***

#### ***References:***

***da Silva, J. S., Calmant, S., Seyler, F., Rotunno Filho, O. C., Cochonneau, G., and Mansur, W. J., 2011. Water levels in the Amazon basin derived from the ERS 2 and ENVISAT radar altimetry missions. Remote Sens. Environ., 114, 2160–2181.***

***Getirana, A.C.V. Boone, A., Yamazaki, A., Decharme, B., Papa, F., Mognard, N., 2012. The Hydrological Modeling and Analysis Platform (HyMAP): evaluation in the Amazon basin, J. Hydrometeorol., 10.1175/JHM-D-12-021.1.***