

## ***Interactive comment on “Modeling surface water dynamics in the Amazon Basin using MOSART-Inundation-v1.0: Impacts of geomorphological parameters and river flow representation” by Xiangyu Luo et al.***

**Anonymous Referee #1**

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This manuscript describes the development of the MOSART river transport model to include a flood inundation scheme which was then tested across the Amazon basin. Excellent detail is given as to the setup of the model including the processing of the DEM and channel geometry parameters. The model is run for a time period longer than 20 years and evaluated against in-situ streamflow observations and remotely sensed satellite data of river stage and flood extent. Results from the evaluation showed good agreement in each of these aspects. A sensitivity analysis was then conducted to assess the impact of the DEM and channel geometry corrections, setting a uniform Manning's  $n$  and using a kinematic channel flow equation. Sensitivities were found in

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each variable due to the influence they have upon the floodplain elevation, channel capacity and flow velocity.

The manuscript's contribution to model development is the inclusion of an inundation scheme to the MOSART model, however this is not explicitly stated until page 6 therefore leaving the reader unclear about the paper's contribution for most of the introductory sections. The authors should revise the abstract to state much more clearly that this is one major contribution of the manuscript.

One aim of the manuscript is to investigate the importance of geomorphic parameters and river flow representation when modelling the Amazon basin. This is done through the results of the sensitivity analysis, however these mostly back up results from previous papers which also describe the parameterisation of large scale river models in the Amazon basin. Therefore the novel contribution from this aspect is minimised, the greatest contribution from this paper is in describing the model development of the MOSART model.

There is no comparison between the results from the model developed in this manuscript with results from the previous version of the model without the inundation scheme. Clearly it is not possible to compare the results of inundation extent but for a model development paper there needs to be some direct comparison between the results of the developed model and those of its predecessor. In this case it should be possible to compare the results of streamflow and river stage. I believe that the model development in this manuscript is significant and merits eventual publication, however I would suggest that it is reconsidered after major revisions so that the authors can include results from a direct comparison between the two model versions.

Additionally please find below the following minor corrections:

Equation 2 define  $v$

Page 7 line 3 how was it decided to combine the neighbouring catchments?

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Page 8 line 9 should read 'lowered to 2.5m'

Page 8 line 9 why was a distinction made between shrubs which were over 5m and those which were lower - why the different treatment when correcting the SRTM?

Page 8 line 13 what was the uniform value that was subtracted from areas located outside the floodplain?

Page 7 line 15 were the elevation profiles not defined from the vegetation corrected DEM?

Page 9 line 13 how were the gauges distributed amongst the 10 regions? Some regions might have only had a few gauges hence the significance of the RMSE value might be low, plus this might override the significance of geomorphological factors in applying this correction

Page 10 line 16 give an example of the literature - a reference to a textbook for example

Page 11 line 15 - can river flow in the upper tributaries really be evaluated using the gauge at Santo Antonio do Ica which is located much further downstream? The steeper gradients of the tributaries are likely to have different flow hydraulics to that in the mainstem, can the authors comment on this and provide further justification for using this gauge to make the evaluation?

Page 11 line 22 there is a positive runoff bias in the Japura basin which goes against the overall trend of negative biases in the western portion of the basin, could the authors explain what may be causing this?

Eq 7 This describes how the simulated river stages are converted into elevations, should this not therefore be included in section 2.5 which describes how the river channel geometry in the model was established?

Page 12 line 12 how were the simulated river stages shifted to coincide with the observations?

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Page 12 line 15 should another metric be calculated alongside the correlation coefficient? In the Negro and Japura basins for example Fig 4 shows there is a very high correlation but the differences between the simulations and observations are very large. Perhaps calculating another metric might capture this?

Page 13 line 2 should read 'lake areas'

Figure 6 the four plots should be replaced with two difference plots, one showing the difference between the simulated and observed during high water and the other during low water. This would better visualise the difference between the two simulations.

Page 13 line 13 the statement that the GIEMS data and simulation agree reasonably well is very vague. Figure 6 appears to show that the simulation overestimates the extent in the lowland portion of the basin, especially at low water. This sentence should be expanded to include more details about where the differences occur.

Figure 7 it could be useful to plot the data by seasons e.g AMJ, JAS, OND, JFM as this might show if the errors are concentrated in a particular season e.g. low water.

Figure 8, why does this figure refer to the average seasonal cycle from 1995-2006 whilst figures 9 and 10 refer to 2007 only? Does this explain why the results for the kinematic simulation are so different between figures 8 and 9 & 10? I would expect the kinematic simulation to be very different to the control simulation (as it appears in Figs 9 and 10), yet does this not appear to be the case for streamflow - could the authors explain why streamflow is not sensitive to the kinematic solution or replot Fig 8 for 2007 only so that it is directly comparable to Figs 9 and 10? Figure 10 is confusing with the y-axis reset for 0-1500 km for the simulations but not for the riverbed profile. These graphs should use the same y-axis for the entire river length in order to remove the confusing jump that happens at 1500 km.

Section 4.2, the greatest effect is shown in the Madeira basin, is this most likely because the multiplicative factor (0.36) has the greatest effect on changing the channel

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geometry relative to the other basins? This should be stated more explicitly in the second paragraph.

Figure 13 needs to be redone as it is very difficult to follow the decision chain that the authors are trying to imply. For example at the second box there are four options but how is a reader meant to decide between these?

Page 18 line 26 should read 'could have an evident effect'

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