Response to Referee #1

We appreciate the time and effort of the reviewer and thank the reviewer for the constructive comments. We provided replies to all the comments, and will include the corresponding changes in the revised manuscript.

In the following text, we use blue color for the reviewer comments, black color for the replies, and italics for the revisions in the manuscript.

Comments

[C1-1]

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 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.

Reply: We thank the reviewer for the positive evaluation and suggestion. In the Abstract and Introduction of the revised manuscript, we will more clearly state the goal of our study as mainly to incorporate and document an inundation scheme in the MOSART model, which is used in Earth System Models.

[C1-2]

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.

Reply: In the initial manuscript, the comparisons between our study and previous studies were not clear. We will add more discussions to compare the results of our study with those of former studies. On one hand, our work was based on the important foundation of previous studies; on the other hand, our study had some new points in terms of methodologies, model results and sensitivity analyses.

While our modeling approach and improvements do not differ conceptually from those already explored in previous studies, we attempted to generalize various methods for application over the entire Amazon Basin, which is important as MOSART is used in global Earth System Models. Our study also provided more comprehensive evaluation of our simulations and analysis of sensitivity of the simulations to various factors, which yielded some findings that have not been discussed in former studies.

[C1-3]

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.

Reply: We thank the reviewer for the suggestion and positive evaluation.

We have conducted a new simulation “NoInund” (with the inundation scheme turned off) and compared its results with those of the control simulation “CTL” (where the inundation scheme was turned on). This comparison revealed the impacts of the inundation scheme on modeled streamflow and river stages. The comparison will be presented in a new subsection (Section 4.1 Inundation representation). In brief, we will update Figures 8, 9, and 10 to include the results of “NoInund” for comparison with simulations that include the inundation scheme in different sensitivity experiments. The three figures are attached in the Appendix at the end of this Response. In brief, including inundation generally improves the simulation of streamflow and river stages compared to the simulation without the inundation parameterization.

Comments

Additionally please find below the following minor corrections:

[C1-4]

Equation 2 define v

Reply: The definition of v will be added (below the newly supplemented Equation (1)).

[C1-5]

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

Reply: The number of catchments is comparatively large, so to show the inundation results more concisely, the catchments were combined to a few subregions. This is done by selecting seven major catchments (i.e., Xingu, Tapajos, Madeira, Purus, Jurua, Japura and Negro) as subregions or the major part of a subregion. Then the Upper-Solimoes catchments were combined as one subregion, and the northeast catchments were combined as another subregion. Lastly, the remaining five catchments were combined with their adjacent subregions.

More explanation will be added in this paragraph.

[C1-6]

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

Reply: The original manuscript is not clear. There should be "an amount of" added before "2.5 m". However, this sentence will be removed in the revision (please see the reply to the comment below).

[C1-7]

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?

Reply: The original manuscript is not clear. Some sentences will be revised.

The resolution of the vegetation height data is coarser than the resolution of the land cover data. Hence within one pixel of the vegetation height data, there may be more than one land cover class, which should not be assigned the same vegetation height. For shrub, any vegetation height larger than 5 m should be an overestimation (according to Junk et al. 2011), so an upper limit of 5 m is imposed. After this correction, 50% of the vegetation height was deducted from the DEM pixel covered by shrubs.

The text will be revised as: *"In the high resolution land cover dataset, shrubs were defined to be less than 5 m tall (Junk et al., 2011). So for DEM pixels with shrub, the vegetation height is determined by the vegetation height data, but with an upper limit of 5 m. After this correction, the elevations were lowered by 50% of the vegetation heights for shrub DEM pixels."*

[C1-8]

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

Reply: The original manuscript is not clear. For the fine DEM pixels within one coarse vegetation height pixel, a unique vegetation height is used, but for different vegetation height pixels, the vegetation height can be different even for the same vegetation class.

The text will be revised as: *"a uniform vegetation height was applied for all the DEM pixels within each vegetation height pixel"*.

[C1-9]

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

Reply: Yes, the elevation profiles were generated from the vegetation corrected DEM.

For clarification, the text will be revised as: "*the void-filled DEM after the correction of vegetation-caused biases was used to generate the elevation profiles of all the subbasins*".

[C1-10]

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

Reply: The coefficients of the basin-wide channel geometry formulae were adjusted for seven of the 10 subregions (except “Xingu”, “Upper-Solimoes tributaries” and “Mainstem”; shown in Table 1). Each of the seven subregions used 3 – 13 gauges.

The channel geometry is important for inundation modeling of the “Madeira” and “Negro” subregions which have evident inundation and large area. The “Madeira” and “Negro” subregions used 12 and 13 gauges, respectively.

[C1-11]

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

Reply: The text will be revised as follows:

“*Following Getirana et al. (2012),* $n_{\max}$ *and* $n_{\min}$ *were set as 0.05 and 0.03, respectively. In addition, a few other studies of the Amazon Basin adopted a similar range of values between 0.03 and 0.05 for the Manning coefficient (Beighley et al., 2009; Paiva et al., 2013a; Yamazaki et al., 2011).*”

[C1-12]

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?

Reply: We agree that the river flow in the tributaries could be quite different from that of the mainstem so the river flow in the tributaries cannot be represented by using results at this gauge. Our description in the original manuscript is not accurate so the sentence will be revised as follows:

“*Most of this subregion is controlled by the Santo antonio do ica gauge at the upper mainstem.*”

[C1-13]

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?

Reply: There is a negative runoff bias in the subregion “Upper-Solimoes tributaries” which is on the west side of the Japura basin (Fig. 3i). On the other hand, there is a positive runoff bias in the western part of the subregion “Negro”, which is on the east side of the Japura basin (Fig. 3g). The western Negro and the Japura basin are adjacent, and both have positive runoff biases.

The runoff biases could be due to errors in precipitation inputs or errors in the land surface water fluxes calculated by the land surface model (e.g., canopy evaporation, plant transpiration, and soil evaporation).

The following sentence will be added:

“The runoff biases could be caused by errors in the precipitation forcing or errors in the land surface water fluxes calculated by the land surface model (e.g., canopy evaporation, plant transpiration, and soil evaporation).”

[C1-14]

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?

Reply: Following the suggestion by the reviewer, the method for estimating the riverbed elevation will be moved to Section 2.5.

[C1-15]

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

Reply: All the simulated river stages of the same subbasin were raised or lowered by a uniform height, to facilitate comparison of the timing and magnitude between the simulated river stages and the observations. A similar method was also used in Figure 7 of Coe et al. (2002) .

The text will be supplemented by *“of the same subbasin”* and *“by a uniform height”*, and now read: *“For better visual comparison, the simulated river stages of the same subbasin were shifted by a uniform height to coincide with the observations.”*

[C1-16]

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?

Reply: The original manuscript was not clear. The standard deviations were calculated and used to indicate river stage fluctuations. It was discussed that the river stage fluctuations were overestimated for the subbasins of 4 gauges (i.e., Canutama[Purus], Acanaui[Japura], Serrinha[Negro] and Santo antonio do ica[Mainstem]).

To make the text more clear, the phrase "*as well as standard deviation for simulated and observed river stages*" will be replaced with "*Moreover, the standard deviations for the simulated and observed river stages were also calculated.*"

[C1-17]

Page 13 line 2 should read 'lake areas'

Reply: This will be corrected.

[C1-18]

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.

Reply: Fig. 6 will be supplemented by two panels showing the differences during high water season (Fig. 6e) and low water season (Fig. 6f). The original four panels were kept in order to show the spatial patterns of inundation. Please find the revised Fig. 6 in the Appendix of this response.

[C1-19]

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.

Reply: We will add some discussions of the differences between model results and GIEMS data in the revised manuscript.

[C1-20]

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.

Reply: Thank you for the suggestion. Similar to Figure 7 that compares the annual averaged biases in flood extent and streamflow, we plotted the averaged monthly streamflow errors and the flood extent discrepancies (i.e., the differences between simulated flood extent and the GIEMS data) during 12 years (1995 – 2006). Please find the figure at the end of the Appendix. This figure shows that the seasonal distribution of streamflow errors varies for different gauges. For example, for "(a) Altamira" and "(b) Itaituba", evident positive biases occur from January to April; for "(c) Fazenda vista alegre" and "(d) Guajara-mirim", positive biases are more evident from about May to October; for "(h) Acanaui", positive biases are more evident from about March to July. Except for three subregions (Negro, Cach de porteira-con, and Tabatinga), the seasonality of flood extent discrepancies follows the seasonality of streamflow errors very closely, indicating the important contribution of streamflow errors to flood extent biases on seasonal time scale.

[C1-21]

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?

Reply: Figs 8, 9 and 10 have been replotted to show the results of the same year. We did not have observed streamflow data for year 2007 so we plotted the results of another year (2005).

The three figures show that the differences in streamflow between the simulation KW (kinematic wave method) and the simulation CTL (diffusion wave method) are not as evident as those in river stages. Previous studies have yielded similar results (e.g., Yamazaki et al. 2011: Fig. 5). The reason could be that the flow velocities in the simulation KW (which are based on riverbed slopes) are also quite different from those in the simulation CTL (which are based on friction slopes). Please see the revised figures in the Appendix. Note that as mentioned in our reply to [C1-3], these figures have also been updated to include the results from a simulation without the inundation parameterization.

[C1-22]

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.

Reply: Figure 10 has been replotted to use the same y-axis for the entire river length and will be updated in the revised manuscript. We thank the reviewer for the suggestion. Please find the revised Fig. 10 in the Appendix of this response.

[C1-23]

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

Reply: Yes, the reason is that the channel geometry changes in the Madeira subregion are larger than those of the other subregions.

To make the discussion more explicit, the following revisions will be made in the second paragraph:

(1) "*channel geometry changes*" was replaced with "*the channel cross-sectional area is multiplied by a factor of 0.36 (Table 1)*" ;

(2) Added one sentence: "*Similar phenomenon is observed at the gauge "Cach da porteira-con" in the Northeast subregion (Fig. 8h), where the channel cross-sectional area is multiplied by a factor of 0.48.*"

(3) Added "*caused by refining channel geometry*" after "*Inundation changes*".

[C1-24]

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?

Reply: The original manuscript was not clear. In general, the phenomena before and after an arrow have the cause-effect relationship.

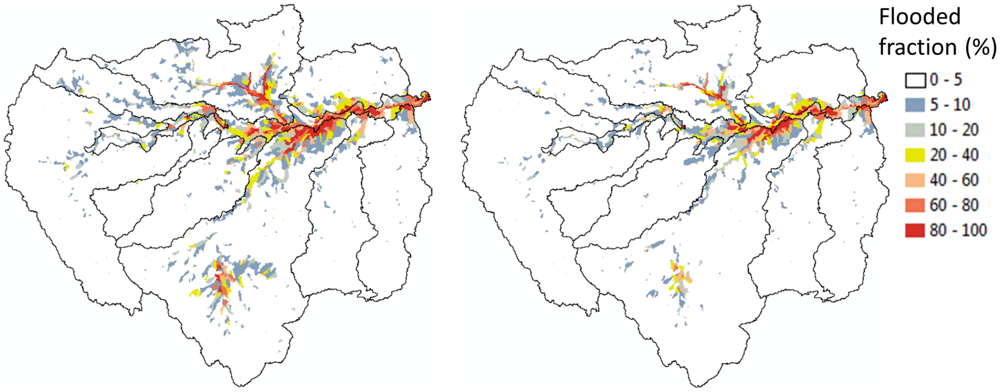
The figure caption was revised: "*An example of the effects of channel cross-sectional geometry on the water depth of the local channel*" was replaced with "*A diagram illustrating how decreasing the width of* *the local channel could bring about changes in the water depth of the local channel through various mechanisms. In general the phenomena before and after an arrow have the cause-effect relationship*" .

[C1-25]

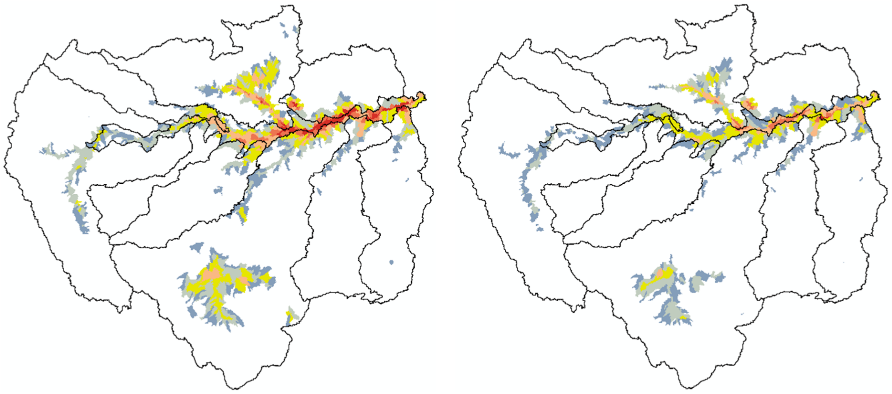
Page 18 line 26 should read 'could have an evident effect'

Reply: "*an*" was added between "*have*" and "*evident*".

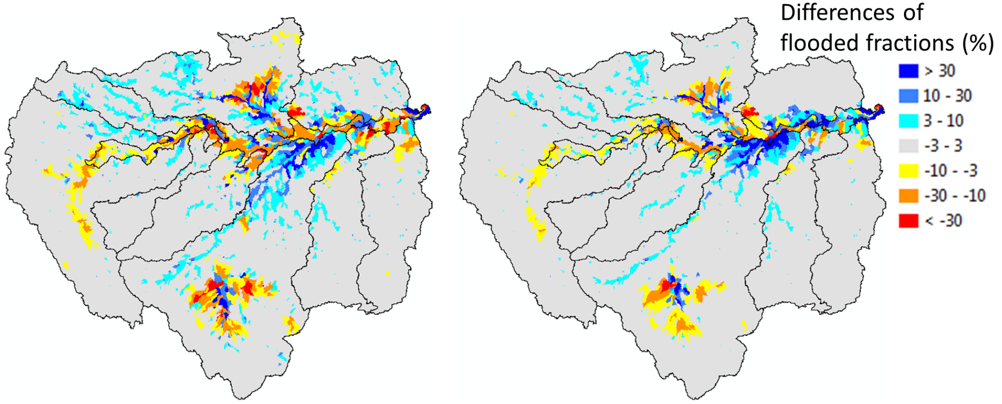
Appendix


a) Control simulation (AMJ)

b) Control simulation (OND)


c) GIEMS observations (AMJ)

d) GIEMS observations (OND)


e) CTL minus GIEMS (AMJ)

f) CTL minus GIEMS (OND)

Figure 6. Average spatial patterns of flooded fractions for all subbasins during 13 years (1995 – 2007): a) Results of the control simulation in the high-water season (AMJ – April, May and June); b) Results of the control simulation in the low-water season (OND – October, November and December); c) GIEMS observations in the high-water season; d) GIEMS observations in the low-water season; e) Differences between the control simulation and GIEMS observations in the high-water season; f) Differences between the control simulation and GIEMS observations in the low-water season.

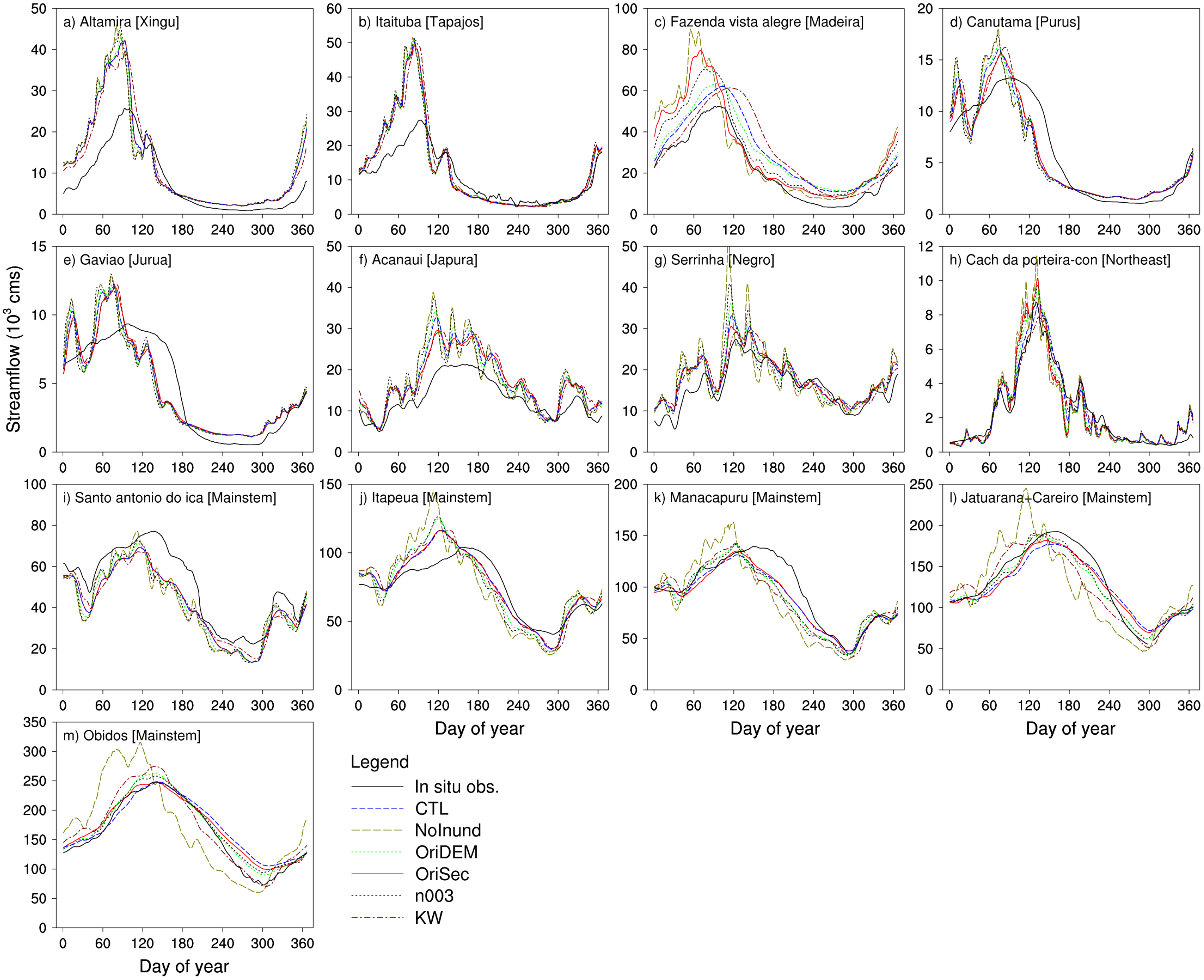


Figure 8. Observed and modeled daily streamflow of year 2005 at 13 stream gauges. Setup of the six simulations is described in Table 2: CTL – Control simulation; NoInund – Without inundation scheme; OriDEM – Using the original DEM (with vegetation-caused biases); OriSec – Using basin-wide channel geometry formulae; n003 – Using a uniform Manning roughness coefficient (i.e., 0.03) for all the channels; KW – Using kinematic wave method to represent river flow.

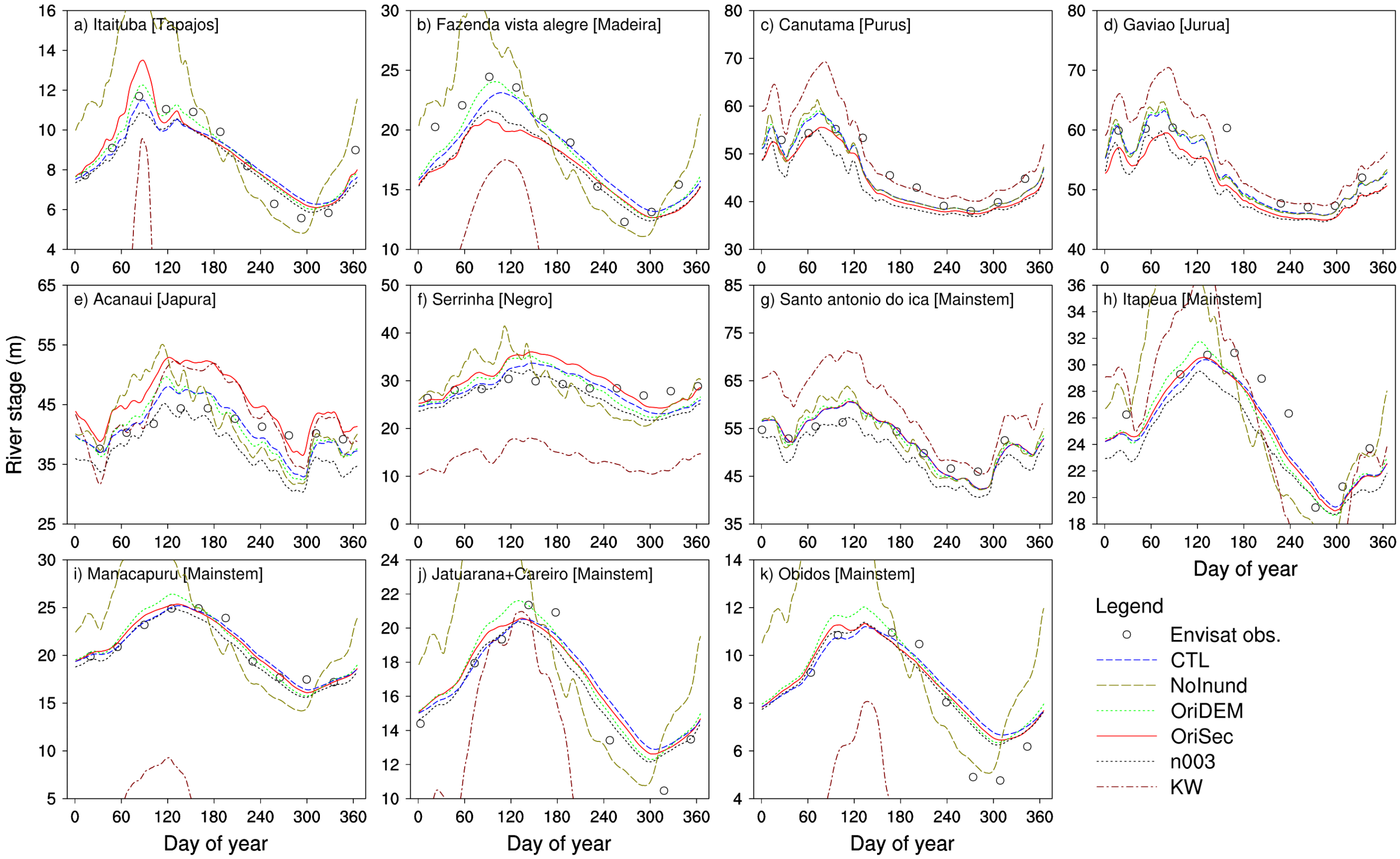


Figure 9. Observed and modeled river stages at the daily scale in year 2005 for the subbasins containing or close to 11 of the 13 stream gauges. Setup of the six simulations is described in Table 2: CTL – Control simulation; NoInund – Without inundation scheme; OriDEM – Using the original DEM (with vegetation-caused biases); OriSec – Using basin-wide channel geometry formulae; n003 – Using a uniform Manning roughness coefficient (i.e., 0.03) for all the channels; KW – Using kinematic wave method to represent river flow.

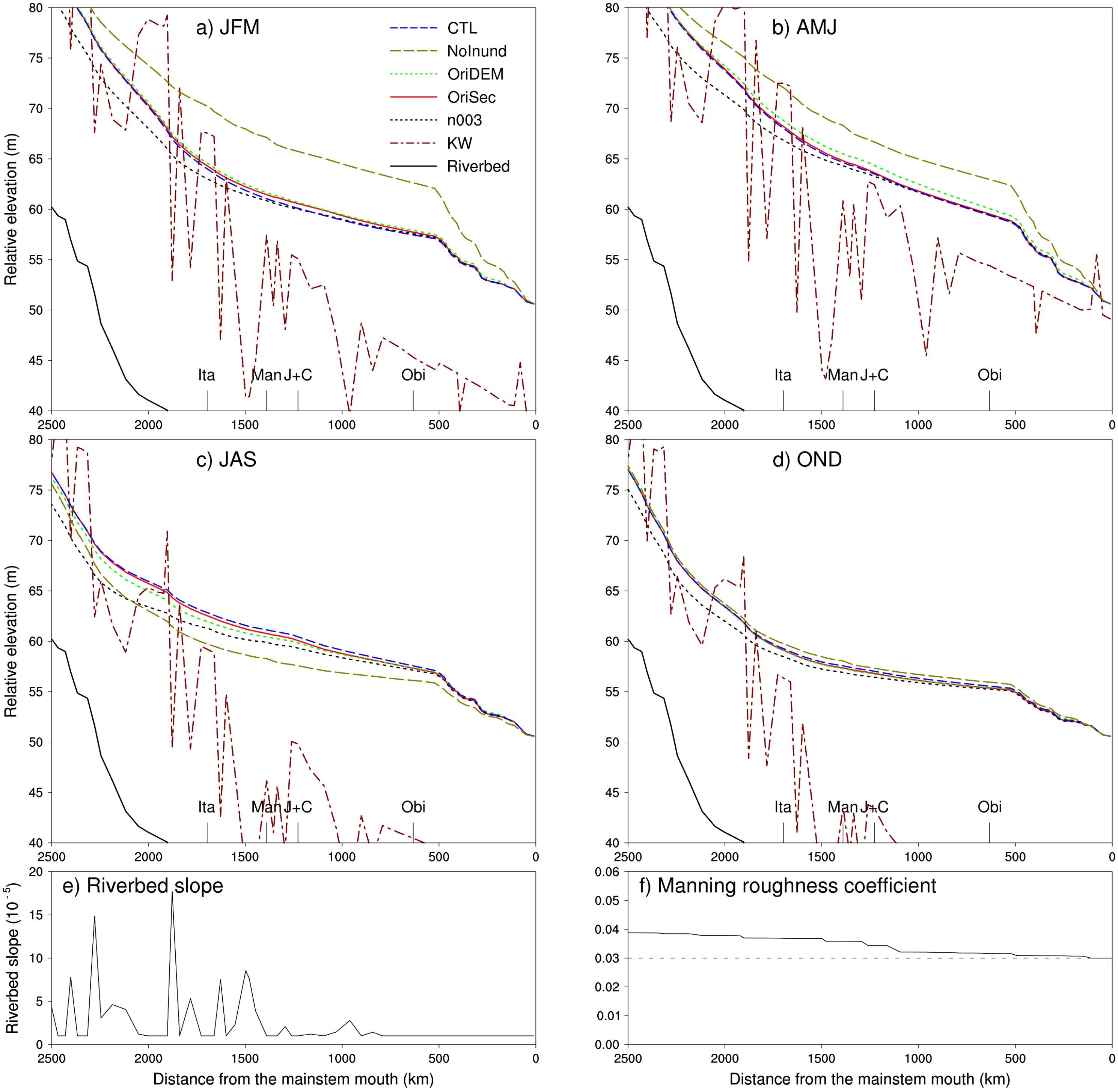
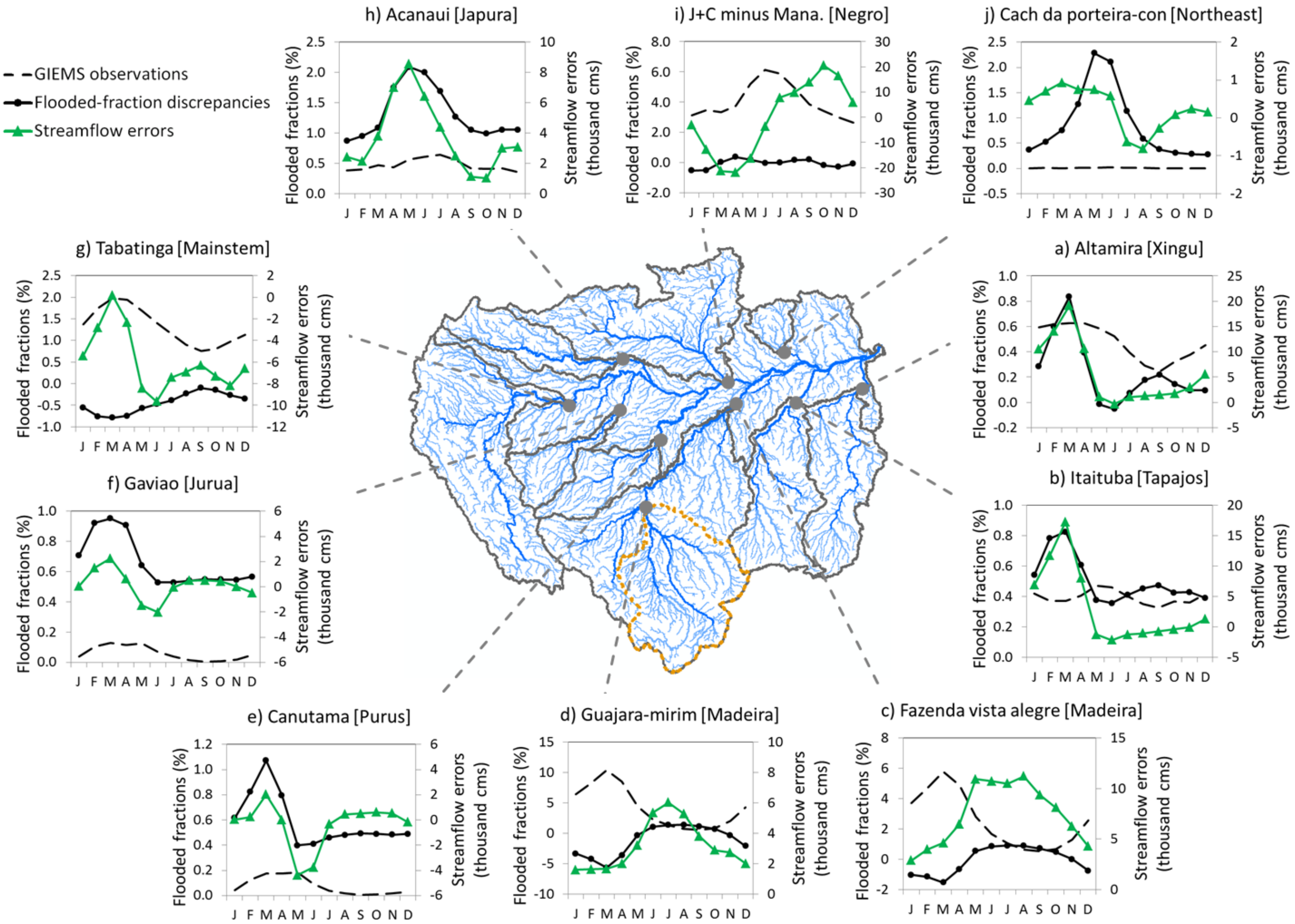


Figure 10. Modeled average river surface profiles along the mainstem in the four seasons of year 2005: (a) JFM (January, February and March; the period of rising flood); (b) AMJ (April, May and June; the period of high water); (c) JAS (July, August and September; the period of falling flood); and (d) OND (October, November and December; the period of low water). Results of six simulations are shown. The four stream-gauge locations are labeled on the x-axis: Ita – Itapeua; Man – Manacapuru; J+C – Jatuarana+Careiro; Obi – Obidos. Riverbed slopes (e) and Manning roughness coefficients (f) along the mainstem are also shown. In the panel (f), the solid curve shows spatially varied Manning coefficients used in five simulations; the dotted line shows the uniform Manning coefficient of 0.03 used in the simulation "n003".



Addition to Figure 7: Averaged monthly streamflow errors and the flood extent discrepancies (i.e., the differences between simulated flood extent and the GIEMS data) in the area upstream of the gauge for 10 gauges during 12 years (1995 – 2006). Streamflow of the Negro subregion (panel (i)) is approximated by the streamflow difference between the Jatuarana+Careiro gauge and the Manacapuru gauge. The upstream area of each gauge is enclosed by gray lines (or brown dotted lines for the Guajara-mirim gauge) in the basin map.