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Discussions

Interactive comment on "Assessing the impact of hydrodynamics on large-scale flood wave propagation – a case study for the Amazon Basin" *by* Jannis M. Hoch et al.

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We thank Dr. Dai Yamazaki for his detailed evaluation of our manuscript and helpful comments, which we will address stepwise here below.

With regard to your first comment on page3/line32 in the manuscript, we refer to CaMa-Flood's ability to determine water level, water storage, and flooded area on basis of sub-grid topography. We acknowledge that this is not precisely equivalent to a sub-grid channel representation as discussed in the manuscript, and thank Dr. Yamazaki for noticing these relevant differences. We will update this in the revised manuscript.

Answering your comment on page7/line6, we thank you for raising some relevant as-

pects regarding the computation of the river bathymetry. Your latter assumption holds, as we computed river depth based on river width taken from GWD-LR and estimated the river depth with Equation 3. This relationship is therefore independent of upstream area. We preferred to use the width from GWD-LR as it better reflects the local conditions than the generic relationship of Paiva et al. (2011) in which only upstream area is the predicting variable, even though the latter was derived for the Amazon. We believe that estimating river depth from river width with Equation 3 holds as the formulas – although empirical in nature – are internally consistent, and moreover derived from data collected at 341 cross-sections (see Paiva et al. (2011), therefore already gaining more general validity. Also, bathymetric information is fed into the model as averages for segments between profile locations, and thus local variability in width and depth is neglected, and the influence of local river restrictions or widenings diminished. Similar to our approach, Neal et al. (2012) estimated river depth from observed width by smoothing river widths and thereafter re-shuffling the hydro-geometric equations of Leopold and Maddock (1953). These approaches have the advantages that they remove the need to include an estimate of bankful discharge or upstream area a priori, and that an educated estimate of river bathymetry can be obtained where no information is available. In a nutshell, we agree with your valid comment that basing the relationship directly on observed width does not ensure downstream continuity as merely using the discharge or the upstream area would. However, we believe that the advantage of using observed widths in combination with estimated depths outweigh this theoretical limitation due to the arguments listed above.

Regarding your comment on page8/line 22, the elevation of each flexible mesh cell is computed as the normal unweighted average of the elevations assigned to each cell vertex. The cell vertices have their elevation assigned by employing a sample-file in ascii-format containing gridded elevation data. We will have improved the description of the method and added the relevant details in the revised manuscript.

In answer to your comment on page8/line8, we indeed used the same equations and assumptions for defining bathymetry in the 1D/2D and 2D schematization, yet in a slightly different manner. While for 1D/2D we ignored upstream area and derived depth directly from river width, we lacked river width in 2D and therefore estimated river depth from upstream area. A systematic comparison between computed depths was not possible as the 1D-network does not necessarily coincide with the LDD use to determine upstream area. However, a manual inspection of points where this was the case showed that differences between obtained depth for 1D/2D and 2D are within of 10%. We therefore are confident that these deviations in methodology do not have a strong impact on model behaviour at the large-scale which is underpinned by experiences made in other case studies. We nevertheless consent that these inequalities, altogether with the still present differences between the set-ups, need to be pronounced stronger in the final manuscript to avoid any lack of clarity.

With regard to your comment on page9/line 13, we understand that the coupling-algorithm is not perfectly clear in its explanation, and thus needs some details added. Indeed, your assumption is correct. With the discharge volumes being the result of PCR-GLOBWB routing upstream of the area where coupling is active, and hence being transferred to the hydrodynamic model only at the boundary cells, surface runoff is used as input to the hydrodynamic model over the coupled domain instead of being fed into the PCR-GLOBWB routing scheme. The "TopWaterLayer" is an additional layer that can be populated with inundation depth information from the hydrodynamic model. Therefore, this layer will only be influential once two-way coupling is performed, and due to that we will remove the layer from both flowchart and text to highlight the other, more important data and processes.

Regarding your comment on page10/line3, we will enhance the specific section to improve the reader's understanding. We will add the GRDC-stations in the plot with river depth observation stations and provide a table showing the specific properties of each station. With this additional information, we are confident that the under-representation of discharge by GRDC-stations will become better understandable.

With respect to your comment on page10/line13, we express our agreement with your statement that different routing schemes may result in different timing of flood wave occurrence as well as possible attenuation effects. Over longer simulations as ours, the water balance should be closed, and the water volumes added to the model should equal the outflow volumes. Since a closed water balance is pivotal for meaningful hydrologic and hydrodynamic computations, we re-assure you that we double-checked it. Our test runs showed that, first, the hydrodynamic model is mass conservative, that is no water is lost or added, which is, given that the model is designed for commercial applications too, a prerequisite. And second, we assessed whether the input volumes (river discharge, surface runoff, and topWaterLayer) from PCR-GLOBWB which are added to Delft3D FM as delta volumes equal the volumes as computed by PCR-GLOBWB without coupling. The answer is yes, they are equal. As a result, we conclude that the amounts of water per model run are correct, but found other causes for differences in observed discharge. Reading the manuscript again, we find that these reasons may not have been explained clearly enough. It can indeed be the case that a model run shows higher or lower discharge values than others, but not because the water balances are incorrect. The reason is that we compare different model set-ups (hydrology-only, hydrodynamics-only in 1D/2D and 2D, coupled hydrology-hydrodynamics in 1D/2D and 2D) which can different rather remarkably between each other. While the discharge timing and shape are governed by the routing module applied, the total volumes are controlled by the hydrologic processes represented. The fact that discharge in coupled runs is consistently higher than other runs may be because of (a) the hydrodynamic-only runs are forced with data from different origin (GRDC), and (b) the hydrology-only runs are forced with data from same origin (PCR-GLOBWB), but account for groundwater infiltration and evaporation processes on all water bodies in the model domain, which lowers the overall simulated discharge. In the final manuscript, we will take your valuable comment into consideration, and add information where necessary to ensure the different discharge results are not taken as result of water balance errors.

To react to your comment on page10/line34, we are thankful that you pointed towards this indistinctness. It is entirely true that not the way of coupling, but mainly the river bathymetry is decisive when it comes to simulating the timing as well dynamics of flood waves. We do not aim to attribute the differences in discharge dynamics to the fact that we coupled two models, but to the choice of model used to simulate discharge and inundations (hydrology-only, hydrodynamics-only in 1D/2D and 2D, coupled hydrology-hydrodynamics in 1D/2D and 2D). It is inherent to the various model set-ups that some properties such as for instance bathymetry differ between them. For the revised manuscript, we will enhance the text accordingly.

Commenting on your remarks to page 12/line18, we are entirely in line with you and agree that the timing of discharge depends on the distance between input and observation station. Our way to account for ‘missing’, that is underrepresented, discharge volumes contains a uniform scaling factor which is applied to each input location, hence not altering the relative contributions of each inflow. Thus, we think our conclusion that using input at a model domain’s edge leads to peak attenuation and lag still holds true.

With respect to your comment on Figure 7, we concur with your remark. For the present manuscript, we decided to plot water depth to indeed highlight the impact of local bathymetry and therefore the impact of general model set-up differences between 2D and 1D/2D. In order to keep this argument, but also to display the important larger-scale hydrodynamics, we will extent the manuscript with a plot of water elevation above sea level and the related discussion in the text.

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