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Geoscientific Model Development Discussions

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Interactive comment on "GLOFRIM v1.0 – A globally applicable computational framework for integrated hydrological-hydrodynamic modelling" *by* Jannis M. Hoch et al.

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We heartily thank Dr. Dai Yamazaki for his detailed evaluation of our manuscript and for his helpful comments, which we will address point-by-point hereafter.

With respect to the first comment, it seems as if the use of jargon resulted in a lack of clarity. Thanks for pointing this out! We thus will update the manuscript such that the functionality of the River-Floodplain-Scheme (RFS) is described in a better understandable way.

Indeed, the causes for vertical inaccuracy of remotely sensed elevation data are man-

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ifold. Since at the time of writing the current manuscript the MERIT DEM was not yet published, we did not refer to it. We will make up for it in the revised version.

Regarding the comment on P8/L4 we are not fully sure how the explanation and comment fit together. Considering only the comment, we thank you for mentioning the missing information. In the revised manuscript, we will add information regarding schematizations not bordering at the sea, for instance mid-stream schematizations. In short, it is possible to employ any hydrodynamic schematization within GLOFRIM as long as it complies with Delft3D Flexible Mesh and LISFLOOD-FP requirements, respectively. This means that not only mid-stream simulations can be run, but adding to your comment, it would also be possible to account for tidal variations of downstream water level boundaries, provided the required information is available.

Thank you for pointing out the lack of clarity on P8/L9 and P8/L30. We will update the manuscript accordingly.

With regard to your comment on P8/L34, we want to clarify that there is actually no channel in the 2-D run of the synthetic test case. In this run, we merely simulated floodplain flow as flow over a 2-D grid, and therefore employed only one surface roughness coefficient (0.07 s m-1/3). While the surface roughness coefficient differs compared to the 1-D run, upstream and downstream boundaries as well as gradient are identical for both runs. To avoid any confusion, we will re-formulate the section under consideration more clearly.

Your comment on P9/L25 is most helpful: both hydrodynamic models have a fixed water level of 0 m as downstream boundary condition to ensure comparability. Since the schematization of LISFLOOD-FP is derived from Delft3D Flexible Mesh, both models employ the same 1-D network. In contrast to other current global hydrodynamic models such as CaMa-Flood (Yamazaki et al., 2014b), the schematizations employed in the current manuscript cannot account for bifurcations and thus the channel complexity of the delta had to be captured with one channel. To that end, we used the Global Width

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Database for Large Rivers (GWD-LR; Yamazaki et al., 2014a) to obtain an effective bathymetry, accounting for islands and other disturbances in channel network. As we acknowledge that the lack of channel representation may have influenced our models results, we will elaborate on that in the discussion part of the revised version.

Thank you for mentioning the upscaling procedure on P10/L6. In line with previously published research (Fewtrell et al., 2008; Savage et al., 2016), we applied the nearest neighbour method to avoid undesired smoothing effects in the elevation values. We will update the manuscript accordingly.

Thanks for your comment on P10/L11. We indeed used a uniform surface roughness value as this has also been done by other studies as stated in the current manuscript. Hence, we do not see the need to further elaborate on this aspect in the revised manuscript.

We thankfully acknowledge your remark on the downsides of the kinematic wave approximation (P10/L14) as you are indeed raising a relevant issue regarding the upstream inflow boundary. Depending on the flow distance covered by PCR-GLOBWB routing until the hydrodynamic domain is reached, the impact of less sophisticated routing schemes may amplify, yet a detailed analysis thereof exceeds the scope of the paper. We will, nevertheless, add this aspect to the discussion of model results. Your recommendation to use a continental-scale model, for instance CaMa-Flood or MGB-IPH, as intermediate step may be one solution to circumvent the impact of the kinematic wave approximation. As GLOFRIM does not yet allow for coupling PCR-GLOBWB with other models, this is not yet feasible but provides a great motivation for future research as the coupling framework, in principle, allows for it. A more straightforward way would be to extent the hydrodynamic schematization over the entire PCR-GLOBWB domain. However, this would result in increased run times that are impractical as yet.

You correctly pointed out that a global application can be locally restricted by data availability for model set-up and we will mention this limitation in the conclusion. We

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will furthermore point out that the “regionalized optimization” (P10/L16) is optional and PCR-GLOBWB can also be run as is in its default parameterization.

We are thankful for your extended remark on the model results. First of all, we agree with the assumption made that the spatial resolution applied by Delft3D Flexible Mesh in upstream areas may impact model results locally as well as further downstream. Due to that we are currently working on a follow-up study concerning the relation of spatial resolution of flexible meshes and model results. Therefore, we desisted from providing a too elaborate discussion in the current version of the manuscript, but will nevertheless mention this interplay in the revised version. In addition, we will re-run out model set-ups with an increased number of observation points, especially for the upstream region, to obtain a clearer picture whether discharge attenuation increases along the flow path. Depending on the availability of observed data, we will perform additional discharge validation for the upstream stations as well. The discussion and conclusion sections will then be updated accordingly.

Thank you for the information on the D4 river network system and the related uncertainties on P11/L16. We will extent the description of this system and its limitations, especially with respect to model results. As the anonymous reviewer #1 already suggested to provide an additional plot with the results of the sensitivity analysis of increased/decreased river length in LISFLOOD-FP, we will put this in context with your remarks to show that possible under- or overestimation of channel dimensions due to the D4 system is not contributing to the gap compared with discharge simulated by Delft3D Flexible Mesh.

This relation between simulated water levels (P11/L29) and cell area is indeed present as suggested by the reviewer. We will clarify this in the revised manuscript.

Thank you for pointing out the lack of clarity on P11/L34. A previous study already showed that the behaviour of simulated water level is not always predictable due to spatial feedback dynamics between neighbouring cells of an observation station (Hardy

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et al., 1999). We will add this vital information to the discussion of model results in the revised manuscript.

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