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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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<General Comments>

This manuscript introduces a new framework for a coupled "land hydrology & river hydrodynamics" modelling, and assessed its feasibility by comparing the results of two river hydrodynamic models (i.e. Delft3D Flexible Mesh and LISFLOOD-FP). The manuscript is well written, and it suits with the subject of the journal "Geoscientific Model Development". However, it contains some unclear points which should be revised/improved before publication.

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Discussion paper

<Specific Comments>

P7. L32: “If RFS is activated, water volume is directly coupled to the 1-D channels of the hydrodynamic model while, when RFS is inactive, water is distributed over all grid cells of the 2-D domain.”

The description of RFS is not sufficient. Please explain the relationship between the hydrology model grid and hydrodynamic model pixels in a more detailed manner. I guess, “water volume of each coarse-resolution hydrology model grid” is distributed to the “corresponding high-resolution hydrodynamic model cells within the coarse-resolution grid”. And the difference due to RFS is whether water volume is distributed only to river cells or both river and floodplain cells within each coarse-resolution grid box. Readers who are not familiar with this topic might misunderstood water volume is distributed uniformly all-over the calculation domain (not to the corresponding cells).

P7. L35: “the accuracy of the 2-D elevation data which is known to contain strong vertical bias, in particular when derived from remotely sensed global data”

The elevation data is affected not only by vertical bias but also by various random/systematic noises. I recommend to add reference to the latest research on this topic [Yamazaki et al., 2017]

P8. L4: “We found that updating fluxes reduces run times compared to states, and hence advise opting for for this option.”

How downstream boundary conditions are treated. The two test cases executed in the manuscript assumes the downstream boundary is river mouth (0m constant). Some potential users might be interested to simulate flooding in middle-stream, that requires a setting of downstream boundary conditions. Without a reasonable treatment of the downstream boundaries, it is difficult to state that the developed framework is “globally applicable”. [I also note that “for” appears twice in the sentence.]

P8. L9: “although PCR runs in spherical coordinates.”

Given that a spherical coordination can be organized by a Cartesian system, it's better to clarify that PCR-GLOBWB runs at "non-Cartesian spherical system" while it is possible to use "regular lon-lat Cartesian system" for hydrodynamic models.

P8. L30. "4 The Synthetic Test Cases"

It's better to explicitly state that PCR-GLOBWB is not used in the synthetic test case. I think this test case is done only for comparing Delft3D and LISFLOOD-FP under an ideal situation. Thus, this test case is directly not related to the GLOFRIM framework, thus readers might be confused.

P8. L34: "0.04 s m-1/3 for the 1-D run and 0.07 s m-1/3 for the 2-D run."

Does this mean the same roughness coefficient is used for river channel and floodplains in 2D run? Usually, river channels have smaller roughness compared to floodplains.

P9. L25: "5.1 Set-up"

Please describe how the downstream boundary was treated as this is critical for simulations. Please also explain how the complex channel network of the delta, bifurcating sections, and braided streams were treated. If they are treated differently by Delft3D and LISFLOOD-FP, this difference could be a potential cause of the disagreement of the simulation results.

P10. L6. "for the elevation data the smoothed 5 canopy-free elevation data was upscaled to a 2 km spatial resolution"

Please explicitly explain how the DEM was upscaled, because this has large impact on flood inundation. Did the authors took the mean within a cell, or the minimum elevation?

P10. L11. "roughness coefficient was uniformly set to 0.03 s m-1/3 for channel and floodplains".

Was the same roughness used for channel and floodplain? If so, please clearly state, because different values are usually used.

P10. L14: "For the hydrological model PCR-GLOBWB, the kinematic wave approach was used for routing outside of the coupled domain."

Please explain that the simple kinematic routing may result in poor upstream boundary inflow, as backwater effects or river floodplain interactions can be neglected. Also, this approach could be a limitation for generating a realistic downstream boundary condition. Probably, using continental-scale hydrodynamic model (such as MGB-IPH or CaMa-Flood) as an intermediate step between the hydrology model and high-resolution hydrodynamic model can be a solution.

P10. L16: "we decided to apply a regionalized optimization technique"

This "regionalized optimization" could be a limitation for using the proposed framework for "global application". The "modelling of flood inundation" may be possible at a global scale, but "global application" can be restricted by the quality of input/boundary datasets. Please state this limitation in the conclusion section.

P11. L6: "5.2 Results and Discussion"

More detailed analysis of the difference between Delft3D and LISFLOOD-FP is needed. As far as I guess from the figures, the flood peak of Delft3D is later than LISFLOOD-FP because it has larger inundation in upstream areas due to its coarser flexible cell resolution. The smaller water level amplitude in upstream must be also related to the larger inundation in upstream. Because floodplain inundation attenuate flood waves, suppress water level fluctuations, and delays flood peak, most of the disagreement between the two models can be explained consistently due to the inundation in upstream regions. The authors can analyze this effect easily, by comparing the simulated discharges by Delft3D and LISFLOOD-FP also at upstream locations other than the Obidos. By analyzing discharge, we can show where flood waves were attenuated. I suggest to include this discussion in the manuscript. And if the discussion above is true, the Delft3D simulation must be sensitive also to the spatial resolution in upstream regions, thus I recommend to include some sensitivity test on the spatial resolutions.

P11. L16: “Since the routing scheme of LFP is based on a D4 system, channel length and dimension in LPF tend to be longer than in other hydrodynamic models”

This is not precise. The D4 river network can generate shorter channel length if the scale of channel meandering is smaller than the size of the cell. Please rewrite this sentence. Furthermore, the D4 system does not only change the flow length. It alters the connectivity of channels and floodplains. Some channels/floodplains which are connected in the D8 system (or a vector system) could be disconnected in the D4 system, because diagonal connectivity is not allowed. To avoid this problem, the DEM should be adjusted to ensure the D4 connectivity. Please make a discussion on this issue, as this could be one of the main reason of the difference between Delft3D and LISFLOOD-FP.

P11. L29: “While at the most upstream station Loc3 DFM simulates lower water levels than LFP”

This is probably due to larger flooding in Delft3D due to its coarser spatial resolution in upstream, as discussed above. Please clarify.

P11. L34: “the more pronounced difference in water levels at Loc1 may simply be a local effect”

What is the “local effect”. Please explain in detail.

<References> Yamazaki et al. (2017), A high accuracy map of global terrain elevations, Geophysical Research Letters, doi: 10.1002/2017GL072874

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