Answers to Anonymous Referee #1 comments

We would like to thank Referee #1 for the constructive comments, very detail corrections, and recommendations towards improving our manuscript. We are improving our writing quality based on your kind suggestion. These comments are all valuable and very helpful for improving our paper. We appreciate that we have a chance to revise the manuscript as you recommend and to resubmit our manuscript will meet your approval.
In the following, we respond point by point to the comments. The referee comments appear in black and the answers appear in blue.

1/ My first comment is directed to the title. Flood prediction is stated as the main aim. This should be rephrased to flood simulation to avoid confusion. Prediction is often associated with forecast, which is not the aim of this manuscript.
The title has been changed into "A Dynamic Bidirectional Coupled Surface Flow Model for Flood Inundation" combined with consideration of the second comment.

2/ My second comment is related with the branding of "2D diffusion wave" with "hydrological model". It seems that the authors have developed a 2D diffusive wave model (line 139). If that is the case, this cannot be categorised as hydrological model. The title should be rephrased to "coupled diffusive-full dynamic". Unless the authors can justify the branding of hydrological model, this must be changed.
Some properties of Hydrological models are: a) only propagate information downstream; b) are inherently one-dimensional when simulating flood routing (channels or links), and c) able to simulate other discharge components such as interflow and baseflow besides direct discharge.
Hence it seems that this not fits to the description of the authors. This requires some rethinking and restructuring of the manuscript, but is required in order to avoid misinterpretation of the good work developed.
Indeed, the method proposed in the manuscript was mainly on the coupling of a 2D diffusion wave model and a full-dynamic model. Whereas, the model developed includes the hydrological processes of precipitation, infiltration and runoff routing, so we thought it belongs to a hydrologic model which will include other hydrological processes in the future, such as, evapotranspiration, snowmelt, satuated zones, etc.

3/ My third comment is related to the time steps. how are the time steps being calculated, and how are the two model synchronized?
Time steps are determined by CFL condition as following:

$$\Delta t = C_r \min\left(\frac{\Delta x_i}{|u_i| + \sqrt{g h_i}}, \frac{\Delta y_i}{|v_i| + \sqrt{g h_i}}\right)$$

This formula has added to the end of section of 2.2 Hydrodynamic model in the modified version.

4/ My forth comment is related with the display of the results. The manuscript is about coupling two different models; however, it is not clear in the plots where the boundary is.

Please add to all plots the location of the boundary between the two.
The evolvement of coupling boundary has been added to the V-shape catchment case and Helin Basin case in the modified manuscript.

5/ minor comments:
line 60, "evolved" instead of "involved"
Thanks for correction

line 60, "overestimate the flood risk in some extent" is too vague, please rephrase
The text was rephrased in the modified manuscript.

line 70 to 73, is confusing. e.g. the same sentence starts with "the next step", and ends with "the next time step", is the former not time? What is the meaning of "present flow state"? is that same as current, as stated previously? if yes, always use same wording for the same meaning.
The text was rephrased in the modified manuscript.

line 80, why is it a "significant problem" please explain.
The problem indicates the bargain of cost and benefit between simulation efficiency and resources as illustrated the following text.

line 85, remove "of"
Thanks for correction

line 86, should read "considers"
Thanks for correction

line 91, "apt to", replace with "adequate". to perform what? (not clear). "doesn't" replace with "does not"
Thanks for correction

line 82 should read "further studies are necessary"
Thanks for correction

line 109, what is "slope runoff" - consider removing the word "slope" and use simply runoff throughout the text
Thanks for suggestion.

line 173, is telemac being used, or it was "re-written". please clarify this.
TELEMAC is not used since it is developed based on unstructured grid, while the model in this paper is based on Cartesian grid.
Change in manuscript: These equations are solved using the finite volume method similar to TELEMAC (Ata et al., 2013). And the convection flux on grid faces is calculated using the HLL scheme with WAF approach (Toro, 2001).

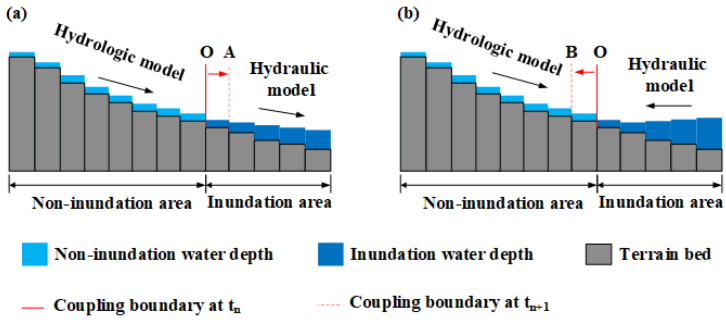
line 193, is the hydrological model 1D or 2D, if it is the latter, it can also produce inundation extents. please add some plots which show the boundary of the "hydrological" and hydraulic model being changed. Overlay these with the flood inundation extent.

The 2D diffusion wave equations were used for the runoff routing process, so it is 2D.

The time varied coupling boundary evolvement has been added to the modified manuscript.

line 212, "is moved to point A", so where was it before?

This indicates the coupling boundary position at the present time step. For the first case, the coupling boundary position will moved to A in the next step, as shown in the following figure(a).



lines 215 to end of paragraph is confusing.

The text has been rewritten in the modified manuscript.

Figures 2 and 4, what is the colour code?

Figure 2 and Figure 4 will be merged into single one as follows:

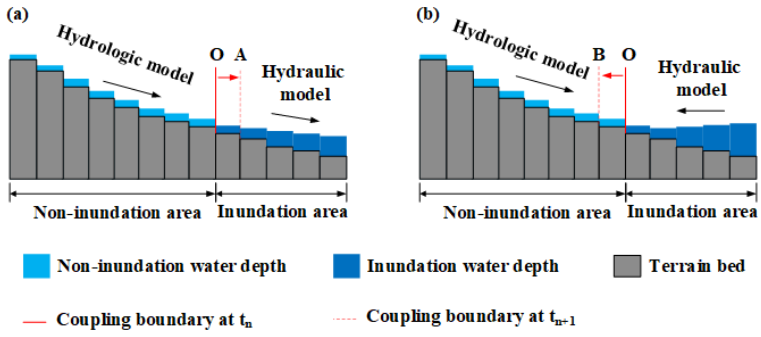
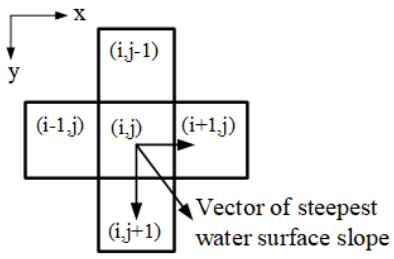


Figure 4, why water cannot flow to the left in the middle figure?

The effects of gravity are that the flow at any point will trend to be in the direction of the steepest water surface slope, as shown in the following figure:



For clarification, figure 4 was modified as follow:

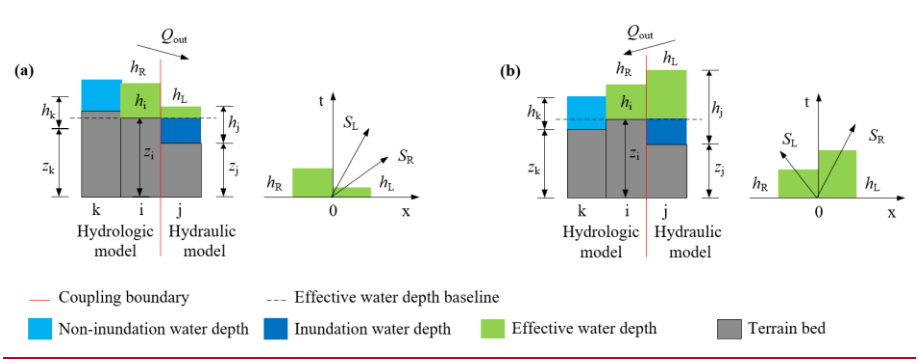


Figure 7, why are there spikes on the depth?

The spikes at the convergent wall is due to the lack of boundary fitting of the Cartesian grid. Same boundary effect can be also found in other studies e.g. (Rogers and Fujihara et al., 2001).

Figure 8, is this already with the DBCM? if yes, where is the boundary of the two models in those plots.

Figure 8 is the results of dam-break test case which is used to verify the performance of the hydrodynamic model. Thus, only hydrodynamic model was implemented in this dam-break case.

References:

Rogers, B. and M. Fujihara, et al. (2001). "Adaptive Q‐tree Godunov‐type scheme for shallow water equations." International Journal for Numerical Methods in Fluids **35** (3): 247-280.