Answers to Anonymous Referee #2 comments

We would like to thank Referee #2 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.

The manuscript presents a novel dynamic coupling approach of hydrologic-hydrodynamic simulation, where representation of flood processes and simulation can be potentially improved, and therefore a substantial research topic. However, unfortunately the performance assessment of the model has not been done properly and extensive enough to justify the strength of the proposed coupling scheme. For instance, the performance is only compared to UCMs, instead of BCM, as that's where the novelty value of DBCM lies upon and should be evaluated. Meanwhile, in the comparison to UCMs (section 4), the hydrological consideration using point source inflow boundary is not an appropriate method to support authors' claim (see below point 3).

The key feature of the DBCM, is the coupling boundary where accounting both mass and momentum transfer between hydrologic model and hydrodynamic model. And focus is on the momentum transfer which consider less in existing UCM and BCM. Since UCM always use SWE for simulation, and momentum transfer information can be readily obtained. Thus, in the original manuscript, lots of efforts were put on comparison between UCM and DBCM. Even though, in the modified manuscript, the comparison between UCM, BCM and DBCM has been added to the V-shaped catchment, see Figure 11.

I also think that the presentation of the manuscript can be much further improved in terms of language clarity, and figure presentation.

Based on the comments of the two referees, most sections of the manuscript were rephrased or rewritten. Besides, lots of figures have been regenerated, as can be seen in the modified manuscript.

More specific comments can be found below:

1) In Section 4, Scenarios in table 2 and their related text: It is not sure how the result from the hydraulic model in Case B being transformed into the spatial distribution of water depth, e.g. in figure 20.b. Is the HEC-RAS setup, i.e. in case B, considers both 1D & 2D unsteady flow simulation (i.e. in HEC-RAS v.5) OR just 1D + GIS "bathtub method" OR just 2D? Also please provide at least table or information for these setups in the supplementary so that it benefits others who may want to compare in the future.

Case B use HEC-RAS v.5 2D, and inundation area can be obtained directly through the post process RAS Mapper tool. However, in the modified manuscript, case B has been removed and this issue no longer exists in the modified manuscript.

2) I would also consider improving the presentation of figures 20 & 22, e.g. to remove the information of the elevation in the backgrounds and they may only confuse readers with many colors. Instead, since the authors validated vaguely with the record of water depth reported in the urban areas, the addition of extent, e.g. hollow polygon of these urban areas would be more useful. This

could also point out further the claim of the author regarding case A failing to simulate flood in the urban areas (lines 488 -495), or rather parts of the urban areas (higher elevation).
Figure 20 in the original manuscript has been removed. And regenerated Figure 22 using a satellite imagery base map to make it clear.

3) Line 495-498, with regards to mentioned reasoning of case A and B failing to simulate the flood depth at the higher elevation further from the river bed, I would add the obvious reasoning is the fact that case A and B contains NO distributed hydrologic modelling, they only consider point sources inflow boundary conditions obtained from SWAT, and therefore none of the spatial rainfall distribution is considered. Such failure has NOTHING to do with the lack of dynamic nor bi-directional coupling method. Therefore, the author's approach for the evaluation/ comparison is not appropriate to prove the strength of the author's DBCM.
Indeed, case A and case B in the original manuscript are not appropriate to support the strength of DBCM. In the modified manuscript, case B has been removed. Besides, the evolution of the coupling boundary added. Actually, Helin town case is just an implementation of DBCM.

4) Since the BCM coupling model (i.e. MIKE SHE + 11) is described timely and only compared conceptually along with author's proposed DBCM and UCM, why is the DBCM is only compared with UCM in terms of performance (instead of all 3)?
Results of Mike she add to V-shaped catchment case in the modified manuscript. See Figure 11 in the modified manuscript.

5) The performance of bi-directional against uni-directional coupling for flood modelling has already been compared/accessed and known overtime in literatures for its improved water transfer dynamic representation and result when setup properly. Since the author's novelty for the coupling approach is emphasize on the improved representation of changing extend of boundary or in other words spatial-dynamic on top of the BCM, I would say the strong focus of the finding should be on the performance assessment of author's DBCM over BCM instead of the obvious UCM vs DBCM.
Thanks for your advice. In the modified manuscript, for V-shaped case, we add the results from other researchers, see Figure 11 in the modified manuscript. And remove case B(UCM) in Helin town case.

6) Line 115, : : :involves the "processes of precipitation"? What does it mean within the context?
Actually, we mean "hydrological processes". The sentence has been rephrased, see line 105 in the modified manuscript.

Line 430-440 and Figure 18, not very clear, please provide a better legend for the soil type, what they mean instead of meaningless abbreviation only and imply for the model, e.g. relating to SCS number or others. Also, please consider providing appropriate reference & source to data inputs like GDEMV2, LULC, and soil type database.
Most of the datasets were obtained from online public data center. In data availability section, we have add links to these websites. These can be found in Data availability section.
The legend of the soil type has been updated, see Figure 18b in the modified manuscript.

Line 443, The selected coefficient of rainfall and floods: : : in table 2 & 3, I think you meant Table 3 & 4 instead. Also ‘coefficient of flood’ is not appropriate.

Thanks for correction. The sentence has been rephrased. See line 422~423 in the modified manuscript.

Line 483, “The red cycle: : :urban area”? Even if it is “circles/ points”, I understand they are discharge comparison site (p1, 2 & 3).

Red cycle indicates the urban area of Helin town, not p1, p2 & p3. The figure has regenerated, see Figure 20 in the modified manuscript.

Minor: Figure 17 & 18, the DEM information/ figures are repeated, and besides they are also shown differently/ not consistent.

DEM information in Figure 18 has been removed, see also Figure 18 in the modified manuscript.

Figure 19, I suggest “Simulated Discharge (SWAT)” instead of “Calculated Discharge”.

The figure has been updated in the modified manuscript. See Figure 19 in the modified manuscript.

Figure 20 & 22, considers revising the legends (incl. the scale) into one single space since they are the same rather than repeated in 2 separate figures, currently It may give the impression that only elevation legend applies to figure a while depth only to figure b.

Figure 20 has been removed from the manuscript because of the removing of Case B. And Figure 22 has been regenerated using a satellite imagery base map, see Figure 20 in the modified manuscript.

Table 2, "outflow" instead of "out flow".

Thanks for correction

Line 340, "slopes" instead of "lopes".

Thanks for correction