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Hydrology and Earth System Sciences Discussions

Interactive comment on "An effective depression filling algorithm for DEM-based 2-dimensional surface flow modelling" *by* D. Zhu et al.

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Thanks for spending time reviewing the manuscript and giving valuable yet inspirational comments. We appreciate your identification on the errors and typos and will make corrections accordingly in the coming revision.

As to the optional corrections A, we agree with most points and undoubtedly it will make the paper much comprehensive. However, as you kindly pointed out, doing that would also make the paper substantially longer and may be too long. In fact, we are in the process of preparing another related paper in which some of points of A will have been considered and discussed. It's a pity that we could not manage to publish the two in parallel but the second one will be ready soon.

Having said that, we still feel obliged to response your suggestion although in a much brief way with some of our thoughts that will be further extended in another paper: Issue 1: The difference between existing scheme and proposed scheme on 2D flow (large or small) would depend on the characteristics of DEM, which means that the impact lies on the number of Type B depressions and the corresponding quantity of retained water in the depressions. Therefore, the difference varies in different DEM.

Issue 2. It would be relatively straightforward to compare the running time of the proposed scheme in dealing with different DEM resolutions. However, it would be much more difficult to compare the running time between existing GIS scheme and the proposed scheme, as one is based on 1D flow solution while the other is designed for 2D flow. Besides, the proposed scheme is based on the algorithm developed by Wang and Liu (2006) in which the result of has been proved to be effective and faster than other filling algorithms in their paper.

For issue 3, the comparison may initially seem to be a bit unfair in terms of the simulated flows from whole-module simulations by MIKE SHE. However, the comparisons of surface flow solely in MIKE SHE are quite fair (see fig 11-13 in the paper) as only the overland flow module was included without any other input from the infiltration and groundwater interaction. As such, the rainfall-runoff process was driven entirely by the terrain elevation and the Manning's number, which have nothing to do with the model parameters. The difference is even more informing without the smoothing effect from unsaturated and saturated modules in MIKE SHE. Again, the impact on the final 2D outflow from existing scheme and proposed scheme lies on the mathematic structure of hydrological models and the proportion of surface flow in the final outflow.

We trust that the points have been sufficiently clarified. Once again thanks for your comments.

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