I have received two reviews of the manuscript. Both Referees have suggested major revisions. Most of them were properly addressed by the authors in their responses. However, there are some criticisms in the Referees' reports, which were answered by the authors in a rather formal manner. I have summarized these criticisms in the following two comments:

(1) The manuscript is devoted to the 2-D floodplain flow modeling, that is the problem whose solution is strongly depended, first of all, on progress in two fields: technologies (from computer capabilities to technologies of measurement and data processing) and numerical methods. Both fields are explosively advanced during the last years and recent opportunities for the above problem solution are much wider than those even 10 years ago. Nevertheless, the most recent publication cited in the manuscript is (Gichamo et al., 2012) and more than half of the references were published in 1990s and early 2000s. I fully agree with the last comment of the 1st Referee and suggest the authors renewing the reference list. Particularly, I suggest reviewing (not just mentioning) publications, where high-resolution LiDAR data were used directly for 2-D simulation, and clarifying weaknesses of this method by the example of more recent publications than (Marks et al., 2000). In my opinion, reasonable choice of mesh type (Cartesian grids, constrained and unconstrained triangular grids, constrained quadrilateral grids, mixed meshes, etc.), which is adapted to specifics of floodplain surface (e.g. takes into account linear objects, as 2nd Referee pointed out), allows one to integrate high-resolution DEM into 2-D model and maximizes computational efficiency (see, for instance, Alexeevskiy et al., 2014; Kim et al., 2014Li et al., 2014)

(2) Inverse distance weighted (IDW) interpolation is used for calculating flood water level in DEM grid cells and is one of the key-pont of the method proposed by the authors for solving high-resolution DEM associated problem. The IDW interpolation is very easy to use but, I agree with the 2nd Reviewer, this method can lead to poor results of interpolation of high-resolution topography into nodes of computational mesh. Weaknesses of the IDW interpolation are well-established and result from the fact that this method doesn't satisfy the major requirements for deterministic methods of interpolation on arbitrary sets of points; such requirements as linearity, monotony, uniqueness (Sukumar et al., 2001). This disadvantage results in the following, just for example, consequences: a patterns are smoothed between the data points; the interpolated pattern can have a discontinuity in slope at the data points; interpolation results depend on size of search window and poor window choice can produces artefacts when use with high-resolution data (e.g. Kravchenko, Bullock, 1999; Hartkamp et al., 1999; Blöschl, Grayson, 2000; Yasterbi et al., 2009). I recommend the authors discussing interpolation methods and showing advantages of the IDW interpolation for the problem under consideration.

I recommend the authors to consider these principle criticisms and re-submit the revised manuscript for the Editor's review and final decision on opportunity of publication in HESS.

Also there are some technical corrections

1. P. 2018, L. 6: the terms $S_{**}$ differ from the corresponding terms in Eq. 2. Please correct.
2. $Z_{cell}$ and $Z_{waterlevel}$ terms are specified after Eq. 4 and repeated after Eq. 5.
3. P. 2024 L. 13: "220 000 Rows"; Fig.3 caption "22 000 rows" What is correct?
4. It is not necessary to repeat three times the IDW equation (Eqs. 6-8)

Alexander Gelfan,
HESS Editor