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Comment on hess-2021-60

Anonymous Referee #1

Referee comment on "A reduced-complexity model of fluvial inundation with a sub-grid representation of floodplain topography evaluated for England, United Kingdom" by Simon J. Dadson et al., Hydrol. Earth Syst. Sci. Discuss., https://doi.org/10.5194/hess-2021-60-RC1, 2021

The objective of the paper is not clear or basically wrong. For England, an accurate appraisal of flooded areas and depths for the 100 year flood is available on a majority of locations so that the results of the calculations are useless. Such calculations as proposed in the paper can be useful for more extreme floods for which the uncertainty is always higher because of both the uncertainty of the flow and the lack of calibration data. They may be also useful for countries in which the data are scarce, maps of historical floods are lacking and for which simplified calculations could permit to obtain a whole coverage of the country without costly studies. If the ultimate objective is one out of the two quoted here above, the text should be reoriented in order to be sure that additional data for extreme floods or raw data would be available.

The structure of the paper is also not clear. First, the organization of the paper is not provided at the end of § 1. Second, the method (the best one) should be first described and second, the validation (or calibration) of the results should follow. Third, the discussion could question some aspects of the method and/or the efficiency of the method comparing it to other methods. However, the paper is not organized like this. It seems to me that the paper begins by discussing te one and then the other component of the method. For instance, if table 1 sums up the comparison between alternatives, a conclusion should be provided just below; in the paper, oppositely, one of the three methods is compared to an other estimate on a data set that is not described and appears to date from before 1991 (30 years ago !). If such data set is a reference, other references should be provided; if not I guess that the conclusions from this data set are questionable similarly to other studies that tried to link bankful depth to drainage area (they are a lot not quoted here!). Similar procedure is used later on for wbf (instead of hbf) without more explanations and any clear justification.

The third parameter to be estimated is the channel roughness: I really do not understand the few lines of explanations (how estimate roughness from a database of river cross-sections?) but I retain that it is so difficult that the authors are using a uniform value of

0.04. Similarly, I could not understand what is the relaxation time at the bottom of page 7 but I am quite sure that this parameter is not linked with the there previous ones and thus the explanation is not at the right place.

- 2.4 describes two sub grid parametrization; however, because the detailed procedure of the whole method was never presented, the reader cannot understand the advantage of any sub grid parametrization compared to the solution to establish directly a relation between depth and inundated area from the DTM source.

From §2.5, I understand that the authors are not using the maps of the flooded area but a percentage of flooded area per Km2 to validate their model. However, form §2.6 I understood that the plot for comparison is 50 m x 50 m. What is correct? I ask the authors clearly explain what means a hit rate of 71%, a FAR of 9% and a success score of 67% in simple words. The following discussion between the various regions does not interest me because the tables 4 and 5 show similar results from one region to another one. It might be more useful to show quantitative results at smaller regions (subregions) for which the results are very different.

For such a type of model, the sensitivity analysis is a key issue and so a wider sensitivity analysis was expected. Moreover, I could not understand why the results are not sensitive to the Manning coefficient : once the geometry is provide transforming a flow into a flooded area or flood depth should depend on the Manning coefficient if hydraulics equations are used.

The first sentence of the conclusion should be written in a different way in order to avoid confusion: I understood that the authors calculated the percentage of flooded area for their studied area quite accurately but they are not providing a map of flooded areas accurately and are not providing at all the peak water depth of the 100 year flood in any location accurately. The objective (for validating the method) should be to provide such results as the ratio of the water depths: reference over calculation for any location (for instance on the 50 m grid).

As a conclusion, I guess that with a new organization of the paper, adding explanations and real validation of their method, the authors could obtain a valuable paper. However, I am not sure that the method is useful for England and that the method can be extended to other countries because a lot of "morphological" equations are very specific to the local geography.