

Interactive comment on “Global flood hazard mapping using statistical peak flow estimates” by C. Herold and F. Mouton

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The authors present a possible approach to global flood hazard mapping using statistical peak flow estimates. The idea of a global flood inundation map that represents 1:100 years events is definitely worth pursuing and very much needed.

I found the paper a very interesting read and I believe this work should be published but I have some major comments/concerns that I like the authors to address before considering this paper for publication, in particular on the DEM suitability and the flood model they apply. In many places, the authors also need to give more explanation or clarify points as they often employ ambiguous words like ‘satisfactory’, ‘doubtful’, ‘good’, ‘best’, etc.

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Below I have given a series of major comments that I'd like the authors to address:

- In their introduction the authors give the impression that there are to date no satisfactory efforts to produce global databases of flood maps but later on they use the Dartmouth Flood Observatory to assess the adequacy of their maps. The authors should mention this database at the start of their introduction more clearly
- P308, line 5: What are these transformations? Please provide more detail
- P308, line 9: Please replace 'satisfactory' with 'meaningful'
- P308, line 23: The authors should briefly outline here what type of model this is
- P309, section 2.1: Are these datasets taken from the global runoff data centre (GRDC)? Please give the source of these data and what the expected accuracy of these data might be, even if it is only in qualitative terms
- P309, section 2.2: It should be stated here that this is based on the SRTM DEM
- P311, section 2.5: How was it established which one of these observed flood maps represents a 1:100 years event, so it can be compared to the modelled one?
- P317, section 3.3.2: How exactly is Manning's equation implemented in this model? How did the authors derive cross-sectional area without depth information and how was the parameter 'n' (roughness) determined? Was the typical version of this formula rewritten to accommodate the many limitations of the data used? If so, this needs to be stated. Also, how did the authors assess that their generated stream centreline is in the true position and still has all the important connectivity throughout? Is this resulting model, after inferring stage, a simple flood-fill operation using the DEM and so the model is not really dynamic? This should be clearly stated and more detail should be given.
- P321, lines 14-22: This paragraph is not very clear and I suggest re-writing it. For example, what exactly does 'best' regression mean, and did the authors apply a test to

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search for outliers, which one? And what exactly did their methodology of adding and subtracting variables from the model involve? Is it a bootstrapping-like method they used?

- P322, line 18: What do the authors mean by 'doubtful'?

- P325, lines 1-10: Here the authors list a number of situations which the applied model does not account for and thus produces low accuracies. Could the authors list a possible 'solution' for each that they might have used to overcome or work around these issues?

- P326, line 4: The authors should quantify these differences observed in Figures 14 and 15?

- P326, line 20: Again, could the authors quantify these observed pattern differences instead of saying 'confirms the trend visible in Fig. 16'? For instance, a simple spatial performance measure reflecting areas in error could be computed

- P327-328: Maybe this section should be called 'outlook' and there should be a section 7 for the conclusions? The present section 6 'Conclusions' to me sounds more like a section outlining limitations and suggesting improvements rather than a concluding section.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 305, 2011.

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