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Hydrology and Earth System Sciences Discussions

Interactive comment on "Towards automatic calibration of 2-dimensional flood propagation models" *by* P. Fabio et al.

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This is a an interesting paper with a detailed study of an approach for automatic calibration of a flood inundation model, based on the variation of friction parameters with accuracy assess based on point measurements of maximum flood depth within an urban area. Ultimately the paper promises to be excellent and is worthy of publication in HESS, but at present there are some short comings with respect to the data used and the flood event studied. Neither is adequate to allow any firm conclusions to be drawn – they will not prevent publication, but need a bit more careful handling to ensure that the best possible paper is obtained.

The validation data which are available are point based measurements of maximum

flood depth and imagery of flood inundation extent. Because the flood event was a large valley-filling event, and the valley studied was topographically well contained, the flood extent data were not used since changes in predicted flood depth do not make any substantial difference to the area of inundation. This is appropriate, although it would still be good to show these data in a figure and illustrate how well the model predicts inundation extent. It is a shame that it wasn't a smaller event which would have allowed these data to be included as a more rigorous test. Without them, the study is limited to using the point measurements of maximum flood depth, which would be ok if they were spread across the whole domain, but unfortunately they only based in the urban area. Therefore, it is entirely unsurprising that the spatially distributed friction values from outside of this area only have a limited effect on accuracy and hence display equifinality. It may also be better to calibrate against water elevations rather than water depth – this would avoid minor errors from the DEM creeping into the calibration. Can the measured depths be converted to water elevation?

Currently the paper is trying to do too much with a limited dataset. To this end, some simplifications are needed in the paper. I suggest the following either (i) only look and channel/ floodplain friction, therefore avoiding spatially distributed friction – this would reduce the equifinality observed but still illustrate the principles of automatic calibration, which is the main focus of the paper; or (ii) reduce the domain size so that it focuses on the urban area and the calibration points are then spread more completely across the domain – this would again reduce the equifinality; however, the domain size may already be rather small and care is needed to avoid boundary effects. Or if additional data for a second event were available, they would be very helpful – although this seems rather unlikely.

Given some changes, the paper will be a useful addition to the body of work on model calibration/ validation. It is a good first step towards an automated calibration process.

Specific comments: P6836, L17-22. The selection of these roughness scenarios seems quite arbitrary. It may be better to use a true continuous friction value based

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on the land cover. See for example Mason et al. (2003, Hydrological Processes 17(9), 1711–1732) or Wilson and Atkinson (2007, Hydrological Processes 21 (26) 3576-3596) for possible methods. These continuous surfaces would provide for a more rigorous approach in defining spatially distributed friction.

P6838, L14. What is the source of the DEM used? LiDAR? You provide a scale of 1:10,000, but scale is only one aspect – precision of the data are very important. 1:10,000 contours are still not good if only every 5 m. Please clarify what topographic data you are using, and provide an estimate of their likely accuracy.

P6841. Comment on the possible use of computational clusters for PEST analysis.

P6842, L21-25. Similar to with the roughness scenarios, the dividing of the land use into principal regions is open to subjectivity. What process did you use to achieve it? This needs greater discussion since it is a fundamental part of the study.

P6844, L17. Why didn't you exclude the unphysically realistic negative parameter values from the calibration set?

P6844, L18-21. This discussion regarding the parameter values and equifinality would be better if put into the context of accuracy – so probably better moved to the discussion.

P6844, L25-29. Expand on this brief discussion of the use of PAR2PAR – this appears to limit you somewhat.

P6845, L2-5. The other calibration criteria should be illustrated here.

P6845, L19-21. The RMSE is quite high ($\sim$0.8 m) and very similar for all. What is the error in the measurements used? You could illustrate the calibration behaviour using dotty plots.

P6845, L24. The roughness values are also suppressed because only maximum flood depth is used as comparison – this misses much of the dynamics which friction values

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will have an influence on.

P6847 – It is difficult to assess quality of DEM here since we don't know where it came from or how it was produced. We also don't know how the calibration points were obtained and their likely error, so we don't have any idea of the relative error in each dataset. These should both be discussed earlier in the paper. But in any case, I don't think that removing calibration points is appropriate here, and doesn't somehow balance the model complexity with available data. It is always better to have as many points in as possible, which then may highlight the shortcomings of the model, as they have in this case. I suggest skipping this last step as I don't think it adds anything to the paper.

Minor points/ typos, etc:

P6834

L1. Suggest rephrasing to "...are an essential tool in many fields and are used, for example, for flood hazard and risk assessments, evaluation of flood control methods, etc."

L8, end. Suggest rephrasing to "This for two primary reasons:"

L9. "...lack the relevant data against which the models..." – insert "which"

L11. "Second, two-dimensional models are..." – delete "–ly, especially the"

L21. "...too simple a model setup." Insert "a"

L22. Insert comma after equifinality.

End of abstract – briefly summarise key findings of paper here.

P6835

L2. "...models which simulate...". Delete "to date"

L13. "...roughness coefficience in models...". Remove "the".

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L15. “In addition, roughness coefficients often have to compensate for insufficient...”

L16. Remove “as well”. Change “efficient” to “effective”: change this throughout paper.

L17. “praxis” -> “practice”

L18. Remove comma after “time-consuming”

P6836

L9. “times” -> “time”

L11. Define PEST acronym here.

P6837

L1. Rephrase to “Sufficient calibration data rarely exist for flood propagation models, but in the study area a flood event in August 2002 was well documented”. Please introduce the type of data documenting the flood event here – i.e. images, point based depths, trash lines etc.

L6. “certainly” -> “necessarily”

L7. “Here” -> “In this paper,”

L12. Insert comma after “In this study”

L17. Remove “follows”

P6839

L25. Remove “follows”

L7. Change “manpower” for gender-neutral language

L23. “use” -> “uses”

P6841

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L22. Rephrase to “The urban area of Eilenburn, located in Saxony, Germany, was used in this study.”

P6842

L1. Remove “the” after “state”

L3. “enormous” -> “large”

L5. “yielding” -> “yielded”

L20. Remove “however”

P6843

Section 4.1 – move to the methods section.

P6845

L9. Insert “greatly” after “without”

L16. Equation – move BIAS to end to match order in text.

P6846

L2. Remove “also”

L14. “and possibly reduced it” -> “and to possibly reduce it”

L18. “For this end” -> “To this end”

P6853 – Table 1. It is unclear what you mean by “none” – do you mean all land covers were used, i.e. fully distributed?

P6854 – Table 2. Label k parameters as A-F scenarios?

P5860 – Figure 1. Label the bypass in this figure to be consistent with the text.

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Interactive Comment

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 6833, 2009.