Hydrol. Earth Syst. Sci. Discuss., 6, C2670–C2672, 2009
www.hydrol-earth-syst-sci-discuss.net/6/C2670/2009/

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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Received and published: 19 November 2009

This is an interesting paper - worthy of publication in HESS. This is the first time I've seen automated optimisation used for a complex flood inundation model like this - this may become a much more widespread technique as computer power increases (especially through multi processor machines to which it is well suited).

There are some points which need further comment from the authors in the text:

In equation (4), Q represents the inverse covariance matrix - this would imply that the method takes into account both variable errors in the observations, and the correlation between them. A major difficulty in calibration studies is dealing with correlations in the

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observed data - this tends to place more weight on areas where more measurements have been acquired, which may not be justified since the errors in these measurements may be strongly correlated. This method appears to offer a way to deal with this - have the authors looked at correlation between the errors, or assumed them to be indepedent?

The authors calibrate the model against water depths rather than water elevations - this will have a large effect on the sources of error. Hydraulic models are generally much better at predicting water levels than at predicting water depths - the latter are much more sensitive to topographic errors. This may explain the spatial distribution of errors, which coincide with steeper slopes on the floodplain (these are the places where locational or sampling errors will have greatest effect). How would the results be affected by using water levels rather than depths?

What is the length of the model reach? Fig 3 indicates it's 6-7km. My concern is that the reach is short, and therefore the dowsntream boundary condition (the authors should describe this) will have a significant influence on water levels throughout the reach (see Horritt et al, 2007, Comparing the performance of a 2-D finite element and a 2-D finite volume model of floodplain inundation using airborne SAR imagery, Hydrological Processes, 21(20), 2745-2759, for a discussion of this for a similar type of reach). If the downstream boundary condition is wrong, it may need to be compensated for by unrealistic roughness parameters. How can the authors be sure this is not happening in this study?

The authors are right to be cautious about rejecting observations because they don't fit the model (it's more likely that the model is wrong). Effectively, observations are rejected if they are not included in the range of model predictions from the range of input parameters. This type of behaviour is generally associated with an inadequate model - is there any evidence for large model errors that may cause this? I notice there are two bridges across the channel (I think!) - are these represented in the model? How confident are we in the hydrometric data? The authors should include a discussion of

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these possible error sources.

Some typos etc to fix:

P6833: Line 11: Suggest change to "Secondly, two dimensional models in particular..." Line 16: The model can't be described as a full 2D model since it neglects the advection terms - suggest calling it a simplified 2D model instead.

P6834: Line 16: "efficient" should be "effective" Line 17: Suggest change "praxis" to "practice" Line 25: Optimisation methods such as these have previously mostly been applied to hydrological models where parameters can be less well defined (ie less physically based) - the authors should mention this.

P6835: Line 5: Suggest change "non-linear" to "complex".

P6836: Line 1: Suggest change "hardly ever" to "rarely do"

P6837: Line 11: Change "explicit" to "varying"

P6838: Line 6: This sentence makes little sense - rewrite.

P6846: Line 19: I'm not sure what the authors mean by "chroncially" - "typically"?

P6848: Line 21: Change to "orthogonally".

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