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Hydrology and Earth System Sciences Discussions

Interactive comment on "Fuzzy set approach to calibrating distributed flood inundation models using remote sensing observations" *by* F. Pappenberger et al.

N. Hunter (Referee)

Neil.Hunter@bristol.ac.uk

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General comments

The paper by Pappenberger et al. develops a novel approach for applying fuzzy set theory (FST) to the problem of calibrating a non-error free, spatially-distributed flood inundation model using non-error free, spatially-distributed observed data. By accounting for errors in the remotely-sensed flood shoreline observations, the proposed FST calibration methodology seeks to provide a more realistic estimate of uncertainties associated with the spatial predictions from a 2D flood inundation model. FST has not previously been used to address spatial uncertainties in this specific field and the paper provides a good overview of how to apply these techniques in practice. Additional references are also cited should more detailed information on the subject be sought.

The FST approach is compared alongside traditional cell-by-cell objective functions for comparing modelled and observed spatial patterns. Although this is a logical step, the application-specific nature of these evaluations means that this comparison does not provide an objective test of the relative merits of each approach. The two methods certainly produce similar results (Figure 5) and so it is difficult to ascertain any direct benefit from adopting the more-computationally intensive FST approach. An extremely limited discussion in Section 3.1 does not help matters in this respect.

In addition to the uncertain pattern comparison methodology, this paper also makes use of a variance-based global sensitivity analysis (GSA) to investigate parameter response within the model calibration. When applied correctly, GSA has been demonstrated to be an extremely powerful diagnostic tool for a range of environmental modelling problems, and can be used to quantify the first- and second-order significance of individual parameters within a particular model structure. Whilst the authors should be applauded for their ambition in combining FST and GSA techniques, the paper's impact suffers as a result. Neither the FST or GSA aspects are explored in sufficient detail to justify their future inclusion in a practitioner's modelling toolbox. Due to a large number of calibration parameters and a limited number of simulations, the authors have failed to maximise the benefits offered by GSA and, as a result, can only speculate on the cause/effect of the observed parameter responses/interactions. To better demonstrate the value of the FST approach over traditional comparison measures (a central aim of the paper), a simplified calibration problem - for example, lumped channel and floodplain roughness coefficients only - could have been adopted. This would have removed much of speculation and conjecture contained in Sections 3 and 4 and Table 3.

Overall, this paper represents a solid attempt to demonstrate the value of FST to the flood modelling community. Non-expert users of FST will appreciate the clear introduction provided in Section 2, although a clear sequence of steps (or flow chart) to

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show how the different functions/measures are interrelated and applied in the analysis would have been useful. For the most part, the paper is well-written, structured and referenced.

The main criticism of the paper relates to the extremely limited discussion presented, particularly in relation to establishing the utility/efficacy of FST-GSA for flood model calibration. This concern is exemplified by Figure 7 and Table 4. This is a real shame and something that I know the authors have remedied in later publications (Pappenberger et al., accepted for Journal of Hydrology). I would therefore encourage interested readers to look beyond the obvious limitations and consider the potential of these techniques for more robust uncertainty estimation of spatial flood predictions in the near future.

Specific comments

1. Predicting inundation patterns versus flood depths. For this initial investigation, both the model predictions and the remotely-sensed observations against which they are compared are assumed to be two-dimensional and binary (i.e. wet/dry) in nature. Whilst this is standard practice in many flood inundation studies (e.g. Horritt, 2000; Aronica et al., 2002; Yu and Lane, 2006; Bates et al., 2006), it may be argued that comparing modelled and observed spatial patterns is of limited use for determining flood risk/hazard. Unlike a simple wet/dry classification of model results, water depth is a continuous variable and will present many more problems for generating useful hazard assessments than alluded to here. It should also be remembered that good pattern predictions often do not equal good depth predictions because of the laterally-constrained nature of many floodplains. Some additional comment/discussion to this effect would be very useful.

2. Calibration strategy outlined in Section 2.4. With the exception of inflow representation, I really can't see how the sampling ranges and distributions for the various calibration parameters are justified. I believe there are several inter-related issues causing confusion/problems here. Firstly, though the roughness parameters are obviously ef-

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fective (although this is not stated), the upper bound on the range specified for the channel is too high to be sensible within the model structure. In LISFLOOD, such high values will almost certainly swamp the influence of the other parameters (possibly with the exception of Qin), and are probably being used to compensate erroneously for errors either in the floodplain topography or in the channel bathymetry parameterisation strategy. Also, in Table 1, what is a ‘log’ distribution? Do you mean log-normal? If so, how is this justified? It would assume some prior knowledge of roughness that you don’t appear to have here.

Second, it is not clear how the error sampling on the cross section data is a useful or meaningful addition to the calibration problem. Sure, there will be errors in the data but fudging this assessment to work with a kinematic wave model (i.e. resampling to maintain a positive channel gradient) is not, in my opinion, the way forward. Also, will the sampled bed elevation channel not just tend further and further away from the original data value as you move further d/s? If so, is this effect being compensated for in the potentially very high values of n used?

3. Limited discussion of parameter (in)sensitivities. No conclusions drawn over the relative significance of parameters within the calibration problem. Is this not one of the intended goals of GSA? I also don’t understand Table 4.

4. No comparison map for Figure 7 using traditional cell-by-cell objective functions. A inundation likelihood map constructed using one of the standard performance measures would be a useful benchmark for the FST approach.

5. LISFLOOD-centric discussion of results in Section 4.1. Many of the results are interpreted primarily in terms of the numerical model used (LISFLOOD-FP) in section 4.1. However, Section 2.2 states that it is the methodology, and not the model, that is the significant development in this paper. This problem is compounded by a number of incorrect statements about the LISFLOOD-FP code - e.g. the depth threshold for flow calculation is 1 mm and not 100 mm as stated in the text.

6. The “disconnected” areas of high inundation hazard observed in Figure 7 are probably due to large ponds/storage areas on floodplain (i.e. topography-based) rather than any model-based explanation.

Technical corrections

p2245, l11: Consider using “relatively” instead of “rather”. Matt Horritt’s work on the topic would suggest otherwise.

p2246, l17-20: Consider replacing existing sentence with “The methodology applies the 2-D LISFLOOD-FP model within a Generalised Likelihood Uncertainty Estimation (GLUE) framework to derive the possibility distribution of inundation extent for an 8 km reach of the River Alzette, Luxemburg.”

p2247, l24: Consider adding “to generate inundation predictions”.

Second paragraph of Section 2.2 is poorly written, particularly the fifth sentence (˜6 lines long). Rephrase.

Section 2.3 is also poorly written & structured. The first four sentences need rearranging to make a coherent argument. What is an “acceptable level of performance”? More specifically: p2248, l16: Consider replacing “factors (particularly distributed parameters e.g. frictional coefficients)" with "parameters (e.g. frictional coefficients)”. p2248, l19: Pappenberger & Beven reference required. p2248, l25: “framework.” p2248, l26-27: Consider replacing “MC approaches consists of running repeated simulations of a model using a range of values for each uncertain input parameter.” p2249, l1: Replace “more normally” with “typically”.

P2249, l21: What do you mean by the “the error due to the raster size”? Rephrase.

P2251, l13: Remove the comma after “thus” and the plural of “formula” is “formulae”.

P2252, l12: Consider replacing “remote” with “remotely sensed”.

P2252, l16: Consider replacing “no” with “zero” onwards.

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P2254, l22: Consider replacing “sub-heterogeneity” with “sub-grid scale heterogeneity”.

Figure 3. Pixels are shown at 50m resolution?

Figure 4. Wrong graphic inserted.

Figure 7. A standard/benchmark graphic would be beneficial to demonstrate clearly the advantage of the FST approach advocated here.

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