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# *Interactive comment on* "Topographical changes caused by moderate and small floods in a gravelly ephemeral river – 2D morphodynamic simulation approach" *by* Eliisa Lotsari et al.

**Anonymous Referee #2**

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The paper provides a detailed account of the calibration of a numerical model (a 2d implementation of Delft 3D) in order to simulate the geomorphic changes during flash flood events. The main novelty and significant findings of the work are the predictions of when sediment transport occurs during flood events of different magnitudes, based on a model simulation calibrated versus observed changes. It is an interesting and thorough piece of research.

However, the purpose of the work and the findings and conclusions get lost in a highly detailed description of the calibration method. The calibration itself is not particularly novel, although as it includes observations spanning two events it is more robust than



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Discussion paper

many other examples. Much of this detail could be moved to supplementary materials, and a concise summary provided in the main manuscript. This would allow the manuscript to be focussed more on the simulations themselves, what they show, and why this is important (I felt this last point was not made clearly enough throughout).

The authors at several points refer to sensitivity testing performed on the model. Although evidently there were some tests performed to assess model behaviour differences with some variations, these are not sufficiently thorough to be considered a sensitivity analysis, and falls short of the level of analysis performed by operators in other modelling fields. For example, the tests performed would not ascertain any non-linear interactions between the parameters being tested. For interest, Ziliani et al (2013) provides a useful and efficient methodology for screening model parameter sensitivity in reach-scale geomorphic models, which might be useful for future work. The authors should be careful using terms such as sensitivity analyses, and the conclusions they draw from the tests should not be presented with as much certainty as they presently are. The tests may be useful for informing the calibration process, but cannot be relied upon to suggest anything concrete about the model behaviour.

Below are some notes for correction or additional clarification:

Page 1, Line 15 – use “caused by the flood” or “resulting from the flood” instead of “performed by”

Page 1, Line 16 (and throughout) – strictly speaking, a 2D implementation of Delft 3D

Page 1, Line 24 – As explained above, I don’t think a rigorous enough sensitivity test has been performed to make these conclusions with such certainty

Page 1, Line 24-25 – Is the total load equation probabilistic? Or is it implemented in a deterministic way too?

Page 2, Line 13-16 – I’d like to see something here to explain why this is important. Why does it matter that we know this?

Page 2, Line 30-34 – The authors are also in danger of overparamaterising the model by using a single calibration against a small set of observations. It risks equifinality with the model matching the data well, but there are many ways that the changes could have come about.

Page 3, Line 19 – With two events the authors could have calibrated with one and validated with the other as an extra check.

Page 4, Figure 1 – It would be more useful to see the reach in context with the wider catchment here than its rough location in Spain. How big is the catchment and where is the reach in relation to the gauging station used?

Page 6, Lines 10-16 – What is happening between in 18km between the gauge and the reach to increase the discharge. Are there more channels flowing in? Has there been more rainfall inbetween? Convective events are likely not to span 18km so rainfall will not be uniform. This is key as the authors make the assumption that the hydrograph is the same shape when flow makes it to the reach, just scaled larger, yet the reasons for the increased discharge will also likely change the shape of the hydrograph and this potentially invalidates the calibration and the conclusions. More detail needs to provided here to justify the above assumption.

Page 6, Lines 17-22 – By calibrating the discharge against water levels there is another assumption that the bed levels were static and had no influence on the water levels in the flood (despite in the discussion stating that bed changes are an important influencer on water levels). This will lead to some uncertainty cascading to the simulations.

Page 9, Line 30-31 – What is the expected level of uncertainty in the measurements – is 1.6mm difference significant?

Page 11, Line 15-16 – If the Manning's n values were set according the geomorphological elements, did they alter with changes during the model runs, or were they stationary?

Page 11 Line 18-20 and Table3 – either present the values in the table in the order of Qx1.3, Qx2 and Qx2.5, or present the values in the text in the order of the table.

Page 12, Line 31-32 – Tells you something about model sensitivity to parameters, but not a great deal.

Page 13, Line 1 – Need to know what the criteria was for this! “best and most interesting” not a rigorous methodology. The authors might have rejected useful information by arbitrarily labelling it as not interesting.

Page 13, Line 2 – Not a sensitivity analysis.

Figures 4, 5 and 6 – These would be easier to interpret if they showed the changes over the whole reach, not just in the area used for the analysis. This could still be identified as in Figures 1 and 8.

Page 19, Line 18 – “satisfactory” not “satisfying”

Page 19, Line 25 – or other factors had an influence? Interaction with other parameters?

Page 22, Line 8 – Has it been shown that it is reliable? It’s shown it is able to reproduce the events it was calibrated against, but not others.

Page 23, Line 2 – Yes, validation of the calibrated model versus independent events is needed to claim the model is reliable.

Page 23, Line 4 – Yes, the uncertainties need to acknowledged, maybe a little more prominently then there are at present.

Page 23, Line 10 – The authors have not provided enough detail to evidence that the hydrograph is of “a known shape”. It may well be, but the gauge is 18km away with half the discharge (according to the calibrations).

Page 24, Line 13 – Both equations are implemented determinisitically are they not?

Page 25, Line 13 – Are the parameter sets non-stationary or is it the initial conditions which need to be set for each event?

Page 27, Line 18 – Instead of “goodness” use “feasibility”

Page 28, Line 6 – Both implemented deterministically?

Page 28, Line 8 – This is a key point, and should be made more prominent earlier in the manuscript as part of the reason why this research is important.

Page 28, Line 16-18 – Can this be said with such certainty, or does it just apply to this reach for these events (and also with these parameters)

Ziliani et al (2013) - doi:10.1002/jgrf.20154

Thank you for an interesting study and manuscript, I look forward to seeing the revised version.

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