

Interactive comment on “Simplified representation of runoff attenuation features within analysis of the hydrological performance of a natural flood management scheme” by Peter Metcalfe et al.

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Received and published: 4 September 2017

This paper investigates the extent to which natural flood risk management might have mitigated the effects of a series of extreme storms in northern Britain. The paper represents an important contribution to the literature which will be of broad interest to hydrologists concerned with the theoretical aspects of this problem and to applied workers involved with flood risk management and I recommend publication in HESS subject to a number of revisions to clarify the text and to resolve a series of queries outlined below.

1. The term “runoff attenuation features” is establishing itself in the literature but it is an

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unfortunate use of overcomplicated language where simple language will do. In some cases RAFs are simply what most people would call ponds! In other cases the use of the term obscures the relative effects of storage and attenuation and glosses over the variety of interventions that might be possible by lumping them together as RAFs. My suggestion is to be clear about which types of intervention are being referred to at the outset, and to prefer simple language if possible.

2. Some clarity on how the drainage time constant is estimated (p3. Line 5) is essential. It appears to be allowed to range over two orders of magnitude for the purposes of the uncertainty estimation exercise. Fine if that's all that can be done but some discussion of what values the parameter might sensibly take is warranted in a paper of this kind. Is it considered an observable parameter that an engineer might design to or control?

3. Given the simplicity of the model there is an overly confident equation of model performance with the operation of the "real world". For example on p. 3 line 20 after only just having introduced the modelling approach it is declared that the model "can be used to examine the drain down, filling and possible overflow of these features during the course of storm events." I understand the point that's being made but it would be as well to note that the model can only provide an understanding commensurate with the fidelity of its representation of the interventions. The opportunity on p5 line 20 to describe whether different types of feature behave as modelled is also missed (and also on p6 line 25 when it is stated that tree shelterbelts can be modelled using this approach too). I suggest that some additional text in these parts of the manuscript would help to improve the paper.

4. A key part of the research problem is to distinguish between storage effects and network / wave propagation effects. This is very clearly articulated on p4 lines 21ff, an explanation that might more helpfully be given earlier in the paper. The comment p17 line 22 that network effects are important is appropriate to the discussion (and has been raised in the literature before) but it is hard to see what new evidence is offered for this view in the present manuscript.

5. The tables indicate the results of the study very clearly but the text is a little more equivocal, and is unclear in parts. For example, it would appear from the tables that the RAF10 intervention shows a 30% reduction in peak flow – an important effect - but this is not discussed until the conclusions and is only mentioned in passing (p16 line 32). More detailed presentation of the results in Section 5 is warranted, in my view.

Minor comments

There are a number of typos / grammatical mistakes / errors of punctuation throughout, which might usefully be corrected at this stage.

The title of the paper somewhat understates the conclusions of the piece. In its current form the title suggests that the paper offers only an improvement in method. No doubt an improvement in the simulation of runoff attenuation features would be worthwhile in its own right but the work presented takes the ideas further and in fact evaluates and seeks to draw conclusions on the performance of systems of RAFs under multiple storm conditions. I'd suggest that the title be edited to reflect the wider scope of the paper as written.

Please clarify the statement at p14 line 6: “Due to hydrodynamic storage utilisation exceeds 100% at the peak of Storm Desmond. . .”

Table 4. Consider the implied precision of the modelled estimates when quoting simulated peak delays in hours to two decimal places.

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Interactive comment on Hydrol. Earth Syst. Sci. Discuss., <https://doi.org/10.5194/hess-2017-398>, 2017.

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