Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2017-398-AC1, 2017

Hydrology and Earth System Sciences
Discussions
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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: 9 October 2017

Comment #1

The term "runoff attenuation features" is establishing itself in the literature but it is an C1 HESSD Interactive comment unfortunate use of over-complicated 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

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Discussion paper

possible.

Response #1

We agree with the Reviewer that the lumped term can be misleading or inaccurate and could be considered unnecessarily technical. Our suggestion is to use "enhanced hillslope storage", and to refer to ponds, gully blocking etc where necessary to clarify what this term is referring to.

Comment #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?

Response #2

Given the assumption of linear storage – discharge relationship (p6, Line 14), residence time is simply the reciprocal of the constant of proportionality in the relationship. We chose the large range to ensure that both very fast draining and very slow draining cases were considered. A much larger range would have been examined but this could have obscured the conclusions drawn. The linear store assumption is not necessary for the solution of the system of equations given on p.6 Line 19, although an analytical solution would no longer be possible. Any storage-discharge can be specified for individual response units, and the system solved numerically. This will be clarified, and it will be stressed that the example relationship is the simplest that could be used and is likely to be more complex. With the applied relationship, estimation of the drainage constant is an issue that will need to be addressed in further experimental work. Applying a more sophisticated hydraulic model, for example to leaky gully barriers , could be more realistic and, in theory at least, related directly to the configuration of a feature in terms of size of and spacing between members etc. This however would introduce further parameters but the identification of feasible ranges could be the subject of further work.

Comment #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.

Response #3

This is true and we will attempt to qualify these statements where they occur.

Comment 4

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.

Response #4

We agree with the first point and we shall try to expand on this section. The example of tree shelterbelts is not actually given as a case of an intervention that could be modelled in this approach, so this section will be clarified or removed. It is included as example of how spare downslope soil capacity could be introduced by an NFM-type intervention, and thus that reinfiltration should therefore be considered and this can be handled by the model.

Comment #5

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.

Response #5

We agree that this is a potentially very significant component of the effectiveness, or otherwise, of NFM schemes. Although the paper in itself provides no major evidence, it does, however, give a potential use of the model presented and will be the subject of further investigation. We shall qualify the statement with the caveat that the paper does not actually address the network synchronisation issue but the model presented does allow the possibility for this to be investigated.

Comment #6

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

Response #6

Some additional text on what these table show is warranted, and, possibly, a change in what is shown. There is an extremely wide range of potential impacts, with magnitudes up to that quoted, but the majority are smaller. The impact on the first storm (Abigail) is largest as all schemes have their maximum storage. A better metric than the maximum difference would probably be the median value. Each realisation is associated with a likelihood determined as described in section 3 and 4.3, and the largest reductions in peak flow are generally associated with the least likely realisation. This could be used to likelihood weight the values and achieve a figure for the mean reduction more representative of the corresponding interventions' actual overall impact.

Comment #7

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

Response #7

We are grateful for the reviewer pointing this out and we will endeavour to eliminate these errors.

Comment #8

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.

Response #8

Our revised suggestion is "A new method, with application, for analysis of the impacts on storm runoff and flood risk of widely-distributed enhanced hillslope storage".

Comment #9

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

Response #9

The weir equation applied to model overflow out of the features will result in a small head clear of the rim that will introduce more storage over and above the hydrostatic amount. This storage cannot be identified beforehand as its is not known how large this head will be. Hence, the static storage of the feature at ”brim-full” is equated with 100% utilisation.

Comment #10

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

Response #10 We shall round these to 1 d.p.

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