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Hydrology and Earth System Sciences Discussions

Interactive comment on "Effects of peatland drainage management on peak flows" *by* C. E. Ballard et al.

Anonymous Referee #1

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General Comments

This manuscript presents an analysis of the impacts land management on peak flows generated in blanket peatlands, specifically the effects of drainage by grips and of the blocking of grips associated with peat restoration. Blanket peat restoration is a major current focus in the UK uplands, and over the last few years the argument that ditch blocking will have benefits for downstream flood risks has been increasingly used to help justify this major land management intervention. Unfortunately, these claims have been made in the absence of clear evidence or support from either empirical or modelling studies. As such the research presented in this manuscript represents one of the first attempts to evaluate the impact of drain blocking on peak flow generation,

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and as such is to be particularly welcomed as a potentially important contribution to our understanding of the hydrological effects of peat restoration. The focus of the paper is therefore certainly appropriate for the scope of HESS. The analysis and findings are potentially of interest to a broad set of constituents, including the scientific community, conservation agencies and peat restoration practitioners.

The approach taken is to use a physics based model to investigate flow responses of intact, drained and blocked drain conditions to a years worth of rainfall events. The scale of study is notionally a 200 x 200 m area. A series of simulations has been made using 100 parameter sets sampled from prescribed ranges of key inputs to the model – resulting in a population of 100 'simulated' peatlands on which the analyses are based. Although the main focus of the analysis are the differences in peak flow conditions between the three land use types, with associated identification of the key driving variables, there is also some consideration of the model assumption of static soil and vegetation conditions, and the impact post-intervention changes in these conditions might have on the model outputs.

Although different approaches might have been taken, the overall structure of the modelling approach is logical. The paper is well written with model construction and assumptions clearly laid out and it is generally possible for a non-modeller (such as myself) to follow the analytical steps - an important consideration given the wide potential readership of the paper (though see specific comments below).

The key findings of the analyses are, for the most part, made clear and are well justified by the analyses. Diagrams and tables are appropriately used. There is (generally) good use of the wider literature and hydrological understanding of these systems when interpreting the results. The interpretations themselves are measured, and it is reassuring to see clear appreciation of the limitations of the analyses and appropriate 'health warnings' on the model outputs where necessary.

The findings themselves are interesting, both in terms of developing our understand-

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ing of the key processes controlling peak discharge changes following these land use intervention and in terms of potential implications for peat restoration practice, and will be of relevance to the research and peat restoration communities. There are some matters of detail which need attention or clarification (see specific comments below), but given the overall scope and approach of the paper I do not think these invalidate the analyses or findings.

Overall I think this is a good topical paper which, after some clarifications and attention to matters of emphasis, is suitable for publication in HESS.

Specific comments

My really key concerns are points 4 and 9-11

1. Page 6535 line 27. Additional support / references just to emphasise the idea that drain blocking is now a major focus of land management in upland UK peatlands. There are lots of potential references for this!

2. The last paragraph of the introduction (page 6537 lines 6-24) is weak on clarity in terms of the aims of the paper, and it does not relate well to the subsequent structure of the analyses as presented in the results. There are too many 'aims'/foci presented here ! The paragraph needs some editing to clearly emphasise the key aims of the paper. I suspect use of a bullet point list would help focus on this. My own understanding of the paper is that it aims to (1) evaluate the differences in peak flow characteristics of intact, drained and drain blocked sites and (ii) explore the site factors which are potentially control these differences.

3. The underlying model is cited through a paper currently undergoing review in the Journal of Hydrology. This JOH paper apparently includes testing of the model against empirical data from a drained, unblocked site, and there is 'good agreement' between model and empirical data. No stats on model performance are quoted in the current m/s, which makes me a bit nervous. Given the scope of the current m/s, evidence

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of strong model performance from such testing is a prerequisite. I hesitate to say the JOH paper needs to be confirmed before this current m/s is accepted ... but at the very least more complete summary information is needed to demonstrate how well the model represents real catchment data.

4. The entire analysis is dependent on the parameters and ranges detailed in Table 1, but the sources of these data are not all clear and the justification for the selected ranges weak. There needs to be a clear, systematic explanation of the sources of these numbers, including adequate citations.

5. Table 1 indicates that in the model maximum drain angle can be twice the maximum surface slope angle. Is this realistic at the scale of 200m by 200m? Given the average depth of blanket peat is often given as 2-2.5 m, generally less than this on steep slopes, this seems unlikely and at the scale of the model maximum drain angle will be constrained by maximum slope angle. If this is the case, you may have completely unrealistic 'systems' in your 100 hypothetical sites. Please clarify and justify these ranges.

6. Given the large set of parameters, is 100 simulations adequate? Is there any way to defend the 'representivity' of this dataset in terms of the 'real' population of blanket peat systems? Difficult questions I know, but they stress why responding to point 4. above is crucial!

7. Although most of the descriptions of the model set-up and analytical approaches are clear, this can't be said for the material in on page 6543 lines 11-21 – in particular the description of the 'vector of events', what this means and how it is derived. This may be a failing of my analytical knowledge, but given the clarity of similar explanations in the paper, some editing to clarify this analysis is warranted.

8. Page 6546 line 12-13. To what extent do the 'greatest reductions in peak flows' occur where the peak flows are highest? Given the other interpretations here, it is perfectly possible that the largest effects are occurring in simulations with relatively small peak

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flows. This would have implications for your interpretation and conclusions, so is worth checking.

9. Table 2 is not well justified. Where are the sources of information for the predicted directions?

10. Table 2 shows positive AND negative changes for some variables. The sources for these need justifying. You also need to be explicit in the text about how these were handled in the analyses.

11. Page 6547 line 15 onwards. Related to the last point, you need to be clearer on how this analysis works, and how the values of the perturbations were derived. This is not clear at the moment.

12. I am not sure anything can be done about this without asking for significant new analysis, which I am not doing, but I note that the analysis in section 4.4 would be much more useful if the effects could be partitioned to the different individual parameters, rather than this global analysis which is rather crude.

13. Your analysis considers overland flow roughness using Holden et al's numbers. However, does this take into account the effects of variable topography on the peatland surface, and subsequent 'surface storage' effects in depressions etc? At a scale of 200 x 200 m, even degraded systems exhibit topographic variations above the scale associated with different vegetation types (and captured by the Holden et al numbers). This may be an important influence on peak flow.

14. The final discussion should be rather more detailed on the implications of the findings for empirical studies – i.e. exactly what hydrological controls should the field studies now be testing? The finding that flood peak reductions at blocked sites are at least partially limited by deeper overland flow lines ('surface streams') immediately downslope away from blocks is a case in point. Have these been observed in the field? If not we need field assessment of their existence and importance, given the influence

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they have on your model and results.

15. At the end of the discussion you introduce the hypothesis that drainage management has less impact for extreme events. This just 'pops' up here, but is a key concept and a little more introduction and explanation is needed earlier in the discussion. Ideally, your analyses should allow some specific comment on support or not for this (see point 8. above).

16. The conclusion and end of the abstract should more clearly state the key controls on peak flow change identified by the analyses – in particular (in my reading) the role of drain roughness and surface (overland flow) roughness – and more fully state the key guidelines for identifying drains that would most greatly reduce peak flows if blocked. If I understand the paper correctly, the message that roughness is key is sufficiently important that it needs to be up-front.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 8, 6533, 2011.