In the following document we have provided responses to each of the individual comments of the reviewers. Reviewers' comments are shown in black, and our responses are shown in red. We have also included a supplement of a .doc format version of the submitted paper with changes associated with the responses to the reviewer comments highlighted using the "track changes" utility.

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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

Received and published: 30 August 2011
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, 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 understanding 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!

    We have added references to Armstrong et al., 2009, Ewen et al, 2010, Holden et al., 2004, Wilson et al., 2010 to add support to the wide use of drain blocking in the UK

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.

    We have left the first half of the paragraph in place, as we believe that this provides an important rationale for the methods selected and for some of the assumptions that had to be made in the modelling process. However, the reviewer has very nicely summarised the main aims of the paper, and as such, we have removed much of the second half of this paragraph (lines 18-24) and replaced it with the two bullet points listed by the reviewer, plus a third bullet point related to an evaluation of the model uncertainty (this point is necessary in order to address one of the other reviewer's comments):

    "

    - Evaluate difference in peak flows of intact, drainage and drain blocked sites
    - Explore the site factors which potentially control these differences.
    - Identify field data that could improve the model structure and assist in the reduction of prediction uncertainty"

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 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.

    The JoH paper is now published (407 (2011) 81–93) and can be referred to by readers – examining the hydrograph performance in that paper is a useful method of evaluating the model performance. We have also added some performance statistics into this paper to support the claim of "good agreement"

    > "During the validation period, those parameter sets that were considered to be "behavioural" led to an average RMSE across six boreholes of 0.06 to 0.07m and RMSE for the flow predictions of 0.07 to 0.08 l/s (the maximum observed flow was approximately 3 l/s)."

    These statistics were not included in the JoH paper.

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.

    We have added some additional references in this section to justify the selection of the parameter ranges.

    The paragraph now reads (with additions shown in italics):

"The drain angle is defined as the angle between the drain and the contours of the site (*as measured in the horizontal plane).* Along with the site slope, the drain angle governs the drain slope and the geometry of the reservoirs in the blocked drains. *As nationwide values for slope and drain angle in peatlands were not readily available, ranges were evaluated from DEM and aerial photographs of the peatlands in the Hodder catchment, Lancaster, UK, which were assumed to be representative of the peatlands across the UK.* The overland flow roughness is parameterised based on field observations made by Holden et al. (2008), where flow roughness was observed to vary both with plant cover and flow depth; *this study is the only known investigation that quantifies overland flow roughness on peatlands.* This parameterisation is represented by the parameter b, which is a proxy for the Darcy Weisbach roughness coefficient (see Ballard et al. 2011 for the full derivation). *Hydraulic conductivity ranges were estimated based on information from Letts et al. (2000) and Holden and Burt (2003).* The acrotelm and catotelm porosities (εa and εc) are set as functions of their respective hydraulic conductivities following the relationship presented by Letts et al. (2000) plus a random term between +/- 0.05 to account for natural variability and uncertainty in this relationship. The drain depth is fixed at 0.6m and the drains were blocked at 12.5m intervals (typical average dam spacing, Armstrong et al., 2009)."

Although it is possible that the ranges do not completely include the full of range of upland peats in the UK, we made these selections with the best available information and believe that it is unlikely that the ranges could be significantly larger than those shown in table 1 (now table 2). We have included in the list of limitations at the end of the discussion the fact that the regression results are likely to change if the parameter ranges are changed:

> "3) The assumption of linearity used in the regression models to investigate parameter sensitivities appeared to be suitable in this instance. However, should the parameter ranges be changed (either widened or tightened) the sensitivities are likely to change as well. Therefore the sensitivities should be viewed as indicative rather than strictly quantitative."

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.

   There appears to be a misunderstanding of both reviewers about what the drain angle is. The drain angle is the angle between the drain and the hillslope contour, as measured in the horizontal plane. Hence, the slope of the drain is determined by both the drain angle and the site slope. In this way, it is impossible to have drain slopes steeper than the site slopes. In order to make this definition clearer, we have changed line 15-16 6540 to read:

   > "The drain angle is defined as the angle between the drain and the contours of the site (as measured in the horizontal plane)."

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!

   To some extent we agree that 100 parameter sets is a small sample. Unfortunately, the small sample size is a reflection of the computational expense of the model. There was no evidence in the literature to suggest that the ranges used in this study were inappropriate in a British context or that any of the sampled sets could not potentially be members of the 'real' population of blanket peatland sites. However, we have taken into account the fact that the ranges may not cover the full spectrum of peatland sites in the UK when making conclusions from this study by (1) recommending that the sensitivities are to be considered as qualitative and (2) by not recommending the use of the regressions as predictive tools. The statistical significance of the regressions were also considered in the conclusions drawn, which accounts for the sample size.

   This study should be considered to be a first step in an iterative process to develop understanding of the processes affecting flood peaks, to inform data collection and development of improved models for prediction. We have used the best available information to develop a conceptualisation of the system and then used this model in order to explore variability in predicted response, which in turn can inform targeted data collection by identifying critical gaps in understanding and identifying the most efficient monitoring strategies. This data in turn could be used to update the model structure and parameter ranges. This iterative approach ultimately would lead to reductions in both the epistemic and aleatoric uncertainty of the model predictions. Given that this study is the first iteration, we have been careful to list the limitations of the study (the last paragraph of the discussion, which is now further expanded) and identify methods to improve future predictions.

While recognising that the paper presents a preliminary modelling step, we agree with the comment indicating its novelty and potential importance: "one of the first attempts to evaluate the impact of drain blocking on peak flow generation, and as such is to be particularly welcomed as a potentially important contribution to our understanding..."

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.

   In total, there were 80 events – each rainfall event can be represented by an index as an identifier. We examined the hydrographs produced by each of these events and extracted the peak runoff associated with each rainfall event. We then looked at the 10 largest runoff events, and identified which rainfall events caused these 10 largest runoff events. As such, the events that lead to the 10 largest peak flows can be included in a vector of events, i.e [1,17,23,45,66,76,89,91,94,99] – which is a list of the indices associated with each of these events. We accept that this may not have been very clear in the text, and think it would be clear if less unnecessary detail was provided; as such we have adjusted the text to say: "For each of the hypothetical peatland sites, the rainfall events that led to the 10 largest peak runoff events (**r**) were identified (including only the largest peaks in the sample is considered suitable in the context of flooding), then the mean of the peak flows produced by these 10 events was calculated ($\overline{q}(\mathbf{r})$). The sensitivity of $\overline{q}(\mathbf{r})$ to each of the model parameter values can be quantified by conducting a regression analysis with the peatland properties (i.e. the model parameters) as the regressors (Saltelli et al., 2004)."

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

   When plotting $\overline{q}(\mathbf{r})$ verus $\Delta\,\overline{q}(\mathbf{r})$, there is a trend of greater change ($\Delta\,\overline{q}(\mathbf{r})$) associated with greater drained scenario peak flow values, $\overline{q}(\mathbf{r})$. This supports the later interpretations about the limited value of blocking drains that are already revegetating and on low slopes, as they already should have relatively small flood peaks. As such, we have not made any changes in the text.

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

   The sources of information used to justify table 2 are provided in the introduction (p6536 line 19 to p6537 line 5). A reference to table 2 has been made at the end of this paragraph : "The evidence of direction of change of physical properties following drainage management change is summarised in Table 2." And a reference back to this paragraph at line 15, p 6541: "This data is based on the literature cited in the introduction, and the assumed reversal of these changes following drain blocking."

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.

    Again, the changes up and down reflect the fact that observations in the literature have suggested both directions of change (i.e. drains revegetating, or drains eroding and becoming smoother). See previous answer to q4 to link to sources of data used to develop this table.

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.

    We have rewritten this methodology: (to replace p6547 line 15 to 20)

    "We have arbitrarily assumed that the maximum changes in any of the site properties listed in Table 1 would be 10% of the pre-change parameter values. To examine the potential impacts of multiple changes in properties following land use change, 1000 random perturbation sets were sampled. Each set contains a value between -0.1 and 0.1 for each of the parameters shown in Table 1, where negative changes are sampled from -0.1 to 0, positive changes from 0 to 0.1 and changes in both directions from -0.1 to 0.1. For each of the 100 hypothetical sites 1000 perturbed parameter sets were derived (θ+Δθ). The change in $\Delta\bar{q}(\mathbf{r})$ related to non-stationarity in physical properties, $d\left(\Delta\bar{q}(\mathbf{r})\right)$, is calculated as:"

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.

    The global analysis was conducted in order to provide a more realistic prediction of the potential influence of parameter non-stationarity, where multiple parameters may change. The influence of the individual parameters is largely quantified by the regression coefficients (although this is made slightly more complicated by the conversion to percentages and the presence of the same parameters in the both the pre and post change predictions). No change made

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.

    The values provided in Holden et al. 2008 are based on a number of sprinkler experiments conducted on real peatland surfaces. Values for roughness were derived based on observed flow velocities. The values should therefore inherently include the influences of microtopography. The Holden et al. study is the only known study specifically examining overland flow in peatland systems. We have used a simplified surface runoff representation because there was no other information to support (either in terms of conceptualisation or parameterisation) a more complex representation. We agree with the reviewer that topographic variations may indeed be an important control on peak flow magnitude. The fact that the surface runoff processes are shown by the scenario modelling to be so important, will hopefully lead to further research to investigate in more depth the processes and response characteristics of peatland surface runoff. We have added a paragraph into the discussion to address the importance of this issue, both in terms of reductions in prediction uncertainty but also in terms of new information needed to improve process representation in the model:

    "Structural simplifications in the models were employed with the rationale that more complex representations could not be justified given the data scarcity, which implies that model improvements could be made if sufficient supporting data were available. The results from the simulations conducted in this study suggest that surface flow paths are the dominant control on peak flow response. Investigations into peatland surface roughness and drain roughness (for example, through sprinkler experiments) that could assist in refining the parameter ranges would lead to significant reductions in the model prediction uncertainty. Such studies may also assist with the conceptualisation of the surface runoff processes. In particular, the field investigations could build on the study of Holden et al. (2008), to include a wider range of peatland plant species, as well as estimates for mixed species sites and to explore the impacts of microtopography. Particular emphasis should also be placed on the drain roughness, for which the Holden et al. (2008) study only collected a limited data set."

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 additional paragraph added in response to the reviewer's comment no. 13 also addresses this comment, identifying surface runoff processes as the hydrological control that field studies should 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 they have on your model and results.

    We have observed evidence of this behaviour in the blocked drains in the upper Hodder, UK, and in some cases we have even seen a slight channel dugout from the blocked drain downslope in order to encourage the spill of the blocked drain reservoir. We have added a reference to a thesis that describes this behaviour:

    "This behaviour has been observed in the field (e.g. Geris, 2012)"
    (added at p6539. Line 3)

    However, we recognise that there may be alternative spill processes with different drain blocking implementations. As such, we have added a paragraph into the discussion noting this point and calling for further investigations into this spilling process:

    "There remains some uncertainty in the conceptualisation of blocked drains. The configuration is a representation of an ideal drain blocking system, but alternative methods are also employed, where overflow from the reservoir created by the drain block spills not downslope, but into the downstream dam (or some combination of the two) (Armstrong, 2009). In many cases, the drains are blocked using peat excavated from the side of the drains; the excavated peat is used to block the drains immediately downstream of the excavation. This leads to increased storage of the newly created reservoir (and will also affect the spilling process), which is not accounted for in the present model. The sensitivity of the reservoir spilling process to variability in the elevation of the top of the drain is not well understood. High variability in the elevation of the top of the drain may lead to more diffuse spilling on to the downslope peat, and hence reduced flow velocities. Given the significance of the differential flow velocities between the peatland surface and drains in controlling the ultimate impact of drain blocking, observations from blocked drain sites could assist in reducing the conceptual uncertainty in these predictions."

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).

    Our analysis did not show any evidence to suggest that the impacts of drain blocking had less effect as the events became larger. However, as stated in the text, the range of events included in the current analysis is limited to relatively frequent events. This is just a hypothesis based on logic and the results of other modelling studies. We have added a qualifying statement and reference: (new text in italics)

> "Further research should include more extreme events, with the hypothesis that drainage management has less impact for larger events *(as has been modelled for other land use change impacts, e.g. Wheater et al. 2008).* "

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.

    At the end of the conclusions we have added:

    > "; in particular investigations of surface and drain runoff response would most greatly reduce prediction uncertainty and also potentially improve the model process representation."

    And at the end of the abstract we have changed the last sentence to:

    > "Based on insights from these simulations we propose guidelines for identifying those steep smooth drains as those that would most greatly reduce peak flows if blocked and recommendations that future targeted field studies should be focused on examining surface runoff characteristics."

J. Freer (Referee)

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Summary: This paper provides virtual experiments to test a simplified 'physics' model for predicting changes in peak discharges for peatland areas. The model is setup to simulate intact, drained and blocked peatland areas with a simulation scale of 200m by 200m. Analysis is conducted using a monte carlo simulation of sampled parameters and the model structure has previously been 'validated' on a drained peatland in the Yorkshire Dales. The authors note the model has been validated but in fact in the paper of Ballard et al. 2011 there are no metrics produced of this validation except visual analyses of the flow and water table dynamics for a validation period. Whilst the flow dynamics are very positive for wetter periods, there is more discussion that could have been developed regarding the water table dynamics and if they show appropriate dynamics (is the model in other words right for the right reasons?). However these points do not reject the model being used for the current assessment of different model structure formulations to test differences in peak flow simulations. But there does need to be a better context in this paper what is meant by the model has been 'validated' and on what basis this has been achieved. The paper is generally well written and the framework is, on the whole, logical. However there are some confusing points and diagrams and I feel the authors need to do a better job with some of their analyses of results so it's clear to the reader what can be concluded from this work. I put forward the following comments in this review in the order they appear through the paper:

1) page 6535 line 17-19: Given the context of this paper I feel these comments need to be broadened as to why (at least somewhere in the discussion).

    We have added some sentences to elaborate on the key hydrological processes associated with open ditch drainage in order to provide some context to line 17-19:

    > "Open ditch drainage changes the hydrological response by (1) creating more storage in the subsurface and (2) by providing a rapid conduit for runoff. Process (1) tends to reduce the flashiness of system response while process (2) increases it; which process is dominant is likely to dependent on a number of site specific characteristics. "

2) page 6536 line 1:Why (again) cannot the impact on peak flow be determined conclusively (better context)

    We have added some more context to this paragraph to highlight the limited data availability on the impacts of drain blocking on peak flows:

    > "This is largely due to a lack of suitable data; most experiments examining the impacts of drain blocking have focused on changes in water table levels (e.g.

Armstrong et al., 2010, Price, 2003, Wilson et al. 2010), but even these studies are limited in number. " (at p6536 after line 1)

And

"Although there are an increasing number of studies of blocked drains within the UK, the efficacy of restoration is still unclear due to strong influences of local conditions and lack of pre-drain blocking data for comparison (Wilson et al., 2010)" (at p6536, line 7)

3) page 6536 line 4: There needs to be some better understanding of the scale differences here. I also wonder why this point is being made, given the fact this application does not investigate changes to the whole catchment response either as it is applied at the 200m by 200m scale and in a hypothetical simulation mode.

This point is raised here because the flow in the blocked channels really gives no information about peak runoff, as most of the runoff from blocked drain peatland sites is due to the spilling processes from those blocked channels. So, perhaps monitoring at a catchment outlet is not necessarily required, but certainly impacts would have to be evaluated at a location where the overland flow generated from the blocked drains is also collected. We have changed the text to make this clearer: (italics are new text)

"There are also methodological challenges associated with the measurement of flow following drain blocking; cases have been noted where the occurrence of drain flow is reduced by up to as much as 70% following drain blocking (Worrall et al., 2007a) but this is just within the drainage channels themselves and not necessarily at a location that also measures water that might spill downslope from the blocked drains. "

This is important because the authors suggest later this model can test 'management scenarios' and I am not sure if this scale is as meaningful for such a purpose or not. . . At least it needs to be discussed in the context of the greater issues of timing and how discharge peaks are realized over multiple channel lengths at the larger 'management scale'

We have added into the limitations something to address this point:

"4) The results from this study only reflect changes at the 200m x 200m scale. At the catchment scale, particularly when only parts of the catchment have changes in land use, the impacts will also be dependent on the stream routing and connectivity (Lane et al. 2004)."

4) page 6537 line 4-5: Again add a little explanation as to why?, how uncertain? – for example if the evidence showed results are highly variable then this might point to the complexity fo these systems which has relevance to the simplified process representation considered here (and especially with regards to the complexity of the surface and subsurface topography

We have changed the wording "uncertain" to "unclear". How uncertain is difficult to quantify, as this is epistemic uncertainty. There were only a few studies to suggest the changes in physical properties associated to peatland drainage management and they are insufficient in number to quantify the degree of uncertainty in these properties.

5) page 6537 line 23-24: It's not at all clear to me there is enough analysis in this paper to justify this statement about what field data will be critical to reduce predictive uncertainty (edit 'most greatly' note).

This sentence is now a bullet point and has been reworded to "Identify field data that could improve the model structure and assist in the reduction of prediction uncertainty". See also the answer to reviewer #1 q 13

6) page 6538 line 2-3: I am all in favour of using simplified models and exploring uncertainty. I'm not sure however the authors have discussed fully what drives the flowpaths and boundaries in peatland systems and been critical enough to suggest a 1D model with highly simplified 'sheet' topography (when of course overland flow roughness is a critical factor to discharge peaks and micro-topography may be a factor) at these low gradients is appropriate. I wouldn't just class this as treating 'minor

processes' in a simplified manner, I would question the validity of important flow domain structure and potential behaviour.

We have added to this statement describing the modelling philosophy used in the development of the Ballard et al. 2011, to explain that processes were not represented in any more complex a way than could be supported by the availability of data. The representation of the surface runoff as sheet flow was driven by the fact that the only available data to characterise the surface roughness characteristics of peatlands was in the Holden et al 2008 paper. The experiments used sprinkler experiments and made measurements of overland flow depths and velocities which were then used to derive darcy-weisbach friction factors. These values were equivalent to Manning's n values of up to 1. The large magnitude tends to suggest that processes such as the influence of microtopography were incorporated into the measured values.

See the addition to the discussion about this point in the response to q13 of reviewer #1

7) page 6538 line 15-16: 'Good agreement with observations' needs an improved context and what was actually evaluated in ballard et al. 2011. Especially as the authors suggest the model was 'validated' (page 6540 line 5), I suggest this is an often overused word which needs an improved context of what the authors mean (do the dynamics of the subsurface water tables in Ballard et al. 2011 really look like they are following the right dynamics in the cases shown?).

   We have changed the word "validated" to tested, and have also provided some additional measures of the goodness of fit observed in the Ballard et al. 2011 paper (see the response to reviewer #1, q3. In addition, we have added a paragraph to provide some discussion about the performance:

   "The performance of the model in predicting the responses demonstrated in Ballard et al. (2011) provides some extra confidence in the otherwise *a priori* model structure. All the of model parameters were identifiable, suggesting that the model is not over parameterised and that all the parameters have some sort of measurable influence on the predicted model response. Significantly, it was possible to calibrate the model using locally measured physical parameters or ranges that were restricted from measured values in the literature. Applying the chosen performance criteria and considering the ranges of parameter values perceived to be possible *a priori* for this site, no evidence was found to suggest that the parameters are inconsistent with their true (measurable) physical meanings. This provides support for using this model structure in other peatland sites in speculative simulations where there is no supporting data, but some knowledge about the range of potential physical properties."

8) page 6539 line 6: Again 1D flows in shallow micro-topography and substantial subsurface flow systems with tortuous channel flowpaths?

   We have not made any changes in response to this comment, as we believe that the additional changes added in the model limitations in the discussion and the comments added in response to question 6) are sufficient "health warnings" about the structural simplifications employed in the model.

9) page 6541 line 1: Not clear what scheme was used to sample the parameters nor if really 100 samples can really capture the dynamics of the model response for different parameter combinations. There are two issues here with the scheme

   1) there are parameters over orders of magnitude so are these sampled differently, if not with 100 samples only you will not be sampling your expected lower orders of magnitude. . .

      The samples are all sampled in the same way as there was no evidence to suggest that likelihood distributions would be anything other than uniform. The upper and lower sample values are shown in the table below:

		Ranges for Monte Carlo Simulations		Actual samples	
Parameter		Lower Value	Upper Value	Lower value	Upper value
Acrotelm hydraulic conductivity (md^{-1})	K_{sa}	0.05	1	0.0583	0.9922
Catotelm hydraulic conductivity (md^{-1})	K_{sc}	0.001	0.05	0.0010	0.0496
Thickness of acrotelm (m)	d_a	0.075	0.2	0.0756	0.2000
Drain angle (degrees)	α	5	25	5.0526	24.8267
Surface slope (degrees)	β	2	12	2.0570	11.9729
Plant cover (overland flow roughness)	b	Sphagnum & Juncus (roughest, 1.91)	Eriophorum (smoothest, 5.05)	1.9252	5.0499
Manning's n (drain roughness)	n	0.05	0.6	0.0511	0.5946
Drain spacing (m)	W	10	25	10	25

We were happy that the upper and lower sample ranges were represented within our 100 parameter sample. The sampling error is treated in the regression by using statistical significance tests. It is possible with more samples we could have identified more effects, weaker effects, and higher order effects. But the main effects will not change. It is likely that many more samples would be needed if, say, we were looking to calibrate optimal parameter sets, or estimate percentiles of posterior parameter distributions, but here we are only looking for the major sensitivities.

2) Can't you sample, without allowing for any correlation not just 'unlikely' scenarios but even scenarios that are physically impossible if I understand correctly. For example what is stopping a sample of a surface slope of 12 degrees and a drain angle of 5 degrees and what does that look like in a modelled flow domain?

See previous response to reviewer #1 about the misinterpretation of the drain angle. Drain angle and site slope are combined to calculate drain slope; as such physically impossible scenarios of this type are avoided. We could not see any other clearly impossible combinations of parameter values

4) It is not even noted if the same parameter samples are selected for each run (intact, drained and blocked), given the sparse nature of the sampling this might be a concern for assessing differences in the simulated output distributions,

Further details have been provided in the text to explain that the same parameter samples are used for each of the land management scenarios: added "The same 100 parameter sets were used for each of the possible peatland land management scenarios." Before p 6541 line 6.

5) It really would have been useful to understand the rationale for these choices of parameter ranges and the likelihood these are realistic sample ranges for 'a given site' of peatland.

See answer to reviewer #1 about the source of these parameter ranges. The likelihood that these are realistic parameter sets is very challenging to evaluate – based on our review of the literature there was nothing to suggest that these ranges are not a good reflection of realistic sample ranges for blanket peatlands in the UK. We have added to the discussion of the study limitations a point about the results to some extent being dependent on the sample ranges.

10) page 6541 line 6-15: I'm not sure that these changes in parameters reflect some of the more critical aspects of expected changes of peatlands. Wouldn't one expect changes in the gully geometries themselves through erosion and deposition processes and consider how these are reflected in these non-stationary changes

    In the model development section we explicitly state that the model only applies for shallow open drains and is not suitable for deep gullies (line 3-5, p6540). We have also added some text to indicate the model does not currently account for any issues related to erosion and sediment transport. "The analysis considers only the potential non-stationarity of the model parameters and does not consider the potential non-stationarity of site geometry (i.e. erosion and deposition within channels) or of the model structures." (added line 26, p 6546)

11) page 6541 line 29: Please explain the method a little more clearly

    We have added another sentence to hopefully make this method more clear "Rainfall events were identified as periods of rainfall followed by a minimum period without rainfall (in order to achieve independence of events)"

12) page 6542-6543: I confess I am confused by the results in figure 3 and much of the discussion, labeling and figure title seems contradictory.

    a. the Y axes, the text notation (6542 line 17) and the figure title are not consistent, please can we have either Intact-Drained or vice versa in all cases for example for figure 3a and equally for the other figures – it's a mess at the moment and actually I cannot follow the reasoning or the discussion because one is not clear what is plotted.

        All of the figures and equations shown are for drained minus intact. We agree that the figure 3 label could be confusing, so have changed it to:

        "Figure 3: Difference in peak flows: a) Drained minus intact (positive values indicate increases in peak flows following drainage); b) Drained minus blocked (positive values indicate decreases in peak flows following drain blocking); c) Intact minus blocked (positive values indicate that blocked sites have lower peak flows than intact sites). Light grey areas are the 5-95% range, dark grey areas are the 25-75% range, and the heavy black line is the median difference."

        We believe that after this correction, we have used a consistent approach in the analysis, that is all changes are calculated relative to the drained scenario.

    b. I am confused as to why figure 3c is so shortened on the x axis, it doesn't seem to relate to the differences from the other graphs.

        The x axis on figure 3c is the flow from the intact peatland. The maximum peak flow for the intact peatland is significantly smaller for that of the drained site. Comparing against other graphs, it should be borne in mind that the plots are stepped and are averaged over a number of events (hence, taking the largest x axis value from 3a and subtracting the 5% value gives a value that is larger than the actual largest intact peak flow). We have added a clarifying statement at p6543 line 9:

        "The maximum peak flow for the intact peatland is significantly smaller for that of the drained site, hence the difference in x-axis scales between Figure 3(a) and (b) and Figure 3(c)."

    c. Is this really a sensible analysis given the aims of the paper?, what the authors have done is to rank storm peaks no matter where they occur into bins (80). This means there is a potential mixture of events in the rank orders. What this does no reflect is the inherent uncertainty in the 100 'peatland sites' driven by individual events. I would have thought it was much more sensible and informative to keep the storm peaks separate and look at the uncertainty overlap in these predictions for the intact, drained and blocked model setups. You could also then reflect more clearly how consistent the picture was for different periods of the year and where main differences expressed by these hypothetical simulations occurred.

We tried a number of alternative methods for analysing the changes between drainage management responses and taking into account all of the factors that influenced these differences. We chose these plots as a simple and informative method of demonstrating the trends in changes in flows as the baseline observed flows increased, across all of the sampled parameter sets. Without the smoothing used in the plots, it is extremely difficult to identify these trends. We have also specifically avoided analysing the influence of seasonality on the response in this paper for a number of reasons – we felt that the 1 year record was already very short, and we did not have a sufficient sample of large flows across the seasons. Also, in terms of practical management of these systems –the weather cannot be changed, so although it is interesting to know that greater changes may be associated with some particular types of rainfall events, we thought that it would be more useful to examine the influence of characteristics that can be managed..

The influences of event characteristics on parameter sensitivities were examined in a recent PhD thesis, and differences between summer and winter events were also explored. In general there were much greater ranges of impacts for summer events than for winter, with greater median impacts simulated for summer events. However, we feel that the significant complexity of these additional analyses is too large to include them in this current paper, but we have added a reference to the Thesis, which is freely available online. We have added a paragraph in the discussion about averaging over events:

"Although arbitrary, the averaging of peak flows for the ten largest events works to remove some of the response dependence on the nature of the rainfall event and initial conditions, which have been found to influence the relative sensitivities of the model parameters (Ballard, 2011). To some extent, the results are sensitive to the number of rainfall events included in (r) (assuming they are still sampled from the larger of the 80 rainfall events). However, the order of parameter sensitivity (at least for the most sensitive parameters) generally remains the same, as does the sign of the regression coefficients. Therefore the method is useful insomuch as it provides a general measure of the magnitude and direction of change in peak flows and the importance ranking of the parameters. However, although averaging over many events is a very useful technique for sensitivity analysis (particularly in order to identify those processes and properties that are generally controlling changes in runoff response), in terms of predicting impacts of change the approach neither accounts for the variability between events nor for the non-linearities involved. For making predictions, the simulation model would need to be run. "

Page 6543 line 5-6 seems at odds with the graphed direction of change, please develop a consistent and clearer approach to all this discussion. In section 4.2 I again don't understand why there is a mixture of events to assess sensitivity but I do find this confusing and so I might not be following the rationale correctly.

Decreases in drain flow following drain blocking would be $q_d>q_b$ and hence $q_d-q_b>0$. Positive values of q_d-q_b as shown in figure 3 indicate decreases in peak flows following drain blocking. We believe that this should be clearer now that the changes to the figure labels have been made.

13) General comment : Given the context of the paper isn't it sensible to show how the 'peak flows' have been generated in some way and the variability of this from the model output (i.e. surface vs subsurface vs drain flow). This is important to understand why these differences are coming about and what are the dynamics that are making them happen

During the model development and testing we did examine the components of flow that contributed to the peak flows. The information gained from this process was almost the exact same information as gained from the sensitivity analysis, i.e. the sensitive parameters are those that are involved in the most dominant processes. The discussion addresses the changes in flow paths and processes controlling the differences between different drainage management scenarios. This discussion is largely based on the outcomes of the regression/sensitivity analysis. To highlight the link between sensitivities and our process understanding/interpretation, we have added some text at the end of the first paragraph of the discussion; the final sentence now reads (additions shown in italics):

> "Sensitivity analysis has been conducted in order to investigate the sensitivity of both the responses and the impacts of drainage management to the peatland properties (as represented by the model parameters), *as well as to identify those processes that are contributing most to modelled differences in flows*."

14) page 6544 line 10-20: Whilst reading this I thought that the length of the idealized slopes have not been changed, I wondered how critical this might be for 'peaks'? – can the authors comment.

    We have also completed the exact same set of simulations for a 500mx500m idealised hillslope. Although the simulated peaks were smaller per unit area due to attenuation of the flow, the regression relationships showed very little difference, i.e. the same processes were still dominating the responses. We do not expect scales much larger than this to exist in any real peatland site before the development of a drainage network occurs, at which point peak magnitudes begin to be influenced by network routing characteristics. No changes made.

15) Section 4.3 : Again I'm not sure this is the most meaningful way to produce these results. The context needs to be % changes based on event magnitude. The results as shown could be driven by small scale events which are not potentially interesting in the context of the papers aims.

    The values presented in section 4.3 are the changes in the mean peak flow for the ten largest events. As such, they are already the largest 12.5% of events during the one year simulation. It was necessary to perform some averaging in order to analyse these results, as the events were sensitive to initial conditions and other event characteristics. By averaging over a number of events, this sensitivity was largely removed and clearer relationships between the average magnitude of peak flows and the system characteristics. (see additional paragraph added in response to question 12 c).

    I think these results need to be ranked by mean storm peak flow and then shown graphically for the percentage changes. In fact with a second Y axis this could be accommodated in Figure 3.

    We do not believe that any additional value would be provided by plotting the values of dq(r) against q(r) for this analysis. The regression analysis can already demonstrate that those sites that already had lower peak flows when they were drained are less likely to show reductions in peak flows following drain blocking – this is how we came to the recommendations about the smoother steeper drains (i.e. those drained sites with the greatest peak flows) being the most effective to block. We believe that adding this information to figure 3 would not add to the information provided and would obscure the main purpose of the figure.

    By the way why use ranges when the rest of the paper uses 5th and 95th percentiles, keep consistency!

    We have changed the values presented to the 5th and 95th percentiles to maintain consistency.

16) page 6547 section 4.4: I have re-read this section a few times and I confess that I do not understand if this can really reflect non-stationarity of peatland properties that might be expected after a change and certainly it's not reflecting process that one would expect in a perceptual sense (drain filling for example after blocking). I find this very confusing as currently written. I'd need to be convinced further there is value in doing this and how this really reflects the title given for this section.

    We have rewritten the methodology to make the explanation clearer and also included some explanation as to what factors are and aren't considered in the analysis:

    > "The analysis considers only the potential non-stationarity of the model parameters and does not consider the potential non-stationarity of site geometry (i.e. erosion and deposition within channels) or of the model structures."

    We believe that the analysis provides a useful measure of the possible influences of non-stationarity of peatland properties on predicted changes in peak flows. The analysis suggests that the property non-stationarity is only a secondary influence. This may be useful to consider when designing field experiments, in particular if only limited resources are available.

17) Table 3 and 4 : The titles do not fully describe what is in these tables and need to be improved. Why is this not principle components analysis to assess which are the controlling parameters rather than this stepwise approach?

We have adjusted the titles so that they provide a more complete description of the information contained within the tables. Due to the sampling method employed, the variables used in this analysis are all independent. If they were not, then the use of PCA may have been more appropriate. The regression analysis was also selected as it was desired to produce equations to estimate the average peak flow; these were critical for the non-stationarity modelling.

18) Conclusions: I'm not sure the comments made about identifying 'steeper and smoother' drains really reflects the real world management decisions that need to be made here and how variable these drainage sections would be 'on the ground' There is no clear statement about any expected variability in the context of the parameter variability shown here. I think this discussion needs improving.

We believe that based on our analysis that the recommendation that focusing blocking efforts on steeper and smoother drains is a valid point. At present, drain blocking is being applied wholesale across the UK, but the current analysis suggests that there may be very little benefit for sites where the sites have shallow slopes and the drains are already vegetated. By stating that the steeper and smoother drains are "most likely " to show reductions in peak flows following drain blocking implies that there could be some variability in the change. We certainly do not intend for the regressions to be used as a predictive tool at this stage of the model development, hence, the qualitative recommendations. We have also included a number of additional limitations of the modelling into the discussion, which we have added in part to address this review point:

> "3) The assumption of linearity used in the regression models to investigate parameter sensitivities appeared to be suitable in this instance. However, should the parameter ranges be changed (either widened or tightened) the sensitivities are likely to change as well. Therefore the sensitivities should be viewed as indicative rather than strictly quantitative. 4) The results from this study only reflect changes at the 200m x 200m scale. At the catchment scale, particularly when only parts of the catchment have changes in land use, the impacts will also be dependent on the stream routing and connectivity (Lane et al. 2004). .... 7) The inferences about causal mechanisms may be dependent on the chosen model structures employed within this study."

See also the paragraph added in response to 12c

Kind regards, Jim Freer

AUTHORS' CORRECTIONS

1. We have updated the affiliation and contact email of the first author
2. We have changed figure 5, as the data in 5b was inverted.