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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.

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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). 2) page 6536 line 1: Why (again) cannot the impact on peak flow be determined conclusively (better context) 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 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' 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 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). 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 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?)... 8) page 6539 line 6: Again 1D flows in shallow micro-topography and substantial subsurface flow systems with tortuous channel flowpaths? 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... 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? 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, 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. 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 11) page 6541 line 29: Please explain the method a little more clearly 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. 1) 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. 2) 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. 3) 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. 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. 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 whay these differences are coming about and what are the dynamics that are making them happen 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. 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. 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. By the way why use ranges when the rest of the paper uses 5th and 95th percentiles, keep 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. 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? 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

Kind regards, Jim Freer

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