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Earth Surface Dynamics Discussions

Interactive comment on "The role of hydrological transience in peatland pattern formation" *by* P. J. Morris et al.

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We are grateful to Anonymous Referee #1 for their detailed, thoughtful and constructive comments, and we are encouraged by the positive nature of their review. Below we respond to each of the referee's main points using the same section headings as in the review.

Background

We agree that it would make sense to reduce the emphasis placed in the introduction on comparing the findings of the two groups of studies. This is easily achieved by removing the text between P. 34 L. 28 and P. 35 L. 20. A small amount of reworking would then allow our existing description of the ponding model (P. 34 L. 16 to 27) to

lead smoothly into our aim and objectives, including our concerns about Type-I errors in the SGCJ models.

We also believe it would help readers if we clarified explicitly in the introduction the mechanisms involved in the long- and short-range feedbacks in SGCJ models. Contrasting microhabitat states (hummock, hollow) between adjacent grid squares allow a positive feedback whereby ponding occurs upslope of hummocks, leading to hollows there; while rapid drainage occurs upslope of hollows, leading to hummocks there. This short-range positive feedback competes against a long-range negative feedback whereby water flow between the model's boundaries tends to homogenise local variations in water level. However, in the current study we demonstrate that the length scale of this negative feedback is effectively governed by the hydrological equilibration time. Long equilibration times allow the effects of the negative feedback to propagate across the entire model domain and override the short-range positive feedback, leading to unpatterned landscapes. We will also alter the first paragraph of the Dicsussion (P. 47, L. 3-20) so as to clarify the mechanisms involved.

Model Overview

For the most part we are happy to clarify the statement of aim and objectives in line with the referee's suggestions. However, we disagree with their suggestion that our stepwise alterations to the model structure have skipped a step, and that an additional model between Models 2 and 3 is in order so as to isolate the effects of the impermeable base layer without the influence of constant delivery of rainfall. We had discussed this possibility during the development of the manuscript but decided against it for two reasons. Firstly, we see little point in improving part of Model 2's conceptual basis while leaving other parts with unrealistic oddities. The transition from Model 1 to Model 2 corrects what we believed to be a flaw in Model 1's numerical integration scheme (calculation of inter-cell transmissivity). The transition from Model 2 to Model 3 corrects a group of what we believe to be conceptual, rather than numerical, flaws (impermeable lower peat; no delivery of rainfall; cross-sectional aquifer shape). We see little point in a model that corrects some of these flaws but not others. Secondly, the referee intimates that a 'halfway-house' model in-between Models 2 and 3 might help to isolate the cause of one small difference in behaviour between those models (upslope spread of patterning on hemi-elliptical aquifer as Δte increases). However, this effect can already be explained by the relationship between slope angle and pattern strength as previously documented by the SGCJ authors, and which we already discuss in the manuscript. An additional model would add little to the overall message from the article and would come at the cost of a substantial increase in length. We believe that our choice of four models strikes a good balance between experimental control and brevity, and – most importantly – is sufficient to address our objectives. In order to aid readers we suggest that we could clarify the objectives and/or model descriptions so as to indicate that Model 2 addresses numerical issues present in Model 1, while Model 3 addresses a group of conceptual issues present in Models 1 and 2.

We are happy to provide more detail on the set of governing equation used in our models.

We disagree with some of the reasoning in the referee's third paragraph in this section (beginning near the top of page C33). Again, the referee suggests that we add another model to the study, this time in between Models 3 and 4, so as to examine the effects of individual rainfall events. Again, although this might shed light on a very specific facet of the models' behaviour, it would add little to the pursuit of our objectives. Model 4 allows us to deduce that patterning in Models 1, 2 and 3 is reliant on the memory effect (which is absent in Model 4), rather than simply on hydrological transience (which is still present in Model 4). In doing so we have used a plausible temporal pattern of net rainfall delivery based on real data. It would have been possible to come up with very many models, each with a small incremental improvement over the last, but this would have led to a very long paper that would have been difficult to read, and which would have approached its overall goals slowly. While the referee raises an interesting question here we believe it to be outside the scope of our article, although

we acknowledge that this may be worth exploring in more detail in future studies.

The referee is correct that we have analysed the response of the hydrological model to equilibration time but we had omitted this analysis for the sake of brevity and in order not to dilute the description of our pursuit of our main objectives. We would be willing to include an additional figure and/or a short passage of text to illustrate the relationship between water-table steadiness and Δte, but we would seek the editors' advice on lengthening the paper in this way.

Results

We appreciate that Figure 5 is dense with information and we agree that a less dense figure could aid readers with interpretation. However, we disagree with the referee's suggestion that the temporal development of the summary metrics shown (Q, R, S) is of marginal importance. We believe that replacing the time-series plots with (for example) a table of summary statistics would hide information that is potentially important for two reasons. Firstly, previous studies using the SGCJ models have demonstrated sizeable 'spin-up' effects, whereby model patterning and state variables change rapidly between early developmental steps. In later developmental steps the model converges on a behaviour that is more representative of the parameter set used, rather than on initial conditions. We believe it is important for the interested reader to be able to see that what we refer to as 'final' model maps (i.e., after 100 developmental steps) are not influenced by any temporal boundary effect, and this is evident from the time series shown in Figure 5 even if we do not discuss this point explicitly in the manuscript. Secondly, without the time series plots of Q, R and S it would not be apparent that the models with high values of Δte exhibit cyclical behaviour in time. The apparent limit cycle found in simulations with Δte greater than 100 hours is a new and valuable finding that helps to explain the behaviour of the model. By way of compromise we suggest reducing the number of time series shown in each panel from nine or ten to three or four, which would greatly reduce the density of information presented to readers. There is little difference in the development of Q, R and S between models with similar

Δte so it would be sufficient to show only one example each of short (e.g. 10 hours), intermediate (e.g. 100 hours) and long (e.g. 10,000 hours) equilibration times for each model.

Discussion

We agree that the DigiBog model suite provides a powerful platform from which to explore our current and future research questions. However, we had deliberately avoided a lengthy discussion of its merits in order to avoid self promotion. Given the referee's request, we would be willing to add a short passage here (perhaps three or four sentences) describing in more detail how model frameworks such as DigiBog could be used to unify cellular models of peatland patterning and cohort models of peatland development. Our use of a "skimmed" (and arguably less mechanistic) version of DigiBog in the current work provided two benefits over going straight to a unified model of peat accumulation and surface patterning: 1) our analysis of the stochastic model has highlighted the role of hydrological steadiness in pattern formation. A more mechanistic treatment of the problem will allow effects of memory and/or scale/strength of long-range feedback to be examined explicitly in future studies; and 2) we wished to replicate the previously published SGCJ models as closely as possible in order to demonstrate that those studies likely also featured a timestep effect.

Minor Comments

We agree with the majority of the referee's minor comments and we are content to make the small clarifications and alterations suggested.

Interactive comment on Earth Surf. Dynam. Discuss., 1, 31, 2013.