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

Anonymous Referee #1

Received and published: 9 July 2013

This is an important paper, providing a systematic analysis of the driving factors of pattern formation in a relatively simple and elegant eco-hydrological model of peatland surface dynamics. The starting point of this paper is a previously published model. In the current paper, hydrological modeling elements are added to this model in a step-wise manner, to identify the factor responsible for previously reported model behavior. Also, the authors perform a sensitivity analysis with regard to the hydraulic conductivities (and transmissivities) used in the model.

The model results reported here strongly suggest that the criteria for hydrological steadiness used in previously published results were not conservative enough to warrant a truly steady hydrological state. Interestingly, the authors show that this artifact might actually have induced an ecological memory effect that may actually be of key

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importance in the formation of real ecosystem patterns. As explained by the authors, an improved approach in future modeling efforts would be to use more stringent criteria for hydrological steadiness, in combination with an explicit consideration of the processes creating ecological memory effects in these systems

The paper is well written; the consecutive steps of the authors' modeling approach are well described and results are presented following this same structure. The discussion clearly highlights the implications of the presented results and provides valuable suggestions for future modeling efforts in this field. I think there are a couple of instances where the authors could provide a bit more explanation about their model structure (i.e. make this a stand-alone paper), their approach and their results, as outlined (per section) below:

Background:

Peatland patterns and their importance for studying peatland ecosystem functioning have been well described in the background. This paragraph ends with an interesting comparison between different model studies, pointing out an interesting disagreement. I have, however, a few comments regarding this latter part of the background section:

1. Indeed, a similarity between the SGCJ models and the Eppinga et al. model is that the water ponding mechanism is implemented by making hydraulic conductivity dependent on water table depth/acrotelm thickness, with dryer conditions leading to a lower hydraulic conductivity.

2. I think that a difference, however, is that the feedback implemented in the SGCJ models contains elements of the peat accumulation mechanism as implemented by Eppinga et al. 2009. For example, imagine a cell which, due to its current water table depth, has a probability of 0.5 to turn into a hummock (thus, also having a probability of 0.5 to turn into a hollow) in the next timestep. Considering that the surrounding matrix (i.e. all other cells in the lattice) remain the same, this stochastic event will affect the development of the cell in future timesteps: If the cell turns into a hummock,

water table in the next development step will be lower than when the cell turns into a hollow, suggesting diverging trajectories caused by a feedback between surface state and water table depth. I think this translates well to the dynamics as seen on the rising limb of the peat accumulation-acrotelm thickness curve as shown in Fig. 3 of Belyea & Clymo (2001), a positive feedback to which Eppinga et al. refer to as the peat accumulation mechanism.

3. In contrast, in their model version including only the water ponding mechanism, Eppinga et al. only assume interaction between the water table depth and surface structure dynamics (including peat accumulation) as observed on the extremes in Be-lyea & Clymo's curves: a slightly higher or lower water table in a cell has no effect on vegetation growth when conditions are wet, and dryer conditions lead to lower plant production (and peat accumulation) when conditions are dry. The latter interaction corresponding to the falling limb of Belyea & Clymo's curve.

4. Summarizing points 1-3, I think that the discrepancies between the two models are mainly due to the fact that Eppinga et al.'s interpretation of the water ponding mecha-nism is more narrow, as part of the elements captured in the SGCJ models has been isolated in a separate peat accumulation mechanism. The disentanglement of the dif-ferent mechanisms involves the breaking up of different feedback loops, and this can be done in different ways. I think the authors can avoid a discussion about semantics (what is the most appropriate definition of 'the water ponding mechanism?'). I there-fore think that addressing this discrepancy is not the most interesting part of the model exercise that the authors perform in this paper. It is also a point that is not returned to in the discussion section of the paper, perhaps an indication that is not a major point in this study. I think the interesting part is that the authors perform a systematic analysis of the reliance of pattern formation on model assumptions, boundary conditions and integration schemes.

Model overview:

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I like the stepwise approach that the authors take, addressing particular questions by means of comparisons between model versions including/excluding specific elements. In terms of structure, however, I think it would be clearer if the first aim was split up into two different aims, the first being able to address using model 1 only, the second requiring a comparison between model 1 and model 2. To me, it seems that the first aim is to assess whether hydrological steadiness is reached in the SGCJ models, and whether reaching steadiness or not affects the patterns generated (this question can thus be addressed varying criteria and parameters in model 1 only). The next aim (effect of using transmissivity versus depth-integrated conductivity) can be reached comparing model 1 and 2. The current third aim can be reached by comparing models 2 and 3, although it is not entirely clear to me why rainfall is added to model 3 as well (later the models turn out to behave differently, and then attribution of these differences to specific hydrological processes/properties becomes difficult). I wonder if the comparison would not be cleaner if model 2 was compared to a model version in which the only difference was the impermeable peat layer. The current fourth aim seems to focus more on the question whether it is likely that hydrological steadiness can be reached in systems receiving precipitation abundantly and frequently (comparison with model 3 makes sense here).

Further, I would like to see more details on the model versions as used in the current paper. The authors start with stating that model 1 was as similar to Couwenberg (2005) as possible, but later the starting point seems to be the DigiBog model, which was then modified to approach the model described by Couwenberg (2005). In the current version of the manuscript, readers are referred to Baird et al. (2012), it is pointed out that the processes included to DigiBog by Morris et al. (2012) are not included and then some modifications are made in the current paper. I think this becomes a bit too much of a puzzle for readers to figure out the particular details of the model versions used in the current paper. I agree that the reader can be referred to specific details (such as numerical implementation) to previous papers, but given the fact that this paper uses a unique (not previously published) set of governing equations, I would like to see at least

these equations for the model versions (perhaps in tabular form) even though they may include rather standard hydrological descriptions.

When going from constant precipitation to variable precipitation (model 3 → model 4), it seems that there is an intermediate step missing; it would be insightful to see the response of the model system to a particular precipitation event (i.e. how long does it take for the system to return to the pre-precipitation event conditions). Such insight could be obtained more easily from a precipitation pattern with constant frequency, for example. Going directly to a variable timeseries makes it difficult to make this interference. Finally, one point that deserves more attention in the methods section is how the authors dealt with hydrological steadiness criteria themselves. In the previous SGCJ models, criteria were set as a fraction x of cells changing less than y units in water table depth during an update step. Although this seemed like a reasonable approach, the authors show in the current paper that hydrological steadiness is probably not guaranteed with such criteria settings. I therefore think it is of crucial importance that the authors show in this paper an approach that enables a check that ensures that hydrological steadiness is reached. I think that one way to show this would be to show that there is a size of Δte above which model simulations converge to the same behavior. Currently, however, the 10,000 h simulations show quite different behavior than the 1,000 h simulations, making it difficult to infer how far away the models still are to converging behavior of Δte. I expect that the authors have done some additional checks on securing hydrological steadiness in the large Δte simulations, and I think it would be useful to report some of those findings in the paper.

Results:

I think the results section reads very well, it clearly follows from the setup presented in the previous section. My only comment is on the presented Figure 5. I find this figure difficult to read, mainly because the 10,000 hour simulations dominate the panels. I wonder if the key statistics from these panels can not be condensed (e.g. the development of the metrics over the developmental time steps seems to be of minor

interest).

Discussion:

The discussion reads well, some important points are made about the role of feed-backs in peatland pattern formation, and to what extent this feedbacks were (or weren't) present in the model simulations studied in this paper. I only have one comment regarding this section. The model exercise presented here is to some extent based on a skimmed version of the authors' DigiBog model presented in previous papers. I think a valuable point of discussion would be how the authors foresee to proceed with future modeling efforts; is the approach presented here suggesting a follow-up that is tangent to DigiBog, or does the current study provide insight into aspects that could be included in future expansions/modifications of DigiBog? In other words, an explanation of the added value of using a skimmed DigiBog model framework in this study could be elaborated upon in a bit more detail.

Minor comments:

P. 32, L 21-22: I cannot follow the reasoning here. A model can reproduce a particular pattern seen in the real ecosystem, but for the wrong reasons (i.e. the modeled mechanisms are different from the one(s) driving the real-world pattern). The authors acknowledge this in the paper (the Grimm et al. 2005 reference is also useful here), so I am a bit confused about the meaning of this sentence in the abstract.

P. 37, L 11: This description is a bit cryptic. It seems to me that you are suggesting two things here: 1) Previously user-defined criteria were not stringent enough to warrant hydrological steadiness at the end of each development step. 2) These transient hydrological dynamics were crucial to generate spatial patterns.

P. 40, L 23: This reads a bit like a circular reasoning. [Our aim (objective i)) is to see how the model responds to parameter variation, so we varied parameters].

P. 47, L 1-20: I agree with the inferences made here, but it might be good to start with

explaining what the positive (downslope) short-range and longer-range effects are in your model framework. Also, when looking at the interaction kernels used in Thiery et al. (Journal of Ecology 83: 497-507, 1995) and Rietkerk et al. (2004), just having two scales (i.e. a 5*5 interaction neighborhood) is sufficient to create patterns. This would mean that patterning in your framework would already be achieved with 2-3 update steps (and that the 1h simulations are thus rather unique).

P. 47 L 21 – P. 48, L 25: Here two arguments are provided against completely discarding the SGCJ models. I find the second argument much more convincing than the first. The fact that models can reproduce an observed pattern does not mean that the modeled mechanisms are driving the real pattern, so I am a bit confused about this being used as an argument here. In contrast, the second argument explains how the short equilibration times introduce a form of ecological memory, which may indeed be important for peatland pattern formation. The follow up suggestion to introduce this memory effect using the cohort-based approach of the Frolking et al. and Morris et al. papers is indeed a valuable suggestion.

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