Earth Surf. Dynam. Discuss., 1, C101–C105, 2013
www.earth-surf-dynam-discuss.net/1/C101/2013/

Earth Surface Dynamics Discussions
Open Access

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

Prof Roulet (Referee)

nigel.roulet@mcgill.ca

Received and published: 19 August 2013

Overall Impression

This is a very interesting paper. It examines the pattering on peatlands. This is a interesting problem because the patterning results from the differential accumulation of peat in space, which in turn, alters the hydrological variability in space. Since peat accumulates it becomes the substrate that governs the hydrology of the peatland. The model presented in the paper is almost certainly wrong - it has some significant short comings that the authors do not hide from. I think this is a good example of the saying "All models are wring, but some can be very useful" . The authors use various configurations of their model to test some of the general hypotheses of pattern formation. It this regard this paper is a valuable contribution.

I think the authors may want to think a little about the difference between patterned fens and bogs. There modelling is more applicable to more ombrogenic than minerogenic systems. I raise this issue because it is relevant to what feedbacks need to included, how strong the individual feedbacks are, and the interaction of the peatland's hydrology to the underlying mineral substrate. The second issue I would stress is issue the authors raise about the link between the hydraulic properties of peat and the degree of decomposition of the peat. The authors discuss the consequences of not including this link, but I think there is more to it. Is there a temporal - spatial dependency to their model that result from the lack of this coupling and how would this affect the develop times for patterns?

Page & line specific comments

Pg 35 Ln 3 & Fig. 1: This peatland is almost certainly a fen. The HBL fens do not fit the Digibog view of the world. Their hydrology appears to very much flow through systems where the 'constant' slope model (models 1 & 2 in your analysis) apply. Also, while they are quite oligotrophic systems, there is likely a buffering of the organic acids yielding a pH > 5 or so - important to the presence/absence of Sphagnum which greatly influences the substrate characteristics. These are not ombrotrophic systems. The slope of these fens is related to the underlying topography rather than being generated by the difference in the spatial accumulation of peat. Further to comments that appear later in the methods, the net precipitation (your U but with snow added) is probably a half or quarter the value you use, and definitely has a strong seasonality of supply - i.e. spring with excess eater due to snowmelt, versus a summer with Et > P. These comments are all repeated from comments below, but I added this comment when I noticed the picture you used. John Riley published an excellent book on the peatlands of the HBL: Riley, J. L. 2011. Wetlands of Ontario Hudson Bay Lowland. Nature Conservancy of Canada, Toronto, ON Canada.

Pg 40 Ln 4: Is this true? The profile of permeability is different between hummocks and hollows and the position of the water table is different but is there evidence that the

ESurfD

1, C101–C105, 2013

depth integrated transmissivity is different from hummocks and hollows. I know this is a common assumption but i have yet to find good empirical evidence that it is true.

Pg. 40 Ln 12 Why not make K a f(z)?

Pg. 41 Ln 6: But the base had the same slope as models 3 & 4? That is the difference was impermeable/permeable but with the same constant slope? Ok - from the description below the slope is also different, given by Digibog. Did you try the intermediate case: permeable but constant slope? Will readers understand the difference between the constant slope and the slopes generated by Digibog?

Pg. 41 Ln 16-18: What is a tine unit fir a development step? The biomass production and decomposition have to be defined in a rate term. How are these translated into development steps?

Pg. 42 Ln 8-9: Given that the existence of patterns seems to have some broad climatological control (see Eppinga et al. 20101 [Eppinga, M. B., M. Rietkerk, L. R. Belyea, M. B. Nilsson, P. C. De Ruiter, and M. J. Wassen. 2010. Resource contrast in patterned peatlands increases along a climatic gradient. Ecology 91:2344-2355] and Numgesser's work [Numgesser, M. 2003. Modelling microtopography in boreal peatlands hummocks and hollows. Ecological Modelling 165:175–207]) is U of 400 reasonable? I would make it clear that net rainfall in annual P - annual ET. Many of the regions that contain patterned peatlands are in areas that receive at least one third of annual precipitation as snow and therefore it is not immediately hydrological active as is rainfall. Anticipating your results based on the paper's abstract do you think seasonality of net rainfall plays a role in the hydrological transience? Many northern peatlands with patterns receive a lot of water (200 -500 mm at snowmelt and then experience a deficit of atmospheric water -i.e. P < Et for summer two to four months. What influence of not have seasonality in the model have on the model output?

Pg. 42 Ln 21-24: Why not complicate it with inter annual variability using a simple stochastic filter given you have records of P?

ESurfD

1, C101–C105, 2013

Pg. 43 Ln 7-9: I agree that our eyes are a very powerful tool and that the S&G variance statistic is a more objective tool, but could you not use some simple geostatistcal tool such as semi-variograms (nugget and sill variances) to provide some quantification? Theoretical studies such as yours will never (should not) reproduce the patterns on an particular peatland but they should be able to reproduce in general the broad statistical characteristics of the range, size , frequency and density of pools as is observed in nature. Are our objective enough as analytical tools for this even basic analysis?

Pg. 43 Ln 23-25: Does this fit with the age development observed by others. It would make the youngest pools at the middle? See lines 22-25 on this page? They migrate downslope but their age would appear to be younger?

Pg. 45 Ln 16-22: This gets back to the question of constant slope (very low) but also permeable underlying substrate. Hydrologically this would be the setting on the peatland featured in Fig. 1.

Pg. 47 Ln 3-7: I am sure the authors have reached a steady-state. I think you mean their model simulations of SGCJ had not reached a steady-state. This is a fairly pointed criticism as one would expect with any model that the authors would want to find when and with what form their model reaches an equilibrium.

Pg. 47 Ln 17-20: This is very relevant given the interplay that is probably needed for the more fen-like systems.

Pg. 47 Ln 24-27: There has to be some small directed transience in peatland systems. Their dH/dt is not equal to zero so the relationship between H and the elevation of the water table is also transience. H and the wtd may reach a quasi - equilibrium on shorter time periods - a basin of attraction or zone of control for patterns which is a function of the relationship between local H and wtd, but the entire system is still hydrologically transient. In others words is the equilibrium scale dependent, and the dependent variables change with scale - does U really matter as much for patterns as it does for the larger SL 2 to 3 form of the peatland? Is there any reason that the

ESurfD

1, C101–C105, 2013

Interactive Comment

dependency needs to be continuous across scales?

Pg. 48 Ln 1-4: Would it be fair to think of this as being the true ombrotrophic configuration with its associated string feedbacks between moisture and peat production, and the previous models some less stringent configuration?

Pg. 48 Ln 12-17: Have you looked at the input and output of mass of peat (or carbon). One would expect a highly differentiate input- output, while averaged across space a near neutral to slight sink for mass otherwise the system cannot continue to grow. The reason this is important is the differential spatial accumulation and loss of mass is how the hydrology changes? I realize this is nit the focus of this paper but knowing that some continuity of peat mass is maintained would give more confidence in your results.

Pg. 49 Ln 1-3: Then why should we believe your results have any relevance and are not an artifact of the model?

Pg. 50 Ln 13-17: It maybe the lack of connection between hydraulic and peat properties that is why the time for pattern development seem off.

Nigel Roulet McGill University, Montreal August 2013

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