Review of Chaudhary *et al*., 'Modelling Holocene peatland dynamics with an individual-based dynamic vegetation model', published in *Biogeosciences Discussions*.

Overview and general recommendation

This is a mostly well-constructed paper that, in general, is easy to follow. Its subject matter is suitable for *Biogeosciences* and it contains novel work. The authors describe an ecosystem model that simulates: (i) the growth of a range of plant functional types found in mid- to high-latitude peatlands and (ii) the accumulation of peat over timescales from decades to millennia. They apply their model to a permafrost peatland in the Swedish Arctic and validate (further test) it for a temperate peatland in Canada. They also apply the model to a range of Swedish sites and consider what impact future changes in climate may have on the carbon store of the Swedish Arctic site. Overall, I think the paper is interesting and worthy of publication in *Biogeosciences*. However, I recommend the paper is revised before publication. I have made multiple comments on the pdf that I encourage the authors to respond to in a revision. My more substantial comments are developed in more detail below.

Substantive comments

1. *Model choice and model scale*

The authors note the following:

> "Model formulations of peat accumulation and decay have been proposed and demonstrated at the site scale (Frolking et al., 2010) but have not yet, to our knowledge, been implemented within the framework of a DGVM, or applied at larger spatial scales than a single study site or landscape."

The authors are right, but they then go on to apply their landscape-scale model (or land surface scheme) to individual sites, so we do not get to see what the LPJ-GUESS model does at larger scales in comparison to a series of smaller site models. The authors also provide a very limited review of other peatland models. At least two other models have been developed – MILLENNIA (Heinemeyer *et al*., 2010) and DigiBog (e.g., Morris *et al*., 2012, 2015) – and it might be useful to acknowledge what these models are capable of doing and their limitations.

The authors note that vegetation in their modelled domain can develop into patches and that each patch is represented by a different soil column. The authors seem to suggest that patches can emerge over time, but, if that is so, how can a different soil column be assigned *a priori* to each patch? The authors also suggest that water can flow between patches, which makes sense, but do not indicate how such flows are simulated (see point 3 below).

Heinemeyer A, Croft S, Garnett MH, Gloor M, Holden J, et al. 2010. The MILLENNIA peat cohort model: predicting past, present and future soil carbon budgets and fluxes under changing climates in peatlands. *Clim. Res*. 45: 207–26.

Morris PJ, Baird AJ, Belyea LR. 2012. The DigiBog peatland development model 2: ecohydrological simulations in 2D. *Ecohydrology* 5: 256–68.

Morris PJ, Baird AJ, Young DM, Swindles GT. 2015. Untangling climate signals from autogenic changes in long-term peatland development. *Geophys. Res. Letts*. 42: 10788–97.

2. *Model complexity and process and parameter redundancy*

LPJ-GUESS is a complicated model – it does many things. In choosing what processes to represent in a model it is important to consider process and parameter redundancy. For example, it may seem intuitively correct to include all obvious plant functional types, but the inclusion of some may add little to the predictive power of the model. For example, how does the model behave if litter production is confined to, for example, a single shrub PFT; do the model's results change substantially? I wonder too whether the litter production functions in the model could be replaced with a simpler function and the model results remain essentially the same? I am not suggesting the authors change the model and re-run it. It would, however, be useful to see *brief consideration* of why the model has been set up as it has been. Currently, the model set up is described rather than justified. An important paper on this topic is that by Crout *et al*. (2009) who show, for example, that a well-established and popular wetland CH_4 model is over-complicated and can achieve the same predictive success in much simpler form. Models are often more complicated that they need to be.

Crout NMJ, Tarsitano D, Wood AT. 2009. Is my model too complex? Evaluating model formulation using model reduction. *Environmental Modelling and Software* 24: 1–7, doi: 10.1016/j.envsoft.2008.06.004.

3. *Hydrological components of the model*

I found the explanation of the hydrological part of the model difficult to follow. In particular, it was unclear how the model predicts the soil moisture content of the peat above the water table. The authors note that rates of peat decomposition depend on peat wetness and suggest that the highest rates of decay occur when the peat is at field capacity, but they do not say how they modelled soil moisture content (as opposed to water-table position). Equation 7 is a balance equation that shows the different inflows into, and outflows from, the model. However, I could not find any discussion of how water inputs are allocated separately between the unsaturated and saturated zones.

The authors are also unclear on how lateral flows of water occur in the model. On lines 254-256 they note:

> "We equalize the WTP of individual patches according to the mean WTP of the landscape. The higher patches loses water if the WTP is above the mean WTP of the landscape while the lower patches receive water."

This description is too general and it is not clear *numerically* how water is moved across the landscape. I assume the model has lateral boundary conditions but such conditions are not mentioned in the paper. These can have a profound effect on how the model functions hydrologically so should be discussed and justified.

There seems to be some confusion too in how different processes are reported. For example, 'R' is defined as surface runoff in Equation 7 but later (in Equation 9) is described as a function of base runoff which seems to be some type of subsurface flow.

I recommend section 2.1.4 is re-written to make it clearer and that it is accompanied by a new diagram which shows all of the components of the hydrological budget as represented in the model (the current Figure 1 is not sufficient for this purpose).

4. *Representation of Stordalen and of soil ice*

The authors compare their simulation of the Stordalen mire to a reconstruction by Kokfelt *et al*. (2010), a paper which I have not read. I think it would be useful if the authors indicated in more detail how Kokfelt *et al*. estimated past peat thicknesses of the mire. More fundamentally, I am not clear on the appropriateness of considering peat thickness from one location at a site. My understanding is that Stordalen is a palsa mire in which case it will comprise elevated palsas – large ombrotrophic hummocks – formed by the growth of ice lenses, and intervening minerotrophic areas that form after wastage and collapse of the ice lenses. The authors note on line 543 that their model cannot simulate peat subsidence due to permafrost thaw. What is not clear is whether it can also simulate the palsa cycles that would have occurred prior to the recent warming of the climate in the region. As far as I can tell the model is not capable of simulating ice lenses.

Figure 4 shows the ‘observed’ peat thickness (the reconstructed peat thickness) at different times during Stordalen’s development and the modelled thickness. The authors provide a 95% CI around the ‘observed’ values but say the CI was inferred from the model runs. Did the model actually produce multiple peat thicknesses for different patches, in which case why don’t the authors show the spread of outputs from the model?

Finally, a more minor issue, but one that is important to address, is that it is not always clear what units are used in different parts of the model. They are given in some places but not others – I recommend that whenever a parameter or variable is first defined its units are given.

I operate a policy of 'open reviewing' and ask that my name be revealed to the authors.

University of Leeds, UK; 17th February 2017.