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Interactive comment on "Modelling long-term blanket peatland development in eastern Scotland" *by* Ward Swinnen et al.

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Overall quality of the discussion paper ("general comments")

This study is an interesting one. It tackles an important issue, our ability to model and predict how blanket bog peatlands evolved and (based on their current C balance) could behave under future climate change. It also highlights the limitation of peatlands to sequester carbon in the long-term: as the C balance of input vs decomposition decreases over time, the ability for net sequestration is limited. This issue is often overlooked and needs to be highlighted.

The authors could have added a few more modelling studies which already highlighted this issue (i.e. Heinemeyer et al., 2010; Frolking et al., 2010) – plenty of peatland de-

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Discussion paper

velopment models show an asymptotic C accumulation over time, leveling off during the past few thousand years. However, this depends on the peatland conditions (climate and topography). A bit more context in the introduction and discussion would be beneficial.

Although the model is interesting there are a few points I raise (also see below). One important, related aspect is the recreated past climate. I do not think that the anomalies for temperature or rainfall are capturing past changes. Just think about the little ice age/medieval warm period etc. Maybe consider Heinemeyer et al„ 2010 and Morris et al., 2015 for some UK specific reconstructions. 8 mm difference in monthly rainfall seem somewhat meaningless. Also, the initial warming impact and wetting at the onset of the warming about 12k ya should be more pronounced. I suggest you consider some specific literature (although I acknowledge that actually data on this is not that easy to come by). I think the issue is mainly in using a large scale model output – actually differences are lost (same as when averaging current climate over large grid scales). I suggest you discuss this limitation in the light of the above concerns and publications.

Moreover, the lack of a link between runoff and erosion within a hill slope context seems very odd to me. I suggest you consider this but maybe I have missed something in the methods. I just cannot find a link – which is crucial to allow slope C accumulation (which is not only decomposition [water table x temperature] driven). But your calibration will lead to overcompensation because of an important C flux process missing in your overall C budget.

This previous point also relates to the possible issue that underlying bedrock slope might not relate directly to peat surface slope (depressions) – another reviewer already made a comment on this. Would be nice to see a comparison in this respect.

Very interesting to see such a high level of heather domination over time. Also the hill slope data are if general interest. Are those data to be made available? I suggest a section and/or doi for the data.

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Scientific questions/issues ("specific comments")

I think my main concerns are some assumptions like: L129 The underlying bedrock is impermeable (drainage). In most cases it is, and that will likely affect water tables during dry periods. They do have hydraulic conductivity and porosity. . .but in line 147 you mention that the mineral layer has been assigned a hydraulic conductivity. Explain and be consistent throughout the manuscript.

L143 The hydrology model just assumes that the water table depth is 'always' near the surface. This is not true. I bet in 2018 the water tables even in deeper and normally wet Scottish blanket bog went down to 30 cm or even more.

L196 Litter input and quality (NPP) are affected by both, temperature and precipitation (Leith's equation). It is a bit odd to use the Moor House equation, which is purely temperature based. Maybe provide a critical assessment of doing so. Particularly the role of trees in peat depend on the water table depth (trees evaporate a lot and can cause shrinkage and decomposition of peat – but the understanding about this is still somewhat limited). Maybe consider recent publications on Scottish afforested bog by Sloan et al (with the late Richard Payne as his supervisor).

L148 Snow evaporation during frosty and dry periods can be important (see Carroll et al., 2015 model description).

L168 AET is also dependent on rooting depth in relation to the water table depth (see Carroll et al., 2015 model description).

L207 It seems a bit odd to have a 2 phase Q10 which will cause a jumping up and down in steps (why not have a continuous change in Q10 – simple function?). Also, the Q10 question is very questionable indeed – apparent vs intrinsic & short-term vs long-term & with roots vs without roots - but that is a complex issue. It would be good to also provide references as to those two Q10 values.

Abstract:

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Please add a bit more information on what kind of model you developed/tested (empirical/process) “spatially-explicit hill slope model”.

Please add more information to what this means “peatland architecture was reconstructed”. Currently this is unclear (did you take core samples/use a pollen and/or peat depth database?). Also, is ‘assessed’ a better word?

Please also add more information on what kind of climate data you used “climate reconstructions” e.g. mean annual precipitation/temperature . . . . The above points will help the reader grasp the complexity of what you did and put it into context for specific interests.

Introduction:

There are a lot of Gallego-Sala references when talking about basics of blanket bog. Is this the first time those explanations are made? I would have thought that Lindsay would have done this earlier and others before him. It is always good to go back to the original people.

I think it would be nice to mention the MILLENNIA (Heinemeyer et al., 2010) peatland model – coming out at the same time as HPM but for a UK context (so I suggest it is important in this context). This is also relevant when considering hill slope (see topography effects on temperature and hydrology, especially runoff and erosion - as in your study also using Garnett peat depth data for a validation at Moor House) and root C inputs. However, a valid point of criticism would be that it is a 2D model – so you are trying to do something better, which is great. However, you might also want to discuss what the disadvantages are of your simpler 2 depth model (root C input vs water table depth and porosity over depth affecting oxygen availability and thus decomposition – see Carroll et al. 2015 using the MILLENNIA model).

Finally, the empirical C input relationship with only temperature seems questionable when NPP is clearly dependent on both, temperature and precipitation – potential

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evapotranspiration functions are commonly used as in the MILLENNIA model, Durham Carbon Model by Worrall et al. etc.).

Typos etc. ("technical corrections")

Field data: L99 Maybe better to use 'assessed' instead of reconstructed. L100 Soil cores were 'taken' along. . . L104 give details of the sections (what depth ranges were assessed – BD changes with depth).

Model Outline L131 Please add more information regarding "Boussinesqequation for the simulation of the hillslope hydrology".

Peat Initiation I like that there is a threshold for 'peat' based on C accumulation in the top mineral layer! Nice work. However, I suggest that a bit more information is needed again for the definition of the threshold (%Corg?).

Model Calibration I think it would be nice to see how all the parameters changed from default to calibrated. Possibly also how they compare to other publications (if applicable; e.g. HPD).

Results I think reporting dry bulk density values is best in g/cm3 – most models and field measurements in peatlands will show those units (unless I am mistaken).

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