Response to referee and other comments, for manuscript bg-2015-21:
Title: Representing northern peatland microtopography and hydrology within the Community Land Model
Author(s): X. Shi et al.

(Referee and other comments in *italics*)

FIRST REFEREE'S REPORT
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This is an interesting study of water table dynamics at a single peatland site, particularly as it deals with microtopography, but is limited in scope. Its contribution to our understanding of peatland hydrology would be improved if the robustness and generality of model parameters were better established to assure us that they are of general application in peats with diverse hydrological characteristics that do not require site-specific parameterization. Its contribution would be further improved with more information about, and testing of, water movement above the water table and transfer to the atmosphere.

Response: Thank you for these encouraging comments. As described below in response to more detailed comments, the manuscript has been revised to include more complete information about parameter optimization. Based on the concern raised here, we have also added a short section to the discussion regarding the generality of the current parameterization as it relates to ongoing application within the experimental site, and to future application in other peatland and wetland settings. One goal for this work is to contribute to a peatland hydrology and biogeochemistry modeling capability that will extend beyond the SPRUCE experimental site. We intend to challenge the current model with more comprehensive observational and experimental datasets as they become available at SPRUCE and to extend the model application to other peatland and wetland settings. We expect the generality of the model will be improved through broader application, and those efforts will be the focus of future reports. We believe the current model serves an important need for the advancement of testable hypotheses in the SPRUCE experiment setting. As documented here, the model has already been useful in focusing attention within the experiment, for example, on the interaction of above and belowground warming with snowpack and seasonal temperature variations.

Introduction p. 3383 l. 16: Grant et al. (2012) modelled, rather than reported, that the productivity of wetlands was strongly affected by changes in water table level. p. 3385 l. 17: ...used ...

Response: Thank you for your corrections of these and we have changed them.

Model Description p. 3389 eq. (1): The physical basis for this equation needs to be presented – what hydrological process or soil attribute does fdrai represent? Is the value used here applicable only to the peat in this study? How can it be derived for peats with differing hydrological characteristics without recalibration? The term 'zlagg' in this *equation is the same as the external water table used to define boundary hydrology in Grant et al. (2012), and so does not represent a conceptual advance on earlier modelling approaches as claimed on pp. 3384 – 3385 in the Introduction, but rather a similar approach. p. 3389 l. 16: mm s-1 or kg m-2 s-1? p. 3390 eq. (3): Again, the physical basis for this equation needs to be presented – what is the rationale for these terms? How robust are they? p. 3390 l. 23: How did 2013 differ from 2011 and 2012, thereby providing an independent test of the robustness of the modifications? It more convincing to test with results from more than one year.*

Response: For this study we use the thermal and hydraulic properties of peat as defined globally in CLM 4.5 (Lawrence and Slater, 2007) with the exception of the maximum subsurface drainage rate, which is calibrated for the site. The parameter fdrai is an exponential decay factor that controls the rate of subsurface drainage as a function of water table elevation. The parameter $q_{drai,0}$ defines the maximum rate of subsurface drainage when the water table elevation is at the surface of the soil column.. We use the globally defined value of *fdrai*, and determine $q_{drai,0}$ through parameter calibration using observed water table depth. The modified text first defines the equation for the original CLM4.5 model (Eq 1), then introduces the modified form for our new model (Eq 2).

We acknowledge that there are similarities in our approach with Grant et al. (2012), and that both approaches are intended to set reasonable boundary conditions for the system of study. A significant difference in the boundary conditions between the two approaches, and the main distinction we intended to call out in the model description, is that we have specified a local geomorphological feature (the elevation of the bottom of the lagg), whereas Grant et al. (2012) specify a bounding hydrological state. One difference is that in our approach the minimum of the bog water table is not constrained by the lagg elevation – especially under warming conditions and in a year with low precipitation the bog water table can drop well below the lag. For a treatment as in Grant et al. (2012), the specification of a regional water table will drive fluxes into or out of the bog depending on the modeled local bog water table height. We think the specification of lagg height is the appropriate boundary condition for the case (like the SPRUCE site) of an ombrotrophic bog, with only very weak connections between the local and regional water tables.

The unit for $q_{lat,aqu}$ is mm s^{-1}, not kg m^{-2} s^{-1}.

The text has been revised to include a description of the physical basis for our original Eq 3 (now Eq 4 in the modified text).

The observed data of 2013 is not especially different to 2011 and 2012. We agree that additional years of observations will provide a more convincing test of the model. We will continue to evaluate the model as the SPRUCE experiment and other ongoing peatland studies proceed.

Results

p. 3393 l 10: What was the lagg depth with respect to the hollow?

Response: The depth of lagg is specified here as 0.4m below the elevation of the hollow. We also note that as we apply the model to predictions in the specific setting of the SPRUCE experimental manipulation rings, the raised-dome nature of the bog means that for rings closer to the lagg this elevation difference will be smaller. In practice we will reset this parameter for each ring based on an accurate survey of the mean hollow elevation in the ring. This information has been added to the text.

p. 3395 l. 25: Under higher temperature, wouldn't soil surface drying with lower water table (Fig. 6) reduce surface evaporation from soil (Table 2: Fig. 7) and moss during summers through reduced soil hydraulic conductivity? Information about modelled water movement in the unsaturated zone is not provided in the paper. Tests of modelled soil water content above the water table should have been included in order more fully to evaluate the model.

Also air temperatures greater than ca. 20 ^{o}C are commonly observed not to raise LE measured by eddy covariance towers over coniferous forests because of decreased stomatal conductance. This response has been modelled with a D0 term in the Ball-Berry equation, although it can be better attributed to lower hydraulic conductivity in coniferous xylem. This response may be less apparent in larch than in spruce. However it does suggest a smaller increase in Ec and hence ET (Fig. 7), and hence a smaller increase in water table depth (Fig. 6), than that modelled here. Information about the calculation of ET in CLM in this paper is inadequate to evaluate model results for ET (e.g. the D0 term was left out of Table 1). How were these issues of LE response to temperature addressed in the model, and was the response of modelled LE to temperature evaluated against flux measurements?

Response:
The suggested mechanism (reduced soil hydraulic conductivity as soil surface dries) is included in the model, but for these simulations the increased evaporative potential being driven by higher temperatures and associated higher VPD provides a counteracting tendency, with the net result of higher simulated ET under warming. We do observe that modeled increases in soil evaporation are smaller later in the growing season, and soil evaporation from hummock is lower in September for the +9 °C warming treatment than for the +6 (Fig. 7), reflecting the simulated reduction of soil hydraulic conductivity. Mention of this has been added to the results (section 4.2).

Site soil/peat moisture observations are being added to the experiment in 2015 and we will further evaluate model performance with these new data as they become available.

In CLM, the stomatal conductance in the Ball-Berry formulation is linearly scaled by relative humidity at the stomatal opening (actual vapor pressure divided by saturated vapor pressure at leaf temperature), as opposed to the more traditional use of VPD/D0. That means there is no additional empirical coefficient for this part of the relationship – at saturating relative humidity the linear multiplier term is 1.0, and for completely dry air the term is zero. CLM does not include a term for xylem conductance control on stomatal conductance or transpiration. Ongoing experimental observations at SPRUCE are considering such a mechanism of water stress response. Detailed information about the implementation of the Ball-Berry equation and other model parameters is found CLM4.5 tech note (Oleson et al., 2013). Unfortunately eddy flux data are not available at this time for the SPRUCE site. Clarifying text has been added to the results (section 4.2).

Discussion

p. 3398 l. 17: The zlagg term in Eq. 1 does in fact, represent a local constraint to lateral boundary flow in the model. There is nothing wrong in having such a constraint, but it is not accurate to indicate that this constraint is absent.

Response: The text has been modified for clarity on this point.

p. 3398. Sec. 5.2: Discuss site-specificity of the fitted parameters in Table 1. How robust are they? To what extent to they reflect the varying hydrological characteristics of different peats?

Response: We added a sentence to the discussion on this point. We find it encouraging that realistic simulations were obtained using the global default thermal and hydraulic properties defined in Lawrence and Slater (2008). The applicability of this model at other sites of course depends on the robustness of the site-specific fitted parameters. Future work will consider evaluation at other sites. Such parameterization for other sites could be obtained by the application of fine-scale models and/or high-resolution remote sensing information to characterize the local microtopography and flows.

Second REFEREE'S REPORT
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GENERAL REMARKS

I have completed my review of the manuscript "Representing northern peatland micro-topography and hydrology within the Community Land Model" by Shi et al. The almost complete inability of current large-scale land surface models to represent satisfactorily the interactions between climatic change and peatland hydrology (and so by exten- sion peatland biogeochemical cycles) is, in my opinion, the 'elephant in the room' that we can't continue to ignore. As such I believe the intention behind this paper is very valuable, and I was excited to read and review it. However, I have some concerns about both the manuscript itself and the new model that it describes. Some of my concerns may stem from deficiencies in notation, or in some cases I may simply have misunderstood what has been done. However, for each of my major comments, below, I believe that the authors should take one of the following three courses of action before the manuscript should be considered for publication: 1) use a physically-based argument to rebut my criticism and justify assumptions or choices of model specifications; 2) make the relevant alterations to the model in line with my criticisms and present a thoroughly revised *paper; 3) clarify the manuscript in any situations where I may have misunderstood what has been done so as to guide other readers away from similar misunderstandings. In the broadest terms, I am concerned that the model is overly reliant on tuned pa- rameters and that the new equations added to the existing CLM model are not phys- ically based. As such I question the general applicability of the new model to other study sites, and indeed its ability to reveal new process-based understanding about peatland-climate interactions.*

Response: We very much appreciate the supportive assessment of our topic, and have followed the suggested approach in responding to the major concerns, below. As a summary statement, the five model modifications identified in the text (Section 3.2) are all based on the physical situation as observed in the bog, in an effort to represent the physical processes which cause the ombrotrophic bog hydrology to differ from the general soil hydrology representation in the original model. We have added a sentence to this section reflecting that intent.

MAJOR COMMENTS

Equation 1: I spent a long time picking through this to try and make sense of what is happening, but I have come to the conclusion that either the model, its description here, or both, are in error. Firstly, in the text immediately below the equation, the defi- nitions seem to have got mixed up. Surely q_drai,0 is the drainage rate when the water table is at the surface (water table = zero), whereas q_drai is the variable drainage rate. Secondly, and more importantly, the authors present this new equation to de- scribe drainage, but offer no justification for why the functional form of this alteration is appropriate or what has informed its development. Please explain why this relation- ship should be an exponential one. What does the parameter f_drai represent? It is described vaguely in the text as a decay factor, but it strikes me that this represents some property of the aquifer such as hydraulic conductivity of deep peat or the distance between the centre and the edge of the bog dome. With that in mind, why not use a more physically based representation of drainage? I think we should be wary of popu- lating models with fitted parameters that have little or no physical meaning, so please explain. Thirdly, what are the assumed (or perhaps fitted values) of q_drai,0 and f_drai in the baseline parameterisation? Large values of q_drai,0 in particular would cause lateral subsurface drainage to dominate the model's water balance, while high values of f_drai would lead to a strong negative feedback between drainage rates and water- table position. As such it is important to know what values you chose for your baseline parameterisation. Fourthly and most importantly, I can't see why f_drai has a negative sign. The negative sign before f_drai is part of the exponent, meaning that as the water- table gradient between the bog and the marginal lag (i.e., the difference between Z_w and Z_lagg) increases, the drainage rate decreases. Surely drainage should increase in this situation? The only possible explanation I can think of is if f_drai is itself always a negative value, in which case the negative sign in eqn. 1 would cause its effect on the exponent as a whole to be positive and the problem disappears. But of course it's impossible to tell because no values are given for f_drai. If this is an error in notation then please also confirm that this error is in the manuscript only and does not extend into the model's numerical implementation. If, on the other hand, I have misunderstood something here *then please clarify the explanation of this equation to prevent others from making the same mistake.*

Response: We sincerely apologize that there was a typographical error made in equation 1. The functional form of the equation used in the model code is now represented correctly in the text.

Equation 2: The simple arithmetic mean used here to calculate an average K between hummocks and hollows is arguably inappropriate because K is a rate coefficient. Harmonic mean is the appropriate mean for an average of two or more rates partly because it emphasises low values. The spatially distributed peatland development and hydrological model described by Baird et al. (2012) provides an example of how to deal with this situation. More importantly, if both hummock and hollow water table depths are measured relative to the surface of hummocks (as stated on P3387, L24) and negative values are below the hollow surface then as far as I can see the variable Z_h2osfc,hol should be added to, not subtracted from, the hollow water table. The presence of ponded water in hollows would act to reduce the hydraulic gradient relative to neigh- bouring hummocks, yet deducting a positive number (Z_h2osfc,hol) from Zw,hol acts to increase the gradient. Finally, if both hummock and hollow water table depths are measured relative to the surface of hollows then the last term in the numerator on the RHS of eqn 2 (to compensate for the height difference between hummocks and hollows) is unnecessary – please remove it. The issue of positive/negative water ta- bles in shallow water-table environments is always confusing, but I think your specified conventions have been applied inconsistently, which has made your equations all but impenetrable. As with Eqn 1, please either rectify or clarify, and confirm that the model implementation is error-free.

Response:

Regarding the use of arithmetic versus harmonic mean for averaged hydraulic conductivity: We agree that in general for a flux calculation across a distinct transition in hydraulic conductivity, some averaging method other than the arithmetic mean is more appropriate. There is considerable literature exploring exactly which averaging method is most appropriate for specific conditions of head and differences in material properties, and the harmonic mean is not always a good choice in practice (see e.g. Srivastava and Guzman-Guzman, 1995). A more problematic aspect in our particular case is that while we have conceptualized the hummock and hollow as discrete adjacent columns (manuscript Fig 2), in the real bog the interface between them is indistinct and smooth, with the largest differences in material properties found between the hollow bottoms and the hummock tops. Given the geometric complexity, it is not obvious that harmonic averaging is more appropriate than arithmetic or some other method. Given these uncertainties, and the fact that the interface between the hummock and hollow is smooth in reality, we opt to use the arithmetic averaging method. We hope to explore this question with a more sophisticated model at a finer spatial resolution in a future study.

[R. Srivastava and A. Guzman-Guzman, 1995. Analysis of hydraulic conductivity averaging schemes for one-dimensional, steady-state unsaturated flow. *Ground Water, 33(6), pp. 946-952.*]

In the original manuscript z_w represented water table depth while z_{h2osfc} represented surface water height, which was confusing because of the opposite sign conventions. The model implementation is correct, and the manuscript has been revised for clarity. The new term $z^*_{w,hol}$ represents the hollow water table depth, with hollow surface water height subtracted (adding surface water effectively reduces the water table depth). This results in the expected effect on the hydraulic gradient. The $\Delta z_{hum,hol}$ term is unnecessary and has been removed as recommended.

P3390 L13-14: Please don't skip over descriptions of alterations to the model (Modification 4) just becau.se they turned out to be unimportant (making this revelation during the model description is also premature). If the process is unimportant then why include it at all? If it is included then you must describe it sufficiently for someone else to understand - and indeed replicate - your work.

The premature results were removed and additional descriptive text added for this modification.

Equation 3: Again, please justify the functional form of this relationship. Why have you chosen this function in place of other possibilities that could have been used to represent this process? I am also concerned that this appears to be another fitted function, in which r_h2osfc seemingly has little physical meaning.

Response: Please see our response on this same point to referee #1, above.

P3391 L7-11: This methodological overview should be right at the very beginning of the model description section so that readers can see straight away what you have done in broad terms, particularly the fact that your model is lumped (aspatial). It is important for you to be up front about this assumption given that the premise of the paper is an attempt to incorporate the effects of spatial heterogeneity.

Response: This overview has been moved to the beginning of section 3.2 describing the changes that were made to CLM to represent bog hydrology

Figures: Presumably the series labelled "hummock" (Fig. 3, blue), "CLM" (Fig. 4, blue) and "CTL" (Fig. 5, black) are the same time series from a single model run with the baseline/default parameterisation. Please clarify this in the figure legends and captions. Use of a consistent colour scheme and naming conventions across all figures would help greatly in this regard. Why is a zero line included in some plots and not others? On my screen the line series are very thick, causing overwriting. The plots would appear less crowded and would be easier to read if the time series lines were a little thinner.

Response: You are right, the three lines in different figures are showing the same model output, and the labeling in the original manuscript was confusing. We have changed "CLM" to "CTL" for figure 4, and changed the color from blue to black for "Hummock" line of figure 3. However, we still wanted to keep using "Hummock" for figure 3 because the purpose of that figure is showing the water table levels for Hummock and Hollow in our control simulation with our new modifications. The zero line for the figures with water table levels means the surface of hollow. We added the zero line for all figures with water table levels followed your good suggestions. We also made the lines thinner for all plots. Thanks for these helpful suggestions.

P3393 L8-12: Is vertical drainage merely "limited" as stated here or is it assumed equal to zero? Also, the use of the word "prognostic" here caught my attention. What do you mean by prognostic? Does this mean that you chose a value for what you thought water tables ought to be and tuned other parameters accordingly? Details of parameterisation are rather thin on the ground. Particularly for parameters that aren't currently being measured ate the study site, it's very difficult to tell how the model was parameterised. If parameters were tuned then it's of little wonder that the model fitted well to observations from other time periods, but it also makes me wonder about the generality and broader applicability of your model beyond your study site.

Response: Subsurface drainage depends on the water table elevation and is zero when the water table elevation drops to or below the level of the lagg. The underlying assumption is that the glacial till acts as a barrier to drainage when the water table is lower than the lagg. In reality, there is an observed "deep seepage" term as described in the manuscript but that term is set to zero for current simulations as we lack the data to parameterize. The text has been modified to clarify this point.

We use "prognostic" to mean that the bog water table height is a state variable simulated by the model, as opposed to an imposed boundary constraint. We clarified by replacing "prognostic" with "simulated".

Discussion and Conclusion: Much of this text, not just section 5.3, comprises a lengthy and at times low-content manifesto for the current and future goals of the SPRUCE project. Although it is noteworthy to read that your work is part of a larger, ongoing effort, a long monologue on the broader goals of the project are likely to be of only limited interest to those not immediately involved in it. This padding could (and in my opinion should) be greatly reduced, and the discussion rewritten so as to serve its primary purpose – interpreting your results in the context of your research questions. Please identify the two or three main findings from your research that add something new to peatland science or biogeosciences more generally, and concentrate the discussion on those. What have your numerical experiments added to process-based understanding of peatland-climate interactions?

Response: The discussion has been reorganized and rewritten following your suggestions.

MINOR COMMENTS

In addition to the comments above, which I believe are central to the reliability of the model and/or the readability of the manuscript, I also have the following minor comments that may help the authors to improve the manuscript:

The introduction is the best part of this manuscript. I found the rationale both concise and convincing. However, I think some references are out of date or missing. Peatland development models have moved on a lot in the decade and a half since Hilbert et al. (2000). Have a look at the more recent studies by Frolking et al. (2010) and Morris et al. (2011). Additionally, although created for a different purpose than your model, the group of cellular landscape models described by Swanson and Grigal (1988), Couwenberg and Joosten (2005), Eppinga et al. (2009) and Morris et al. (2013) all deal explicitly with fine-scale variability of peatland hydrology. As such their hydrological routines are substantially more sophisticated than the model presented here, and it might be appropriate to acknowledge where your model lies on this scale of complexity.

Response: Thank you for these suggestions. We have incorporated the additional studies in the introduction and added text placing our efforts in the context of a spectrum of peatland model complexity.

P3385 L16-17: typo here I think - is the new model called CLM_SPRUCE or CLM-SPRUCE?

Response: It is a typo, thanks for pointing it out.

P3385 L26-28: This part of the rationale reads as somewhat weak. I would argue that the CLM model itself is of little interest, and that it is merely a tool to address interesting questions about biosphere-climate interactions in the real world. As such, the fact that this is the first time peatland hydrological routines have been introduced into CLM is similarly of little interest. What would be much more interesting was if this were the first time that such routines had been included in any such model, making your study genuinely the first of its kind. Is this the case?

Response: As per our responses above, we acknowledge that other more sophisticated models exist. The point we hope to make regarding novelty is that this is the first time that the unique hydrological constraints which characterize ombrotrophic bogs have been included in a land surface model that is designed to operate as a component of a fully-coupled Earth system model. To strengthen the point, this sentence is rewritten as: “The model improvements reported here represent the first time that the isolated hydrologic cycle of an ombrotrophic bog, with its characteristic raised hummocks and sunken hollows, has been represented in the land surface component of an Earth system model.”

P3388 L25, and P3389 L8: What you have done is more than merely parameterization, which I would take to mean adjusting the parameter values in an existing equation. You have changed the functional form of the governing equations.

Response: We agree, and have changed 'reparameterization' in P3388 L25 to 'reformulation' and 'reparameterized' in p3389 to 'reformulated'.

P3395-6: This description of changes in ET is vague. Please summarise in the text the magnitudes and/or temporal behaviour of the most important changes between model runs.

Response: We added additional descriptions for ET based on table 2 to the 'influence of warming on simulated evapotranspiration' section.

P3394 L13, P3396 L4, P3396 L27, and elsewhere: Please reserve the use of the word "significant" and its derivatives for describing statistical significance.

Response: Following your suggestion, we have made these changes throughout the manuscript.

Discussion, section 5.1: The representation of peatland hydrology in your model, although improved relative to the previous CLM, is still a long way behind that in a number of other ecosystem-scale peatland hydrological models, particularly those that deal explicitly with two- or three-dimensional spatial heterogeneity (see also above). Although there is obviously a very valuable role for large-scale, lumped models such as yours (not least as components within global scale simulations) it would be prudent to acknowledge here that more advanced peatland hydrological model schemes exist, albeit ones that are designed for different purposes.

Response: We have taken this suggestion into consideration in our re-write of the discussion. We have also modified the introduction to include a brief review of more complex ecosystem-scale peatland models (as above), and highlight our contributions in the context of land-surface models and global climate models.

The reference to Hilbert et al. (2000) is missing from the reference list. LITERATURE CITED

Response: All references have been updated.

Short Comments from Theodore Bohn.

This paper is a timely and valuable contribution to high-latitude wetland modeling. I am writing because I noticed an important omission in the discussion of previous attempts to model peatland microtopography, and its effects on hydrology and carbon fluxes, in the introduction.

On Page 3384, line 6, the authors state that: “to raised-dome bog peatlands. The absence of this important detail may limit the predictive capabilities of existing peatland models. Other ecohydrological models couple hydrology and carbon cycles in peatlands, but differ greatly among each other with respect to their hydrological schemes and the way they treat (or ignore) terrain topog- raphy (Dimitrov et al., 2011). Some models, such as Biome-BGC (Bond-Lamberty et al., 2007), and Wetland-DNDC (Zhang et al., 2002) only simulate vertical soil water flow, neglecting lateral flow components (Dimitrov et al., 2011) within peatlands. Others, such as BEPS (Chen et al., 2005, 2007) and InTEC v3.0 (Ju et al., 2006) include sophisticated ecohydrological and biogeochemical sub-models capable of sim- ulating three-dimensional hydrology (for large scale topography) coupled to peatland carbon dynamics. Sonnentag et al. (2008) further adapted BEPS to model the effects of mesoscale (site level) topography on hydrology, and hence on CO2 exchange at Mer Bleue bog. To the best of our knowledge, only one ecosystem model currently includes representation of microtopographic variability (hummock-hollow topography), that being the “ecosys” model (Grant et al., 2012). Ecosys tracks horizontal exchange between hummock and hollow elements, but its prediction of water table dynamics is constrained by specifying a regional water table at a fixed height and a fixed distance from the site of interest."

However, at least two other ecosystem models have represented hummock-hollow topography: LPJ-WHyMe (Wania et al., 2010) and VIC (Bohn et al., 2013). Both of these models described their formulations in papers that focused on methane emissions (rather than net carbon exchange), which perhaps explains why the authors might have missed them in their literature searches. But both of these models simulate other aspects of the carbon balance and should be included in the current paper’s literature review.

In addition, several other studies have attempted to account for sub-grid water table heterogeneity via a TOPMODEL (Beven and Kirkby, 1979) approach, including Bohn et al. (2007), Bohn et al. (2010), Ringeval et al. (2010) and Zhu et al. (2014), although he TOPMODEL approach’s validity is questionable in flat areas such as peatlands and its results can only pertain to heterogeneity at the spatial scale of the DEM that is employed (typically much coarser than the size of individual hummocks and hollows). Still, I think it is worthwhile to mention these studies, if only to contrast their approach with the current approach.

References

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Geosci. Model Dev., 3, 565–584, doi: 10.5194/gmd-3-565-2010, 2010.

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After re-reading my comment, I think I should clarify: the TOPMODEL approaches were intended more to account for general water table heterogeneity, across an entire grid cell rather than within the wetland portion of a grid cell, and were not necessarily attempting to account specifically for the effects of peatland microtopography. So I leave it up to the authors to decide whether it is worthwhile to mention them..

My main point is that the authors should mention Wania et al. (2010) and Bohn et al. (2013), since these did, in fact, address peatland microtopography specifically.

I apologize for any confusion that the wording in my first comment ave caused.

Response: Based on your comments, we included Bohn et al. (2013) in our introduction section. We now also cite Wania et al. (2010) in the introduction: based on our reading, Wania et al. (2010) considered wetland hydrology, but they did not explicitly consider microtopography.