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Interactive comment on "Representing northern peatland microtopography and hydrology within the Community Land Model" *by* X. Shi et al.

Anonymous Referee #2

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GENERAL REMARKS

I have completed my review of the manuscript "Representing northern peatland microtopography 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 extension 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 parameters and that the new equations added to the existing CLM model are not physically 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.

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 definitions 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 describe 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 relationship 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 populating 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.

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 neighbouring hummocks, yet deducting a positive number ($Z_{h2osfc,hol}$) from $Z_{w,hol}$ 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 tables 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.

P3390 L13-14: Please don't skip over descriptions of alterations to the model (Modification 4) just because 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.

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.

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.

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.

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.

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?

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.

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

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?

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

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.

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

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.

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

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