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Discussions

Interactive comment on "Resolving terrestrial ecosystem processes along a subgrid topographic gradient for an earth-system model" *by* Z. M. Subin et al.

Anonymous Referee #1

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General comments:

This paper describes a new tiling approach integrated into the GFDL LM3 land surface model. The tiling approach simulates land tiles that are linked hydrologically along an upslope to downslope gradient. The approach, dubbed LM3-TiHy, allows diagnosing of water table and soil moisture differences between uplands and lowlands. The LM3-TiHy model is tested against global wetland distributions as that is an obvious application of such a technology. The results are, however, mixed with relatively little evidence of improved wetland simulations, although much of the deficiencies are blamed on an inadequate ability to parameterize the model effectively due to lack of

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observational datasets. The paper is generally well written and clear.

I feel that the approach taken here is possibly a good one and I certainly am fully supportive of their attempts to more effectively simulate global wetlands using a technique capable of distinguishing upslope to downslope gradients. However, I do not think the paper, as is, provides a publishable outcome. I feel that the authors should provide more site-level analysis demonstrating that the model formulations are valid, before attempting these global-scale simulations. If the authors can take their model and run it for sites where the model parameters can be appropriately set and the model outputs evaluated, it would provide much more confidence that the model formulations are appropriate. The authors do show three example regions with different water table regimes (Fig 3), but these are just taken from the global simulations and have no observations for validation against. I do understand that water table information is sparse, but with no effective evaluation of the model outputs, it is hard to accept the global simulation results. The authors propose something similar on p. 8471 line 24 and I would suggest that, as they say, evaluation at sites or regions where the topography, substrate properties, forcing climate and resulting hydrology are well characterized should improve confidence in the model. I feel that this has to occur before the underlying model structure behind these global scale simulations can be assumed to be correct. I think these proposed simulations could form a first paper, with this manuscript reviewed here as a follow-up paper after having laid the groundwork and demonstrated the appropriateness of the model structure in the first regional and site-level paper.

Other comments:

I also feel that the authors missed an opportunity for a further interesting application of LM3-TiHy. While their results demonstrate the LM3-TiHy, as this present stage, does not represent a significant advancement for modelling wetlands, it could provide a means for better capturing vegetation producivity differences between uplands and lowlands. The authors do look at LAI in Fig 7 and also evapotranspiration, but no attempt is made to compare to observations. The authors also themselves suggest comparing their results to observations (p. 8469 line 6) and I encourage them to do just that.

The authors present some results derived using the unpublished CORPSE model. Since this model is not published and no description of the model is provided, I have no way to evaluate if these results are in any way reasonable. I request the authors to remove the CORPSE results from the paper since it is not possible to adequately evaluate these results.

Abstract, last sentence.- The authors suggest that the LM3-TiHy model can provide a new approach to investigating the vulnerability of boreal peatland carbon to climate change. I am not so sure of this as peatlands are generally areas of low-relief and the West Siberian Lowlands and Hudson's Bay Lowlands were both poorly simulated by LM3-TiHy. This is important as the WSL and HBL are the predominant regions of boreal peatland carbon.

How plausible is it to realistically parameterize the model using observations? I think this is a reasonable question given how much uncertainty there seems to be in how to parameterize the model globally. Is this something that is ever going to be possible? Would it just take a concerted effort to derive from presently available datasets? New observations required? From my reading, it seems appropriately parameterizing the model would be a monumental task.

p. 8449 line 12 - Expand on what is meant by 'simple plant functional type transition dynamics'. This sentence is too vague as is.

p. 8453 line 3 - The surface runoff is assumed to flow directly downstream, bypassing the intervening tiles. Any idea how important this simplification is?

p. 8459 line 1-4 - I am not sure I understood this. By only considering the water table to a depth of 0.1 m, the authors are not looking at where water table is most important in wetlands, i.e. shallower than 0.1 m. Perhaps expand on this.

p. 8459 line 12 - I assume I am being thick here, but the description of the sensitivity and precision read as though they would give the same result.

p. 8461 line 10 - This suggests to me that the perched water tables in permafrost zones might correspond to a separate regime from the three identified by the authors as the underlying processes would differ from other regions, i.e. the regions could shift regimes as the active layer deepens.

p. 8470 line 20 - The authors suggest that observations of inundated area are better than those of wetland ecosystem extent. I don't agree considering the problems with inundated area observations in areas with high canopy cover and flooded agricultural fields (see discussion of GIEMS in Melton et al. 2013). I ask the authors to provide more support for the statement.

p. 8471 line 13 -17 - Yes, including ground water pumping, irrigation, and drainage should improve wetland simulations. However the LM3-TiHy model seems to perform most poorly in regions that are relatively unaffected by these processes. What about floodplain processes or improving processes in areas with low-relief?

— Literature cited:

Melton, J. R., Wania, R., Hodson, E. L., Poulter, B., Ringeval, B., Spahni, R., Bohn, T., Avis, C. A., Beerling, D. J., Chen, G., Eliseev, A. V., Denisov, S. N., Hopcroft, P. O., Lettenmaier, D. P., Riley, W. J., Singarayer, J. S., Subin, Z. M., Tian, H., Zürcher, S., Brovkin, V., van Bodegom, P. M., Kleinen, T., Yu, Z. C. and Kaplan, J. O.: Present state of global wetland extent and wetland methane modelling: conclusions from a model inter-comparison project (WETCHIMP), Biogeosciences, 10(2), 753–788, 2013.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 11, 8443, 2014.