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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 #2

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This paper stems from a brilliant idea to describe sub-grid soil moisture variability in land surface models, based on a tiling approach allowing to account for water table depth (WTD) gradient along hillslopes. Yet, the paper struck me more by what it lacks (a good structure; a comprehensive description of the model, simulations, forcing and validation data; a critical look at some results and the work's limitations) than by what it brought me as an interested reader. Basically, I felt the paper wanted to say too much, and couldn't say it well in the standard length of a scientific paper. I thus recommend major revisions, and hope the comments below can help.

1) Introduction: The paper claims to pioneer the implementation of below-ground,

hydrologically-based tiling, but proper credit should be given to Koster et al., 2000, who proposed the Catchment model with the same purpose 15 years ago, although with tiles of dynamical extent, based on TopModel's analysis of the relationships between topography and WTD. Note also that the underprediction of wetlands in flat areas (p8447, L10) is not an intrinsic failure of TopModel, but is restricted to its simplistic implementation in most LSMs, following Gedney & Cox, 2003.

2) Models:

2.1) LM3: p8451, L10: if several representative hillslopes are used in one grid-cell, are they hydrologically independent (i.e. no exchanged water)?

2.2) LM3-TiHy: The approach is far from being straightforward, and the paper is not self-consistent to explain it, as it is necessary to search for many important information in Milly et al., 2014, and in the technical note provided as Supplementary material. I recommend to present in the paper all the equations and parameters that are further used to discuss the results. If the authors feel some equations need to be annexed, then restrict the technical note to what is needed for this paper, and make sure the notations are consistent with the ones of section 2.1. It would be nice to illustrate the variables of Eq. 4 in Fig 2, including the relative positions of the tiles (j-1), j, (j+1). I suggest that the beginning of Section 2.3 is used in 2.2 to explain Ln (adding the definition of Z(x)). I didn't understand the rationale of Eq.5 and how it is used to define Kl in Eq. 4. Finally, it is said several times throughout the paper that inundation processes are not fully described in LM3-TiHy, but I couldn't find a clear description of what is implemented and what is lacking. It should be added since the authors justify many of the poor performances of the model by these shortcomings.

3) Sections 2.3 to 2.5 of the Methods: I found this part really messy, as it requires lots of back and forth reading, and I never had the information I wanted when I wanted it.

3.1) An alternative structure could be to separate the input data (slope, permeability), the performed simulations (including the way prescribed parameters are prescribed, and ending with the spin-up procedures), and the validation data, which lack important details as the spatial and temporal resolution and covered period. The interpolation procedure for GLWD (p8460) was not crystal clear to me, while it is called as a potential explanation of the model's poor performances (p8465). By the way, if the authors really believe this, why not improving this interpolation procedure before further analyses?

3.2) More importantly, I had problems really understanding the assumptions behind the simulations and the differences between them: do all the tiles of a grid-cell have the same characteristic slope (zeta) ? Does zeta take only three possible values using the FAO data? Are Untiled and CORPSE-Untiled defined by having only one tile per grid-cell? How do they differ from the LM3 simulation in Milly et al., 2014? Why is the hillslope length L=1km ? How can you describe 2°x2.5° grid-cells using one 1-km hillslope? Why do you assume a depth to bedrock B=200 m and what are the implications regarding the results? Why don't you build the Concave and Converging simulations from the same simulation? HSWDSlope Bimodal is not clear to me, does it assume a rectangular shape with two different slopes? If so, how do they alternate along x (zeta1, zeta2, zeta1, zeta2, etc...; or 5 times zeta1, then five times zeta2)? I also suggest to add some information in Table 1 to clarify the differences between the simulations, and to explain CORPSE only once, when presenting the simulations.

3.3) Regarding the details about the specific analyses (how to define WTD, wetland and inundated fractions, Budyko's index, the WTR), I would have preferred reading them directly in the Results section, when these specific features are analyzed. Regarding the diagnosed inundation fraction, the authors write p8458, L16, that they didn't attempt to to tune topographic parameters to match the observed inundation, but it is a bit abusive, as they did tune zm, which is not very different from correcting topographic indices from their known dependence onto DEM resolution. Regarding WTR, some variables in Eq. 10 do not have the same meaning as in Gleeson et al., 2011a (B replaces maximum terrain rise, total runoff replaces recharge): please discuss the consequences.

4) Results:

4.1) All the maps require a higher resolution, as we need to zoom in a lot to see anything.

4.2) In 3.1, I would present Fig 4, with the main forcings of simulation Base, before the typical WTD regimes (Fig3). More importantly, I was intrigued by the selected hillslopes shapes, with 5 to 10% slopes over zones supposed to extend 1km from the streams: I suspect this is quite rare, and that river beds usually have flatter cross sections; and what happens further from the streams, since it can take more than 1 km for overland flow to reach the closest stream? Can you discuss these strong assumptions in the paper?

4.3) I also regret that no attempt is made to validate the simulated results: Fig 4a could be compared to the map of Gleeson et al, 2011a, over the USA (with significant differences over Florida for instance); couldn't the authors find any monitored transect showing the kinds of WTD gradient they highlight? One could think of the Sleepers River catchment for the first regime. Refer to above comment regarding the second regime illustrated by a Florida grid-cell. And regarding the third one, with groundwater level below stream level, there should not be any baseflow in such a case, so streamflow could become ephemeral: is it consistent with typical hydrological regimes in the Rocky mountains?

4.4) Upland vs Lowland: this analysis is split between Sect 3.2 for WTD, and Sect 3.3 for the surface properties (ET, Ground temperature T, LAI). (i) I didn't find a clear definition of the upland and lowland tiles, even though I supposed they were the two extreme tiles along the simulated hillslopes. (ii) I would have liked seeing the difference in WTD between up and low to better understand the corresponding differences in ET, T, and LAI. (iii) I was very surprised that the mean values of these surface properties (Fig 7, left) are not at all assessed against a reference, whether from LM3 or from observations. (iv) An important result is that the LAI increase in lowlands compared to uplands may be excessive, but it is only said in the Conclusion (p8469, L7-9); then, is it related to the hydrology or to the vegetation parameterizations?

4.5) I have serious doubts on the comparison between CORPSE, Base, and Untiled (Sect. 3.6, p8467): if I understood well, the main result here is that CORPSE simulates a much larger accumulation of soil carbon than CORPSE-Untiled, despite the two simulations having the same WTD. This accumulation is attributed to larger areas with wetland fractions (Fig. 8b). Yet, Fig 8b is not from CORPSE but from Base, and Table 2 shows problematic inconsistencies between Base, Untiled, CORPSE and CORPSE-Untiled. On a global mean, Base and CORPSE have the similar WTD, wetland and inundated fractions, but Untiled and CORPSE-Untiled don't; Untiled has lower WTD, wetland and inundated fractions than Base, which makes sense, but Untiled-CORPSE has the same WTD than CORPSE, yet much lower wetland and inundated fractions (by an order of magnitude); as a result, Untiled-CORPSE has a larger WTD than Untiled, but much lower wetland and inundated fractions. There seems to be a problem here. Does it come from the CORPSE simulations (spin-up ? feedback of soil carbon onto hydrology?) Does it come from the Untiled simulations? All this has to be checked and elucidated before any conclusion can be drawn.

4.6) Sensivity analyses (Sect. 3.7): I suggest not to discuss ConstGeo before this section (L14-20 p 8463 is rather distracting there, especially since no explanation is given to the defects of this simulation. In particular, I didn't understand why ConstGeo lowers WTD so much. The same applies to Untiled with a mean WTD which is 5 m lower than Base (Sect 3.5). Some explanation should be proposed to explain these important differences and help understand LM3-TiHy. I also found problematic that the two experiments showing the effect of hillslope shape (Concave and Converging) did not proceed from the same reference simulation. Finally, what conclusions can be drawn from the sensitivity to the slope input (Base vs HWSD and HWSDSlopeBimodal)? Can you conclude that one slope dataset is better? Can this be separated from interpolation procedures? From the influence of hillslope shape? And how do all these simulations

compare to Untiled (Sect. 3.5) and why?

4.7) Overall, I found the "Results" section too descriptive, and not enough focused onto giving useful insights on the pros/cons of the model, and the difficulties to parameterize it. An example of what could be discussed is the respective contribution of the studied parameters and recharge to control the WTD patterns, the effect of the latter being very overlooked in the paper. The differences between the different simulations, or the upland and lowland tiles, would be more convincing if they were compared to the mean values and to variability metrics, as often realized owing to statistical significance analysis.

Cited references (in addition to the papers' ones)

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