

Interactive comment on “Physically-based modeling of topographic effects on spatial evapotranspiration and soil moisture patterns in complex terrain” by M. Liu et al.

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We thank the referee for taking time to review our manuscript and give valuable comments on our manuscript and suggestions for improvement. Your suggestions will surely contribute to improving our paper and we will follow them as much as possible to revise the paper. In addition, we will send the revised manuscript for professional English correction. In the following, we will discuss each of the points one by one.

<Specific comments> 1. “Page 7060, line 23,24 How accurate can r.sun deliver daily radiation data? How accurate can METRAS PC deliver daily wind velocities?”

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This is a very important point, “garbage in, garbage out”, the quality of the input data has an critical impact on the model output. The authors have an another paper submitted to “International Journal of GIS” which is in press. It is related to solar radiation modeling in complex terrain. In that paper, the adapted r.sun model shows a correlation higher than 0.90 and an RMSE around 40 Wh/m² for daily actual radiation, which is pretty good. The METRAS PC model has been validated by the developer (Schlünzen et al., 2001) in Hamburg University, and has been widely adopted in wind modeling. Massage about the performance of the model will be added in the revision.

2. “Page 7061, line 2; Figure 1 Suddenly here the terms ‘inner and outer domain’ are used. Introduce these terms in the legend of Figure 1.”

The missing introduction of the terms which define the two domains will be added in the revision.

3. “Page 7061, lines 9-11, Figure 2 It seems that the legends for inner and outer domains are mixed up in the figure. Text and figure are contradicting.”

The reviewer is really sharp to find the mixing-up of the two figures. The figures are wrong and will be exchanged.

4. “Page 7063, line 3 Show the Richards equation, including the root water uptake term.”

The authors have thought that Richard’s equation is well-known to almost all readers. However, given that other vertical water flows caused by root extraction, macropore drainage, tile drain, etc., it would be useful, as suggested by the reviewer, to provide the Richards equation for this specific case.

5. Page 7065, lines 6-10 What is the source of the hydraulic parameters of the four soils?

The soil parameters are obtained from State Institute for Environmental Protection Baden-Württemberg. The information should be and will be added to the manuscript.

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6. "Page 7067, lines 18-25 Both the bottom boundary condition and the lateral drainage condition determine in concert the simulated groundwater levels. The current description is not clear. I assume the mentioned linear relation between groundwater level and elevation refers to the initial soil moisture conditions only. How did you calculate lateral drainage, if you assumed a zero flux condition at 3 meter depth? Which drainage resistance and drainage level did you apply? Please clarify!"

"Page 7071, lines 6-11 + lines 26,27 Indeed the assumed drainage condition will have a pronounced effect on the simulated soil water contents. This will also affect the simulated ET fluxes. Therefore, how hard is your conclusion that 'the spatial ET pattern is well related to topography'?"

"Page 7072, lines 15-17 In such a mountainous terrain, the use of a groundwater model might be problematic. A refinement with respect to the current simulation might be achieved by specifying the local soil depth to the bedrock with fast drainage in high elevations and groundwater level – drainage flux relations at low elevations. You think this might be feasible for your experimental area?"

These three comments all concern about the boundary conditions of the numerical simulation. Here, we have to emphasize that the purpose of this manuscript is to investigate the spatial ET/SMC caused by vertical water transportation assuming identical or at least similar boundary conditions. It is of no doubt that the drainage condition will have a pronounced effect on the simulated soil moisture, but it has less effect on ET. In another numerical experiment, we have applied the same groundwater level and fast drainage for each grid, the resulting absolute ET/SMC, but the spatial pattern is similar to the presented case for which a linear initial groundwater table is assumed. As mentioned by the reviewer, simulation of the groundwater system in such a complex terrain is a very complicated and even problematic without detailed hydrogeological data. Therefore simplification of the boundary conditions aiming to separate the water transportation in horizontal and vertical direction is made in this manuscript. As specified in the description of the lateral drainage condition, a local drain at the height 5cm

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below the groundwater table is assumed. The drainage system is a common practice in agriculture for the low-land area. For the high land area, a given soil depth and fast drainage, as suggested by the reviewer, may be applied, which has the same effects. Under such condition, groundwater table will be maintained by fast drainage through the drain system, and later flux of groundwater is negligible under such conditions. We realize that in the manuscript we have not presented the boundary condition very clearly which has led to the question on the later flux from the reviewer. In the revision, we will emphasize this point again.

7. "Page 7069, lines 14-16 I don't understand this sentence" Table 4 shows that the difference between potential and actual transpiration is much smaller than the difference between potential and actual evaporation. The authors believe that this shows that the plants have a stronger water transportation capacity which leads to a actual transpiration as high as the corresponding potential value.

The authors thank specially the reviewer for correcting the detailed language bugs. During the revision, the manuscript will be changed accordingly.

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