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Hydrology and Earth System Sciences Discussions

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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General comments:

"This paper touches on important topics on modeling of surface interactions (1) the influence of topographic features on evaopotranspiration (ET) and (2) the spatial resolution necessary to capture these effects. The authors use spatial fields of wind and radiation to show their respective influence on modelled ET by applying a surface exchange model (Soil Water Atmosphere Plant, SWAP) at two different model resolutions (100m versus 1000m), and they compare with results obtained at two individual points.

They find that wind effects play a substantial role for creating spatial heterogeneity of ET. “

Reply: Thanks for the reviewer for supporting this study focusing on topographic induced spatial ET/SMC difference in vertical direction. Most soil moisture related studies concentrated on spatial patterns caused by the lateral flow. It is of no doubt that lateral flow is the major driving force of the spatial soil moisture pattern, however, what we would like to show in this study is that the spatial difference driven by the vertical water flow is not negligible.

“The spatial distribution of other variables is also investigated, but not in the same detail. The results are very relevant for the readers of HESS.”

Reply: Actually point-wise difference is just an example of the spatial difference. In this paper we want to emphasize the regional or catchment-wide differences in statistical terms. Therefore we conducted 4 types of simulations under different conditions to investigate the spatial patterns. It is by any means more in detail than the point results. However, we agree with the reviewer there is some space to extend the results, such as the seasonal variation of ET/SMC caused by radiation and wind. This aspect will be added in the revision.

[1] The abstract states that “The results show a strong spatial and temporal intracatchment variability in daily/annual total ET and less variability in soil moisture.” However, I did not find a direct quantitative comparison of the same measure of soil moisture and evapotranspiration. A comparison is given in Table 5, but this is for the “maximum daily spatial soil moisture variation over the year” versus some aggregated (not daily) measure of spatial variation for ET. These results need a clearer presentation in the results and discussion section.

Reply: Because the variation in soil moisture is very small for daily average values, therefore in the table the maximum daily difference is used. We agree with the reviewer, in order to rectify the statement, we need to use the same measurement. In the

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revision, the daily average spatial difference for soil moisture will be added in addition to the maximum daily.

[2] The abstract also states that "The spatial variability in ET is associated with a difference in total amount of runoff generated". However, in the results, we only find runoff discussed in a numerical experiment comparing two points (no spatial variation). Additionally, the difference of total runoff between those two points was small (which is also stated in the discussion). This seems a contradiction, and should be rectified.

Reply: As stated in the title and abstract, the aim of this paper is focusing on spatial ET/SMC. The difference in the generated runoff is an interesting massage the authors have found during the investigation of ET/SMC, a detailed discussion of the spatial runoff generated is beyond the goal of this paper, because the lateral moisture flux has to be considered. The difference in the runoff amount in this paper is derived purely based on the ET difference without considering the lateral moisture flow. The magnitude of the runoff difference is related to soil type. If both surface and subsurface runoff are considered, the total runoff for more permeable soil is larger than less permeable soil, but the difference is not so much (around 4mm), whereas is only surface runoff is considered, the less permeable soil generate >10% less surface runoff. The conclusion in the manuscript is not complete, and should be rectified.

[3] Important variables used are the wind fields extracted from a meteorological model (METRAS PC) and the radiation fields from another model (r.sun). It would be really necessary to give more information about these models, since both spatial fields are the basis for the conducted experiments. Also, how representative is the spatial variation of the wind fields compared to the spatial variation of radiation?

Reply: We completely agree with the reviewer that a more detailed introduction of the radiation and wind model, as well as their performance in modeling the respective meteorological parameters. This has also been mentioned by the reviewer #1. Actually the authors have also thought about it during the preparation of the manuscript. How-

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ever, to make the manuscript not so lengthy, we eventually discarded the idea to write a paragraph for the meteorological model. But we will add it in the revision.

[4] The topographic influence on modeled evapotranspiration caused by wind is far greater than the one caused by radiation at both modeled scales. This issue should be explored more fully. I suspect this is due to the topography leading to larger variation in the wind velocities than in radiation, shown also in Figure 2. It would also be important to discuss, how this result changes with the season, since spatial variation in ET in summer would relate to much larger absolute values of ET fluxes than in winter. Also, variation in wind velocities is similar throughout the year, but variation of radiation has a seasonal cycle, with much larger variation in winter. An aggregation of the year (as done in Fig. 6-9, Tables 4-6) probably obscures important effects.

Reply: The variation of wind is stronger than radiation, except in the winter time. This might be one of the reason that the wind-induced ET variation is larger than the solar radiation. However, the effect of both wind and radiation on ET is nonlinear. We highly value the idea suggested by the reviewer that it is definitely worthwhile to investigate the seasonal effect, and present the results for different seasons, and present the results for two different seasons. We will add this investigation in the revision.

[5] Simulations are conducted for one specific year (2002) and there should be mention of the representativeness of that year.

Reply: Thanks for the the reviewer to remind us to present the representativeness of the simulation year. The year 2002 is relatively a normal year with a moderate weather, especially precipitation. We would mention it in the revision.

[6] The SWAP model is explained in great detail, in some parts, but other parts are lacking. For example, soil hydraulic properties are explained in great detail, although they are never varied between the numerical experiments, and not mentioned later. On the other hand, the spatial distribution of the soil types is never shown. The methods used for modelling runoff in SWAP is never explained, although runoff is discussed in

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the results section.

Reply: We agree with the reviewer that there should be a balanced introduction of the SWAP model. SWAP model has many components and it will be trivial to introduce all of them. Therefore, we have taken the parts which are most relevant for ET/SMC. Soil hydraulic properties are presented for the demonstration of point investigation. For the spatial investigation, we did not consider the soil effects, therefore, a soil map is not presented. We don't consider a soil map as completely necessary. However, a more systematic introduction of the SWAP model will be presented in the revision.

[7] The structure of this paper should be improved. It would help orientation, if a methods section would be introduced, which includes sections 2-3. Also, I am not sure of the role of section 4 ? Is this a review of sensitivity analysis done on the Penman-Monteith-Equation? The title and position in the paper suggests that a sensitivity analysis is conducted here, but this is not the case. If this is a review indeed, it should move to the introduction, and needs to be more related to the content of the paper. For example, the cited conclusion by Bois et al (2008) on the seasonal cycle is relevant and important previous work.

Reply: The sensitivity analysis by Bois et al. (2008) is meant to be a review in the manuscript. People may argue that, part of this work can be considered as a more specific sensitivity analysis of radiation and wind. Actually, we intended to carry out a more in-depth investigation of the topography related effects, not just a sensitivity analysis. Therefore, we will follow the suggestion of the reviewer to restructure the manuscript and put this part into the introduction.

[8] It would help, if the table and figure legends were more informative. In present form, they are only intelligible with close reference to the text. For example, just looking at Table 5, it is unclear over what period the spatial variation was evaluated.

Reply: We apologize for the incompleteness of the table and figure legends. These will be improved in the revision.

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Detailed comments “Page 7057, Line 15-16: I do not agree that actual evapotranspiration is the one achieved under water stress, but it is the (often unknown) real evapotranspiration that occurs at a specific site.”

The reviewer is in some sense right, because the phenomenon that the actual ET is less than potential ET is caused by some kind of stress, not necessarily, even though in most cases it is, the water stress.

“Page 7060, Line 23-24: Both the r.sun and METRAS PC model and their application should be explained better. For the latter, a reference should also be given. Could you present some data indicating that your generated values are reasonable?”

Yes, we will. One paper about the simulation of solar radiation in complex terrain from the authors is in press for International Journal of GIS. For this manuscript, a short introduction of the model and their performance will be provided in the revision.

“Page 7061, Line 2: Please introduce the ratio (P90-P10)/ s (maybe by rephrasing the next sentence). Also explain how you integrate these daily values to the values for the entire experiment shown in Tables 5 and 6 ?”

We applied the interquartile range to measure the spatial difference, because it is more general in the case that the spatial data is normally distributed. The data in Table 5 and 6 are mostly daily mean, except for the soil moisture. We will follow your suggestion to make a detailed description in the revision to avoid any misunderstanding and unclearness.

“Pages 7061, Lines 17-19: I do not understand why you did not aggregate from 100m once you had the data produced? Even if the 500m aggregation was not much worse than the 100m one, it seems an unncessary intermediate step. Could you place a note on this?”

For this test region, we tried both aggregation from 100m and 500m, and found that the improvement from 100m to 500m is really minor. Given that the calculation for 100m

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resolution is 25 times more than for 500m, we suggest for practical use the aggregation from 500m data, and for the demonstration we have applied the aggregation of 500m radiation.

Page 7062, Line 2: “In case humidity data is not available, it can be” You probably mean that you used an alternative method (Thornton) for obtaining humidity data during times when no measurements from the tower were available? If this is the case write: “During times when no humidity data were available from the tower, it was ..” Also, it would be good to state, how often were data taken from the tower, and how often were they calculated from temperature? Did you check, whether the humidity data obtained with Thornton method compare well to measured relative humidity?

This is a just a technical issue. For our study, the observed humidity data from German Weather Service is available. The alternative is given only for the readers who want to apply the same methodology under data limitation. We have not conduct a validation of the Thornton method. As a generally applied method, we also do not think it is necessary to check it in this manuscript.

Page 7072, Lines 2-9: Here it is stated that different soil conditions have an influence on certain fluxes (i.e. Evaporation/Transpiration partitioning), but this is based on investigating only two points (P1 and P2), if I am not mistaken. Also other factors but soil conditions changed between P1 and P2, as Table 4 states. So I do not understand where this conclusion comes from, and this should be stated more clearly.

Yes, this statement is made based on the point investigation, which is a special case of spatial investigation. For the point investigation, we compared the case with actual soil (less permeable) and the case with the test soil (more permeable soil) without changing any other parameters. Therefore, the effect of soil properties is investigated with point data, not spatial grids. We will specify this clearly during the revision.

Comments on language We appreciate the reviewer’s efforts so much for really taking time and going into details including the language of the text. We will follow each of the

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comments to improve the manuscript.

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