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Interactive comment on "Validating a spatially distributed hydrological model with soil morphology data" *by* T. Doppler et al.

Anonymous Referee #2

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The manuscript "Validating a spatially distributed hydrological model with soil morphology data" by T. Doppler et al. presents an hydrological modelling exercise using a distributed hydrological model applied to a small agricultural basin, with the objective of predicting the areas contributing to saturated overland flow, as critical source areas for diffuse pollution of waters. The modelling results shown by the authors are good for flow simulation, average for water table reproduction and rather poor for predicting the patterns of saturated areas in the basin; the authors argue that a more complex model would be needed for better results, although there are data and computational difficulties for implementing such an improved model.

In my opinion, the causes for the relative failure of the modelling exercise should be attributed to an insufficient calibration approach, as the authors show that topography is a better predictor of the patterns of frequently saturated areas than the model results. Although the manuscript may deserve publication for several reasons, a substantial part of the model implementation should be revised before publishing in HESS a paper which claims the need for a more complex model whereas another calibration of the same model or the implementation of a simpler model could provide better results.

General comments:

The discussed manuscript affords a new contribution to the approaches made in several published papers stressing the interest and difficulties of using hydrological models for simulating maps of saturated areas. The use of the information from soil maps is not completely new, as it was already attempted by Merot et al (1995), but I do not know any former trial analysing the role of subsurface drains on soil saturation patterns, which is frequently eluded (e.g. Beven and Kirkby, 1979).

The difficulties found by the authors for the use of piezometer data for constraining basin models, attributed to the role of local controls, is a new example of the difficulties found elsewhere by other authors (e.g. Anderton et al., 2002; Blazkova et al, 2002).

On the other hand, the authors show that there are acceptable similarities between the observed water table depths and the map of the degree of saturation of the soils, as well as between the last and the "topographic wetness index" map. But the more remarkable point is that the tested model provided worse predictions of saturation patterns than this much simpler topographic information.

A key for understanding this unexpected result may be found in the model calibration process. If well understood, the calibration of the model was made searching a unique parameter set that maximized the likelihood of the groundwater level simulations at 11 piezometers, along with the simulation of a transformed function of water discharge at the outlet. The results of this exercise were good after the graph on Fig. 5 (observed and simulated discharges). But this choice of calibration approach hides a real multi-objective calibration; the poor simulations of the dynamics of water tables shown in Fig.

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7 suggest that the model converged to an acceptable simulation for a few piezometers (3, 4 and 8) as well as discharge, but had difficulties to simulate the remaining piezometers because of the disparate values. As suggested by the authors, the model seems to optimise discharge by simulating an excessive role of tile drains, but this is not necessarily because of the model structure, but may be attributed to the results of the calibration procedure used. One may suspect that the degrees of freedom of the model mean that other parameter sets might provide flow simulations of similar quality with diverse simulations of water table depths and tile drains contribution.

Irrespectively of the successfulness of the simulations, it is difficult to accept the publication of a hydrological modelling exercise that does not take into account the present awareness of multi-objective calibration (e.g. Efstratiadis and Koutsoyiannis, 2010) nor the uncertainties associated with model simulations (e.g. Beven, 2006). The authors may choose their way to offer a more convincing manuscript. If a more explicit multi-objective calibration taking into account the uncertainty associated with the simulations is not possible, the comparison with an alternative calibration of the model with other optimisation criteria or cancelling out the role of tile drains might provide more insight on the problems addressed. Alternatively, a test with a simpler model based on topography (e.g. TOPMODEL) might provide comparative elements for a wider discussion on the needs (or not) for more complex models.

Specific comments

The list and values of used parameters should be preferably shown in the paper instead of in the supplementary material.

Page 11, line 15: A more formal definition and citation of the “topographic wetness index” is needed. Is this the TOPMODEL topographic index ln(a/tanïĄć) index (Beven and Kirkby, 1979)?

Page 23, line 20: It should be noted that Fig. 6 provides a false impression of good results, because the large differences among elevations hide the errors in water table depths.

Page 29, line23: The topographic wetness index map may be used for modelling the spatial dynamics of the saturated area. TOPMODEL performs a lumped simulation of the average depth to the water table and the map is then used to distribute it, so the extent of the saturated area at every time step can be mapped.

Fig. 7: Some quality cross-check of records should be made in order to avoid errors (piezometer 2 shows an odd decrease by the end of the calibration period) as well as too local responses, before being used for model calibration.

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