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Hydrology and Earth System Sciences Discussions

Interactive comment on "Catchment-scale non-linear groundwater-surface water interactions in densely drained lowland catchments" *by* Y. van der Velde et al.

Anonymous Referee #1

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This manuscript presents an interesting approach to model the hydrology of lowland catchments using water storage as the central variable. Spatial variations in storage are represented by assuming u to be normally distributed. This is an interesting approach which allows considering spatial variations in a simple way. The authors formulate then a water balance model which explicitly takes the coupling of unsaturated and saturated storage into account. This is an important improvement over many models where this coupling is neglected. To large degree one could say that this study follows the ideas out lined by Regianni et al. (1998). Compared to Regianni this study, however, is much more concrete when it comes to specifying the expressions used for the different fluxes. This makes the paper valuable, but on the other hand also easier to criticize. My major concern is that treating the unsaturated storage as random variable without any specified spatial pattern is a crude simplification which might limit the usefulness of the presented approach considerably.

Major comments:

In order to simplify the spatial descriptions it is assumed that the unsaturated storage can be described as a random variable following a normal distribution with the std dev varying with the mean in a specified way. To really be able to simplify things it has to be assumed that u (or the rate of change of u) is not correlated to other 'random' variables such as the porosity (p 3766, line 14) or the drainage depth (p. 3771, line 7). The assumptions of being uncorrelated might be hard to justify. In the first case one would expect u to change faster if the porosity is higher. In the second case one has to remember that drainage tubes are installed for a reason, i.e. they are installed depending on local wetness conditions. For yet other variables it is assumed that they are uniform in space, which also simplifies things (soil parameters, p. 3767, line 20ff). However, I am not really convinced by the argument that considering soil parameter variations is not meaningful *because* of the assumption of hydrostatic equilibrium. To me, these are two independent assumptions and it would be useful to consider soil heterogeneity even with the hydrostatic equilibrium.

A question which comes up is also why there is a spatial variation of u if we assume that u is not correlated to the variables which otherwise could explain a spatial variation. My point is that there might be an inconsistency.

Another problem with the treatment of u as random variable is that any spatial correlation is ignored. This means that connectivity issues also are not considered. A location produces, for instance, surface runoff no matter whether it is located close to the stream with only other wet locations along the pathway from the location to the stream or whether it is located away from the stream separated by dryer locations.

HESSD

6, C1340–C1343, 2009

Interactive Comment

In other words, the hydrological descriptions becomes rather one-dimensional. This should be stated more clearly.

Lateral flow within the catchment is basically ignored as I understand the equations. Is this realistic? Consequences? This is an issue which should be clarified (i.e., state the assumptions and discuss the consequences)

The authors use another model to validate their model. This of course is always a dangerous approach and I am not convinced that this particular part is an important contribution. I am also a bit surprised that different relations between u and evaporation are used for modflow compared to the rest of the study (. 3778, line 9) (see also next point)

Distribution functions make things smoother and more robust at the catchment scale as correctly stated by the authors. However, I would think that we still should start with realistic descriptions at the local scale. This is not always the case in the paper. For evaporation it seems to be assumed that evaporation is switched on and off with no transition at the local scale. First at the catchment scale there is a more gradual transition due to the distribution of u. The local water balance, however, seems to be unrealistic, since there is no evaporation at all in some areas.

I can see the argument for a decreasing std of u for wetter conditions, but looking on fig 11 and 13 this effect might be minor. For observed data (Fig 13) no tendency of this behavior at all can be seen (simply because the catchment does not get wet enough), and even for the modeled case the tendency is small. Would it be possible to simulate a 'wetter' catchment?

Other models also work with distribution functions (e.g. TOPMODEL, soil routine in HBV). Could you discuss the differences more?

Minor comments:

The use of u, i.e. the unsaturated storage, is sometimes confusing when the vari-

HESSD

6, C1340–C1343, 2009

able which is actually meant is the saturated storage, which is increasing when u is decreasing. Please try to clarify this.

p. 3755, lines 3ff: I agree that TOPMODEL has been developed for sloping catchments, but for the other mentioned models I don't see why there should be a limitation based on topography

p. 3776 and p 3781: I am a bit confused, first there is only one well mentioned and later there are 31 wells, please describe all field installations in 3.1.3

p. 3778, line 24: a subgrid variability of 5 cm sounds small to me, the value might be appropriate, but could it be motivated by some data?

p. 3780, line 11. How were these weighing factors determined?

p. 3780 ff: I find it always confusing to have results and discussion mixed and would recommend to distinguish between these two parts.

p. 3782, line 11: Please specify the calibration criteria and procedure. From which group of parameter sets was the optimal set selected? How where the criterion 'close to ranges for Dutch sandy soils' evaluated? I also wonder whether the contribution of drains was used for calibration and/or (independent) validation (p. 3778, line 20, and p.3784, line 4)

p. 3784, line 21: I find it difficult to see any hysteresis in fig 15&17, could this be displayed in any other way?

References: McDonnell with two n, Seibert et al should be 2003

Table 1: Could you add a short description to each parameter (or have this as separate table), given the many parameters this might be helpful.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 6, 3753, 2009.