Hydrol. Earth Syst. Sci. Discuss., 10, C6199–C6202, 2013
www.hydrol-earth-syst-sci-discuss.net/10/C6199/2013/

Hydrology and Earth System Sciences
Discussions

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

Anonymous Referee #1

Received and published: 10 November 2013

The manuscript HESS 2013-384 "Validating a spatially distributed hydrological model with soil morphology data" by T. Doppler et al. presents a spatially-distributed hydrological model developed to simulate discharge and groundwater levels with subsequent detection of saturation areas and the potential generation of saturation excess overland flow. The model is calibrated using quantitative (saturation frequency) soil information additionally to discharge data, which is a novel contribution to the literature and of interest to the hydrological community. However, I think that the paper could benefit from more focus on the model development and testing and more discussion (or additional analysis) of model structural adequacy and uncertainties which I will detail below. Nonetheless, I recommend this paper for publication in HESS subject to the clarification of my comments.

General comments: - The paper to me (and I invite the authors to consider this) should be all about the model development and testing and the usefulness of additional soil data (with the procedure described to derive soil saturation frequencies) for model calibration with the potential to be applied for practical delineation of CSAs in the context of herbicide transport. However, already in the introduction quite a lot of material is presented about the practical issues of CSAs. I think it is important that the model was developed for this purpose, but it is an application that follows from what is presented in the paper. Therefore, it would be possible to shorten the introduction (section from p. 12907 to 12908) quite a bit which also results in more focus on the main objectives of the paper. I also recommend to clearly stating two or three main objectives.

- Reading the detailed model description I was immediately struck by the separate and decoupled (or unidirectional) treatment of the unsaturated and saturated zone. Despite that one goal for model development is stated as the representation of groundwater levels and the dynamic behaviour of the unsaturated and saturated zone in soils (p. 12911, l. 28), the decoupling is only mentioned as a limitation much later in the discussion (p. 12932, l. 6 citing work by Seibert et al., 2003 who used a conceptual model to achieve coupling). My concern here is that this in fact is one of the reasons the model failed to predict the groundwater levels with a higher degree of precision. From this model failure it is then concluded that a more complex approach would be needed to improve simulations (abstract, l. 28). I don't agree with this conclusion as a slightly different model structure with coupled saturated and unsaturated zones might already do a much better job at simulating groundwater levels and the frequency of soil saturation. Did you try this or is this planned for future work? Furthermore, from the result section it becomes evident that the drainage system is over-emphasized (p. 12929, l. 4) in the model structure resulting in fluxes reaching the drains too quickly. You rightly identify this issue, but was it somehow addressed in the model building process or do you plan to do this in the future? I therefore think that it is premature to conclude about the need for a more complex model if different perfectly plausible concepts were not exhaustively tested. Recognising that this is an interesting and difficult scientific prob-

HESSD

10, C6199–C6202, 2013

lem specifically considering the low-relief and tile-drained catchment, I would urge the authors to more clearly state the potential impact of a different model structure with reference to e.g. Gupta et al. (2012) and/or incorporate the testing of competing model concepts of similar complexity.

Specific comments: Abstract: I think the abstract could be substantially shortened. As it stands it contains a lot of methodological information which is not really needed at this point.

Abstract, Line 28: I don't agree with the conclusion that you necessarily need to increase model complexity to adequately simulate groundwater levels if other competing model structures were not tested (see general comments).

Page 12908, Line 28: In the context of hydrologically active you could refer to work by Ambroise (2005).

Page 12910: In the context of using HOST as additional information for model calibration and evaluation I would suggest including work by e.g. Dunn and Lilly (2001). See reference below.

Introduction: I would ask the authors to consider formulating clear study objectives.

Introduction and Discussion: Consider including physics-based Integrated Surface-Subsurface Hydrological Models as the more complex counterpart to the model presented (see e.g. recent work by Partington et al., 2013).

Page 12915, Line 15: Please, spell out Dinf and/or explain what type of algorithm this is.

Page 12925, Line 10: I think the information on calibrated parameters, initial ranges and best-fit parameters is best presented in the paper rather than as supplementary material.

Page 12926, Line 4: Delete one "of".

HESSD

10, C6199–C6202, 2013

Page 12927, Line 21: Please, consider including a more quantitative assessment of fitted groundwater levels.

Page 12927, Line 24-26: Please, give the reason why it's not shown or just don't mention it.

Suggested references: Ambroise B. 2004. Variable 'active' versus 'contributing' areas or periods: a necessary distinction. Hydrological Processes 18: 1149–1155, DOI: 10.1002/hyp.5536

Dunn SM, Lilly A. 2001. Investigating the relationship between a soils classification and the spatial parameters of a conceptual catchment scale hydrological model. Journal of Hydrology 252: 157–173.

Gupta, H. V., M. P. Clark, J. A. Vrugt, G. Abramowitz, and M. Ye (2012), Towards a comprehensive assessment of model structural adequacy, Water Resour. Res., 48, W08301, doi: 10.1029/2011WR011044.

Partington, D., P. Brunner, S. Frei, C. T. Simmons, A. D. Werner, R. Therrien, H. R. Maier, G. C. Dandy, and J. H. Fleckenstein (2013), Interpreting streamflow generation mechanisms from integrated surface-subsurface flow models of a riparian wetland and catchment, Water Resour. Res., 49, 5501–5519, doi:10.1002/wrcr.20405.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 10, 12905, 2013.