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Hydrology and Earth System Sciences
Discussions

Interactive comment on "Predicting land use and soil controls on erosion and sediment redistribution in agricultural loess areas: model development and cross scale verification" *by* U. Scherer and E. Zehe

Anonymous Referee #3

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This paper focuses on the topic of spatially distributed and physically based modelling of runoff and erosion on multiple scales. This is an interesting and important topic, especially since many models exist which all have their advantages and disadvantages. This paper promises to unfold a new model that is simple, but physically based, with measurable parameters and also applicable over multiple scales. This raises high expectation which are, unfortunately, not fully met in my opinion.

Most importantly, it is not completely clear what this particular new model adds to the

already existing 'zoo of models'; is it not another 'animal in that zoo' to speak with the terms the authors use? Although the model seems to do well, a more in depth discussion on this issue is needed, clearly pointing out the advantages of this model as compared to existing ones. Related to this is the question: did none of the existing model simulate the hydrological and sediment redistribution processes satisfactorily for the catchment? Secondly, the model is referred to as 'simple' or, at least, as simple as possible. It remains subjective what simple is, but in my opinion, the model is far from simple, including many processes and equations. See for an interesting discussion on simplicity of models e.g. Paola (2011). Related to this is the number of parameters this model has. The aim of the authors to include only really 'measurable' parameters (as opposed to lumped ones) is indeed something to aim for. However, this model needs very many parameters. In the catchment that is being simulated, a very detailed, extensive and long measurement campaign has been carried out, which provides this data. However, for most catchments and studies, such amount of data is not available and it is often too expensive to obtain. So, how feasible or useful is this model for general use in other, more data-poor, regions? Thirdly, in the calibration and validation of the model, the biggest (three) storms are used. I agree that usually most erosion is caused by large storms, but still also quite some sediment redistribution might occur in smaller storms that are much more frequent. Why were smaller storms not tested? A final major issue of the paper is its length; in my opinion the paper is too long and contains too many topics: a new model, its calibration/validation, sensitivity analysis, data collection and requirements and scenarios and effects of land use change. I think the latter is too much and should be skipped from the paper; or, alternatively, two consecutive papers can be prepared covering all these subjects. However, I also would like to compliment the authors; for such a long paper with so many subjects, it was still good to follow most of the time.

Some minor issues and questions are: - Abstract: last sentence difficult to follow '..to mitigate...loess landscapes' (p.3529), please rewrite. - 'emissions' (p. 3529 L7): rather say 'inflow' or comparable. - 'burrows' instead of 'borrows' (p. 3529 L21; happens more

often, please check) - ‘landscape’ (p.3530 L19) - if this complies with the journal’s regulations, please give references in chronological order; this is not always done. - (section 2.2.1) The model uses ‘model elements’ to discretize hillslopes and the catchment; so it is not grid or cell based. What’s the advantage of this? Is this also applicable to more complex catchments, as each hillslope section is assumed to act similarly? Many cell-based models are sensitive to resolution; how is this for this model? - ‘up to’ instead of ‘to a massive’ (p. 3545 L7) - ‘thick’ instead of ‘huge’ (p.3545, L18) - consider showing the land use types in cake-diagrams instead of in a table (Table 2) - p 3556 L27: ‘This value is slightly larger’: which value? The values mentioned are actually the same?

Ref: Paola C. 2011. In modelling, simplicity isn’t simple. Nature 469: 38.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 12, 3527, 2015.