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Hydrology and Earth System Sciences Discussions
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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 #2

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This paper addresses the topic of distributed and physically based soil erosion modelling with a multi-scale approach that ranges from the plot scale to the small catchment scale (~ 3 km^2). The study focuses more specially on the issue of the parameterization of soil erosion defending the concept of the right balance of process parameterisation to avoid over-simplification and model simplicity. My opinion is that the topic is very timely and the approach is very interesting. However, the conclusions reached by the paper are somewhat disappointing compared to the expectations that one can have on this subject. The main result is that the model reproduced well the observed data but

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the validation process seems incomplete. I noted several points that need improvement. These main points are as follows: 1) The scope of the paper is too wide and the paper is a little too long, posing the risk of losing the reader. Many things are considered in the paper: the authors presents a new erosion model, they present a set of unpublished data, they make the calibration of the model, they try to evaluate the model and to make a sensitivity analysis and finally they use the model as a tool for prediction to test different land-use scenarios. It seems too much for a research paper and it affects the fluidity of the article and the coherence of the speech. This comment also applies to the title since the authors emphasize first the predicting capacity of the model related to land use changes and then they focus on the development and validation of the model across scales. I believe removing the part which deals with the prediction of different land use scenarios with the model would be relevant, especially since there is no data to verify these simulations. 2) I think it is more appropriate to limit the paper to the issue of the parameterization of soil erosion in the model based on the calibration-validation at various scales and to the issue of simplification in the selection of processes and their formulation. However, I feel that the part dedicated to the calibration of the model parameters is not completely treated. Some parameters such as the erosion parameters P1 and P2 have not been taken into account in the analysis and the justification of this choice is absent. On the other hand, one wonders why the authors have not used a framework such as the GLUE methodology to calibrate the parameters of infiltration and erosion processes. Nothing is said about the spatial distribution of rain in the methodology. 3) The data used for this work are multi-scale and involve both experiments and observations, it is a very positive point. It is also interesting to promote the use of data collected 20 years ago to calibrate and evaluate the model. The multi-scale data involve rainfall experiments at the plot scale, observations of overland flow and erosion at the hillslope scale and observations of overland flow and erosion at the small catchment scale. Nevertheless the extent of the data and the diversity of the selected events are open to criticism. For example, the data used at the hillslope scale are cumulative exports of eroded particles at the event scale and are limited to a single event. This seems insufficient to confidently assess a distributed model of this type. 4) Finally, I do not understand why the authors consider the multi-class approach in the description of the model and do not say anything about it in the rest of the paper, nor in the presentation of the experimental dataset or in the calibration and evaluation of the model. It seems contradictory with the idea to try to simplify the model. Why do they take into account several soil classes without mentioning specific parameters to each class and without evaluating the model results with field data? Is there an interest to preserve the multi-class approach in this study? 5) A list of symbols is missing in the paper to help the reader navigate.

The following comments regard more specific points: - p.3531, L.6-8 : this sentence is not clear. Please re-formulate - section 2.1.1 The authors refer to detachment of soil particles and aggregates but they forget to specify that it is detachment by flow. For instance, p.3533, L.14 or p.3535, L.10. - Eq(4) p.3536 : I do not understand why the detachment by rainfall impact and the detachment by flow are dealt in a bi-linear way. This expression means, for example, that if the energy of rainfall is sufficient to overcome the resisting forces of the soil, the soil can therefore be eroded by overland flow even if the shear stress of the flow is very low and inferior to the resistance of the soil. It is surprising as detachment by flow is usually conceptualised as a threshold process in cohesive soils. - Eq(5) p. 3539 : it is not clear how Tc is calculated - p. 3539, L. 12-15: what is the relevancy of presenting a multi-class approach if it is not discussed in the rest of the paper? - p. 3541: the discussion about the Cdep coefficient is not clear. What is this coefficient for? - section 2.2.2.: Eq(15): what is the purpose of this integration? What do you mean by discretization element exactly? It is surprising that you do not talk about multi-class modelling in this section. - p. 3547, L.7: burrows instead of borrows - p. 3547, L. 22-23: the date and the rainfall amount do not match with the data in Table 3 - section 3.3 : that would be much easier if you would be more precise about which data are used for calibration and which data are used for validation. You could make a Table that explains the distinction between calibration and validation and the method used in each subsection of sections 3.3 and 4. - p. 3550, L.

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7-10: these are hypothesis but they are not proved - p. 3551, L. 2-4: the discretization is not clear to me, in the 3 dimensions. - p. 3551, L. 10: why not to calibrate the Manning coefficient as well - section 3.3.3: there is no hydrograph and sedigraph at the plot scale to evaluate the model? - section 3.3.5: why do you perform a sensitivity analysis at the end? That should be done at the beginning and taking into account more parameters. Furthermore, the methodology used to carry out this sensitivity analysis is not clear. - p. 3554, L. 1-18: I would remove this paragraph. - p. 3555, L. 11-13: hopefully the agreement is good as it is the calibration - p. 3555, L. 17-29: I do not understand the meaning of these regressions? Why do you perform this statistical analysis? Is that for the regionalisation of the parameters used in section 4.1.3? This is not clear to me. - section 4.1.2: only one event with cumulative data at the event scale. It seems not sufficient to evaluate such a distributed model. - section 4.2.2: I would remove this section - section 5: Be careful with the terminology. It is important not to mix calibration and validation. Moreover, it is preferable to say “CATFLOW-SED reproduced the sediment loads” instead of “CATFLOW-SED predicted the sediment loads” - p. 3561, L.22: burrows instead of borrows - Figure 1 : term "precipitation (qlat)" shouldn’t be in the middle of the figure? - Figure 2: that would be fine to add the exact coordinates of the site - Figure 2 and 4 : could you add a soil map or a map of soil parameters (soil resistance for example) to one of these figures?

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