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Interactive comment on "Modelling soil and landscape evolution – the effect of rainfall and land use change on soil and landscape patterns" *by* W. Marijn van der Meij et al.

Anonymous Referee #1

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Review of "Modelling soil and landscape evolution- the effect of rainfall and land use change on soil and landscape patterns" authored by W. Marijn van der Meij, Arnaud J. A. M. Temme, Jakob Wallinga, Michael Sommer.

I found this a very interesting article that could prove to have a major impact. As far as I know it is the first time that water flow as driving pedogenetic process was added to a landscape evolution model, resulting in a (one of very few) non-empirical (but functional) soilscape model that could be used for global change studies. The article is well-written and a pleasure to read. My comments below focus on some points that would benefit from clarification or that could be named as assumptions behind some

choices in the modelling approach.

Remarks: 1. The authors have chosen to work with a hypothetical hilly landscape covered by loess rather than an existing landscape. This choice makes a full confrontation of model results to measurements impossible. This choice is understandable, as the history of real landscapes (and their agricultural history) is not easy to reconstruct and thus any inaccuracies could be resulting from the model, reconstructed boundary conditions, etc. Thus, a synthetic study is likely easier to interpret. The authors could pay some attention to this in their discussion: synthetic studies may avoid ambiguous interpretations of simulated versus real landscapes. At the same time, and as partial support, I would refer to http://dx.doi.org/10.1016/j.scitotenv.2016.07.119, where it was concluded that inaccuracies of boundary conditions over the simulation time did not significantly influence model results of the profile model SoilGen, while inaccuracies of initial conditions (like initial texture) did have significant influence.

2. The usage of bulk density estimated by a PTF like that by Tranter, to translate (simulated) mass per compartment to the volume (thickness) of that compartment makes good sense but is sensitive to the quality and the independent variables of that PTF. Tranter gives R2 of 0.49 of the best model, thus there is still considerable uncertainty. Furthermore, this PTF takes only texture and OM (as a proxy for soil structure) as inputs. In this sense, bulk density change (hence volume change) cannot be caused in the model by processes like decalcification and bioturbation. The authors smartly avoid the decalcification issue by assuming non-calcareous loess, which however limits the application domain. Bioturbation and tree throw are considered in the model but apparently do not directly affect bulk density. Perhaps these limitations could be mentioned in the discussion.

3. Unless I missed it, it seems to me that any climate change (in terms of precipitation) during the simulation period is absent (there are 3 scenarios, but these appear to be constant). It is well known that the precipitation surplus (as well as temperature) varied considerably, especially in the late glacial period but also afterwards. Can the authors

comment on possible effects that considering climate change might have had on co-evolution on soils and landscapes, additional to what has been stated already? I can imagine that cold and dry periods like Younger Dryas might have affected erosion for the reason that vegetation was less well developed or even absent. Is the reason not to include climate change related to the computational consequences of varying water flow dynamics?

4. I do not particularly like the 1:1 coupling of vegetation to infiltration regime; a forested site will not change into a grassland site on December 31st. There may be some more resilience there. This is also recognized by the authors, but I do not understand how they dealt with it. Lines 180-183 appear to suggest that outputs were time-aggregated, but inputs of vegetation type were not. Perhaps some clarification is useful. Btw, annual variation in infiltration is caused by the sum of precipitation and (re-)infiltration. Given the above remark, am I correct in concluding that the variation in re-infiltration is non-zero, whale the variation in P is zero? This would strengthen a terrain control on vegetation type, while there could also be a climate control. Additionally, for tree throws to result in a serious pit/mound topography, trees must have been present for a number of years and counting the "tree years" is not the strongest point in the model setting.

5. How thick is the loess, and what's below it? Line 195 states that shallow rooting depths do not occur (even after erosion), so the bottom of the loess is never reached? The effect of armouring (e.g. by coarse material originating from below the loess) on erosion is included in the model, so there seems to be no limitation there.

6. Can well-expressed Bt-horizons (such as present in Meerdaal as well) affect the rooting depth in the model?

7. line 433: SOM stocks in natural areas are estimated higher than often observed: Could this be because ectorganic material (O-horizon) is not simulated and thus this SOM is added to the mineral horizons?

8. line 574-577: I am not sure about the conclusion that in agricultural systems co-occurrence of non-interacting processes rather than co-evolution occurs. Reason: 14500 years of natural history are compared to 500 years of agricultural history. Is this a fair comparison? If you would compare the first 500 years of natural history to the 500 agriculture years, what would you conclude then?

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