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.

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Dear reviewer,

Thank you for your time and effort to review our paper. We appreciate your kind words and constructive remarks. Here we respond to your remarks one by one, with your comments in italic.

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.

Response: The choice to simulate a synthetic landscape was indeed made to avoid a full confrontation of model results and field observations, because this comparison can be distorted by uncertainty in local climate history and land use history and potential other factors that might have played a role in the development of the current soil-landscape. Also, to compare the effect of different rainfall regimes on

soil-landscape evolution, as we did in the paper, other drivers of soil formation should preferably remain constant or vary only as consequence of the varying rainfall. We will expand our discussion on pros and cons of the simulation of synthetic landscapes in soil-landscape evolution in the revised version of the paper. The paper you suggested will be a useful support in this discussion.

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.

Response: The pedotransfer function (PTF) of Tranter et al. (2007) that we used required soil texture, SOM and depth below the surface as inputs. Depth below the surface acts as proxy for the effects of soil structure formation, weathering, bioturbation and other soil reworking. Tree throw and bioturbation do have an effect on the bulk density in the model through their effects on soil and layer thicknesses.

We agree that the uncertainty of this PTF is relatively high. However, PTFs that yield a higher accuracy often require soil hydrological or soil structural information,

which is not readily available in Lorica and HydroLorica. As we discuss in Van der Meij et al. (2018), the estimation of these parameters often gives biased or highly uncertain results, which would propagate into the calculation of bulk density. Rather than stacking pedotransfer functions, we decided to use a PTF that required input that is readily available in HydroLorica.

We will motivate the choice of our PTF in the Methods, describe the bulk density PTF in the manuscript and discuss the consequences of the chosen PTF on the model results in the Discussion to provide more information on this part of the model.

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 coevolution 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?

Response: We indeed did not include the effects of a changing climate in our simulations. Next to a synthetic, simplified landscape, we also used a simplified climate scenario. The calculation demands would be a bit higher when more overland flow would occur, but this is not the reason we did not include climate dynamics. We agree that changes in climate have played a substantial role in the development of soils and

landscapes, especially more extreme climatic periods such as the Younger Dryas. However, the introduction of the model HydroLorica and simulation of three simplified rainfall scenarios already resulted in a complex and lengthy paper. Therefore, we think the effects of a changing climate on soil-landscape evolution are out of the scope of this paper, but this can be an interesting topic for a subsequent paper.

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.

Response: Vegetation type is indeed controlled by two factors: climate (precipitation) and terrain (re-infiltration). With these two factors we can for example simulate vegetation differentiation on north- and south-facing slopes and the occurrence of deciduous vegetation in locations where water flows converges, such as valleys and depressions (e.g. Metzen et al., 2019).

As we discuss in the paper and you indicate in your remark, we consider the

long-term effects of vegetation change on soil-landscape formation rather than the year-to-year variations (lines 180-183 in the original manuscript). This is similar to the simulation of clay translocations, where we consider the long-term changes in the soil profile rather than the differences between two consecutive years. We will clarify how we simulated and interpreted vegetation dynamics in the revised version of the manuscript.

The dimensions of the pit created by tree throw are a function of tree age (line 195-198 in the original manuscript, Eq. S6 in Supplement 1). This will lead to small root clumps for young trees, which will only cause a partial turbation of the upper layers in one raster cell in the simulations. Only when the dimensions of the root clump exceed the size of a raster cell (radius of 1.5 m in our case), a pit-and-mount topography is created. We hope that this explanation resolves your concerns on the creation of pit-and-mound topography in our simulations. We will clarify this point in the revised manuscript.

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.

Response: In our simulations we assumed an infinite layer of loess to avoid potential effects of lower layers. However, for computational reasons we worked with an initial loess layer of three meters with free leaching of water and potential clay at the bottom of the soil columns. This approach reduced the amount of soil layers, avoided numerical instability from the pedotransfer function for bulk density which is

depth-dependent and was still thick enough that the bottom of the loess was never reached by erosion. We forgot to mention loess thickness and the free leaching in the original version of the manuscript and we will include this in the revised version.

The shallow rooting depths we refer to in line 195 can be a cause of tree throw, for example when the roots are blocked by rocks or impermeable soil layers. We did not include such limitations in our simulations, but this would likely not have occurred since we limited the thickness of the root clump for tree throw to a maximum of 70 cm. For the calculation of bioturbation and SOM cycling, we varied the potential rates to account for rooting differences between vegetations. The effect of armouring is indeed included in HydroLorica, but did not play a role in our simulations, because there was no coarse fraction present that would lead to armouring.

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

Response: As we stated at the previous remark, we did not include such limitations in our simulations. Depending on the settings, Bt horizons can limit root growth, but also facilitate root growth by structure formation and increased nutrient availability. The occurrence, type and grade of soil structuring is very difficult to estimate and therefore we did not consider this effect in this paper.

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?

Response: We indeed did not include the formation of O-horizons in our simulations. However, we compared the stocks from our simulations with soils including O-horizons from the paper of Wiesmeier et al. (2012), and still our estimations were higher. As we argue in the paper, the agriculturally-derived SOM depth profiles were not representative for forested sites, because other factors and processes affected uptake and decay of soil organic matter under forested conditions (lines 436-439). We are currently not able to simulate and calibrate these processes properly. We will mention this in the revised manuscript.

8. line 574-577: I am not sure about the conclusion that in agricultural systems cooccurrence 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?

Response: We derived this conclusion from our findings that under natural conditions the formation of soils, terrain, the hydrological system and vegetation are intertwined. Changes in one domain in the landscape have effects on the formation of all other domains. These interactions, or co-evolution, occur on both short and long timescales, but become more pronounced over longer timescales, due to progressive soil and landscape formation. This is visible in the animation in Supplement 2, where there are already considerable differences between the soil patterns from each scenario after 500 years of natural soil formation, due to the role of water and vegetation in soil-landscape co-evolution. These differences become more pronounced over time. In comparison, the differences between the patterns after 500 years of agricultural

land use are much smaller. Anthropogenic processes do not show co-evolution, because the rates of for example tillage erosion far exceed any rates of natural soil and landscape change (see Fig. 5 in the manuscript). Tillage can introduce new processes or accelerate other processes e.g. by breaking up aggregates. However, these processes do not affect the rate at which a plough transports sediments through a landscape. If these interactions do not occur on shorter timescales, they will also not emerge over longer timescales. We think that the differences in time scales between the two land use periods do not affect our conclusion, because co-evolution is not time-dependent, but process-dependent.

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