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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.

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General Comments

The manuscript presented by van der Meij and others is interesting and well written, and will prove to be an important contribution to current pedogenic models. The manuscript presents a new formulation of the Lorica model, with the addition of a soil water balance component. The authors present model simulations for a 15 kyr period on a loess mantled landscape, with 14.5 kyr period of natural/ambient conditions and 500 yr period of intensive tilled agriculture; simulations were performed under three different rainfall scenarios.

I agree with the authors approach throughout much the manuscript. I think that reducing complexity, simplifying processes, and avoiding over parameterizations of models are principles that soil modelers should follow. There are several points that the authors could clarify or include throughout the manuscript.

Specific Comments -Intrinsic thresholds – There is a substantial discussion of progressive and regressive processes in the introduction, and a section about the potentially co-evolution/co-occurrence of soil-landscape processes and their relationship to external soil factors and drivers. The authors conclude that rainfall and landscape position, i.e. climate, are the dominant soil forming factor that generates soil spatial heterogeneity under ambient and agricultural contexts. However, there is no discussion of the possibility of intrinsic thresholds driving soil property change and variability across landscapes. Intrinsic thresholds also have the ability to create heterogeneity in soil properties without the influence of external soil factors. I think this may be an important component of soil evolution, particularly in natural settings, as some soil hydraulic changes can occur without changes in climate or water balance, such as argillic horizon formation leading to perched water tables and reducing conditions.

-Climate change – there was no discussion about the possible influence of climate change on pedogenesis throughout the manuscript. While this is a simulation, and as the authors note can only be used to understand general trends in pedogenesis, changes in climate over the last 15 kyr would like be a major driver of soil variability. However, this may be more of concern on a regional-continental scale, rather than the scale of the catchment consider in the present manuscript. Additionally, if we are to assume that these sites could exist at generally the same latitudes, I would expect soil responses to Holocene climate change at these hypothetical sites to be similar scaled.

-Vegetation switching – the authors tied vegetation type to the water availability. Depending on the annual water availability for the year, this may lead to annual transitions in vegetation. For this reason the authors consider vegetation type on multidecadal time scales. However, these "quick" transitions seem problematic in the model scheme.

Why were vegetation types not set for each simulation given that only one rainfall scenario could generate these transitions (humid)? Could the authors not have considered a savanna-type ecosystem for this precipitation level? I think this issue should be clarified in the manuscript text to aid in understanding of simulation parameterization and simulation results.

-Role of bulk density – based on the authors description it seems that estimating bulk density is central to estimating a number of soil variables from the model simulation, but there is little discussion in the main text on how this is done, other than with a PTF. I'm assuming this information is listed in the supplemental text. However, this should be included in the main text. Currently, it is unclear how these relate. This is especially important due the relationship between bulk density and OM and clay content. I may not understand the model architecture, but this would be greatly clarified with the inclusion of this information.

-Definition of intensive ag in loess mantled landscapes – The authors considered tilled agriculture in loess mantled landscapes. A recent trend, ∼50 years, of no-till ag has been prompted in many parts of North America, and I'm assuming the EU as well. Have the authors considered running similar simulations with no-till agriculture? This would be very timely, and may help us better understand SOM trends and long-term storage in soils in no till systems.

Technical Corrections

Line 89: "whereas" is not needed, please delete.

Line 90: "Therefore" is not needed, please delete.

Line 125: what are the two types of OM considered in the model?

Line 151: Please replace "didn't" with "did not".

Line 219: There are not other chemical information in the HydroLorica model. How did CEC evolve with the simulate soil landscape model?

Line 227: What is meant by SOM uptake? Accumulation in the soil? I would use a different choice of words for clarity.

Line 391-393: This sentence is unclear as written, I would remove the negative ("does not only"), and revise the sentence for clarity.

Line 408: Please delete "well" in "is well visible"; it is not needed

Line 410: Replace "get" with "become"

Line 436-438: I think that litter quality and input would also be a major driver of differences in SOM accumulation between natural and agricultural sites.

Line 450: "only" is not needed, please delete.

Line 476: "Especially" is not needed, please delete.

Line 511-512: Sentence starting "SOM cycling is heavily influenced..." This sentence is unclear as written. Please remove the "vice versa" and just say that erosion is not dependent on SOM cycling.

Line 515: I would also look at the work of Berhe et al. (2018). Ann. Rev. Earth and Plan. Sci. 46: 521 - 546, she has written extensively about the influence of erosion on SOM cycling.

Line 530-532: This sentence needs to be revised, it is not clear as written. Please revise the portion of the sentence starting with "...because changes in soil properties..."

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