

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 Dr. Shepard,

Thank you for your extensive review of our manuscript and the encouraging words. We reply to your comments below one-by-one, with your comments in italic.

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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.

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Response: We acknowledge the role that intrinsic thresholds can play in the evolution of soils and landscapes. We discuss these thresholds and following



co-evolution in van der Meij et al. (2018). But, as we mention in the same paper, such intrinsic thresholds can currently not be modelled, because we lack the methods for estimating accurate soil hydraulic properties which drive the threshold behavior.

Ideally, the model shows such threshold behavior without explicitly incorporating these thresholds in the model code. However, such hard thresholds can cause problems when calibrating the model by creating sharp discontinuities in the model results as a response to slight variations in parameters (Barnhart et al., 2019). Also, the occurrence of such thresholds depends on a large variety of factors, such as clay type and clay content, moisture dynamics and land management. When such factors cannot all be modeled and a hard threshold is assumed based on one of these factors (e.g. clay content), the model can give wrong results. For these reasons we focus on heterogeneity related to external causes in this manuscript. We will mention the potential role of intrinsic thresholds on soil heterogeneity in the revised manuscript and we will argue why we did not include them in our simulations.

-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.

Response: We agree that changes in climate have played a major role in soil and landscape variability over the Holocene. However, our aim was to isolate the effects of different rainfall and land use regimes on soil and landscape evolution. We decided to vary only the amount of rainfall to reduce the amount of variables that could have influenced the model results. The effects of a changing climate on soil-landscape evolution are thus out of the scope of this paper, but this can be an interesting topic for a subsequent paper. We will mention in the revised manuscript that we simulated a present-day Holocene climate, but that our simulation period extends beyond this climate period.

-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.

Response: We linked vegetation type to moisture availability to include both the effects of hillslope aspect and local convergence of water flow to gulleys or depressions on vegetation type (e.g. Metzen et al., 2019). This variation in moisture and vegetation can occur very locally, especially in semi-arid regions. On top of this spatial variation, the infiltration patterns also vary in time, due to changing infiltration capacity and surface water routing. Because of this co-evolution of soils, terrain,

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vegetation and the hydrological system, we think it is not a good idea to fix vegetation type depending on external rainfall amount. We will clarify the choice for dynamic vegetation modeling in the manuscript.

-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.

Response: The PTF for bulk density is indeed not properly discussed in the reviewed manuscript. We will include the description of the used pedotransfer function in the Methodology section of the manuscript and we will discuss the consequences of the chosen PTF on the model output in the Discussion of the revised manuscript.

-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.

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Response: Reduced-tillage or no-tillage is indeed also an occurring trend in Europe to reduce land degradation. But, just as with the rainfall, we simulated a simple scenario of intensive agriculture to facilitate interpretation of the model results. The implications of different tillage regimes on soil-landscape evolution can be the topic of a subsequent study.

Technical Corrections

Response: We will address the questions and process the corrections in the revised manuscript.

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.

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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. . .”

Interactive comment

References

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