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# Reply on RC1

W. Marijn van der Meij et al.

Author comment on "ChronoLorica – Introduction of a soil-landscape evolution model combined with geochronometers" by W. Marijn van der Meij et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-934-AC1, 2023

*The manuscript entitled "ChronoLorica – Introduction of a soil-landscape evolution model combined with geochronometers" by van der Meij et al. presents a novel model to simultaneously simulate soil and landscape evolution, respectively. This contribution starts filling an important knowledge and tool gap. To the best of my knowledge, such models commonly simulate either landscape or soil evolution but only rarely both. I therefore highly appreciate this contribution. To this end, the authors combine lateral matter fluxes, i.e. diffusion, advection, to simulate hillslope formation with vertical processes that shape the soil evolution, i.e. bioturbation, clay translocation. I enjoyed reading the manuscript a lot and I think this manuscript nicely suits the focus of GChron. Moreover, I think the presented modeling here will find use not only in geochronology community but will raise attention among both soil scientists and geomorphologists. In fact, this study could have a high impact. One of the most interesting and potentially impactful implications of this study is arises late in the discussion (L 548ff: 'These [reconstructions] are often made using different chronological methods, such as pollen analysis and 14C dates for climate and vegetation reconstruction (Mauri et al., 2015), or OSL and other dating methods for regional land use history and landscape change (e.g., Kappler et al.,2018, 2019; Pierik et al., 2018). These reconstructions serve as input for SLEMs, but, interestingly, SLEMs such as ChronoLorica can also be used to better understand the chronologies that have been used for developing these reconstructions.' The chosen approach is plausible and the implementation into a freely available software provided on Github is consistent. The English is well written and the figures are clear and easy to follow. However, I have several major observations that I wish to address to the authors.*

**Response**: Dear referee,

Thank you for the kind words and thorough review of our manuscript. Soil-landscape evolution models (SLEMs) indeed fill the gap between one-dimensional soil evolution models and two-dimensional landscape evolution models. However, the model we present is not one of the first models to couple soil evolution and landscape evolution. In Minasny et al., (2015) and Van der Meij et al., (2018), several SLEMs are reviewed, including the SLEM Lorica (see also lines 60-62 in the manuscript). The model we present is an extension to Lorica, where we added the geochronological modules (line 96).

Below, we address your remarks one by one. Your comments are marked in italic.

With best regards, on behalf of all authors, Marijn van der Meij

*1. The authors present a first and single simulation of soil and landscape co-evolution. To this end, the authors chose a (thankful) example of a synthetic, sigmoidal-shaped hillslope that is based on the shape diffusion dominated. While I clearly see the scope of this study and I also acknowledge the aim of a 'proof-of-concept', I was wondering why the authors chose such a hillslope shape and did not test for other hillslope shapes that are not as much controlled by diffusive processes. I was wondering if the model can perform on distinctly shaped hillslopes comparably well?*

**Response**: The model also works well on distinctly-shaped hillslopes, as is illustrated in earlier publications of the Lorica model (e.g., Temme and Vanwalleghem, 2016; Van der Meij et al., 2020). In these landscapes, diffusive processes act in two dimensions instead of on a one-dimensional hillslope, and advective geomorphological processes, such as water erosion can be simulated as well. We chose to simulate the one-dimensional sigmoidal-shaped landscape for two reasons. The first is that the simulation results for such a simplified landscape are easy to visualize and explain, which serves the purpose of this introductory paper of the model. The second is that advective processes in the model, such as water erosion and deposition, rarely lead to the development of depositional layers in the simulations, which is not useful to illustrate the development of chronologies in depositional environments. We will mention these reasons in Section 3 of the manuscript.
The application of the model in distinctly-shaped landscapes, that are formed under different processes, is something that we are planning for future publications.

*2. I missed a detailed description of the boundary conditions applied to the modeling raster. I assume that the hillslope extends from $x = 0$ at the ridge to $x = L$ at the valley bottom. I anticipate that the boundary conditions are set to $\partial z/\partial x\ |(0,t)=0$ and $z(L,t)=0$. Could the authors please provide more information on the boundary conditions. Also, a description of the initial conditions would be appreciated and may improve the readability of the manuscript allowing reproduction.*

**Response**: We will add the following initial and boundary conditions on the modelling raster to the manuscript: "The simulated hillslope was created to present stable, eroding and depositional positions under conditions of diffusion and has the shape of a Gaussian curve. The hillslope extends from $x = 0$ m at the ridge to $x = 500$ m at the valley bottom. $z(0,0) = 40$ m and $z(500,0) = 0$ m. Through the simulations, $z(x,t)$ changes under the influence of the simulated pedological and geomorphological processes. There are no restraints on $\partial z(x,t)$. "

*3. In this context, I also miss a description and reasoning for the parameters chosen (Table 1). I understand that the aim of this study is a proof-of-concept. However, I cannot see any explanation of how the estimated parameters are chosen. I see this as an important gap given that in L 434 the parameters are presumably chosen 'to create outputs that could be expected'. This may introduce a bias.*

**Response**: The selected parameters are loosely based on values that we got from literature and from previous modelling studies. The parameters are in the same order of magnitude as reported values in literature, but we did not want to add these references to Table 1, because we didn't use the exact same parameters. Other parameters we estimated to get illustrative outcomes for the model. An example is the tillage constant. With a too low value, the build-up of colluvium would be limited and we would not be able to illustrate how the model simulates the development of geochronometers in the colluvium.

However, we agree that the justification of the selected parameters is very limited. We will add an elaborated justification of all parameters to the text in Chapter 3. We will add the references on which we based our parameters and we will better explain what we mean with selecting parameters to get illustrative outcomes.

*4. The authors state that the current model includes several important geomorphological processes including tree throw. I missed the application here, as this process is prominently mentioned in line 110-111. I did not find this specific process in the Github repository neither. Any chance to include this process into the current study, too?*

**Response**: The process is called calculate_tree_fall() in the github code (code lines 18016-18410). This process is included in the simulations in Van der Meij et al., (2020), where it was shown to be a major process causing soil heterogeneity. We did not include it in this study, because tree throw is not commonly studied using geochronological tools, and we deemed it not a representative process to illustrate our model. The geochronological module for this process is also not yet written.

To avoid this confusion, we will mention specifically which processes were included in the simulations in this study. We will list the other processes in the model separately, in case someone else wants to apply the model in a study area where other processes are occurring.

*5. Given the clearly stated focus of the study that restricts on proof-of-concept and does not claim to reconstruct existing topography and soil landscapes, I was wondering if the authors should not, however, better context their modeling approach into the 'real world'. Any idea of how plausible (in quantitative terms!) the model may simulate existing landscapes?*

**Response**: The performance of SLEMs for simulating real-world landscapes depend on several aspects: the spatial and temporal extents of the simulations, the complexity of the actual soil and landscape evolution, and the data availability for calibrating the model and reconstructing initial and boundary conditions. Based on these aspects, the quality of the simulations will differ a lot between different landscapes and is difficult to estimate in advance. We also explained this in Section 5.2.1.

Actually, one of the motivations for developing the geochronological module for Lorica, was to improve the application of SLEMs for simulating real-world landscapes. The module provides an extra possibility for calibration and validation of the model. We will mention this again in Section 5.2.1, and we will also stress the importance of providing quantitative evaluation of the model when it is applied in real-world settings.

*6. Generally, I see problems in the organization of the results and discussion section. In many occasions, e.g. LL, the authors mix the results with an interpretation under the umbrella of a results section, e.g., L357 'indicating' or L370 'This is a consequence of…' . I would recommend to more rigorously split results and discussion. Given the current version, I have problems in objectively assessing the results. In addition, I miss more quantitative statements of the results. In many cases the authors remain unclear by stating 'more than' etc, e.g. L 385 'the inventories are higher compared to…' or L 415 'show very different dynamics…'*

**Response**: Thank you for this remark. We will adjust the Results section where necessary, to remove any interpretation. Concerning the remark about quantitative statements, in this paper we want to illustrate how the model works and how different processes affect different geochronometers. We think that relative statements, such as 'more than' or 'higher than' better serve this purpose than quantitative statements, because we want to show what happens in the model. Quantitative statements will be relevant when the simulated data is confronted or validated with field data, which is not the scope of this paper.

*7. I see a conflict in the modelled rates of vs measured rates of erosion. How do the authors explain the difference of 2-3 orders of magnitude between observed and modeled values excluding tillage as the process responsible? If I understood the manuscript correctly, such high discrepancy also occurs under presumably 'undisturbed' conditions during the 'natural phase'. Thus, I am not sure if comparing these data with tillage is plausible. The other explanation of the potential creep rate as multiplied by the slope gradient needs more explanation at least.*

**Response**: The simulated creep rates in the natural phase are indeed much lower than measured creep rates in the field. In the manuscript, we provide several explanations, such as shallower slopes and conservatively estimated parameters in the simulations. The point that we want to make is that the model needs to be confronted with field data to improve creep simulations and derive better model parameters, among others with our geochronological module. We will stress this point more in the manuscript. We will remove the comparison of creep rates with tillage rates.

*8. Also, the authors claim a probabilistic approach for choosing the particles. Yet, there are no clear descriptions of how the probability is computed. I see the reasoning of the fractions of sand etc. Yet, this is not unambiguously clear here how the probability (that is not the fraction) is estimated here.*

**Response**: The probability that a particle is transported, equals the fraction of sand that leaves a certain layer. So, if 1% of all sand is removed from a layer, also 1% of the particles should be removed, because they are associated with the sand fraction. Because the number of particles is variable, we need to calculate a probability for particle transport. This probability equals the fraction of sand that is transported, in this case P = 0.01. This probability is then used to randomly assess for each particle, if it is transported or not. We will better explain how we derive the transport probabilities in the manuscript.

*9. How do the authors define here transient landscapes? Do the authors refer here to the fact that the modeled curves of hillslope elevation still evolve over the period of simulation without convergence? Or do the authors refer here to landscapes changing in terms of erosion processes, i.e. tillage is activated after some 'natural' and undisturbed periods? Or are the boundary conditions changing over time. In that case, I suggest to be more explicit and I refer to my comment above.*

**Response**: With transient landscapes, we actually mean all the aspects that the reviewer suggests. By changes in boundary conditions, for example land use intensification or introduction of tillage erosion, erosion causes landscapes, soils and geochronometers to change with rates that are much higher than natural changes, that are often convergent or steady-state. We will better define what we mean with transient landscapes in the Introduction.

*10. I personally liked the clear, fair and honest discussion on the 'weaknesses' of the model. Yet, in the way it is written now, I have had the impression that many 'easy to apply' ('easy' is commonly mentioned here) additions to the model can be achieved. If so, and if these extensions are that easy, why aren't they already implemented in the current model version?*

**Response**: With Section 5.3 we want to indicate for what kind of scientific questions ChronoLorica could be used, and maybe inspire other researchers to use SLEMs such as ChronoLorica in their research. Adapting the model to answer these questions requires additional data and knowledge, that is not readily available to us. Also, each of these topics will need justification of the model adjustments and calibration and validation of the model results, which in itself can fill up an entire paper. Therefore, we think it is outside of the scope of this introductory paper to include the proposed adaptations. In hindsight, 'easy' is not the right word to refer to the model adaptations. We will leave out this word when we discuss the possible model adaptations in Section 5.3.

*11. Finally, I think a sensitivity analysis of the parameters (and thus the underlying processes) would improve the manuscript a lot. Up to now, it is hard to assess the efficacy of the distinct processes implemented on the simulated patterns. Such an addition, which is a lot of work, I know, may help to disentangle quantitatively the impact of controls on the simulated results.*

**Response**: We agree that a sensitivity analysis can shed more light on the effect of the individual parameters on the simulated results. This was also shown in a sensitivity analysis for the original Lorica model (Temme and Vanwalleghem, 2016).

A sensitivity analysis of the geochronological module would be most useful when focusing on a single process instead of a collection of processes, because different processes require different settings on the geochronological module (see Section 5.2.2). We believe that quantitative analysis of parameter selection and sensitivity can best be preserved for future studies where we will focus on individual processes and where we can confront the model with experimental data, and is therefore outside the scope of this paper.

*Minor*
*1. I suggest to avoid words like 'complex'. Every landscape is complex. What does complex refer here to?*

**Response**: With complexity, we mean that landscapes and geo-archives are formed by multiple processes, sometimes under non-linear behaviour. This complicates the disentanglement of the effects of individual processes. This is in contrast to 'simple' landscapes, where there is a dominant shaping process. The term complexity is commonly used in geomorphology (e.g., Temme et al., 2015) We will add this reference to the manuscript to indicate what we mean with complexity in this context.

*2. LL 127f I did not fully got the 'division of the slope gradient' and 'factor p'. Maybe the authors could better describe here the procedure. Similarly, L 142 on the 'convergence' factor.*

**Response**: The part of the equation with the convergence factor p determines diffusive transport through the landscape. We will add the following clarification to the text: "This last part of the equation controls the diffusive transport through the landscape, using the multiple flow algorithm (Freeman, 1991). The parameter p determines the division of the transport over all lower lying neighbouring cells. With higher values of p, transport becomes more convergent towards the lowest neighbouring cell."

*3. L 153: 'is lost from the soil column'. How? Here, again the boundary conditions are important. How can soil be lost assuming the conservation of mass? I assume that this principle applies here, too as loss and gain in elevation equals 1.04 m in both cases. If this is not the case, please state clearly.*

**Response**: Clay particles that eluviate from the lowest layer, are assumed to leach from the soil columns. This resembles leaching to deep layers, of colloidal transport. In modelling terms, these clay particles are removed from the modelling domain. This is necessary, because otherwise the clay particles will accumulate in the lowest soil layer, creating an unrealistic clay fraction. The conservation of mass still applies, when we consider this loss term. We will mention the boundary condition of conservation of mass in Section 2.1: model architecture.

*4. L 262-265: This sentence is hard to understand. Please consider rephrasing.*

**Response**: We will rephrase this sentence to: 'The total local input, $A_{me,local}$, is divided over all soil layers at that location, based on the depth of the respective layer and the depth decay function (Eq. 11)'

*5. Table 1: What is LSD in the table exactly? I missed an explanation here.*

**Response**: LSDn is a production rate scaling scheme used to normalize measured cosmogenic nuclide concentrations to globally distributed calibration sites, adjusting for variability in production rate with altitude and magnetic field influences. LSD is shorthand for Lifton, Sato and Dunai, the authors of the paper describing the scheme (Lifton et al., 2014). By accident we omitted the reference to this paper, so we will add it to Table 1.

*6. L 503. 'This suggests..' I did not fully understood this sentence. Please consider rephrasing.*

**Response**: We will rephrase this sentence to: "This suggests that quantitative erosion rates can be determined by the level of truncation (i.e. 'decapitation' of depth profiles) of bioturbation age-depth profiles, similar to truncation of radionuclide profiles (Arata et al., 2016a, b) or soil horizon profiles (Van der Meij et al., 2017)."

*7. The paragraph LL506-520 reads a bit out of context. I suggest to better connect this section to the discussion of the results obtained by modeling.*

**Response**: This Section is indeed out of context. We will leave it out of the next version of the manuscript, because it does not add to the Discussion of the model results.

*8. L 559 Please provide more specifics on the computing infrastructure. A laptop of year 2022 can be anything.*

**Response**: We will add the specifics of the laptop that was used for the simulations to the manuscript (Intel Core i7 processor with 6 cores and clock speed of 2.7 GHz, 16 GB RAM).

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