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 ^{14}C 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.

(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?

(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 $\frac{\partial z}{\partial x}|_{(o,t)} = 0$ 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.

(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.

(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?

(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?

(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…'

(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.

(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.

(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.

(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?

(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.

Minor

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

(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.

(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.

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

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

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

(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.

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