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Comment on soil-2021-133

Anonymous Referee #2

Referee comment on "Evolutionary pathways in soil-landscape evolution models" by W. Marijn van der Meij, SOIL Discuss., https://doi.org/10.5194/soil-2021-133-RC2, 2022

This short forum paper applies the concept of evolutionary pathways to the results obtained with the use of soil-landscape evolution models (SLEMs). In that specific paper, the author used outputs from the HydroLorica SLEM. The paper is short, well written. The authors are convincingly presenting this application of the evolutionary pathways concept, with a very interesting second part of the discussion section to broaden the scope of this paper. It uses extensively the results obtained in an other previous paper (Van der Meij et al., 2020) to calculate two types of evolutionary pathway parameters at a given timestep, that are accounting for time and space variations : the soil-landscape development stage (SLDS) using depth to the Bt horizon and SOC stocks as proxies, and the complexity using the standard deviations of these paramaters as proxies. These two indicators are simple to calculate, and allow to convincingly summarize the soil/landscape evolution. Moreover, SLDS and complexity can be easily applied to other model results, which shows their versatility and generalization potential.

Overall, I think this forum paper is worth publishing, providing a few issues are dealt with :
- The main issue deals with the interpretation of figures 2 and 3 that does not always seem to me to be correct, and require many changes in the text (lines 104-145 and some parts of the discussion, see below). This should however not be a difficult task.
- The second issue deals with the difficulty to read this paper without reading extensively the previous paper in which the HydroLorica model is applied. As it is, this paper does not always give sufficient details about the structure of the HydroLorica model, to be able to understand the explanations given for the evolution of both SLDS and complexity parameters in the discussion section. I would therefore recommend to give a bit more details about the structure of the model, and specifically some details about the components of the model that indeed have an influence on the behaviour of the SLDS and complexity evolutionary pathway parameters.

Detailed comments :
- Line 68 : can the author present a figure with the topography of the artificial catchment (contour lines) ? This would allow to have a better understanding of the initial conditions.

- line 77 : the depth to Bt horizon is used as a SLDS parameter. Does the author infer that the conditions for clay translocation processes are met over all the catchment area ? It if iyt the case, it would be worth to write it. I would actually need more explanation about the processes that provoke clay translocation in the model, to actually understand why this translocation occurs in any topographic situation.
- lines 106-107 : replace 'than the dry sceanario' by 'for the wet scenario'.
- lines 107-110 : this part is not clear to me and would deserve to be rewritten, as some facts are repeated (=> slight increase of SLDS for the dry sceanrio). Moreover, according to figure 2A, I do ot completely agree with what is written : I see the complexity for the wet scenario increasing again very shortly after the onset of agriculture, and not 250 years into the agricultural phase (some 50 to 100 years after according to Figure 3C ?). The evolution of SLDS for the dry scenario does seem to have a more complex behaviour than just a slight increase after the onset of agriculture : according to figure 3A, there is first a significant decrease of the depth to Bt.
- lines 115-119 : again, this paragraph is not clear to me and needs rewriting. Lines 116-117 : the part of the sentence 'due to higher SOM input in grassland systems and less redistribution processes' should be transfered to the discussion section. Moreover, I do not think data presented in this paper justify this statement.
- line 125, figure 3 caption : please specify that each point in the plot corresponds to a 500 years timestep for thez natural phase, and 50 years timestep for the agricultural phase.
- lines 132-134 : looking at figure 3D, despite what is stated, it is not at all obvious for SOM that natural evolutionary pathways in the wet scenario are more divergent and converge more slowly to the origin of the graph. I however agree this is the case for depth to Bt horizon. This should be corrected. I guess we can not see any difference for SOM for the natural evolution between both scenarios because steady state is reached faster than the 500 years timestep of the model. Perhaps we would have more information for SOM stocks evolution in the natural phase considering smaller timesteps at incipient stages of soil development, due to the inherent quick response of SOM dynamics compared with depth to Bt horizon.
- lines 138-139 : looking at figure 3, while the magnitude of change for SOM is clearly higher for the first step after cultivation, it is not the case for depth to Bt horizon. Please consider modifying.
- lines 140-141 : again, this sentence should be transferred to the discussion section. I indeed do not understand why "potential clay translocation in clluvial positions" would induce shallower depths to Bt horizons. Indeed I would infer this would be the opposite.
- lines 142-145 : consider transferring these sentences in the discussion section ?
- lines 145-146 : change to "all pedgenic pathways either kept or turned divergent". It is indeed not so obvious for the wet scanario, where the divergent behaviour seems to have taken a bit more time, especially for SOM.- lines 151-152 : I do not see what to conclude from this statement, where increased water erosion and clay translocation have opposite effects on the depth to the Bt horizon.
- lines 152-155 : I do not understand. The author states that a steady state is not reached in the natural setting for the wet scanario, however, figure 3 shows the opposite : at the end of the natural phase, a steady state is reached, illustrated by the position of the point at the origin of the graph. I understand there is no steady state that is reached for a given point in the landscape, but a strady state is reached on average.
- lines 156-158 : the figure 3 does not show in an obvious manner that in the wet scanario, the equilibrium takes longer to be reached than for thre dry scenario (and not at all for SOM stocks).
- lines 161-163 : I do not think reference can be made to a figure in an other paper to explain processes that are described in this paper. It is necessary to have more information in this paper
- lines 163-164 : I do not understand this statement. Can the author justify it ?
- lines 169-171 : I do not understand this statement. It has been shown on Fig. 3 that a steady state is reached in the natural phase for both SOM stocks and depth to Bt. Why is

it stated here that this steady state is only reached for SOM stocks ?
- lines 171-172 : it is not the case for delta SLDS for the depth to the Bt horizon, if the first stage of soil development is accounted for (figure 3).