Comment on soil-2021-133

Anonymous Referee #1

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-RC1, 2022

Review of the manuscript entitled « Evolutionary pathways in soil-landscape evolution models"

This short manuscript compares mechanistic soil landscape evolution modelling (SLEM) to the theoretical evolutionary pathway approach and discusses the complementarity of the two approaches with as initial idea that evolutionary pathways could be an efficient tool to summary the modelling results and thus better communicate. The demonstration is made on modelling results from an already published study (van der Meij et al., 2020) that are reinterpreted here in term of evolutionary pathways for two soil characteristics: the depth of the Bt-horizon and the soil organic matter (SOM) stock. The work demonstrates that evolutionary pathways of these two soil characteristics differ due to differences in dynamics of these two characteristics. This idea could however have been pushed forward clearly introducing another concept of the soil sciences: the inherent and manageable soil characteristics as defined by Dominati et al. (2010) among others; depth of the Bt-horizon representing the inherent characteristics and SOM the manageable ones.

In addition, the potentiality of evolutionary pathways to be used as a communication tool should be specified, notably by clarifying toward which community this communication is thought to. Indeed evolutionary pathways are a convincing tool to sum up the SLEM results but they are not easy to understand and thus probably not suitable to communicate with most of the soil end-users.

Out of these two main comments, the manuscript is very well written and easy to read and follow. I added detailed comments below to be addressed.

Once these comments addressed, I think that this manuscript is worth publishing in SOIL.

Detailed comments

Method section

Model study. This is the summary of a study published elsewhere. The presented model does not contain weathering processes at all, which can be a severe limitation when used over 15 000 years. I recognised that the aim of this manuscript is not to discuss the model. I nevertheless think that this limitation should be mentioned in this section and discussed in the evolutionary pathway section of the discussion line 153.

Evolutionary pathway. Please add the units used in the different equations

Results

Page 5 line 119, replace "the complexity" by "it" to avoid useless repetitions.

Page 7 lines 133-137, these statements seem to be true mainly for the depth of the Bt-horizon.

Same page, lines 137-138, on the opposite, this statement seems to be true mainly for SOM.

Same page, sentence lines 142 to 145, please refer to figure 2 (more appropriate than figure 3).

Same page, lines 145-146, add "for SOM" after "new steady state" and "for the depth to

Bt" after "a steady rate of change".

Discussion

Page 8 line 161. A reference to results found in van der Meij et al. (2020) is made. This should be extended to be clearer to the reader as I had to go to that paper to understand. It presents semi-variogramm and when the statement seemed clear to be for SOM, it is not so much the case for the depth of the Bt horizon.

Same page lines 172-173, this statement mainly apply to manageable soil characteristics according to the present study. See comment above.

Figures

Fig. 1. Please don't use the acronym in the figure title so it can be understood by itself

Reference cited

Dominati E, Patterson M, Mackay A (2010) A framework for classifying and quantifying the natural capital and ecosystem services of soils. Ecological Economics 69, 1858-1868. oi:10.1016/j.ecolecon.2010.05.002