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Comment on esurf-2021-70

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Referee comment on "A geomorphic-process-based cellular automata model of colluvial wedge morphology and stratigraphy" by Harrison J. Gray et al., Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2021-70-RC2>, 2021

I would first like to thank the authors for this contribution, it was very interesting to read and opened my mind to new ideas. This manuscript attempts to decipher the geomorphic processes that control the evolution of a colluvial wedge following normal faulting and rapid surface deformation. The research of fault scarp evolution has been studied for many years, mostly as a tool to reconstruct paleoseismic activity. The authors use a relatively new physical process based cellular automata numerical model, which is an interesting alternative to the commonly used continuum-style models and by that avoid several limitations and over-simplistic assumptions. Similar models have been recently and successfully used to model various natural landforms, but not fault scarps. The results of this study give new perspective on the evolution of colluvial wedges but also on the much studied and poorly understood processes of soil covered hillslopes evolution.

The authors chose not to compare the model with real-world examples but rather run sensitivity analysis using very generalized settings. This limits the application of this study but gives profound basis for further research, which I hope to see in the future.

The manuscript is well written and original and could be published with minor changes. Below are suggestions that I think would benefit the manuscript, some might require moderate changes and I leave it to the editor and authors to decide if they are required.

Main comments

The resulted grouping, as shown in figure 8 is very interesting and deserves a deeper discussion. Several model runs show distinctly different grouping than the ones used (e.g. figure 8 C,G,K). It might hold valuable information on the evolution of colluvial wedge morphology and stratigraphy. It would be very interesting to connect the observed groups

with the different facies, instead of using fixed cutoff values. Reading the manuscript, I was expecting to find a convincing, physically based, argument for the chosen threshold values of the different facies, but did not. I strongly suggest using a simple grouping method, or at least discuss the physical logic in using the same threshold values in mean velocity for different model scenarios.

The authors chose to use fixed morphological parameters to focus the analysis on geomorphical processes. Looking at the results, it seems that the comparison between 60 and 90 degrees dip shows high sensitivity and results that are sometimes trivial and related to bigger accommodating space and steeper initial dip. It might be better to use fixed dip angle and focus on the process-based parameters, which are less known (covering several orders of magnitudes each) and very challenging to decipher with any other method (unlike dip angle that can be measured directly in a trench).

According Wallace 1977, which his work is fundamental for this study, the debris-controlled phase ends when the fault scarp reaches the angle of repose. This was also the basis for initial conditions for several models that were used to morphologically date fault scarps. I believe that your results could shed light on the validity of this assumption if addressed more specifically.

I suggest adding another parameter to the analysis - the time that has passed since a particle last moved. It could give valuable information on the timing of facies formation and give a more complete picture of the geomorphic evolution of the colluvial wedge. It would also make results more comparable with luminescence dating data.

Minor comments

Lines 166-167: How small is the initial sediment layer? This should be more methodically defined and described. It is not unlikely that a pre-faulted surface is covered with up to tens of centimeters of mobile regolith. I assume that the thickness of the initial sediment cover could influence results.

Lines 184-185: The classic definition of a pecllet number, to my knowledge is different, and the referenced paper is about soil mixing, a process that is not described by the model.

Line 185: In figure 11 you show combinations of D and W_0 values that range over 3-4 orders of magnitude. To my understanding of your definition of the pecllet number

(D/W0), this must result on much wider range of values.

Line 189: Why choosing the specific 4 orders of magnitude?

Lines 206-208: It is not clear to me where the threshold value of $\sqrt{3}$ radians comes from.

Line 331: Nelson 1992 is repeatedly and rightfully cited in this study, however it is worth addressing the fact that his pioneering work was limited to arid regions.

Line 351: I expect that the collapse dominated stage will end when the surface angle will approach the angle of repose. It would be interesting to test it. It will validate a physical basis of the model and also contribute to studies of long-term modeling of fault scarp evolution that commonly assume rapid evolution until angle of repose is reached.

Technical corrections

Lines 103-104: Consider citing BenDror and Goren, 2018, JGR: Earth Surface.

Line 153: I believe it was Culling, 1963 who first used the term 'diffusivity' in soil transport.

Line 215: add reference to figure 8.

Line 340: Give references to these observations.

