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Interactive comment on "A coupled soilscape-landform evolution model: Model formulation and initial results" *by* W. D. Dimuth P. Welivitiya et al.

Reply for the comments from A.J.A.M. Temme (Referee #2) in italic font

The manuscript by Welivitiya and co-authors presents a simulation performed with their novel soil-landscape evolution model. The model and the rationale behind it are pre- sented in detail. The model simulation is on a simplified 2D landscape (i.e. a row of cells) representing a plateau over a hillslope and a valley. Two scenarios are simulated, with different depth-dependent weathering functions. Findings are discussed in details, and appear to indicate that the model functions well, and that basic expectations about the joint development of soils and landscapes (co-evolution) are met. The paper is interesting to me as a soil-landscape modeler, and I greatly enjoyed the detailed model layout and accompanying figures.

General Reply

First of all the authors would like to thank the referees for expending their valuable time and energy to review our manuscript. We also greatly appreciate the constructive criticism and the comments of the referees very much. The authors will consider all the comments and suggestions made by the referees and accommodate them in the manuscript wherever it is possible.

I have detailed suggestions in the attached annotations, that amount to minor revisions in and of themselves. Below, I add three general concerns.

1. Although the paper is very interesting to me, I am not sure that it is to the general audience of ESurfD. The meat of the paper is the model presentation, to my mind. That makes me wonder whether an outlet such as Geoscientific Model Development of Computers and Geosciences is not a better choice.

The authors do agree that the main objective of this manuscript and the bulk of its content has to do with model formulation and implementation. If the manuscript was just model formulation, we would agree with the Referee #2 s suggestion on submitting this manuscript elsewhere which caters to more mathematically oriented audience. Although a manuscript concerning only model formulation may be interesting, we strongly felt that we needed to include some initial model results (albite simple) to illustrate how the model performed at the simple scale. Also we wanted to highlight some of the geomorphic signatures emerging from the modelling results itself. We believe that publication here is important for not just the modelling community but also for the general geomorphology and soils community who through this work can observe some important physical insights but also raises some important questions. The other important issue is that lack of field data to calibrate/evaluate such models. We hope that we may inspire fieldwork (or reveal existing data) to advance such models as that described here. With this in mind, We strongly believe that Earth Surface Dynamics is the better choice for publishing this manuscript.

2. The simulation that illustrates the model's performance, is interesting but hypothet- ical. The paper does a great job of explaining what the results are, but does not give much attention to what that means for how we should think about landscape evolution. This may be possible only in a limited way, given the hypothetical setup, but I do think some comparisons with existing thinking and findings from others are possible. I do one suggestion in the annotations. In another outlet, a lack of connection with exist- ing thinking would be no problem at all, but I think that in ESurfD the readership is particularly interested in that aspect (and perhaps

less in model workings).

The authors do agree that the simulations presented in this manuscript concerns a hypothetical situation and not much attention was made to compare results with real world scenarios. As the Referee #2 has understood the main objective of this manuscript was to introduce the new coupled soilscpae-landform evolution model and demonstrate its ability to co evolve soilscape and landform together resulting in real world trends. So to keep the focus of this manuscript to the model development and model mechanics and to keep the manuscript at a reasonable size, the authors decided not to include a result comparison section to the current manuscript. In fact we have already done some comparison study on the model simulations (particularly the deposition region of the landforms) with experimental flume scale studies and fluvial fan development and the results will be published in 1 or 2 separate manuscripts in the near future. We also highlight the issue of a lack of field data in the previous comment.

3. Although the model description is detailed and accurate, it would benefit from pointing out differences with existing models or a few more tradeoffs between accuracy and efficiency, especially where some of the innovative aspects are covered. How do these differ from existing models such as Lorica? I give a few suggestions for improvement.

I am glad to see this work in manuscript form, and I am happy to have it in the public domain so it can be used by colleagues. I wish the authors good luck in considering the changes suggested.

Please also note the supplement to this comment:
https://www.earth-surf-dynam-discuss.net/esurf-2018-16/esurf-2018-16-RC2- supplement.pdf

Specific comments from the supplemental document

Referee #2 comment Page 9: I continue to struggle with these issues myself. Propagating resupply seems perfectly reasonable, and is necessary if layer thickness/mass is constant but don't you in this way always provide the top armouring layer with easily erodible soil? I imagine the result is a less effective armour. Does this reflect the real process?

The authors appreciate the Reviewers comments, understanding and grasp of the practical difficulties of material propagation in a model like SSSPAM. However according to the method used here the resupplied material to the armour layer is not always easily erodible. Size selectivity only applies to the material movement form the armour layer to the flowing water layer above (i.e. depending on the surface water discharge rate, smaller particles are easily removed from the surface armour layer while larger particles remain). The material resupply to the surface armour occurs from the layer below and the resupplied material have the same grading as the subsurface layer grading (no size selectivity) so both small particles and large particles are resupplied to the armour layer. Most of the time the net effect of this material resupply and the size selective erosion will be enrichment of larger particles and armour strengthening. Depending on the depth dependent weathering function the relative coarseness of the subsurface layers can be less compared to armour layer. But once the armour layer is reconfigured with the added material from below and removal of small particles through erosion, again the net effect is armour strengthening. This layer restructuring is explained later in the paper.

Referee #2 comment Page 10: If I see it correctly this all means that if you need to deposit 20kg of material and all you have in transport is clays, then you'll deposit some/all of the clays right? Regardless of the settling velocity.

Yes the referee #2 is correct in recognising this factor. If all there in the transport is clay the amount of material which need to be deposited will be deposited from the available clay. Size selectivity comes in to play when there are a range of particles in the transport (which is almost always the case). Then the larger particles with higher settling velocities will deposit first.

Referee #2 comment Page 13: Ok, this makes me doubt what I said three pages ago. –at the cost of my understanding. Tell me is ∑deposition in a cell equal to L_{in} -T_c , which I thought before or is it dictated by the settling velocity? The later seems to allow for ∑deposition to be < L_{in} -T_c .

The Referee #2s earlier assumption is correct. The ∑deposition at a pixel is always equals to Lin -Tc. The settling velocities and the critical immersion depths for that matter dictates the distribution of the deposited particles. The Referee 2 is also correct that at the first glance it seems like ∑deposition can be < Lin -Tc . This is why we have the adjustment

vector K (equation 5) which ensures
∑deposition = Lin -Tc (always). The authors previously had a worked example on how this correction vector is calculated. However the authors thought that this may confuse readers with less mathematical/modelling experience and decided not to include it in the final version of the manuscript. The example is again added to the main body of the manuscript. Following is this omitted section with the example.

Calculation of deposition mass vector

Following simplified example shows the need to have this adjustment vector and the method we used to calculate it.

Table 2 Example calculation of adjustment vector $\underline{K}$.

Size Class	*Elements of* $\underline{\psi}_{in}$ (ψ_z)	*Entries of* $\boldsymbol{J}$ $(J_{z,z})$	$J_{z,z}\,\psi_z$	*Adjusted* $J_{z,z}\,\psi_z$	*Deficit / Surplus*	*Diagonal elements of* $\underline{K}$	*Entries of* $\underline{\Phi}$
1	*5.00*	*1.0*	*5.00*	*7.29*	*-2.29*	*-2.29*	*5.00*
2	*10.00*	*0.7*	*7.00*	*10.21*	*-0.21*	*-0.21*	*10.00*
3	*20.00*	*0.4*	*8.00*	*11.67*	*8.33*	*2.00*	*13.67*
4	*40.00*	*0.1*	*4.00*	*5.83*	*34.17*	*0.50*	*6.33*
Total	*75.00*		*24.00*	*35.00*			*35.00*

Consider the example values given in Table 2. The total mass of the incoming sediments is 75 kg and the sediments are distributed in four size classes. Here the size class one is the largest and has the highest potential *for deposition (with* $J_{1,1}$ *=1) while the size class four has the lowest potential for deposition (with* $J_{4,4}$ *=0.1). If the transport capacity* T_c *is 40 kg, 35 kg of incoming sediments should deposit at the pixel as the total deposition D. Using the* $\sum J_{z,z}\ \psi_z$ *value (which is 24) and rescaling these values with D (total deposition mass), we can calculate the masses of sediments which need to be deposited from each grading class. In some cases (when the total deposition D is higher than the* $\sum J_{z,z}\ \psi_z$ *value) the mass of material which needs to be deposited can be larger than the available sediments in that particular size class. In this example there is 5 kg of sediments in the 1st size class and 10 kg of sediments in the second size class respectively. However, our adjusted calculation dictate that there should be 7.29 kg deposition from the 1st size class and 10.21 kg from the 2nd size class which is not possible. So these values needs to be adjusted to reflect maximum possible deposition from size classes one and two which are 5 kg and 10 kg respectively. This adjustment introduces a deficit of 2.5 kg to the total deposition and it needs to be deposited from the* 3^{rd} *and* 4^{th} *smaller grading classes. According to the deposition matrix values* $J_{z,z}$ *the deposition probability ratio between* 3^{rd} *and* 4^{th} *grading class is 4:1 (0.4:0.1). The deficit mass 2.5kg is deposited from the 3rd and 4th size class with 4:1 ratio which accounts to an additional deposition mass of 2 kg from 3rd size class and 0.5 kg from the 4th size class. In this way the entries of the adjustment vector* $\underline{K}$ *are calculated. Depending on the number of size classes and the distribution of the sediments, this adjustment vector* $\underline{K}$ *needs to be calculated iteratively.*

Referee #2 comment Page 16: The section 2.3.2 feels excessively detailed and long for a paper (Better in a model manual perhaps?) consider shortening drastically.

The authors agree with Referee#2s comment regarding the section 2.3.2. The section is modified and some paragraphs were removed.

Referee 2 comment Page 17: Also it conveniently avoid possible negative effects of d8. Please comment here, or where you present the d8 choice.

At the time this manuscript was prepared, the authors had already ran the simulation for 3dimentional synthetic landforms as well. However the authors did not find any significant issues with using d8 for flow calculation.

Referee #2 comment Page 21: I’m trying to imagine implications. Does it follow from this that nick points cutting back into higher regions slow down less than otherwise expected since they meet fine soft soils as they proceeds further into the plateau? Please comment and harvest for your readers.

The Referee #2s understanding is correct. When the erosion region cuts back in to the plateau area, weathering process has already broken-down the coarse material in to finer constituents where it can be readily eroded away by the erosion region cutting in to the plateau region. If we did not have weathering the rate at which the erosion region cutting back to the plateau region would be slower as it requires the removal of coarser soil particles. A small discussion mentioning this implication was added to the manuscript as per Referee #2s comment

Referee #2 comment Page 24: I have the opinion/impression that the discussion section is quite detailed in describing your results. Perhaps a bit much so, and that there is not enough in the way of comparing dynamics with results reported by others, empirically or otherwise.

As Referee #2s Comments the Discussion section is modified and some paragraphs were removed

Referee #2 comment Page 26: It would be good to shorten conclusions to about ½ or 2/3 of their size.

As Referee #2s Comments the conclusions section is modified and some paragraphs were removed