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Earth Surface Dynamics Discussions
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Interactive comment on "Exploring the sensitivity on a soil area-slope-grading relationship to changes in process parameters using a pedogenesis model" *by* W. D. D. P. Welivitiya et al.

Reply for the comments from Anonymous Referee #1 in italic font

General comments

This is a nicely written and well-conducted model study that fits well in ESurf. The au- thors present a sensitivity analysis of the pedogenesis model SSSPAM5D. The results are quite interesting and have important implications for understanding the spatial vari- ability of soil properties in the landscape. I only have two minor concerns. The first is that the setup is relatively simple: straight, planar slope and a relatively limited One- At-a-Time sensitivity analysis, but given the computational demands of the model, this is understandable and hard to work around at this point. Another minor concern is that in this model, there is no feedback between evolving soil properties and runoff rate that controls erosion (Equation 4). Basically r is fixed by the authors. It would be very

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Discussion paper

interesting for future work to have some kind of feedback between infiltration rate or saturated hydraulic conductivity or soil water holding capacity and r. For this paper, I would be happy if the authors could include a minor discussion point on this matter. In conclusion, the quality of this paper is excellent and I believe that the point made by the authors, that these results confirm the generality of the area-slope-d50 relation, is important and holds significant implications for geomorphology and soil science. It is definitely a very interesting study to publish in ESurf. Apart from my two minor concerns, I only have some specific comments for the authors to take into account, see below. I also believe the number of figures and tables is quite high and could be reduced. My recommendation is therefore minor revisions in order to streamline the presentation of the results to the reader, by eliminating a few figures and explaining a few statements in the text better.

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.

In the general comments section the referee has expressed 2 minor concerns regarding the modelling setup. The first concern is the simplicity of the simulation setup where we used a planar slope and the parametric study where we changed a single parameter for each simulation. The second concern is that the model doesn't account for any feedbacks between the discharge rate, erosion and the infiltration. Authors do agree that the simulation setup is relatively simple. The objective of this manuscript is to analyze the robustness of the area-slope-grading relationship under different process parameters. In order to directly compare our parametric study results with previous work by Cohen et al. (2009,2010) we decided to use the same model setup used in those simulations. Using a planar hillslope was also due to the previous model setup and it's a easy way to create

to understand what is going on. Because the erosion mechanism used is detachment-limited the erosion at one node is not impacted by erosion at upstream nodes, and only by the cumulative area coming from upstream and the local slope. Thus the geometry upstream (i.e. 1D hillslope versus 2D catchment; planar, convex and concave slope) does not impact on the erosion/incision at the node. In this way the model acts upon each node (pedon) individually. In this context the authors believe the simple model setup presented in our manuscript has enough complexity to explore the parameter space of the model.

As the referee suggested the infiltration rate can change with time due to the changing soil grading, particularly in the surface, in the armour layer. Since we are mainly interested in exploring the parameter space of the model and their influence on the soil reorganization, at this initial stage the feedback mechanisms between soil and infiltration has not been modelled. However the authors are in the process of combining the pedogenesis framework presented here with a landform evolution model in the future. The referee's suggestion will be taken in to account when the authors are formulating this coupled soil and landform evolution model.

Specific comments

- I like the S-A-d50 plots and the explanation of how to use them in figure 4. Just as a suggestions to make it more attractive, it would be nice to fill in the contours with colours (using the same scale for all figures makes comparison very fast)

-At the time of the preparation of the original manuscript the authors also discussed regarding using different colors for different contours. This would indeed improve the manuscripts appearance greatly in its online version. However the authors decided against using color contour lines due to 2 main reasons. The first is that if the manuscript is being printed in black and white the colors turned in to gray scale can be very hard to read. The other reason is the large difference in the d50 produced for different parameter simulations. In some simulations the d50 range is larger (eg:10-4mm) and in other cases it is very small (eg:0.5-0.01mm). in such cases assigning colors for contours (even the same scale) for the contour plots will not work because the colors will not change much from plot to plot. If we use different scales (as we have used in this manuscript) and assign

different plots will have different color contours for the same value according to the scale.

-p.5 line 12-13: The authors can call their model as they will of course, but I am not quite sure if I agree with the 5 dimensions. Because a point-based model simulates 2 (or n) soil properties does not make it a 3D model (or n+3 model)? . Also: "depth down the soil profile", why not just name it z and talk about the 3 spatial dimensions?

-The authors do agree with the above comment. The model itself is capable of modelling the soil evolution in a large number of points which can be spread in a spatial grid. Also the evolution of the soil profile gives the model a vertical dimension as well. With the other soil properties calculated by the model the authors thought the "5D" is justified. However to alleviate any concern on this matter the authors will change the name of the model to "SSSPAM" instead of "SSSPAM5D"

-p.8 use consistent writing of Shields criterion (if've found Shield's, Shield and Shields in the text)

-We have changed the wording to be consistent

-p9 line 12: delete "and": smaller particles, the cumulative. . .

-the manuscript updated as per referee's comment

-p.9 lines 14-17: repeated from p8 line 15-17. I suggest to eliminate the former.

-the manuscript updated as per referee's comment

-p. 10 lines 4-7: Is it important how you define the thickness of the armour layer?

-typically the armour layer depth in the literature is about 2.5 times the diameter of the largest particle in the grading. Since the largest particle diameter of the soil grading we used is 19mm, the armour depth should be 47.5mm or higher. So we used 50mm as the thickness of the armour layer.

-p.10 Eq4. Define r and x.

-p.13 The authors suddenly talk about chemical weathering here. Yet in paragraph
1.2 no distinction is made. I think it is important to mention this earlier and what the authors mention on p13, line 5 that “we do not explicitly model chemical weathering”.

-The authors intention was to mention that we only model physical weathering in SSSPAM modelling framework but the dynamic reversed exponential weathering function can be used to define the weathering rate change through the soil profile. The sentence was clarified. See comments to reviewer 2 for more detail.

-p.14 paragraph 3. Mention here what a and b stands for (see p 16, lines 14-17)

-in this context 'a' stands for the Actual measured soil grading and 'b' stands for the Synthetic bedrock grading derived from the actual soil grading. The authors do admit the notation is somewhat unclear and clarified in the revised manuscript.

- p14, line 28 gradings

-the manuscript was rectified

-p.18 lines 11-15. I can not see this in the mentioned figure. No mention of 1a and 2b, just 1 vs 2?

-the authors did not present a separate figure to demonstrate that no matter the surface grading used, the final surface d50 values only depend on the subsurface grading used. This is because the final equilibrium figures are identical to that of the figure 5 with the all other parameters (eg: weathering rate) As the referee has pointed out the figure caption for the figure 5 is unclear it the final sentence should be changed to ;(left column) Ranger1a for surface,Ranger1b for subsurface and (Right column) Ranger2a for surface, Ranger2b for subsurface. The required changes will be done in the revised manuscript

-p.18 lines 25-26. See one of my main concerns, that grading does not change the discharge/infiltration rate. Can the authors include a short discussion on this? Fine-textured soils will be more prone to crusting, coarse fragments generally promote water infiltration in addition to armouring the surface and reducing erosion. See a good review by Poesen and Lavee (1994, Catena)

is given in the reply to the general comments section. The authors will add a small discussion regarding this in the revised manuscript

-p18 para- graph 5.2.2. What about changes in the intensity-frequency of events? Authors now only change the absolute amount of discharge. Would these conclusions hold when a time-varying behavior in the event series (with the same annual mean) is applied?

-This is a really interesting and important comment. At the time of writing of this manuscript the authors did not consider the changes brought forward by intensity-frequency events. However at the moment the authors are testing a coupled soil and landform evolution model and performed some simulations with constant discharge and some simulations with a lognormal distribution of discharge values with the mean value being the average discharge at different time steps. The results seem to suggest that only dynamics of the soil evolution will change but not the equilibrium final soil grading.

-p.19 line 8: Ranger 1 or 2?
-the "Ranger site data set 1" needs to change to "Ranger1a" and "Ranger bedrock grading" needs to change to "Ranger1b". the necessary changes implemented in the revised manuscript

-p.19, line 17. This is an important conclusion! –paragraph
We agree and this is a primary justification for including all the figures … to see this effect graphically rather than just have the mathematical summary of equation (13). WE will include a mention of this result in the conclusions to the paper (this somehow slipped out attention).

5.2.4 suggest to change to "changing the erodibility and selectivity exponent"
-Changes were made to the manuscript as per reviewer's comments

-p.19,line 27: suggest to change to: "the d50-exponent beta"

-p20,line 4: geometry? -Figure

Changes were made to the manuscript as per reviewer's comments

4. Is it not possible to read in the points from Surfer and plot the left figure in mat- plotlib to streamline all figures?
-The figure was redrawn according to reviewer's comments

-Figure 5. The authors need to indicate not only the weathering rate, but also the second variable (Ranger 1 or 2 grading) in the figure.

-As per reviewer's suggestion the grading used for both surface and subsurface layers added to the figure captions

-I suggest to eliminate figure 6 in order to reduce the total number of figures -I suggest to eliminate figure 7. The main results are summarized in fig 8 anyways and there are more simulations in fig8 which are also not shown.
-Fig 13 is cut off on the left –Table

-The authors believe that to be completely thorough we require at least one figure for each change of parameter. The authors believe that all the figures presented in this manuscript are the bare minimum that could present the model results in a comprehensive manner. However the authors will explore any other ways to reduce the number of figures without degrading the quality of the manuscript.

1. The authors just downgraded their model one dimension here. -Table 2 caption: "to generate" -Table 3 doesn't add much extra over figure 8. Suggest to eliminate.

Table 3 has been eliminated from the manuscript.

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