

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.

Anonymous Referee #2

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The authors present a model (SSSPAM5D) to investigate the relation between soil grading, surface erosion and weathering. Minor changes to an existing model (mARM3D) are done and a number of 2D simulations have been performed at the soilscape level in the framework of a sensitivity analysis. The main conclusion is that an earlier published area-slope-grading relationship holds when model parameters are altered. Interestingly, the sensitivity analysis shows a dependency of soil grading on the ratio between area and slope exponents, similar to what has been found for other earth surface processes.

The setup of the model runs, the results and the paper in general are very similar to what has been published earlier [Cohen et al., 2010]. A major problem with this choice

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is that major soil processes such as creep, bioturbation and clay translocation are not considered. Furthermore the model does not reach its full landscape (3D) potential because only 2D soilscape simulations are performed. Moreover, observations are only used for initialisation of model runs and not to calibrate, let alone validate, model results.

Although a numerical soil-hillslope coupled model could indeed serve as an insightful tool to better understand the interdependency between soil grading and earth surface processes, the paper needs significant improvements at several points to be of value for the geo-scientific community. Nevertheless, I think that this paper can be published eventually after addressing some major comments listed below.

Major points, not in order of importance

1. In the abstract it is stated that the authors developed a new numerical model (SSS-PAM5D) which generalises and extend findings from mARDM3D (Cohen 2010). However, only small adaptations to the existing mARDM3D model are made and similar synthetic simulations are performed (with altered parameter values). Findings from the previously published mARRDM3D model are repeated and confirmed. In principle, this should not be a problem but the authors cannot sell this as 'a new model' and more complicated model configurations should be additionally tested (preferably in 3D, see further).

2. In comparison to the mARDM3D model, the presented model is not substantially extended in any significant way: nor in numerical methods, nor in the physics behind the model. Therefore, giving it a new name is spurious. Rather, this paper is an application of the existing mARDM3D model and it should be described alike.

3. The described model does not take into account major processes active at the soil-hillslope scale. Recent advances have shown the importance of creep, eluviation/illuviation, deposition and bioturbation on the depth dependent shape of soil properties such as the particle size distribution [Johnson et al., 2014; West et al., 2014;

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Campforts et al., 2016]. Moreover, except for creep, those processes are described and implemented in an earlier release of the model [Cohen et al., 2010]. In this earlier publication, the authors state that the impact of these processes on the self-organisation of soil properties was not yet discussed in order to avoid overcomplexity and enhance interpretation. Currently, I do not see the relevance of this work without including those processes, known to be major driving forces of soil development [Braun et al., 2001; Roering et al., 2007; West et al., 2014; Temme and Vanwalleghem, 2015]. Investigating the impact of these processes could be an interesting and probably necessary research question to be answered.

4. There exists a significant body of literature describing experimental findings where the relation between soil grading and erosion are discussed [Poesen et al., 1998; e.g. Govers et al., 2006]. It would be good to describe these and/or other empirical findings in the introduction of this manuscript.

5. Similar to what have already been done in mARM3D [Cohen et al., 2010], the model is only applied to synthetical landscapes. The simulations performed are almost equal to what has been presented in Cohen 2010 (5 hillslopes profiles). Moreover, the authors only discuss 2D profiles and do not perform landscape simulations. This is in contradiction with what the authors promise in their introduction where they describe a '5D' landscape model. Only performing 'transect' simulations is therefore unfair to the reader. Application of the model to different (3D!) landscapes, similar to what have been proposed by Cohen et al. 2010 seems essential to me. Moreover, the matrix structure of this model is especially of use in the framework of 3D landscape simulations.

6. Given the fact that the model and its functionality has already been discussed in literature, this paper would be of much more value to the geo-scientific community if the model is calibrated with field data (directly, not by calibrating it to the mARM3D model that has been calibrated with the results of the mARDM2D model, as described in line 13-17 on page 15).

7. I am wondering why you use observed soil gradings as an initial condition for your model runs. Shouldn't a good model be able to reproduce these observed gradings by starting from a bedrock? Now the observed distribution is used as an initial condition whereas I would think the observed distribution is representing the outcome of natural processes which are in equilibrium. Showing time series on how these grain size distributions are evolving through time could help.

8. It is unclear how soil production was modelled in this study. I assume the model is based on the soil production function as proposed by Heimsath [1997] but this is not stated clearly.

9. The bibliography is poor and not covering current state of the art knowledge on soil formation processes at the landscape scale (see e.g. references above).

10. Presentation of the results could be much better; The first three figures are more or less copy pasted from Cohen 2010. The others are mainly log-log plots. A few of these plots should be enough to illustrate the presented results.

Comments by line

p1

Line 1: The title is difficult to understand. I would suggest something more explicit and would consider mentioning the name of the applied model in the title (mARM3D).

p2

Line 5: Here and throughout the paper, I do not see the need to introduce this model as a 'new' model because it is almost similar to the previous mARM3D model.

Line 16-18: The influence off different depth dependent weathering functions has already been discussed in previous work [Cohen et al., 2010]. It is unclear to what extend findings reported here are different from this earlier publication.

p3

line 2-6: First paragraph is a bit difficult to follow. I agree that soils play an important role in environmental processes but try to give some hints to the reader on how to frame this.

Line 8: Add references.

Line 9: What do you mean with 'optimum performance'. Model efficiency?

Line 10: What are 'high quality spatially distributed soil attributes'? This is very unclear for readers who are not familiar with this matter. Try to be more specific, maybe give some examples.

Line 12. Grading: I suggest you explain this term the first time it is used.

Line 14 .. it are the soil. . .

Line 29: "However useful these PTFs are". Rewrite

Line 22-14 on page 4: This paper isn't about soil mapping, right? These paragraphs seem to be redundant given the nature of the paper. Rather, the authors could elaborate a bit more on soil-pedogenesis processes which are of real matter in soil genesis models.

Page 4:

Line 9: Please ad references to original work other than McBratney et al.

Line 17: I do not see how GIS products would have revolutionised the society through modelling. They can trigger each other but are not necessarily linked.

Line 24: Define armouring, example is given in Cohen et al. [2009]. First line of the introduction.

Page 5 Line 2: soil profiles

Line 3: remove 'using pedogenic processes'

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Line 6 I do not see how the need can be clear. In Cohen et al [2010] it is written: “The paper aims to confirm the robustness of the log $\hat{\sim}$ log linear relationship between area, slope and d50”. And further: “This suggests that the log $\hat{\sim}$ log linear relationship between area, slope and d50 is a robust result.” So, it has already been shown that there is a robust relationship between topographical properties and particle diameter. In my opinion, adding initial model configurations and plotting different particle sizes (d10,d90) is only of marginal additional value. What would be of real value is the integration of processes like creep, bioturbation and illuviation as suggested in Cohen et al. [2010]: Additional processes (e.g., chemical weathering, translocation) will be integrated in the future. This will allow for more complex studies of soil evolution processes and relationships. Our vision is that with additional development and validation mARM3D will provide insight into the quantitative processes leading to soil spatial organization and a detailed description of functional soil properties for environment models.

Line 8: shortly summarize what is meant with ‘generalize’ and ‘extends it numerics’. The reader should be triggered to continue reading which he isn’t. In the contrary, in its current form, the paper is not attractive to read and it is very vague up to this point what you are going to do exactly and why.

Line 12-13: SSSPAM5D: showing off with 5 dimensions does not really make sense here. Each LEM or soil evolution model has a temporal dimension. Soil grading is a property, not a dimension. I would suggest to just call it 3D. Moreover, I would avoid the use of another word for basically the same model and suggest the use of mARM3D rather than SSSPAM5D throughout the paper (see also comments before). If the authors insist on using a new name I would propose mARM3D.v2.0.

Line 14: It is recommended to restructure the introduction. First describe the processes you are going to deal with (Armouring, Weathering), then explain why there is a need for your study.

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Line 15-16: there is still not a good definition of what armouring exactly is up to this place.

Line 21: In line 15 armouring is a result of fluvial erosion . Here it is fluvial or wind erosion. Please clarify.

Line 21-31 Finally, the definition of armouring pops up. I would recommend moving it up. Refer to existing literature throughout the text rather referring to citations at one line (e.g. line 17). Try to avoid repeatedly citing the Sharmeen and Willgoose paper of 2006 and diversify the references .

Page 6

Line 14-15: Again, refer to the literature throughout the text.

Line 26-27: This is already mentioned before. What is exactly the influence of weathering to armouring, sediment fluxes and erosion rates. Have they observed positive feedbacks, negative feedbacks,... ?

Line 27-2: Mostly redundant or already mentioned before. Rather give the reader insights on what exactly were the findings of the ARMOUR model runs.

Page 7

Line 4. According to the reference list, mARM1D seems not to be the right terminology. It is mARM [Cohen et al., 2009] OR mARM3D [Cohen et al., 2010].

Line 4-5: “complex nonlinear physical processes” Which processes are complex? Which process is nonlinear?

Line 10-17: this is interesting and well explained, it would be good to summarize the results of ARMOUR in a similar way on page 6 instead of line 22-23.

Line 26: The authors should also explain in more detail what has been done with the mARM3D model as this is the model they use throughout the study.

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Line 28: “and allows more general assessments and predictions of pedogenesis” very vague. Clarify.

Line 29: ‘the extensions in’: the updates to the existing modelling framework mARM3D

Line 29: comparing your model with the mARM3D model is not a calibration, let alone a validation.

Line 31: Here you clarify that you are only going to investigate the soilscape. As already mentioned before, I find this of little additional value to existing literature [Cohen et al., 2010] where ‘more complex’ systems are already studied (using a DEM from a real landscape).

Page 8 Line 5-14: any differences with this model in comparison to mARM3D? It would be interesting to add or at least discuss the effect of a depth dependent creep function where in depth grading can be influenced by differences between incoming and outgoing grading properties of soil fluxes [see e.g. Roering et al., 2007; West et al., 2014; Campforts et al., 2016].

Line 6: builds on rather than extends

p 8-11: The methodology section is mainly a copy from Cohen et al. 2010. I suggest the authors refer to this publication for full details of the methodology and clearly indicate what exactly has been changed. I see two minor points of adaptation in comparison to the mARM3D model:

1. The introduction of an asymmetric distribution of weathered soil particles to smaller classes (a concept already used in other pedogenesis models [Vanwalleghem et al., 2013; Temme and Vanwalleghem, 2015]).
2. The use of a third depth dependent weathering function with the highest erosion rate at the bedrock-soil interface.

p14

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Line 13-14: what do you mean with 'input'. Are these used to constrain the initial particle size distributions of the uppermost layers? What about the other layers, are they set to bedrock?

Line 16-17: what do you mean with third and fourth gradings? From Table 1, I guess the 'third and fourth' layer are representing the bedrock of the first and the second gradings? Please rephrase. Very unclear what exactly Ranger 1b and 2b refer to.

p16

Line 10-14: Finally, the authors clearly explain what they are going to investigate and how it differs from earlier literature. To me, these tests are not of sufficient additional value. Verifying the impact of other soil processes mentioned before and evaluating these at the catchment scale would strongly improve this contribution.

Line 17-20: time series of particle size distributions would help to understand this.

Page 17

Line 20: Here, the authors admit they are reluctant to study the interesting soilscape-landscape coupling at its full potential. As mentioned earlier, there is already a good understanding of the 'simple' relationships both in terms of modelling [Cohen et al., 2010] and field data [Govers et al., 2006]. This paper would therefore be an excellent opportunity to indeed focus on these coupled models which can easily be tested with the efficient structure of mARM3D.

Page 18:

Line 21-22: Can this be confirmed by field data?

p19

Line 1: Cohen 2013, missing in the reference list.

Line 6: I am wondering why d50 values are so low in figure 7.a2 in comparison to figure

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7.a1. If all the parameters remain constant except for α_1 and α_2 , I would expect higher erosion rates (equation 3) for figure (a2) where α_1 increases from 0.639 to 1.359. Consequently, I would expect larger particle sizes for simulation a2 instead of smaller particle sizes. Can the authors clarify or explain this?

Line 8: which Ranger number for the bedrock? 1a/1b?

P 20

Line 18-20: as to no surprise because symmetric redistribution attributes the largest amount of material to the second daughter.

Line 23: Rephrase.

Line 32: Can the model also evolve to this equilibrium if one starts from bedrock as initial condition? p21

Line 16-20: Although logical and model simulations are not essential to get this, this is indeed interesting.

p22

Line 25: It would be good to illustrate how that can be done.

Line 26- p 23 line 9: Making the coupling to land evolution models is indeed interesting. Scaling up this finding from the soilscape to the landscape would be a very interesting contribution to this study. Given the architecture of the model I assume such upscaling can be done relatively easy. Also, it would be interesting to elaborate more on the physical implications of this finding. Are there datasets available confirming this trend?

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