Reviewers comments in italics; Authors response in non-italics

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 a 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 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.

We agree that SSSPAM is part of the ongoing development of the approach outlined in the mARM3D papers (we'll address detail differences in response to comments below). The reviewer sees it as a "major problem" that we have not included the full range of processes that are known to be important in soil profile process (though creep is a lateral transfer process rather than one that impacts on the vertical profile directly). The reviewer, however, has missed the intent of this paper which is to examine the generality and robustness of the area-slope-grading dependency of the soil profile arising from a vertical pedogenesis model. Digital soil mappers have discovered similar relationships with terrain attributes (personal communication with GRW) but Cohen's mARM papers was the first to have shown this with a process model. However, Cohen's work was calibrated to one set of climate, geology and process representation (the Ranger field site we have studied since the early 1990s) and before we can compare model results with other field sites we need to know what is the range of behaviours possible from the model. Then we see differences with the field we can, with some degree of certainty, say whether the differences we observe (we are not so naïve that we think we will get a perfect match) are outside what the model can generate, or whether it might simply be that the parameters in the model can be adjusted to match the field. Without an analytic solution for the area-slope-grading relationship (i.e. an equivalent to the slope-area result for topography in Willgoose, Bras, Rodriguez-Iturbe, WRR,

1991) we are limited to doing the general sensitivity study that is in this paper.

Finally we note in one of the comments below that one of the useful features of a sensitivity study like in this paper is that it can highlight what are testable predictions of the model, making it possible to design experiments and field studies that can potentially reject the model if it is wrong.

Major points, not in order of importance

1. In the abstract it is stated that the authors developed a new numerical model (SSSPAM5D) which generalises and extend findings from mARM3D (Cohen 2010). However, only small adaptations to the existing mARM3D model are made and similar synthetic simulations are performed (with altered parameter values). Findings from the previously published mARM3D 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).

As noted for reviewer 1 more complicated geometries of catchments (e.g. concave or convex 1D profiles or 2D catchment drainage) are not necessary because they generate the same results. The only link from node to node on the hillslope is via the runoff-erosion model, otherwise the soil profiles are independent. The erosion model used in these simulations is a detachment-limited model where the erosion rate is a function only of the upstream area and the local slope. For any given node it is irrelevant what the geometry of the upstream area is (it could a catchment or, as in this paper, a linear hillslope). We believe that the paper is complicated enough as it stands and that adding a 2D catchment as suggested by the reviewer would only add to the figures unnecessarily (particularly as the results are exactly the same). The addition of the sample hillslope profiles in Figure 4b was to highlight how our simulations apply to more complex geometries (commented on favourably by reviewer 1).

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.

The models are different and we have only presented enough detail of the model so that the reader can look at this paper and understand what the various parameters control without having to flip backwards and forwards to the both of the Cohen mARM and mARM3D JGR papers. Just to be clear the differences in the models are:

- mARM is written in Fortran and doesn't fully implement the matrix methods in the Cohen papers (the matrix methodology and mARM were developed in parallel, even though they were presented together in the papers). SSSPAM is written in Python/Cython and fully implements the matrix methodology and is as a result significantly faster (about 10,000X) and more flexible. Accordingly while they solve the same problem there is 0% overlap in the code.

- The fragmentation physics in mARM is hard coded in, whereas in SSSPAM it is quite flexible (the main reason for starting again with the code development).
- There are significant improvement in the numerics in SSSPAM which mean that the mass balance for a given time step size is significantly improved over mARM. The improvements are most noticeable for the sediment particles in the grading fraction encompassing the Shields stress threshold. This is the reason that the results in Figure 4 are slightly different (note that while the contours in both figures have the same slope the d50 for any specific combination of area and slope is slightly higher for SSSPAM). This is also true for the time varying approach to equilibrium (for brevity not shown in the paper) where SSSPAM provides a better to the ARMOUR model (which is a detailed physically based model).

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; 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.

Referring back to our response to point 2 above while Cohen 2010 described, using the matrix methodology, how SOME of these processes could be incorporated into mARM NONE of these have ever been implemented into mARM.

We now address the main point of the comment which is about processes that we have not tested. We are aware of all the work described by the author for natural soils and while many of these processes mentioned are important in different locations we know of other locations where none of these are important (our Ranger field site, and most other rehabilitated mine sites, where there is a predominance of granular material) so we feel its perfectly legitimate to start with the simplest model that has exhibited the slope-area-grading behavior and see if Cohen's results were just good fortune given the process parameters for Ranger or whether we might expect this behavior at other sites. This then provides the confidence that we can look at the sensitivity of the slope-area-grading result to these other processes at some later stage. What the reviewer is asking is that we do further sensitivity studies on the effect of these excluded processes (i.e. another paper) not that this paper is unnecessary.

The selection of the papers [Braun et al., 2001; Roering et al., 2007; West et al., 2014; Temme and Vanwalleghem, 2015] is also rather idiosyncratic and focused on the role of creep. Braun et al., 2001 does not model any soil grading properties and only models creep and soil depth; Roering et al., 2007 also model creep but do not study soil in any form at all, only topography; West et al., 2014 likewise looks at experimental evidence for creep in soils but does not model the implications; Temme and Vanwalleghem, 2015 do not model soil grading but do model a number of other processes though they do NOT show any results using these other capabilities.

In conclusion this comment primarily focuses on the lack of downslope creep in the model. Granted it is missing but you could equally criticize these papers for not including depth variations in the soil properties (which we are simulating) which will no doubt influence the depth dependent movement of soil downslope. You have to start somewhere and we've started on the processes important on our field site at Ranger. If the soil is dominated by granular material then the material grains tend to lock together and not creep downslope.

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.

Good point. We had referred to them in previous papers but neglected to include them here. They have been included.

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.

This is the first of a series of studies that we are currently writing up using SPPPAM5D so we thought rather have to repeat the model description and adding a discussion of the capability being evaluated in each paper we would write up the general model here and refer back to the single description in subsequent papers. These subsequent papers do the 2D catchments that the reviewer requests but go beyond the sort of analysis in this paper (e.g. looking at the impact of deposition and coupling with a landform evolution model). A minor point; the reviewer describes these landscape wide simulations as 3D but they are strictly speaking scalar fields on a 2D grid.

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).

The reviewer has misunderstood the work done in our Cohen et al papers. The calibration suggested by the reviewer is exactly what was done for the mARM3D papers, which means that the results are specific to that site and material. This paper is about generalizing results from that study. This point is clearly stated at P7 lines 23-2. We have however reworded his section to make this clearer.

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?

This is part of the sensitivity study. For constructed waste containment sites it is important to understand how the initial conditions might influence the results (do different initial conditions lead to fundamentally different results or is the effect of initial conditions lost over time … also an important fundamental question for natural soils but crucial for constructed sites). We have also done simulations from bedrock with no significant differences.

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.

The initial grading is from a mine site which has ben exposed to erosion and weathering for about 20 years and is far from equilibrium (see Sharmeen and Willgoose, 2007, who predicted that it will take about 100 years for the site to reach equilibrium, see their figure 13). The time evolution of the grading down the profile and down the hillslope is the subject of a separate paper which will be submitted shortly. We excluded temporal issues from this paper to keep this paper focused on the equilibrium area-slope-grading relationship.

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.

There is no explicit soil production function. Saprolite is turned into soil by the weathering function at depth. This is also how it was done for mARM3D though in this paper we have explored the impact of using of functions other than the exponential function. We can generate the exponential production function of Heimsath without having to input it (see the Cohen mARM3D 2010 paper for a discussion of this behaviour).

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).

As previously mentioned we're not sure that we'd consider creep a soil formation process, rather it's a soil transport process. Nevertheless, we have included some of these papers in the introduction.

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.*

Figures 1, 2, 3 are from our previous papers (as indicated in the figure captions) but in terms of understanding the model they are a very concise summary of the 3 key aspects of the model (the profile, the fragmentation model, and the depth dependent weathering function) so we think they are required (based on feedback from AGU and EGU presentations).

Figure 4 shows that SSSPAM can replicate mARM3D. Since it is an entirely new code base with improved numerics this provides confidence that SSSPAM is working correctly. Also Figure 4b shows how to interpret these contour plots for a more realistic hillslope profile.

Figure 5 is very similar to a figure in Cohen 2010 but with the new model, and is the nominal case against which all the subsequent figures are compared.

With respect to the other figures we have include one contour plot for every parameter changed if there has been a significant change. In several cases a parameter change did not change the contours so we just noted that in the text. Yes there a lot of contour plots but we have only included those that are needed to substantiate the conclusions of the paper.

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).

The model is not mARM3D. We have changed the paper title to something simpler but feel strongly that it should not include the name of the model since the results are likely to be independent of the details of the model

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.

As previously indicated this is a completely new code from the ground up so deserves a new name, but we have quite explicit in the paper that is based on the pioneering work of mARM.

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.

This paper examines the depth dependent properties of the soil than Cohen and a new one (an analytic solution to chemical weathering that is presented in Willgoose (2016) “Models of Soilscape and Landform Evolution”, Cambridge Press, currently in the editorial process). Cohen modeled them but did not examine the slope-area-grading relationship below the surface so was not able to conclude what the spatial organization of soil was below the surface.

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.

-The paragraph is changed to make it clearer

Line 8: Add references.

-new references added

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

-In this context the authors meant “provide accurate predictions” The manuscript is edited accordingly

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.

-the authors meant soil attributes such as Hydraulic conductivity, soil moisture content etc.

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

- The manuscript is edited according to referees comment

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.

The digital soil mapping community is very interested in pedogenesis models as a way of providing a better physical basis to their empirical regression methods for digital soil mapping (i.e. the GlobalSoilMap initiative). The World Soil Society in 2015 instituted a focus group on this subject for this very reason. Thus these paragraphs are not redundant but link to major initiatives outside the geology/geomorph community, which many readers may not be aware of.

Page 4:

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

-references added

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.

Poor wording on our part. The ease of use of GIS has revolutionized modelling by making distributed modelling easier to do and interpret.

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

-The structure of the introduction section changed and the definition has been moved up

Page 5

Line 2: soil profilesLine 3: remove 'using pedogenic processes'

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 logâĂŘlog linear relationship be- tween area, slope and d50". And further: "This suggests that the logâĂŘlog 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 par- ticle 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 valida- tion mARM3D will provide insight into the quantitative processes leading to soil spatial organization and a detailed description of functional soil properties for environment models.

The sentence has been removed and the rest of the paragraph reworded.

Cohen et al (2010) showed the robustness of the relationship with changes in in-profile weathering relationship but did not investigate the full range of parameter values because it was calibrated to field data. We agree about the importance of other processes but have prioritized developing a model that can be coupled to a landform evolution model (so we can do some of the lateral transport processes like creep). This coupling work is in the process of being finalized and will be published in due course.

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.

We have sharpened the introduction section to make it clearer earlier what the objective of the paper is.

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.

Since both reviewers are (unnecessarily) uncomfortable with the 5D terminology we will remove any reference to it and refer o the model as SSSPAM. However, as a matter of principle we disagree with the reviewers. The model is 5D. As the reviewer states the x,y,t coordinates are 3D, the soil depth is another dimension and the cumulative distribution function of the soil is a 5th dimension (technically the soil grading is a 5 dimensional scalar field). Again as mentioned above SSSPAM is a completely new code base. We thought long and hard about whether to continue to use mARM name for this version but decided not to, to avoid confusion with the original codebase for mARM (which the 4th co-author has extended with aeolian processes separately to this project … see, for example, JGR 2015, and another paper currently on ESDD). In passing we note that to be technically correct mARM3D is also 5D but at the time of that publication we were swayed by review comments along the lines of those here.

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.

-Introduction restructured

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

-Introduction restructured the definition is given earlier

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

-manuscript updated to clarity this issue

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 .

-Introduction restructured

Page 6

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

-references added

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,. . . ?

-The weathering process breakdown the coarse particles in the amour layer. This would increase erosion and the increased erosion draws coarse particles from subsurface layers depending on the rate of erosion. In the end it becomes a balance between erosion and weathering rather than negative or positive feedback. The introduction section restructured

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.

-sentences removed and consolidated

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].

Typo on our part.

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

-the entrainment of sediments by the flow is a complex non-linear process. Manuscript is updated to clarify this matter

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.

-summary of ARMOUR was added to the manuscript

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.

-a small discussion on mARM3D was added to the manuscript.

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

-re worded the sentence to clarify the aim of the manuscript

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

-As explained earlier the SSSPAM is a newly coded model based on physics of mARM3D. hence we believe what we have presented here is actually extensions that carry SSSPAM beyond mARM3D

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

Yes it is a calibration (it is also a validation because it showed that the model can mostly replicate the older mARM3D model). The model is calibrated against another model so we are comfortable that SSSPAM will reproduce the output of mARM for the similar parameters (this is part of the reason for Figure 4 to provide some assurance that this is the case). We are not claiming it is calibrated against a field site. For instance, mARM was calibrated to a much more sophisticated model (ARMOUR). ARMOUR, however, WAS calibrated against field data. We know this is very indirect but it is not possible to calibrate mARM or SSSPAM directly to the field data because of the approximations required in their conceptualisations. This conceptualisation is the key to the high speed of the model. If it weren't for this high speed we would have continued using the ARMOUR model, which has a strong physical basis, but which is so slow that's its infeasible to couple it with a landform evolution model (we'd some serious supercomputer time to even generate a single slope-area-grading contour figure using ARMOUR).

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).

Well actually the landscape used in Cohen 2010 was not a real landscape either but a design proposal for a waste encapsulation structure. We used it because it shows many features common to waste encapsulation structures so gave us some insight to what soils might look like on such structures. And as noted previously our previous work has confirmed that the results form a 1D hillslope are the same as for a 2D catchment given the formulation we have used for erosion (i.e. detachment limited), and the 1D hillslope is significantly easier to understand.

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].

The reviewer seems to be rather fixated on creep. Not all sites have creep as a significant process, particularly ones with a high proportion of granular material (the grains tend to lock together and not move).

Line 6: builds on rather than extends

-How the SSSPAM model is differentiated from mARM3D has been addressed earlier

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.

See our comments at the beginning of the review response. We disagree with the reference to the papers by Temme and Vanwallaghem as we are currently collaborating with T&V to do the exact work the reviewer claims is in those papers.

p14

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?

-Yes for most of the simulations the authors used the Ranger1a grading for the initial surface layer and Ranger1b grading as the initial grading for all other subsurface layers. Ranger1b is the corresponding synthetic bedrock layer for the Ranger1a actual grading. Ranger2a actual grading and the Ranger2b synthetic bedrock grading was used to assess the influence of different initial conditions on the area-slope-d50 relationship. The last paragraph of this section was changed to clarify this point

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.

-See the previous response

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.

I guess we have to agree to disagree about strategy of what should be done first. It only makes sense to do creep in the context of the coupled landform-soilscape modeling rather than on the standalone soilscape model because creep forces the landform to evolve as well as the soilscape. Nothing in this paper caused the landscape to significantly evolve (e.g. the amount of erosion is really quite nominal relative to the scale of the landform) so using it as is as a standalone soilscape model is OK.

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

Added

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.

As the reviewer has indicated we have already done this in Cohen 2010 and there is no value repeating the 2D simulations done by Cohen (it gives the same results as the 1D hillslopes). The coupling of the soilscape model and landscape evolution model has been largely completed and we are shaking the bugs out and analyzing the rather complex results as we speak. This work will deserve a paper in its own right building upon the insights of this paper.

Rather this paper is to look at the sensitivity of the results to changes in process rates and functional dependencies, which was not done in Cohen 2010. Sharmeen and Willgoose (2007) (where the behavior of the more complex physically based model ARMOUR model was examined) compared ARMOUR with results from Govers et al 2006. The comparison is quite good so since SSSPAM has been indirectly calibrated to ARMOUR we expect that the SSSPAM comparison will also be quite good. Some words to that effect have been added.

Page 18:Line 21-22: Can this be confirmed by field data?

We're not sure how you might collect the required field data. Clearly this would be a useful test of the model performance. We note here that one of things that the sensitivity study potentially highlights for us is what types of experiments and/or field studies might be able to test the model predictions.

p19

Line 1: Cohen 2013, missing in the reference list

Fixed

Line 6: I am wondering why d50 values are so low in figure 7.a2 in comparison to figure 7.a1. If all the parameters remain constant except for $\alpha 1$ and $\alpha 2$, I would expect higher erosion rates (equation 3) for figure (a2) where $\alpha 1$ 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?*

-Here $\alpha 1$is the exponent value for discharge and $\alpha 2$ is the exponent value for slope. For comparison the discharge rate is in the order of 10^{-6} $m^3/s/m$ while slope is in the order of 10^{-2}. So changing the exponent of discharge reduces the erosion at all the nodes with slope-area combinations as a whole, while changing the exponent on slope only change the slope dependence. Because of this low erosion rate in this figure weathering dominates all the nodes and the d50 of all the nodes reduce significantly.

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

Typo. Wrong naming convention was used. Edited the sentence and clarified the manuscript

P 20

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

Agreed but its worth explicitly noting that the model gives the result expected.

Line 23: Rephrase.

-There were some words missing from the sentence. Rectified and the manuscript updated

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

Yes

p21

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

A nice backhanded complement. Some results are obvious in retrospect, but it's the simulations that highlighted the behaviour in the first place.

p22

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

We are currently in discussions with members of the digital soil mapping community about this. Briefly they see the methods here as a physically based supplement to the empirical statistical regression methods they currently use to generate soil properties on grids. Their regressions are derived from regression of observed soil properties against terrain properties. We may be able to highlight terrain attributes that will provide more rational terrain covariates in their regressions.

Line 26- p 23 line 9: Making the coupling to land evolution models is indeed interesting. Scaling up this finding form 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?

The results of this coupling is quite challenging to understand (the effect of localized deposition; timescales of landform evolution versus soils evolution; spatial organisation of the topography … slope-area … when the soil is spatial organised as well; 1D hillslope versus 2D landscape) and will be in a future paper. The challenge of the coupled model, plus the field observation that soils at some of our field sites evolved faster than the landform (so soils equilibrate to the slowly evolving landform), is the reason why we think a focused study on understanding the soils model on a fixed landform is needed prior to looking at the coupled evolution.

References

Braun, J., A. M. Heimsath, and J. Chappell (2001), Sediment transport mechanisms on soil-mantled hillslopes, Geology, 29(8), 683–686, doi:10.1130/0091-7613(2001)029.

Campforts, B., V. Vanacker, J. Vanderborght, S. Baken, E. Smolders, and G. Govers (2016), Simulating the mobility of meteoric 10Be in the landscape through a coupled soil-hillslope model (Be2D), Earth Planet. Sci. Lett., 439, 143–157, doi:10.1016/j.epsl.2016.01.017.

Cohen, S., G. Willgoose, and G. Hancock (2009), The mARM spatially distributed soil evolution model: A computationally efficient modeling framework and analysis of hillslope soil surface organization, J. Geophys. Res. Solid Earth, 114(3), 1–15, doi:10.1029/2008JF001214.

Cohen, S., G. Willgoose, and G. Hancock (2010), The mARM3D spatially distributed soil evolution model: Three-dimensional model framework and analysis of hillslope and landform responses, J. Geophys. Res., 115(F4), F04013, doi:10.1029/2009JF001536.

Govers, G., K. Van Oost, and J. Poesen (2006), Responses of a semi-arid landscape to human disturbance: A simulation study of the interaction between rock fragment cover, soil erosion and land use change, Geoderma, 133(1-2), 19–31.

Heimsath, A. M., W. E. Dietrich, K. Nishiizumi, and R. C. Finkel (1997), The soil pro- duction function and landscape equilibrium, Nature, 388(July), 358–361.

Johnson, M. O., S. M. Mudd, B. Pillans, N. a. Spooner, L. Keith Fifield, M. J. Kirkby, M. Gloor, L. K. Fifield, M. J. Kirkby, and M. Gloor (2014), Quantifying the rate and depth dependence of bioturbation based on optically-stimulated luminescence (OSL) dates and meteoric 10 Be, Earth Surf. Process. Landforms, 39(9), 1188–1196, doi:10.1002/esp.3520.

Poesen, J. W., B. van Wesemael, K. Bunte, and A. S. Benet (1998), Variation of rock fragment cover and size along semiarid hillslopes: a case-study from southeast Spain, Geomorphology, 23(2-4), 323–335, doi:10.1016/S0169-555X(98)00013-0.

Roering, J. J., J. T. Perron, and J. W. Kirchner (2007), Functional relationships between denudation and hillslope form and relief, Earth Planet. Sci. Lett., 264(1-2), 245–258, doi:10.1016/j.epsl.2007.09.035.

Temme, A. J. a. M., and T. Vanwalleghem (2015), LORICA – A new model for linking landscape and soil profile evolution: Development and sensitivity analysis, Comput. Geosci., doi:10.1016/j.cageo.2015.08.004.

Vanwalleghem, T., U. Stockmann, B. Minasny, and A. B. McBratney (2013), A quantitative model for integrating landscape evolution and soil formation, J. Geophys. Res. Earth Surf., 118(2), 331–347, doi:10.1029/2011JF002296.

West, N., E. Kirby, P. Bierman, and B. a. Clarke (2014), Aspect-dependent variations in regolith creep revealed by meteoric 10Be, Geology, 42(6), 507–510, doi:10.1130/G35357.1.