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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 P.A. Finke (Referee #1) in italic font

Journal: ESurf Title: A coupled soilscape-landform evolution model: Model formulation and initial results Author(s): W. D. Dimuth P. Welivitiya et al. MS No.: esurf-2018-16

General Comments

The authors describe a quantitative model suitable to estimate evolution of some soil physical properties over the landscape. The model description and the presented mathematical formulations look OK. The manuscript is well-written. My major comments relate to the over-selling of the model as a pedogenesis model (see comment 1), to the linkage to the real world (2, 3) and to the clarity of model assumptions (4).

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. Referee #1 has raised 4 main issues regarding the current version of the manuscript. The authors will consider all the comments and suggestions made by the referees and accommodate them in the manuscript wherever it is possible. Authors response for each specific comment is *presented under each referee comment.*

1. One major comment, even an objection that I have is that the paper states at many locations that it concerns a soil genesis model. This illustrates a narrow vision on soil genesis, and comes entirely from a geomorphological perspective. In fact, only soil physical processes are considered, and not even all of these (e.g. heat flow and clay migration are no part of the model, the effect of SOC on erodibility is unaccounted for). It ignores that soil genesis involves many other processes, of mineralogical, chemical and biological kinds. See Bockheim and Gennadiyev (2000) for a list of soil formation processes and Minasny et al. (2015; Fig.5) for a check if these processes are covered by the soil models of to date. I therefore advise the authors to be clear in the ambi- tion level of this model, which is the mechanistic simulation of 3D-redistribution of soil particles of various size over the landscape. Mention perhaps “soil texture evolution model”, but not soil evolution model s.l.

   The authors do agree with the Referee #1s comment that only limited soil formation processes has been considered in SSSPAM. The model is based on physical fragmentation of parent soil particles and it does not model chemical transformations. The modelling approach used here is complimentary to the chemical weathering modelling work done by Michael J Kirkby [Kirkby, 1977; 1985; 2018]. However we will be incorporating a physically based chemical weathering model described by Willgoose [2018] into SSSAPAM in the future. Also at the current time we decided not to consider SOC and its influence in the soil formation and evolution processes. All available evidence suggests that in order to effectively model SOC, it will require an extremely complicated coupled model with soil grading, soil moisture, SOC as well as vegetation. Although formulating such a model is very desirable for the entire scientific community, it is well beyond the scope of this current research work. Considering the Referee #1s comment and the limited number of soil formation processes simulated in the model we have decided to use “Soil grading evolution” model instead of “pedogenesis” model

2. The evolution of the soilscape is only to a limited degree connected to physical boundary conditions such as rain, evaporation, heat/temperature. As I understand it, water plays a role to redistribute topsoil material, but does not influence the subsoil (linkages to weathering of minerals, clay migration). The weathering mechanism entirely concerns physical weathering, and the process is driven by 2 parameters n and alpha, which are empirical (section 2.4). True drivers of physical weathering are related to temperature fluctuations, and specifically the occurrence of frost. For these reasons the model is not fully mechanistic, i.e. does not represent the actual processes, but rather “functional”, it describes what happens and uses empirical factors to achieve this. This means that the model cannot

be used for studies on effects of global change on soilscapes, where differences in P, PE and T should drive the processes. I would invite the authors to discuss this item in the paper.

The authors believe that the Referee #1 may have misunderstood the complexity of the weathering mechanism and how the weathering rate of each soil layer at each pixel (node) is calculated due to the concise manner which it is presented. The 2 parameters mentioned by the Referee #1 only controls the weathering geometry (how many daughter particles and their relative sizes) the weathering rate of each soil layer is controlled by the depth dependent weathering function. The rationale behind these depth dependent weathering functions and how they relate to the "drivers of the physical weathering (specially temperature fluctuations through the soil profile)" are extensively described in Welivitiya et al.[2016]. The authors decided not to repeat the material in previously mentioned paper here due to manuscript length concerns. Also SSSPAM is capable of using depth dependent weathering functions for each simulation node (pixel) depending on the geographical distribution of various physical weathering drivers such as temperature. Also a slight modification to the weathering module in SSSPAM will be able to simulate temporal variations of these weathering drivers as well. So SSSPAM can actually be used to study the effects of global change on soilscapes in the future. For the initial simulations the authors decided to keep the simulation setup as simple as possible to better observe and study how the core parts of the models perform and to see whether the results reflect general trends observed on hillslopes in nature. In later stages more and more capabilities of SSSPAM will be activated and a wide range of multidimensional soilscape simulations will be possible. As the Referee #1 suggests a small paragraph is added to the section to state this fact.

3. To allow model testing beyond plausibility testing ("face validity"), which is attempted in the paper, additionally, confrontation to field data would be needed. This is clearly beyond the scope of this paper and, unfortunately, of most soilscape modelling studies. Some sensitivity experiments are done in this paper, which is commendable. I would expect a strong sensitivity of projected landscapes to the initial landscape as well, but this was not studied. This again touches the ambition level of this model: is it meant for synthetic studies or for real world cases?

The authors appreciate the Refree #1s understanding and grasp of the practical difficulties of comparing results produced through a model like SSSPAM with field data. 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. However 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 studies of 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. In these simulations we have identified that the model (even with a reasonable synthetic setup) was able to generate similar trends identified in nature in terms of particle size distribution and landform morphology (particularly for alluvial fans). So we believe that although highly simplified in terms of the number of pedogenetic processes, the model still can be used to explore real world cases.*

4. In general, some assumptions are not so clear. For instance: how does mass redistribution relate to the elevation of the soil-atmosphere interface, in other words, how are mass and volume connected. OK, via the bulk density (for erosion in eq.4; for deposition in eq. 7), but is bulk density then assumed a constant and not affected by bioturbation, strain by weathering? Is this valid over 60.000 years? Are there other assumptions that should be known?

   The authors agree that some assumptions made during the model development phase may have been omitted from the manuscript. Revised the manuscript and assumptions are clearly presented

A few specifics:

- l.83: “scorpan” not introduced; this is in fact clorpt+soil point data+position (see McBratney et al. 2003), thus not so different.

- *The authors agree that scorpan is in fact a further development of clorpt as the Refree #1 has pointed out. The sentence regarding scorpan was revised and the association of scorpan to clorpt is introduced.*

- l.573: erosion and d50 correlate: is this a model artefact? For instance, if the organic matter content would be simulated as well, would it not become part of the correlative complex?

Yes, all soil components are part of the correlative complex however extensive work has demonstrated that d50 is strongly related to erosion.. However this relationship may be true for natural hillslopes as well due to the process of armoring. i.e

High erosion means ability to erode relatively large particles which leads to higher d50. If we incorporate the effects of SOC we would imagine that it will definitely come in to the correlation complex

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- l.689: soil formation and its evolution? =repetition.

There seems to be a repetition in processes describing soil formation and evolution. The sentence was reworded

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- Refs:

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