Geoscientific
Model Development
Discussions

Interactive comment on "SaLEM (v1.0) – A Soil and Landscape Evolution Model for simulation of regolith depth in periglacial environments" *by* Michael Bock et al.

T. Coulthard (Referee)

t.coulthard@hull.ac.uk

Received and published: 14 November 2017

The overall aim of the paper is good - and the contribution of a new/alternative way of integrating a soil development model, within a Landscape Evolution Model (LEM), within the SAGA GIS is great.

There are some issues that I think the paper would benefit greatly from being corrected. Mainly structure/writing/presentation but also some science. There are more detailed comments and corrections in the annotated PDF attached.

Firstly, the introduction is very hard to follow, and would benifit from a re-structuring.

C1

Most of the main items, points and justifications are there, but could be far more clearly presented with a structure first outlining the background science, the research gap and how this model/paper will address that gap. This should also bring in background of existing models that look at Soil/LEM interaction (including that by Willgoose and co-workers, VanVallegan and Temme etc..).

The description of the model operation and its parameterisation is generally good and easy to follow. With the odd sentence that is hard to follow (identified in the PDF).

The validation section is rather downbeat $\sim$ this does beg the question is it worth developing a model for this area if you have no/so little data to compare it to? It's interesting to note that whilst the model predicts the pattern of soil development correctly (slope, peak, base etc..) it over estimates the regolith thickness considerably (by >3). I can't help but wonder if this is simply having too high rates in the weathering/generation of soil from the parent material - and this could be greatly improved by reducing these rates? In effect calibrating the model? I would suggest this as a good improvement for the model performance and that section of the paper.

Some of the arguments in the discussion/conclusion seem a bit out of place for a paper that is describing the development of a new model - these may be better saved for a different future publication that may look more at what the results mean (rather than how the results were made).

Please also note the supplement to this comment:
https://www.geosci-model-dev-discuss.net/gmd-2017-218/gmd-2017-218-RC1-supplement.pdf

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2017-218, 2017.

C2