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Earth Surface Dynamics Discussions

Interactive comment on “Soilscape evolution of aeolian-dominated hillslopes during the Holocene: investigation of sediment transport mechanisms and climatic-anthropogenic drivers” *by* Sagy Cohen et al.

Anonymous Referee #1

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This paper describes the application of a coupled pedogenic-geomorphic model to a semi-arid field site in Israel. The authors demonstrate that a model that combines transport by diffusive hillslope processes (creep and bioturbation) with transport by overland and rill flow does a better job at reproducing observed soil depths than a model that contains just one of these transport types. The paper concludes that different parts of the hillslopes tend to be dominated by one of the two transport mechanisms: diffusion at the top and fluvial at the bottom.

That a model that includes both fluvial and diffusive processes works better than one

with just diffusion or just fluvial processes does not strike me as a significant conclusion. I don’t understand why the authors would run simulations with diffusive processes only or fluvial processes only, given that all landscapes clearly have both of these transport types occurring.

Model concerns:

Equation 1: Nothing like equation 1 appears in Engelund and Hansen (1968). In sediment transport the flux usually goes as the square root of the excess density, not the square of excess density (which is usually s-1, not 1-s as shown here). The assumption that sediment flux is linear with water discharge (i.e. n_1 = 1) is inconsistent with all sediment transport formula in the literature. The publication year is 1967, not 1968, and Engelund’s name is misspelled.

Equation 2: Why do the authors assume n_4 = 0.1? No reason is given. This is an extremely low scaling relationship between discharge and area.

Routing: D8 routing is inappropriate for hillslopes. The authors need to use a multiple flow direction algorithm. As the authors state, the unrealistic “striping” of the model output along 45 deg angles is a result of the routing algorithm. However, it is very strange that the striping occurs only for the diffusive simulation, which does not involve routing at all as far as I can determine from the text.

Equations 3&4: This does not look like hillslope diffusion. The authors have assumed that the colluvial transport rate increases as the 0.1 power of slope, not the usual linear formulation (or the nonlinear formulation of Bucknam and Andrews, Roering et al., etc.). No results of the calibration used to obtain beta = 0.1 is provided. The units of the various parameters are very hard to keep track of and clearly wrong in some cases. D_s should not have units of time because the time step is included in equation 4 (D_s should have units of m, not m/yr). What is k? What are its units and its value?

Equation 5: The reader is referred to Minasny and McBratney (2006), which is not in the reference list. When I tracked down Minasny and McBratney (2006) I found a rather different equation (their equation (4)). Equation (5) is dimensionally incorrect. It is wrong to have the steady state weathering rate appear inside the exponential – the argument of any exponential should be unitless. Why are delta_1 and delta_2 equal to 4 and 6? What are the units? If they are meters these are very large values (i.e. they imply that weathering rates fall off by a factor of e only once the soil is at least 4 m thick. This is a very thick soil).

How the equations are combined is not clear. There must be some conservation equation being used in the model (e.g. erosion rate is related to the divergence of sediment flux), but this is not shown. I did find something like a conservation equation in Cohen et al. (2015), but that equation is dimensionally incorrect (the erosion rate (which has units of L/T) is equated with sediment flux, which has units of L^2/T).

Fluvial erosion from hillslopes is generally modeled as a 2-step process: 1) rainsplash disturbance of soil aggregates to liberate them into the water column and 2) size-selective transport. Only the second process is considered in this model.

The model does not include the vertical redistribution of aeolian material (aeolian deposits stay on the surface). In nature, the reason why an argillic horizon forms is that aeolian fines are redistributed downward in the soil profile. Therefore, I don't see how this is a realistic model for pedogenesis.

Calibration concerns:

Some of the model parameters are chosen ad hoc (i.e. beta = 0.1, n_4 = 0.1) with no apparent calibration. Some are simply chosen based on the default values in other studies that may or may not be realistic for the study site in Israel. No data were used to relate climate changes to the model parameters. The "change factor" values and how they were modified over time may be qualitatively correct but the absolute values appear to be ad hoc. Some data must be used for calibration.

More broadly, the model has so many parameters (I lost count – a table of parameters, their units, and their chosen values would have helped) that I cannot see how a search of the parameter space could possibly have been done to find the optimal values, except via a Bayesian approach such as MCMC. Calibrating a model with 10 or 20+ parameters to a dataset that constrains only one element of the system (soil depth in this case) has to be done very carefully if it can be done at all. In cases where model parameters were matched to the observed data using an "extensive parametric study", no details are provided. This makes it very difficult to have confidence in the conclusions. When the model "fails" to match the data for the fluvial case or the diffusive case, perhaps it is simply that the model hasn't been properly calibrated.

The paper concludes that different parts of the hillslopes tend to be dominated by one of the two transport mechanisms; diffusion at the top and fluvial at the bottom. I don't see how the numerical experiments support the conclusion that fluvial processes dominate at the bottom. This conclusion is inconsistent with Tarboton et al. (1992) and many more recent studies (Perron et al., 2008; 2009) that conclude that the transition from diffusive to fluvial dominance occurs at the channel head. As long as one is on an unincised hillslope, diffusive processes should be dominant everywhere according to the published literature. The discrepancy between the results of this paper and previous studies could simply be a result of the very unrealistic value of beta (0.1) chosen with no justification. Certainly fluvial erosion must become relatively more important at the base of the slopes compared to the top because the contributing area goes to zero at the top (hence the importance of fluvial transport must go to zero at the divide). However, I do not see any evidence in the paper that fluvial processes dominate diffusive processes at the base of the slope.

Other concerns:

The authors state that soil development is dominated by aeolian processes at their study site but no evidence is provided to demonstrate this. There are now a number of studies that use immobile elements (Ti, Zr) to quantify the relative dominance of aeolian input versus in situ weathering of parent material in soils. So, it may be possible to constrain this yet I don't see how this was done in this study. Similarly, the key motivating question of the study is whether soil degradation is caused by climate change or anthropogenic forcing. I don't see any evidence in this paper that soils were depleted (i.e. that they were thicker and have now thinned). Moreover, the question of whether soil degradation occurred by climate change or human activity relates primarily to the timing of the soil degradation. There is no geochronology or other evidence presented to address this question. These are two examples of many in which facts were assumed about the study site without evidence.

I don't understand why the observed soil depth (Fig. 7D) shows "spikes" in the plot. The color map from Fig. 2 does not show these spikes in the data.

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