Anonymous Referee #1

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.

First of all we would like to thank the referee for this very thorough review. Below we provide a point-by-point response to the reviewer's comments.

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. The reason why is clearly and repeatedly described and discussed. In a nutshell: the relationship between these transport types are well explored in bedrock-weathering dominated but not in aeolian soilscapes. Moreover, we investigate the effects of temporal variability in external drivers (i.e. climatic/anthropogenic scenarios, a key aspect of the overarching research) that seems to, based on our literature review for this region, differ for the two transport mechanisms. We therefore need to isolate each mechanism in order to identify their specific spatial and temporal dynamics. The conclusion of this study is NOT that both transport mechanisms are in play but rather a suite of insights on their specific dynamics and interactions.

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 We disagree with this statement (see page 48 in Engelund and Hansen (1968)) but acknowledge that we should have been more specific. This equation was based on the TOPOG model sediment transport calculations which used the Engelund and Hansen (1968) equation (http://www-data.wron.csiro.au/topog/user/contents/frame1.0.html). This is now clearly outlined in the revised manuscript.

(which is usually s-1, not 1-s as shown here). True, this was a typo but make no difference to the result as the expression is squared and s is constant.

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 n1 and n2 values are in line with the TOPOG values.

The publication year is 1967, not 1968, and Engelund's name is misspelled. Corrected.

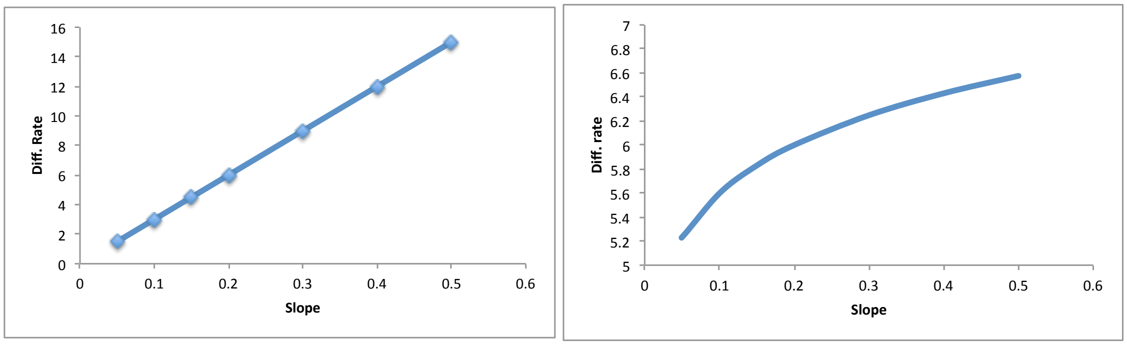
Equation 2: Why do the authors assume n_4 = 0.1? No reason is given. This is an extremely low scaling relationship between discharge and area. It is explained: *"In Cohen et al. (2010 and 2015) the relationship between contributing area and runoff discharge was assumed to be linear (n4=1). This assumption could not be justified in our field-site as Yair and Kossovsky (2002) showed that runoff generation in this region does not increase linearly downslope. We therefore use n4=0.1 in this study."*

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. This limitation is discussed in previous papers. One of the main advances in the mARM model is its computational efficiency which allowed, for the first time, for such explicit and long-term simulations at landscape scales. Recent advances with this framework and similar models may now allow us to consider more runtime-expensive routing algorithms. This is a topic for a whole different paper.

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. Diffusion absolutely involved routing, how else does the model transport the sediment down the slope? It is, however, not directly affected by contributing area and so there is no downstream scaling and the stripping occurs along the hillslope in places where the flow directions are parallel.

Equations 3&4: This does not look like hillslope diffusion. This stem from our attempt to simplify a complex 5D (x, y, z, t and PSD) algorithm in a couple of equations. As we describe, this is a novel equation and is described and explored in more details in Cohen et al. (2015). It is now better explained in the revised manuscript.

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. In Cohen et al. (2015) we used a linear relationship (b=1) and have conducted an extensive parametric study for beta for this study. See the relationship between slope and diffusion rate for beta = 0.1 and 1 in the plots bellow. The fact that beta differs from 1 to such an extent in this field-site is actually very interesting! We propose that it could be another aeolian-driven affect (fine PSD, absence of armoring etc.) or is due to the relatively steep and concave down shape of the hillslopes in this site (so site-specific). This is now explained (in the model description) and discussed (in the discussion section) in the revised manuscript.



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). Not true! Equation 4 doesn't have a time step parameter, it describes changes in D (m/s) down the profile. In practice it makes not difference.

What is k? What are its units and its value? "…k is the surface diffusivity (m)" its values are described in section 2.4. We changed k to D_o to distinguish it from k_a.

Equation 5: The reader is referred to Minasny and McBratney (2006), which is not in the reference list. Corrected

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. The equation was indeed (as stated) modified, the Po parameter was moved to allow for an above-zero watering rate at the surface while depth rates down the soil profile asymptote to zero. Cohen et al. (2010) focused on the model weathering equations and algorithm. We can see how the description of the model weathering calculations is confusing and misrepresentative. This stems, again, from our attempt to simplify a complex algorithm into one equation with time-varying parameter. The full algorithm includes compiling a transition matrix which control the transition of PSD in each particle size class to smaller class(s) in each soil-profile layer. The algorithm was described at length in Cohen et al. (2009 and 2010). The depth-varying weathering rate equation in the model (see the actual model FORTRAN function below) is not directly used to calculate weathering rate, rather the relative (normalized) change in weathering down the profile. As part of the revised model description we removed the section describing the weathering calculations as it does not include parameters that are modified in the simulation scenarios we analyzed in this paper.

```
!##########################################################################
! A function to calculate the decline in weathering rate as a function of depth
! It return the WeatheringAlpha which is different for every layer
! The exponential function is taken from Minasny & McBratney (2006)
! de/dt=Po{Exp(-k1h)-Exp(-k2h)}+Pa ;
! Their original values are: Po-potential WR=0.25; k1=4; k2=6; Pa- steady state WR=0.005
! The function was changed in version 4.5.1 to account to close to zero weathering in
lower layers
!###########################################################################
    REAL*8 Function DepthWeatheringAlpha(WeatherAlpha,i,LayerDepth)
    IMPLICIT NONE
```

```
     Integer i
     Real*8 WeatherAlpha, LayerDepth, Ratio, Depth
     If (i==1) Then
     Depth=0.5
     Else
     Depth=(i-1)*LayerDepth-(LayerDepth/2)
     End If
     Depth=Depth/100 !convert to meters
     Ratio=(0.25*((EXP(-4*Depth+0.02))-(EXP(-6*Depth))+0))/0.04 !We divide by 0.04 to
normalize it
     DepthWeatheringAlpha = WeatherAlpha*Ratio
     Return
     End Function DepthWeatheringAlpha
```

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). Following on the comment above, the model algorithm uses the Minasny and McBratney (2006) equation to get the relative change in weathering rate down the soil profile. These variables therefor control the shape rather then the actual weathering rate. Their values were based on Minasny and McBratney (2006) to maintain the relative change (i.e. shape of the hump function) and so in reality they are unitless.

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). Again, as we showed above, there is no dimensionality issue with the actual implementation of the equation. As we stated at the start of section 2.2 the full description of the model architecture is provided in Cohen et al. (2009 and 2010). The model description in this paper is limited to the equations "that include the parameters that are modified by the simulation scenarios we analysed here." A short description of the model architecture was added to the revised manuscript.

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. Not exactly, the sediment transport process is lumped. We argue that this is appropriate for the scales (primarily temporal) we simulate. Soilscape evolution is extremely complex and numerical models cannot, and are not intended to, exactly and fully mimic it. Models are useful for simplifying complex dynamics, allowing us to isolate parameters and processes and test hypotheses and concepts. Modeling results must be interpreted within the model assumptions, a concept that we carefully follow.

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. Same comment as above, consider the scales and focus of this study.

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. Expending on our previous comments, the goal of this study is not to precisely predict soil dynamics but rather to isolate and conceptually analyze specific processes and dynamics. That can only be done with numerical models given the complexity and longevity of many of the processes involved. Over the years we have used best available data to calibrate some of the model parameters. In a limited and qualitative ways this is what we have done here with observed soil distribution. However we have found that using observed data to calibrate a specific model parameter is extremely problematic as, almost always, the parameter dynamics (spatial and temporal) cannot be sufficiently isolated form the observations. For this field site we actually have quite detailed paleoclimate and hydrological data (references are provided in the manuscript) but we intestinally simplified it. We did so for two reasons: (1) it allows for a much clearer analysis and (2) we are not attempting to precisely predict soilscape dynamics. This again relate to our previous comment about the use of models for soilscape evolution studies.

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. See comment above, these issues were address in the last four papers about this model.

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 is explained at the results and discussion sections: each of the transport mechanisms resulted (when simulated alone) in fairly distinct soil dynamics. When the two were simulated together their signatures were visible in different parts of the soilscape.

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.

Good, now you are getting to a main goal of this paper – investigating the potential differences between aeolian and bedrock ("normal") soilscape evolution. These comments support our assertion and results that the two are indeed different - though the references and your comment focus on channel-hillslope interaction and we are only looking at hillslope processes here. We admit that the use of the term 'fluvial' is confusing in this context but we explicitly explain this in the introduction. This is stated and discussed in the manuscript, even in the context of our previous study: "In Cohen et al., (2015) we found an opposite trend for bedrock weathering dominated soilscapes." (section 4. Discussion).

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. As stated above we did provide justification to beta. See comment above about equation 4.

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). True but this is a highly spatially and temporally dynamic change, not as simple as you describe it.

However, I do not see any evidence in the paper that fluvial processes dominate diffusive processes at the base of the slope. This is discussed in the manuscript. In a nutshell: if you compare soil depth evolution between the three simulations (Figures 4-6) and the cross section results (Figure 7) you will see that diffusion led to thick soils at the base in contrast to the fluvial simulation. The combined simulation looks a lot like the fluvial simulation at the base and like the diffusive at the middle part of the hillslope. We acknowledge that this is a qualitative observation but (as commented above about model assumptions and simplifications) we assert that it is most appropriate.

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. The loess belt in the northern Negev has been well studied; including the rates, extent and dynamics of aeolian deposition in this region (some references are provided in the manuscript). Indeed all one need to do is walk the site and see the extent of loess deposition and how little soil is produced by bedrock weathering. That been said (and repeating an earlier responses), this is actually not a crucial point for this conceptual 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*. No, this is the overarching motivation not the goal of this study. The goal of this study is to gain conceptual insights into the soilscape evolution and the impacts of time varying parameters. As described above, we intentionally used broad-brush estimates of climatic and anthropogenic changes. Ongoing research (which will couple field and modeling efforts) is looking into the question of whether or not soil ever accumulated on the hillslopes. These points are clearly stated in the manuscript.

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. The soil map (Fig 2) is the product of interpolation between measurement points with exposed bedrock (classified form aerial photography) "burned" as zero depth (white color in Fig 2). This was explained in section 2.1. If you look closely you could see these zero-slope ("spikes") along the transect.