Anonymous Referee #2

This paper presents a simulation of soil-landscape evolution in a semiarid zone of Israel under fluvial and diffusive sediment transport.

The paper is nicely written and the method is clearly described. The results are stimulating, offering possible soil-landscape evolution pathways.

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

My comments are that some of the parameters are arbitrarily chosen, e.g. the humped model of weathering. While theoretically it is sound, but no published result yet showing such weathering parameters. As discussed at length in the response for review #1, it is true (and acknowledged in the manuscript) that many of the model parameters cannot be explicitly and directly calibrated even if extensive field data was available. This stems from the difficulty in isolating specific parameters from field observations, the longevity of the processes simulated and their complex interconnectivity and spatiotemporal dynamics. That is one of the motivations for developing a soilscape evolution model and for this study, isolating processes and parameters, allowing us to develop (and conceptually test) hypotheses on soilscape evolution pathways and drivers. That being said we have, over the last several years, focused our analysis on specific processes and parameters, gaining important insights into some of the model parameters. Most specifically, and relating to this comment, is our extensive analysis of weathering equations and parameters in Cohen et al. (2010). Moreover the hump weathering equation we used was adopted from Minasny and McBratney (2006) which is based on their extensive research.

The authors wrote: "Limestone bedrock typically results in limited soil production by weathering except for producing a Mollisol" The statement on limited production and Mollisol is not necessary true, Mollisol development is due to accumulation of loess and organic matter. So why is this not true? We state: "…typically results in…" and go on to explain what is and is not actually simulated.

Another limited assumption is aeolian deposition which is uniform, what are the particle sizes of the aeolian deposit? Silt-size? 2-20 um? Good point. The aeolian deposition PSD is the same as the one we used in Cohen et al. (2015) and is shortly described there. We now clarify that in the manuscript: "We use the same PSD as in the fine-grained simulation in Cohen et al., (2015), with a d_{50}=0.06 mm." (section 2.1.3).

The simulation is run for 16,000 years. It would be beneficial to see how much of the "soil" is due to bedrock weathering and how much is due to aeolian deposit. As describe in section 2.4, bedrock weathering is assumed to be very low (0.01 mm/y), an order of magnitude lower tan aeolian deposition (0.1 mm/y). Also an explicit analysis of the two soil production mechanisms was explored in Cohen et al. (2015).

There is no mention of vegetation effect? Is there a possible feedback between vegetation

and erosion? Of course, as well with bioturbation, crusts etc. These kind of caveats were discussed quite extensively in previous papers (particularly in Cohen et al., 2015). This fact is now described in the manuscript.