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Interactive comment on "Origin of the power-law exponent in the landslide frequency-size distribution" *by* Ahoura Jafarimanesh et al.

S. Hergarten (Referee)

stefan.hergarten@geologie.uni-freiburg.de

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The manuscript addresses the scaling exponent of the power law distribution that has been widely found for landslide sizes. As this power-law distribution appears to be universal, understanding its origin could indeed be a major steps towards understand landslide dynamics and finally improve hazard assessment. So the topic is clearly important and fits very well into the scope of NHESS.

As the topic is also close to by personal interests I enjoyed reading the paper. However, although I am familiar with the topic I had difficulties at many points, so that I am afraid that the majority of the readers will not be able to follow. My feeling is that the explanations are much too short.

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Beyond this, I am not completely convinced whether the results are really as good as they look, and a more thorough discussion is needed. The main result seems to be that the fractal topography makes the power-law distribution and the propagation process has a minor effect. In this context I would request the following questions to be answered.

1. Several approaches for calculating the factor of safety (FS) are considered. However, as long as the parameters are constant, FS is a function of the local slope. What would be the result if we just assume that points where the slope exceeds a given threshold are unstable instead of the rather complicated FS?

2. If the result was basically the same if we only measured the sized of patches steeper than a given threshold slope angle as suggested in (1), would the result be surprising for an artificial fractal topography? And how close would a real topography come to this?

3. The thickness of the soil layer is rather low at least in the pristine fractal topography. This means that even removing the whole soil at one point cannot lead to a very strong decrease of the FS at the upward neighbors for the 10 m grid spacing used here. So is it really surprising that the propagation does not have a significant influence compared to the initiation? And would this change if we used a finer grid at the same soil thickness?

4. The statistics seem to be rather small, and the range of sizes where the power law distribution is visible is rather narrow. Why are the data sets so small?

In the following I list some more points where the explanation should be improved, provided that the points raised above can be addressed.

Page 1, line 23: I would not consider weathering as a trigger, but rather as a long-term driving force.

Page 2, lines 21–22: The results of Pelletier et al. (1997) should be explained a bit more in detail in order to see more clearly in which sense the new results differ.

Page 5, line 1: I did not get what it means that the number of unstable cells reaches a plateau.

Page 5, line 28: I thought a relationship like $\alpha = 1.5\alpha_V$ would be valid in the cumulative sense, which would imply $\alpha - 1 = 1.5(\alpha_V - 1)$ instead of the relationship given in the manuscript.

Table 1: It would be good to include a short list of the parameters here.

Figure 1: The three plots are so narrow that it is not easy to read them.

Figure 2: What is this topography good for in the context of this manuscript?

Figure 4: The meaning of m is not immediately clear here.

However, I would like to emphasize that these points are only details, while the four aspects mentioned above are more fundamental for me.

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