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Interactive comment on "Slope–Velocity–Equilibrium and evolution of surface roughness on a stony hillslope" *by* Mark A. Nearing et al.

Anonymous Referee #2

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Review of Slope-Velocity-Equilibrium and evolution of surface roughness on a stony hillslope by Nearing et al. (2017) for Hydrology and Earth System Sciences

General comments

This is a very interesting paper that I really enjoyed reading and commenting, on a topic known to be an open question in environmental fluxes, in absence of any well-established modelling framework. I have provided quite a large series of comments (more on the conceptual side of the work, to enhance its genericity or enlarge its views) but I am certainly willing to recommend publication once my comments have been addressed. Congratulations for this work!

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Discussion paper

Specific comments

P1 L25-26

Here is where I first wondered whether the paper would be generic enough, in its methodology or at least in the discussion section, to also encompass stream or river processes in the mentioned similarities. In accordance with the suggestions and comments I have provided to enlarge the discussion section, I think "formative discharges" leading to step-pool structures (for example) could also be mentioned here to run the comparison over several scales in size and energy dissipation, because the processes at play seem to be pretty much the same to me. Later in the paper, you also mention headcut erosion which typically refers to an intermediate scale but appeals to (or may fit within) the same description.

A few papers I had in mind: Bennett, S. J., Alonso, C. V., Prasad, S. N., and Römkens, M. J. M.: Experiments on headcut growth and migration in concentrated flows typical of upland areas, Water Resour. Res., 36, 1911–1922, 2000. Church, M. and Zimmermann, A.: Form and stability of step-pool channels: research progress, Water Resour. Res., 43, W03415, doi:10.1029/2006WR005037, 2007. Grant, G. E.: Critical flow constrains flow hydraulics in mobilebed streams: a new hypothesis, Water Resour. Res., 33, 349–358, 1997. Grant, G. E., Swanson, F. J., and Wolman, M. G.: Pattern and origin of stepped-bed morphology in high-gradient streams, Western Cascades, Oregon, Geol. Soc. Am. Bull., 102, 340–352, 1990.

P2 L17-18

The interplay between bed material and the geometrical patterns of erosion is very interesting, we are near the generic issues of geomorphology here, hence my encouragements to keep describing the aims of the present paper without losing the connection to similar processes at different scales. This is certainly a strength of the present approach that is to be kept and reinforced.

P2 L19-20

Here I see the same stability argument as in the P1 L25-26 comment. It is not completely explicit in the introduction though, at least not in the terms I propose. Or am I wrong?

P4 L31 - P5 L20

The experimental procedure is clearly and precisely described. However, I suggest to add a figure that would bear all temporal indications to help the reader have all in mind. I am asking this especially to have a clear picture of the succession of transient and asymptotic or stable conditions in terms of rain intensity and flow discharge (with the associated variations of erosive power). Attached is the type of figure that I was missing and I think it is needed to discuss a few points (thus maybe more in the discussion section but relying on the temporal aspects of the experiments).

- My first point is about the duration of the transient stages and the part they may (or may not) play in the overall results and thus in the analysis. It is not clear to me whether this point has received a dedicated attention or not, and if not, why. In other words, do the authors think that their interpretations are more suitable for steady-state conditions or will hold whatever the succession of conditions? I am very interested in the answer and the issue could probably be handled through the precise indication of "which recordings were made and when".

- My second point is a corollary: if transient periods were not supposed to play a significant role, then the conclusions drawn would certainly be related to the "peak levels" (here steady-state levels) only. Then there is a very interesting feature of fine system dynamics: is there a time needed at a given discharge level for erosion to effectively take place? And if so, is it within reach of the present study or out of reach and maybe out of scope?

- I have a third point with another ricochet from the experiment section to the discussion section. Protection measures were taken to prevent splash erosion here but to the authors knowledge, is there a way (or literature elements) to indicate whether and in which conditions splash erosion would have a significant effect or even a dominant effect?

If I correctly remember (and among others) the Nearing and Bradford (1985) paper deals with "energetic" arguments to explain particle detachment, Planchon and Mouche (2010) discuss splash effects on microtopography and Josserand and Zaleski (2003) expose the favorable conditions for splash erosion to be the most effective. In my opinion, given its quality, the present paper is not far from reaching wider goals when insisting just a bit more on its genericity, possibly through a reasoning in terms of energy.

Josserand, C. & Zaleski, S. 2003 Droplet splashing on a thin liquid film. Phys. Fluids 15, 1650

Nearing MA, Bradford JM. Single waterdrop splash detachment and mechanical properties of soils. Soil Science Society of America Journal. 1985;49: 547–552.

Planchon O, Mouche E. A physical model for the action of raindrop erosion on soil microtopography. Soil Science Society of America Journal. 2010;74: 1092–1103. doi: 10.2136/sssaj2009.0063.

P7 L4

Is there a physical meaning attached to the coefficients? Could the authors comment on the information "hidden" in the differences between (3) and (2)?

P7 L25

This emphasizes the role of antecedent conditions in delineating the conditions of validity of the present study or similar studies. More generally, is it possible, in the opinion of the authors, to infer the conditions of validity of the present study from the ranges of values of the key parameters and variables?

P8 L4

In connection the previous comment, is this statement attributable to the fact that flow depth stays smaller than random roughness? Here I have in mind a few studies that dealt with shallow flows through emergent obstacles, for various spatial densities of the obstacles, in laminar to weakly turbulent flow regimes.

Dunkerley, D.: Determining friction coefficients for interrill flows: the significance of flow filaments and backwater effects, Earth Surf. Proc. Land., 28, 475–491, 2003. Dunkerley, D.: Flow threads in surface run-off: implications for the assessment of flow properties and friction coefficients in soil erosion and hydraulics investigations, Earth Surf. Proc. Land., 29, 1011–1026, 2004. Lawrence, D. S. L.: Macroscale surface roughness and frictional resistance in overland flow, Earth Surf. Proc. Land., 22, 365–382, 1997. Lawrence, D. S. L.: Hydraulic resistance in overland flow during partial and marginal surface inundation: experimental observations and modeling, Water Resour. Res., 36, 2381–2393, 2000.

P8 L9-10

Headcut erosion is mentioned here and I also think of step-pool formations.

P8 L11-16

This is another very interesting point according to me. The derivation of the Manning and Chezy relations not only hypothesized uniform flows but also steady-state conditions and fixed boundaries. There is again this need to better identify the conditions of validity (either empirical or based on physical arguments) for theses formulae, or ad hoc adaptations outside their nominal "terms of use".

Technical corrections

P1 L11 Insert blank spaces between values and units

P2 L3 I am not sure the equation is needed here: the statement seems enough.

P2 L4 No need to repeat "(>0.5 cm)"

P2 L10 I think more precision is needed for non-specialist readers. "Is also associated with increased hydraulic friction thus with reduced soil erosion, as the energy needed for detachment is larger"

P2 L13 How was erosion assessed? Total mass export?

P3 L7 "rates" is a bit vague

P3 L11-13 The formulation could be made clearer, in my opinion.

P4 L2 "m" instead of "meter"

P4 L6 Here and in the following the correct notation is "h-1" and not "h-1". Please also check the Figures.

P4 L7 "h" instead of "ha"

P4 L7-8 The oldest reference should be cited first (I think there is at least another occurrence)

P6 L3 "of all" instead of "all of"

P6 L4 Insert a blank space after "10"

Table 1 I suggest to add indications of the starting Random Roughness in all cases

P7 L21 "appear"

P8 L20 A line jump (or carriage return) should be used before the paragraph that starts with "The fact that", because it pushes the previous arguments towards other implications.

Fig. 2 "(>0.5 mm)" to be coherent with the rest of the paper

Fig. 8 The (a) to (d) labels are missing The legend would look better in the same format in all sketches

Fig. 9 The (a) to (b) labels are missing The number of significant digits should be corrected

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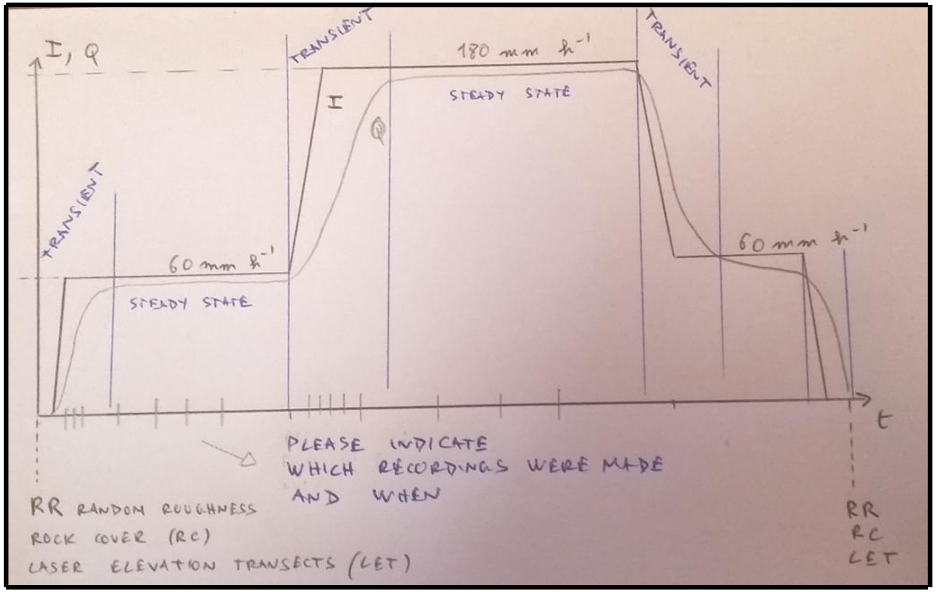
I, Q
TRANSIENT
TRANSIENT
180 mm h^{-1}
TRANSIENT
I
Q
STEADY STATE
TRANSIENT
60 mm h^{-1}
STEADY STATE
60 mm h^{-1}
t
PLEASE INDICATE
WHICH RECORDINGS WERE MADE
AND WHEN
RR RANDOM ROUGHNESS
ROCK COVER (RC)
LASER ELEVATION TRANSECTS (LET)
RR
RC
LET

Fig. 1.