Earth Surface Dynamics Discussions

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Comment on esurf-2020-107

Anonymous Referee #2

Referee comment on "Particle energy partitioning and transverse diffusion during rarefied travel on an experimental hillslope" by Sarah G. W. Williams and David J. Furbish, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2020-107-RC2, 2021

The authors present high-speed camera measurements of rounded and angular grains interacting with a rough surface in drop-rebound and downslope transport experiments. Their major findings are that (1) grain-surface collisions with angular grains convert more gravitational energy to rotational than do collisions with more rounded grains, leading to generally shorter travel distances; (2) the transverse rate of spreading is dependent on the available downslope translational energy; and as a consequence, (3) the rate of transverse spreading is contingent on grain shape as well as surface roughness, as more angular grains have generally less downslope momentum available for conversion to cross-slope momentum due to their more rapid loss of gravitational energy into particle rotation.

The paper is good and I see no major issues with the scientific ideas. The presentation is mostly easy to follow, the experiments are well-designed and thoroughly explained, and the analyses in the paper easily convince the reader of its main conclusions. Although these conclusions may seem rather specific to grain-scale sediment transport processes, the authors do a nice job of explaining the wider implications for hillslope evolution, so the work should be of broad interest to many ESurf readers. However the writing includes some perhaps unnecessary terminology, some variables and concepts are explained at length without ever being directly linked to the rest of the paper, certain explanations intended to provide intuition can be rather slow moving, and several figures could be improved to better explain the experimental configuration. I therefore recommend publication with minor revisions, as I explain below for the authors.

Main suggestiouns:

A key issue is that a lot of background info you included in the paper (Secs. 1-3 and appendix A) does not directly contribute to the central findings of the paper and is readily available elsewhere. For example, Eq 1. and appendix A describe the evolution of topography under sediment transport, but you never measure or calculate elevation change. Similarly Eq 2. represents the sediment flux, but you never measure or calculate a sediment flux. Eq. 7 introduces the length scale of deposition, but this quantity is never mentioned again. The exceedance probability Eq. 13 is used repeatedly, but the non-exceedance probability Eq. 12 is trivially related, available elsewhere, and is not used once. The text at L175 seems to constitute "relevant theory" suggesting it belongs instead in section 3. The presentation of the Furbish et al (2020a) model for the generalized Pareto distribution in section (3) is described carefully but this material could be briefly outlined instead, as this theory is available in Furbish et al (2020a). Finally, much of the discussion setting up a conceptual particle cohort between L110 and L125 could easily be cut without causing any confusion.

Given these observations, I suggest to reorient the material in section 3, 4, and Appendix A toward the main stream of the paper. One way to do this is to cut Eq 1, Appendix A, Eq 2, and Eq 12, paraphrase L110-125, and incorporate the paragraph at L175 into Section 3. Perhaps given the reference to the ideas underlying the 2D Exner equation near L444 and your desire to include it, you could state the equation there at L444 without a derivation by referencing Paola and Voller (2005), then use its structure to support your discussion about topographic smoothing. Finally, since you analyze two dimensions, I suggest to make Figure 1 two-dimensional, so it shows diffusing down-slope particles, collisions without friction, the coordinate system, and the definition of β_x and β_y if possible. This would help the reader to clearly understand all elements within your experiments. For brevity, you might consider combining Figure 1 and Figure 6 in a two-panel figure, showing the experimental setup with the particle launcher as an inset in one panel, and the conceptual diagram in the second. This is just one suggested set of revisions to focus the introduction of the paper toward the problem at hand. However you choose to do this, the paper would certainly be easier to follow after some effort to focus the introduction (mainly in section 3) to fit the paper's narrative, highlight the additional spatial dimensions of hillslope sediment transport you've analyzed, and define important variables in a modified Fig. 1 concept sketch.

Minor comments:

L1: The abstract is nice. However, self-citations of recent papers in the first line of the abstract to support well-known ideas runs the risk of appearing egotistical. I suggest "Particle motions down rough hillslope surfaces act to balance energy supplied by gravity against energy dissipated by collisions" (or similar) so the authors claim less ownership of these well-established ideas (from Kirkby or earlier).

L28: Isn't it Einstein 1937, not '38?

L32: Do you mean to say "with the associated mechanics of particle *transport*"? -- are you discussing disentrainment in particular? Or do you mean to say that the entrainment rate and the particle travel distance distribution leads to the deposition rate?

L41: What is your distinction between disentrainment and deposition? This is individual grains vs many grains? If so, is the distinction used consistently?

L50: Suggest "summarize the relevant theory" with regard to the long text I wrote above about reorienting the intro toward the objectives

L79: Regarding the "heating" and "cooling" terminology, as mentioned on the reviews of some other recent ESurf papers by the authors, it is certainly not consistent with temperature concepts from gas theories, which define temperature as the velocity fluctuations away from the ensemble mean velocites, not the ensemble mean velocities themselves (as in the present context). It's much like calling a second moment of position a variance, which is wrong. The terminology does not cause conflicts at this stage, but we have to wonder if it will as granular gas ideas become more integrated in the grain-scale sediment transport theory which you are advancing here. The more standard acceleration/deceleration terminology poses no such issues as far as I can tell, and in fact it makes ideas more clear (to me). For example consider a modified Fig 2 caption: "(a) A<0 representing collision-dominated deceleration and (b) A>= 0 ... representing gravity-dominated acceleration." What about this well-established (probably hundreds of years old) terminology needs reworking?

Fig 2 caption: "Plot of..." is not needed unless you really want it. It's clearly a plot of something! This same comment applies to many figure captions in the paper.

L180: This is not exactly accurate. Plenty of granular gas theory studies consider the restitution coefficient a random variable, whether directly or by parameterizing it by the particle velocity. See for example Serero et al (2015).

L185: Gravel-sized ?

L194: 20202020

Figure 4. This is a nice plot! I see that you fit the data to the CDFs. I am curious though why you choose not to indicate the empirical frequency distributions in panels (b) and (d) regardless? This would lend visual symmetry to the plot and provide an alternative perspective on departures from the Gaussian/beta fits.

209: "Between" these figures?

L232-L235: This along with Fig. 5 is a nice explanation of how roughness encourages noncollinear collisions, which then give rise to torques about the center of mass. Yet the continuation of this explanation from L238-249 seems to add complication without producing additional insight

L255: Videos are great !

Figure 7: If it's easy enough, you might modify the y-limits on the S=0.28 (bottom right) panel to remove the excessive white space.

Figure 7 caption: The concavity of these semi-log plots indicate whether particles are accelerating or decelerating - or am I wrong? Thermal collapse = deceleration toward zero velocity?

L291: generalizeD

L294: high-speed, particles launched

L295: Pareto

Table 3: high-speed: actually you should search for all missing hyphenations. There are more.

L336: Not sure how the "dictated by the geometry of the porous medium..." is relevant here. Are you presenting diffusion in porous media as an analogue of top-down diffusion? If so this was not clear to me. I am also confused by your distinction between dispersion and diffusion, or are you using these terms interchangeably? It seems up to now the word "diffusion" has been avoided, but later it gets drawn in with relation to Seizilles et al 2014.

Figure 11. This figure appears twice (although I expect this would not survive the copy-edit anyway)

L399: Wouldn't Fickian have a slope of 1?

Figure 12. S=0.18 panel subtitle you can see little accidental icons from your image-

editing software.

L425: Particle properties, ... , influence ... (not influences)

L535: Chartrand ?

Citations:

Paola and Voller (2005): https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2004JF000274

Serero et al (2015): https://www.cambridge.org/core/journals/journal-of-fluid-mechanics/article/hydrodynamics-of-binary-mixtures-of-granular-gases-with-stochastic-coefficient-of-restitution/4AAD60FEC0F3FE86B8BBF979A589DF7A

Abramian et al (2019, 2020): https://journals.aps.org/prl/abstract/10.1103/PhysRevLett.123.014501 , https://journals.aps.org/pre/abstract/10.1103/PhysRevE.102.053101