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Interactive comment

Interactive comment on "Rarefied particle motions on hillslopes: 1. Theory" *by* David Jon Furbish et al.

Joris Heyman (Referee)

joris.heyman@univ-rennes1.fr

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1) Theory

This first companion paper is the master piece of the serie, presenting all theoretical developments.

State of the art: The literature review has been placed after the theoretical developments (Section 5 Related formulation), which, in my opinion, do not help to globally envision the originality of the proposed formulation with respect to existing ones, and understand the main challenges of the hillslope problem. I would suggest the authors to better highlight the originality of their approach based on a succinct literature review from the very beginning. This could also help to introduce the important variables.

Summary of findings: In addition to this originality statement, I believe that a simple summary of findings should precede the detailed theoretical developments. In contrast to the book format, we expect in a journal article to have a rapid understanding of the main results. I had to wait for the summary provided in the second companion paper to make me a clear mental image of the main ingredients of the theory proposed, which I have expressed this way : 1. Particle Mass conservation dN/dx = - N/Ea 2. The variation of the ensemble average energy is constant (since forces are constant ?): dEa/dx = Cst → Ea = Ax+B Thus, the mean disentrainment rate is P=- 1/N dN/dx = 1/(Ax+B), and the PDF of travel distances is a Pareto distribution, in place of the classical Exponential distribution found when P is a constant. Such ultra-simplified preamble would ease a lot the navigation into the details of the theory latter on.

Terminology: I understand the analogy between statistical physics of gas and motion of particles down a slope, although I am a bit skeptical on translating all the technical vocabulary for this situation. For instance, the terms “thermal collapse”, “iso-thermal” and “net heating” are not fully transparent with respect to gravity driven motions, and will remain obscure for a majority of readers. In my opinion the notion of “heat” in a gas refers to zero-mean velocity fluctuations, and is thus not perfectly suited to describe a net shift of mean velocities as is the case in non-equilibrium particle motion driven by gravity. I understand the authors conceive the thermal collapse as a net decrease of particle energy and the heating as a net increase of particle energy. However, if they would extend their statistical formulation to the evolution of higher statistical moments of energy states, there will be a confusion between drift (mean velocity) and diffusion (fluctuations around the mean). My suggestion would be to simply use the transparent terms of mean “deceleration” and “acceleration” of particles ? One of the drawback of using energy balance instead of mass and momentum conservation is that well defined (and measurable) variables such as particle velocity and acceleration are lumped into an energy state, which is less tangible to the observer. Then, it is very easy to understand the disentrainment rate in terms of a decelerating particle (disentrainment probability growing with x, A>0) or accelerating motion (disentrainment probability decreasing with x, A<0).

Fokker-Planck equation: I understand the authors objective to cast their analysis into a fully probabilistic framework, although I did not get the necessity here to derive a complete Fokker-Planck equation for E if none of the higher moment are used latter on. Indeed, the authors introduce betaˆ2 (diffusivity of the energy state), which is never used afterwards. Why ? In my opinion, the shape of the pdf (Pareto) is only dependent upon the evolution of the disentrainment rate probability, not on the FP description of energy states. This is a ‘simple’ non-homogeneous Poisson process. Introducing the FP formulation is thus somewhat confusing for the main message. If this FP equation had an importance for the description of the difference between harmonic or algebraic average of the energy states (Ea, Eh), it might have been preferable to introduce this concept differently (I personally did not get this distinction entirely).

Meta-stability : Being familiar to the study of Quartier et al. 2000, I wondered if the theoretical description proposed by the authors is also able to explain the occurrence of meta-stable states of motion due to micro- roughness. Indeed, depending on the initial particle velocity, a particle may be trapped by bed roughness or continue its motion indefinitely. I would have liked to find a mention of this somewhere in the text.

Quartier, L., et al. "Dynamics of a grain on a sandpile model." Physical Review E 62.6 (2000): 8299.

Specific Points : - p5 l8 : I did not get in which sense these probabilistic formulation are “scale independent” - p5 l17 : “can be a constant determined” - p8 l9 : “The law of the unconscious statistician” ...which means for an unconscious reader ? - p9 l15: This sidebar could come before, at the beginning of the section - p10 l20 : “So bear with us” . This do not presage good... - p11 l25 : Think of moving this didactic sidebar in annexe - p 12 l 23 : What does “immaterial” mean in this context ? -p15 : The authors mention “deposition” in granular gases. I do not understand well how particles can deposit in absence of boundary. Do the authors mean “aggregation” ? -p16 (39) and

(40) : beta and betaˆ2 have the same units ? -p19 l6 “disentrainment rate, consistent with the deposition rate.” I do not understand this. -p20 l25-30 This paragraph is very confusing for me. Could you reformulate it in simpler way ? -p28 l17 m g mu cos theta -p30 l 24 : What is thus the importance of gamma in a model then ? -p32-l18 : Why is it problematic ? -p37 l5-10 : This could have been introduced at the beginning! -p38 l21 : recall what is alpha

Please also note the supplement to this comment:
https://esurf.copernicus.org/preprints/esurf-2020-98/esurf-2020-98-RC1-supplement.pdf

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