Review of "Rarefied Particle motions on hillslopes" (Joris Heyman)

Global comments:

These 4th companion papers are all very relevant for the different messages and new results they convey. I have fully enjoyed this tough but inspiring reading. I have no major comments to make, although, as explained in part 3 and 4, I suggest submitting the last two studies separately (in esurf or other journal), since their scope is much more general than the hill-slope problem.

While pleasant, the writing style contains many "didactic sidebars", "anecdotes" or humor that do not ease the understanding of an already complex message. I sometimes felt more like reading a book than a journal article (the 4th papers format do not help concision neither). Beside precise structural points (see part 1 of the review), I would tend to think that it is possible to globally shorten the text, summarizing the ides, without altering the important results and transferring extra materials in supplementary material.

Specific comments:

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 beta^2 (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 beta^2 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

2) Analysis :

The second companion paper present results from an experimental study of particle travel distances down a slope, launched by a catapult system. Data is compared to previous experiments and field studies in an exhaustive manner and tested again the theoretical elements provided in the first companion paper (e.g. the expected Pareto distribution of travel distances). Data is well presented and well detailed so that I believe the 2nd study can be published within minor changes.

First, I do not exactly see why high speed imaging is used apart from determining launched velocity. Indeed, all the results shown in Figures present travel distances that can all be determined without video.

Second, I am not sure to understand how the Pareto fits to the experimental distributions are obtained : by fitting the Pareto parameters, or by estimating them independently with high speed imaging (such as the \beta_z collision restitution parameter) ? I believe the theory would prove very robust if all parameters could be estimated independently via imaging (or other technique). This point is not clear enough and I would suggest the authors to clarify this while presenting their experiments.

Third, it is somewhat disappointing not to see any particle trajectory plotted, that would show the 'heating' (acceleration) for steep slopes, or 'cooling' for milder slopes. I believe much information can be extracted from an acceleration / velocity diagram, as was done for bedload transport in the authors' 2012 paper serie.

Other comments:
Fig 9 and 10 (and maybe others) : recall what is \beta_z in the caption so that each figure is understandable by itself.

3) Entropy :

The third paper has been the hardest for me to follow, because it touches concepts from statistical physics, that are less frequent in the earth science research community. For what I understood, the author claim to generalize the maximum entropy principle to several energy-based physical constraints. With this approach, they find similar Pareto distributions as with the varying deposition probability framework developed in companion paper 1. I believe this is an important result that goes far beyond particle motion on hillslopes, so that I am not convinced that associating this study as a companion paper is a judicious choice. In my opinion, proposing this study to a more physically sound readership journal than esurf would have a greater impact (Physical review ?). However, I rely on the editor's and other reviewers point on view for this.

Other comments:
p1 l15 "… that is heavy-tailed for net cooling and light tailed for net heating" Isn't it the other way around ?!
(3) precise that A can be between -B and infinity ?
(16) What is notation E[] for ? You have already used it for energy…
p19 l 7: What is Occam's razor ?

4) Philosophy :

The fourth paper present a general discussion on probabilistic approach to rarefied particle motions. It correctly points the generality of such approach, and shows how continuum equation of motion extend (within some subtle extra terms) to ensemble average quantities or probability distributions, even when the instantaneous particle flux is strongly intermittent.

While I completely agree with this viewpoint, and I believe the paper has its importance for the community, I am not sure how this relates specifically to the hillslope motions. Indeed, the use of ensemble averaging/probabilistic description to describe rarefied gas, bedload, or avalanches, and the scale dependence of fluctuations, is a much more general discussion that could fit in a standalone study, with dedicated title. Indeed, the 4th papers format dilutes in my sense the distinct messages the authors convey. Nevertheless, if the editors and reviewers think the inclusion of this paper as a companion paper is justified I will not argue against this.

One minor comment is the following. The authors point 2 equivalent probabilistic viewpoints, the Fokker-Planck equation (the linearization of the master equation) and the maximum entropy approach, originating from statistical physics (they discussed in the 1st and 3rd companion paper). In the discussion, I would include a third way, the Poisson representation [1], which has the attracting characteristic of being exactly equivalent to the Master Equation, while leading to continuous, analytically tractable PDEs. This approach, developed by Gardiner, can be used [1,2] to compute the exact particle number pdf and correlations from basic entrainment/disentraiment rules, without requiring a “small” noise or Kramer-Moyal expansion that assume a large number of particles. As pointed by Gardiner, it has the potential to describe “low density-high fluctuations” states of granular gases, for which large deviations play an important role. A mention of such alternative could be relevant.

1 Gardiner, C. W. (1985). *Handbook of stochastic methods* (Vol. 3, pp. 2-20). Berlin: springer.
2 Ancey, C., & Heyman, J. (2014). A microstructural approach to bed load transport: mean behaviour and fluctuations of particle transport rates. *Journal of Fluid Mechanics*, *744*, 129-168.
3 Heyman, J., Ma, H. B., Mettra, F., & Ancey, C. (2014). Spatial correlations in bed load transport: Evidence, importance, and modeling. *Journal of Geophysical Research: Earth Surface*, *119*(8), 1751-1767.