Review of:
Scales of collective entrainment and intermittent transport in collision-driven bed load
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General Comments

As Lee and Jerolmack point out, the recent work of Ancey, Heyman and others concerning collective entrainment represents an important step forward in focusing our attention on this problem as a significant part of the behavior of gravel-bed rivers, particularly near threshold. The innovative birth-death formulation of entrainment and deposition developed and elaborated by Ancey and others provides a natural probabilistic lens for highlighting the possible significance of collective entrainment. In turn, the work reported by Lee and Jerolmack in this paper represents a natural experimental extension of the idea of collective entrainment, with a focus on particle collisions, in terms of how this process contributes both to entrainment and to the intermittency of transport.

A straightforward, and clever, element of the experimental design is that transport begins only with the introduction of particles to the flow. This in principle isolates the effect of collective entrainment by collisions, rather than needing to parse these events from entrainment by fluid motions (a particularly difficult experimental problem) — although the flow certainly contributes to the continuing motions of introduced particles, and of particles following their entrainment by collisions; and it also likely contributes to entrainment of particles previously destabilized by collision events. The set up allows for direct observation of collective entrainment by collision. The ideas concerning the release of clusters similar to avalanching is particularly interesting, and the analysis concerning the relation between mobilized particles and the extraction of kinetic energy during particle collisions highlights the importance of examining this problem probabilistically.

The comments and questions below are aimed at seeking clarification of certain elements of the presentation, in part because the results likely will be used by others, and in part because I am curious about aspects of the interpretation.

Specific Items

Page 1, Line 23: Although I appreciate the intention of the phrasing, I suggest rewording, or just deleting the word "unfortunately."

Page 2, Line 15: The idea of "hysteresis" used here is unclear. Is this point needed?

Page 2, Line 21: Is it correct that the mean hop distance can be calculated from the entrainment probability?

Page 2, Line 32: Does the collective entrainment rate increase, or just its relative contribution, as the transport rate decreases?

Page 3, Line 7: I recommend replacing "exponential" with nonlinear.

Page 3, Line 9: I recommend replacing "continuum" with "continuum-like," as the rarefied conditions involved in this problem do not satisfy the continuum assumption. The referenced expressions are continuum-like only in that they have the continuous form of continuum equations, when in fact they pertain to ensemble expected conditions.

Page 4, Line 28: typo present []

Page 8, Lines 11–19: It is unclear what quantity is being calculated. (I gather later that this is either the activity or the flux, normalized by the average?) What is the total time series length from which the 500 samples are drawn (for specified τ)? Do these samples overlap (representing the possibility of non-independence)? How might this algorithm differ from calculating the quantity of interest directly from the series with successively increasing duration τ (a standard approach for examining convergence)?

Page 9, Line 9: Were these identified acceleration spikes used in a specific manner later? For example, in the later calculations of kinetic energy changes? If so, are these component accelerations, or computed as changes in trajectory resolved speeds? At 120 fps, I can readily imagine that the acceleration time series are quite noisy, such that identifying impacts involves significant uncertainty?

Page 9, Line 25: The appropriate comparison is with an exponential distribution rather than a Poisson distribution. The latter is a discrete distribution of the (integer) number of events occurring within a specified interval, assuming a stationary Poisson process. The exponential distribution describes the wait times between successive events, assuming a stationary Poisson process. (Is this why the dashed line appears to segmented parts in Figure 6? Where the kinks in the line coincide with integer values $\geq 10^0$ and the left part of the line is extrapolate toward zero?)

Certainly the data (if expressed in terms of numbers of events) could be compared with a Poisson distribution using the average number of emigration events occurring during a fixed interval — if it is demonstrated that this number converges to a finite value, if the number of events is large, and if the probability of the occurrence of an event in the fixed interval is sufficiently small. If these conditions are not satisfied, then a comparison with the binomial distribution is preferable.

In any case, the key point that is being examined is whether the emigration events can be treated as being a Poisson process (whether viewed in terms of the waiting times, or in terms of numbers of events per specified interval), or whether the data in fact suggest absence of randomness in emigration events related to the collective entrainment process upstream. The exceedence probability of an exponential distribution plots as a line that curves downward in log-log space (Figure 1), as the data do in Figure 6. (Might I suggest also exploring a semi-log plot as is done late with respect to cluster sizes in Figure 9.)?

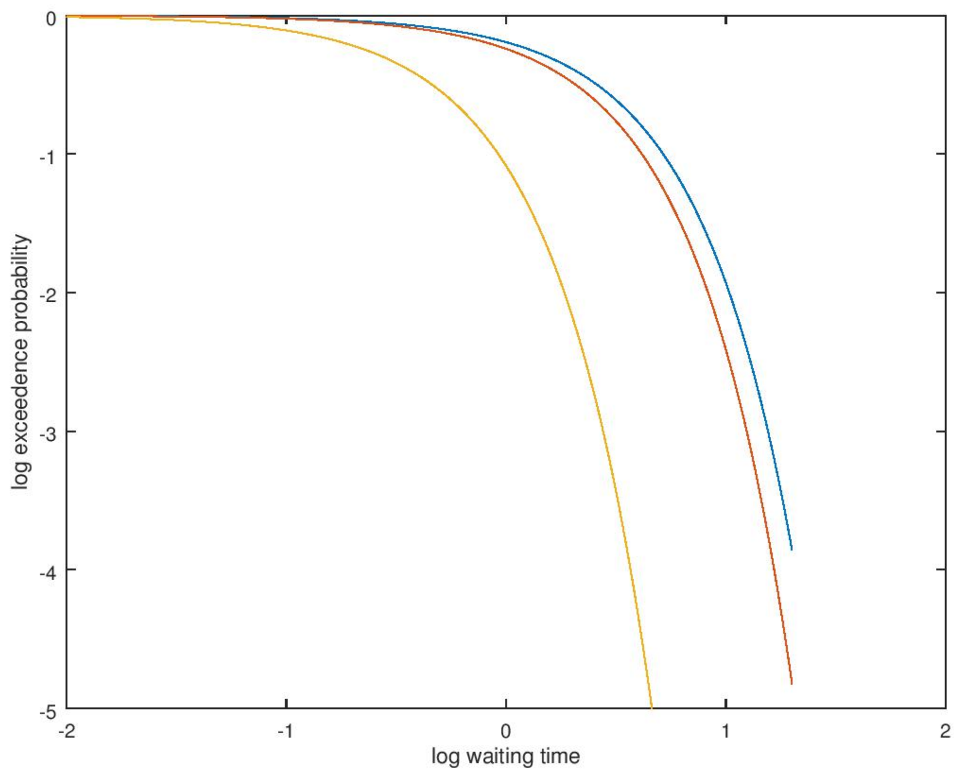

Figure 1: Plot of the exceedence probability function associated with an exponential distribution of waiting times for three values of the average waiting time.

When I use three of the average waiting times from Figure 7 (including the smallest and the largest) and compare this figure with Figure 6, my sense is that the author's conclusions stand, that an exponential exceedence probability decays too fast relative to the data.

This in turn begs the question of what these heavier-than-exponential tails imply. The plots suggest that relatively long waiting times are more likely to occur than what is expected from a random process, presumably bookended with increasingly likely clusters of events dispersed amongst smaller waiting times, hence intermittency. When considered from the point of view of survivorship analysis, the plots suggest that the likelihood of "ending" a waiting time decreases with increasing waiting time (relative to that expected for a random process). The longer waiting times presumably reflect temporary upstream "storage" of particles... essentially a preconditioning of bed conditions that yields to avalanching... whose effects tend to be separated in time with small feed rates, then involving increasing overlap as feed rates increase?

From Figure 7, I estimate mean waiting times between emigration events of 2.3, 1.8, 0.75, 0.4 and 0.35 s. Recognizing error in my estimates, these translate to rates of 26, 33, 80, 150 and 171 MPM. The last three are close to the input rates (40, 60, 80, 160 and 200 MPM).

The first two are a mismatch. Do these reflect a mismatch in the (total) time averaged mass balance? Namely, if I understand correctly, the mean waiting time W_m in minutes is $W/60 = 1/f_{output}$, where f_{output} is the number of particles emigrating per minute. Averaged over a long period of time, then with steady conditions, $f_{input} = f_{output}$, so $W \sim 60/f_{input}$. Correct? The forms of the distributions in Figure 6 are unknown. Perhaps calculations of the (changing) average and variance as the time series lengthen might offer a hint whether these moments converge.

Page 10, Figure 4: Might I suggest simplifying this figure such that it just shows the unit impulses versus time? As presented, one might be tempted to conclude that an emigration event can involve a fraction of a particle.

Page 12, Line 6: How are particle speeds defined? Are these streamwise speeds ($u_p = \Delta x/\Delta t$)? Or speeds involving two dimensions ($\sqrt{u_p^2 + w_p^2}$)? Would this matter?

The phrase "slow speeds (< 0.1 mm/s) are associated with essentially immobile particles" implies that all particle speeds are being calculated, not just the speeds of those particles defined previously (Page 8) as being mobile. Is this correct? If so, does the bimodal behavior vanish if only mobile particles are considered?

Page 17, Figure 10: I am a bit confused by these figures. First the horizontal axes. In 10A, KE has only positive values, so I assume the definition means that a decrease in particle KE means a positive KE "deposited into the bed," and I assume this represents the KE deposited by individual collisions. In 10B, the horizontal axis spans negative values (although it appears no negative values are plotted), but the magnitude plotted on the positive part of the axis is very different from 10A. So what is the significance of the negative values shown on the axis? According to the caption and the axis labels, this axis (and the magnitudes involves) should be the same as in 10A? Now the vertical axis in 10B. The orientations of the violin plots represent distributions of KE, not of the number of moving particles. But the caption implies that these pertain to the numbers of moving particles — which is the vertical axis. Do the violin plots actually need to be oriented vertically?

Regarding Figure 10A, once a particle is set in motion, presumably there is some hand off to fluid forces, which, together with particle-bed collisions, sets the total travel distance (a random variable) of any individual particle. If so, then this travel distance should not "care" about the KE extracted from the collision that started it, only its own collisions with the bed, which generally tend to shorten its travel distance relative to what would occur in the absence of these collisions. In turn, the "cumulative displacement of all mobilized particles" (which I am assuming is the sum of all travel distances of particles initiated from a collision) must therefore strongly depend on the total "number of particles mobilized," which does care about the KE extracted. It seems that 10A and 10B share information.

Both the KE change with a particle collision and the number of particles mobilized from this collision are random variables. From a statistical mechanics perspective, these define a potentially valuable joint probability distribution. (Each of the violin plots in 10B then reflects a conditional distribution of the number of mobilized particles for a given KE extraction.) Might it be possible to illustrate some rendition of this joint distribution? And its marginal distributions?

If I correctly understand the information contained in Figure 10, it highlights the idea that neither the KE extraction nor the number of mobilized particles is a deterministic quantity. This also means that, in this problem, the effective coefficient of restitution does not "belong" just to the impacting particle, but rather, it belongs to the particle, the microstructure of the bed, and the associated stability of the bed, perhaps preconditioned by preceding impacts.

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