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Interactive comment on "Controls on the rates and products of particle attrition by bed-load collisions" *by* Kimberly Litwin Miller and Douglas Jerolmack

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Review of "Controls on the rates and products of particle attrition by bed-load collisions" by Kimberly Litwin Miller and Douglas Jerolmack

This manuscript presents the results of a fascinating set of experiments, and provides semi-empirical predictions for both rates of sediment attrition and the size distributions of the attrition products. The experiments were nicely designed and the measurements were detailed and complete (except in cases where certain material properties weren't possible to measure). The authors used a pair of pendulums to repeatedly collide two

grains of the same size and rock type. Characteristic velocities (and kinetic energies) were measured with high-speed cameras, and masses were measured between sets of collisions. SEM imagery of thin sections of particles after the experiments were used to observe and measure the development of fractures. Material properties were measured with various standard methods.

The manuscript presents several interesting findings, but, from my perspective, the most important claim is that the rate of mass loss (as a function of impact energies) can be predicted from material properties and empirical constants measured in this study. However, this claim is not adequately demonstrated for several reasons that are outlined below. These issues should be addressed before the manuscript is accepted for publication. Otherwise, the manuscript is well-written, easy to follow, and full of cool observations. I do list several minor points by line number below.

Issues with the prediction of the rate of mass loss

Equation (8) presents an elegant model of mass loss as a function of impact energy, material properties, and two empirical constantsâĂŤC1 and C2. However, the observational basis for both C1 and C2 is shaky, based on the information presented in the manuscript.

C1 is based on the best-fit curve between Ab (based on material properties) and the ratio of mass lost to cumulative energy, shown in Figure 7b. This plot shows a cloud of 8 data points in the middle with no strong relationship, and two outliers–one with high Ab and one with small Ab. There is no legend to identify these data points, but based on information in Table 1, the small-Ab data point represents the volcanoclastic cubes. The authors were unfortunately not able to measure the Young's modulus for the volcanoclastic samples because they specimens were too short, and they instead rely on values from the literature, which span an order of magnitude: Y is between 5 and 50 GPa. Therefore, possible values for the Brittle Attrition Number (Ab, the x-axis of Figure 7b) also span an order of magnitude. Strangely, based on the Figure 7b and the other material properties for the volcanoclastic rocks presented in Table 1, the authors used a value for Young's modulus that is outside the range found in the literature: Y = 4 GPa. The authors should double-check the values used in their calculations and be explicit about the material properties used to estimate Ab. For experiments where material properties are looked up from the literature, the manuscript should present a range of values of C1.

The issue with C2 is less important, particularly since the authors note that this early mass loss is mostly due to the cuboid shape of the particles and is likely unimportant for natural particles. Still, I was confused as to why Figure 7c only shows experimental results from the sandstone and quartz diorite experiments, while the manuscript claims that the early mass-loss behavior is universal. If it's truly universal, it would be more compelling to show that behavior for all of the particles.

Finally, Equation 8 is calculated from several steps, not one regression, and the predictive ability of Equation 8 is not tested. This model should be compared against the data shown in Figure 5a to show how well it predicts mass loss. This isn't a true test of the model, since it's comparing it against the data used to create the model, but it's better than nothing.

Comments by line number

23. Consider replacing "it" with "attrition" to avoid confusion with "abrasion"

58-73. I'm not an expert in fracture mechanics, so much of this discussion here and in the discussion section (354-365) was difficult for me to follow. That said, my reading of this section is that it is commonly assumed that attrition processes occur by the progressive development of cracks through the entire particle, rather than local fractures (surface parallel or otherwise). This certainly isn't the conceptual model that I would normally assume (I would local fracture around the impact site to be important), but if that's what people do normally assume then it's very good to point that out. Right now this statement of what is "typically assumed" is supported by only one reference from

60 year ago.

114-115. These sentences require the reader to differentiate between "significantly smaller" and "much smaller." Consider rewording.

262. Why are dM and dE used for cumulative values of mass loss and energy expenditure? Aren't M and E defined the same way? If there is a need to differentiate these values from M and E, consider using big delta, rather than the derivative.

313. Exponents should be negative (-2.5 and -2)

367. "attrition" spelling

Table 1. Consider adding columns for total number of impacts, Ab, and As.

Figure 7c. Show data from all experiments here

Figure 8b. Why are lengths not shown for the volcanoclastic rocks?

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