The work of Miller and Jerolmack entitled « Controls on the rates and products of particle attrition by bed-load collisions » deals with **earth surface dynamics**, by taking into account **fracture mechanics**, leading to a very interesting and well treated **multi-disciplinary approach**.

The authors realized **well controled experiments** of **particle impact and attrition** and **clever data analysis**, as well as precise size measurements of attrition products, allowing them to get their main experimental results: **impact erosion can be treated « as brittle fracture in the purely elastic regime »**. Additionally, their **fine observations** of chipped particles allow them to support that « the common fatigue failure model is inappropriate », but « propose that Hertzian fracture is the dominant mechanism ». Again, **materials mechanics** appear surprisingly as a relevant tool for bedrock erosion, sand production, bed-load transport, … The authors also consider the limitations of the methods and take time to explain them to the readers, that is really appreciated.

The whole work is realized rigorously. High numbers of different experiments are done to ensure good statistics (450 collisions to test the randomness of the grain rotation, 50 to 10 000 collisions, 20 000 collisions, …), that is really appreciated. I also really appreciate the analysis of experimental data by using dimensional analysis and knowledge from elasto-plasticity, as well as the desire of the authors to use « physically-meaningful quantities ».

For all these reasons, I agree the publication of the paper. However, I have some suggestions below that are reported in sequence.

Maybe it would be interesting to say a word about the case of an **impact of a 'pebble' with a granular material** (with or without cohesion), the last one as a model of some river beds. Such non consolidated (discrete) materials would be another class of materials, where there is not any true fracture, but where mass loss can occur. I know the papers from Beladjine et al 2007 (PRE), where they found a relation between the mass loss Nej, the effective restitution coefficient e function of the impact angle, and the Froude number. Are the experimental data or the current knowledge enough conclusive to be compared to your Eq (2) page 2: Delta M = A Delta E? However, maybe these experiments are not realized « under conditions relevant for bed-load transport »?

I would like to see **some references about papers from the mechanical community about brittle fracture in the purely elastic regime**, if relevant for your work.

Main text

1) Introduction.

When I read **« attrition rate »** for the first time in your paper (line 52, page 2) and later (line 259 page 9), I am wondering to **which definition** you refer to: a time derivative of mass or a derivative of the mass according to the impact energy? I guess it is the derivative of the mass according to the impact energy, however it is not obvious in general.

Maybe you should refer to the Charpy impact test, that look like your experimental set-up, even if boundary conditions are not the same. With such a test, do you think that the measurement of absorbed and/or released energy during the impacts and rebounds would be possible and interesting for your open questions? I think this may be related to one of your conclusion l. 440 p. 14 « We hypothesize that this coefficient is primarily controlled by the details of the collision process, which determine how much impact energy contributes to damage as opposed to friction or rebound of the target. »

2) Methods.

2-1)

Dimensional analysis is really appreciable, I think that it may be very useful.

However, whereas it is clear for the brittle/purely elastic regime, I find it a bit less clearly written for the semi-brittle case. I do not know if you should present the ratio H/Kc as the third dimensionless group and/or you should explain why D, the sample diameter appears as an input parameter in the semi-brittle regime but not in the brittle regime (localized plastic deformations at the surface have to extend to the bulk, on a size D?)?
At this end, it is not crucial, because your experiments are well described by Ab, and not by As. But, this lacks.
Also, it is not clear **what are the units of parameters** appearing in As: what is the unit of H (and Kc), so that As have the same units as Delta M/ Delta E and Ab? Nowhere in the paper, values of H are given, whereas As is computed?

2-2)

How do we know typical impact velocities or energies involved in bed-load transport to state that the values reported here in the experiments are **« under conditions relevant for bed-load transport » ?**

l.176: Why the area coming into play in the expression of the tensile strength is pi l D /2 instead of pi D^2 / 4 ?

l. 208: The number As is called here the Attrition Number for the first time, you should have introduce this name for As and Ab, when these latter are introduced page 4.

3) Results

l. 252: In the sentence « Mass loss curves for all experiments are in good agreement with each other, and with a single linear trend (Fig. 6). », the group « a single linear trend » would suggest that the slope is the same, that is not true. Maybe you should add « with different slopes » or something like that.

I am wondering why you choose to write your Eq (7) page 9 as M/M0 = k E + b , instead of writing k as 1/Es; this would be more direct and this would avoid introducing two variables (k and Es) instead of only one.

l. 260, I would make a remind here on what is A: the Attrition number.

l. 265: « The brittle Attrition Number A_b is plotted against long term attrition rates dM/dE (Fig. 7b) and demonstrate good correlation », it should be added here that **there is some scattering**. Fig 7b: To which materials correspond the plotted data? Maybe, it should be written somewhere that Ab is between 0 and 1 and As between 0 and 2, thus are of the same order of magnitude. Say something on the precision or uncertainty of Ab (and As). Say something about the slope of the order of 10^-5. As it is very far from 1, it should mean that some physical understanding still lacks, that will need further investigations in the future.

l. 272: « The parameter b is related to the initial mass of each particle, with an average value of b $=M/M_0 = 0.0018$ and is approximately constant for all experiments (Fig. 7c). » **I don't see that b is approximately constant for all experiments in Fig. 7c**.

l. 276: « results show that the former tracks the latter, and becomes approximately constant when rock mass $M/M_0 = 0.0018$ » I would write instead « $M/M_0 >= 0.0018$ »

l.277: « This value is the same as b , » **The value of b is not shown (in a Figure) or given (in Table 1). This lacks.**

l. 281: Why introducing a new symbol C2, since it is b? I would change the sense of presentation of Eq.(8) as:
M/M0 = C1 Ab E + C2 = C1 rho Y E /sigma^2 + C2
Also, it seems to me that there is a slight approximation because k is not equal to C1 Ab but is equal to C1 (Ab - 0.2) as can be seen in Fig. 7b. So, the term - 0.2 C1 E lacks in Eq(8). You should say a word about this.

l. 285: In accordance with Eqs (7) and (8), M should be divided by M0 in Eq (10). As a consequence, the sentence in l. 286 should be added by « and divided by M0 ».

l. 299: Instead of « diverge », I would use « differ » to avoid suggesting wrongly tend towards infinity.

l. 312: Add the reference to Eq (6) in the sentence « We then solve for the best fit power law to all data points. »

4) Discussion

l. 322: I would add that the sentence « However, rocks achieve the secondary linear mass loss curve quickly while their shapes are still very close to cuboids. concern rocks observed here » or something like that.

l. 324: I would begin a new paragraph to highlight the equation M/(M0 E) = k = C1 A and I would change the first symbol = by the symbol $\simeq$.

l. 329: « It appears our data are reasonably well described by A_b and not by A_s , indicating that material failure may be considered **to be in the brittle regime. » This interesting conclusion stem from the correlation of your measurements with the brittle Attrition number AB: could we have infer it without your measurements, but from the values of elasto-properties (or from the comparison of the values of Ab and As)? If not, which data (impact stress?, impact deformation? …) should we have access to so that it becomes possible?**

l. 331: When using Ab here, I would recall its relation with Ab=rho Y / sigma^2.

l. 346: I would refer to Fig. 4 and 8 in the sentence « The SEM images of sectioned rocks show a zone of damage accumulation in a shallow region below the surface. »

l. 367: attrtition-product -> attrition-product

l. 395: « In the limit where k = 0 , the brittle Attrition Number, A_b , does not likewise approach zero, but instead is associated with $A_b = 0.25$. » It is not clear to me where this rationale comes from? **From Fig 7b, I can read that Ab(dM/dE=0)=0.25. But this not the value your reported.** Explain better, please. And what is the unit of Ab? Here it appears that it is without unit. But in Eq (4), Ab has the unit of time square / meter square. Clarify it please.

l. 402: « However, the pebbles from the field were all at least 4 times larger than those used in the laboratory, while estimated collision velocities were comparable. » Does it mean that your experiments are done at the same velocities as in the field, but at smaller energy? Do you think that normalizing the mass loss by the initial mass allows suggesting that your results to become independent on M0? Can you still write that you are in the same conditions as in the field (« under conditions relevant for bed-load transport »)?

Figures

Figure 1: I found it not clear at the beginning that grains were free to rotate, maybe your sketch Figure 1 should be completed with additional arrows to indicate the movements direction, and maybe some words should be added in the main text.

Figure 2: I would add x=40mm, 80mm, 120mm or add the word « square » to « rock » and one additional value x, so that it is clear that the distances x=0, 40, 80, 120mm correspond to the square rock corners.

Figure 5: Both lin-lin and log-log plots are really appreciated. You should cite your Eq (7) p. 9 in your legend, when talking about « linear fits ».

Figure 6: In your inset, you show average mass, we would like to see also some error bars describing the standard deviations of mass data. How your data shown in inset are compared to your Eq (7)?

Figure 7a: In the legend, you should add the precision « for all samples ».

Figure 7b: To which materials correspond the plotted data? Maybe, it should be written somewhere in the text that Ab is between 0 and 1 and As between 0 and 2, thus are of the same order of magnitude. Say something on the precision or uncertainty of Ab (and As).
Also, there is some scattering in the data: according to you, what is its origin?

Figure 7c: Write in your legend that data are for two samples: which ones?

Table 1: You shout add some properties here: Attrition numbers Ab and As, H and Kc? The tensile strength is referred to as sigma_t here, whereas in the whole text, one finds sigma, sigma_t, sigma_f, … one may get lost. Can you harmonize it please?

Sometimes, figures or equations are mis-referred:
p. 10 l. 288: Fig 5 b/c -> Fig 4 b/c
Figure 8: In the legend, Eq (5) -> Eq (6).
Figure 9: In the legend, Eq (3) -> Eq (6).