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Comment on esurf-2021-17

Anonymous Referee #2

Referee comment on "Breaking down chipping and fragmentation in sediment transport: the control of material strength" by Sophie Bodek and Douglas J. Jerolmack, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-17-RC2, 2021

This paper explores experimentally the erosion processes of particles during sediment transport, in particular the transition between abrasion and fragmentation. To this end, the authors have carried out a series of experiments in a drum equipped with a paddle that causes a series of drops (about 40-50cm high) applied to artificial particles made of a mixture of variable proportions of sand and concrete.

The experimental approach is not new in itself (the use of drums has been quite classical for more than 50 years to study the abrasion of particles) but the idea of focusing on the transition between abrasion and fragmentation is original.

On the basis of their experimental results, in particular the interpretation of the variability of the mass loss undergone at each impact and that of the evolution of the roundness index of the particles, the authors propose a threshold value of the mechanical strength of the particle material which could correspond to the transition between abrasion and fragmentation.

Some parts of the introduction could be improved. Explanations on calculations or variables present only in the figure legends should be developed in the text and some ambiguities could be clarified. But on general, the text follows correctly, the general idea and the figures are understandable.

In spite of its relative clarity and its potential interest, I see this study more as a preliminary study or as a trial run allowing to set up a more elaborate study in the future, and not as a study sufficiently completed to lead to a scientific article that will advance the knowledge on the tackled problem. I give the reasons for this in what follows by exposing the different conceptual and interpretative problems I encounter in this paper.

Problems with the experimental protocol (probably the major issue):

At the first reading of the paper and its figures, I was struck by the strong dispersion on the strength data, and especially on the density data (between 1.6 and 2.7!). There is very little information on the preparation protocol of the particles and the specimens. Even the composition of the concrete material that will be mixed with the sand is not given (what is its content in cement?). The literature on concrete is more than a century old, and it is well known that many factors can modify the strength of a concrete. In this study only the ratio between concrete and sand is considered. However, the proportion between cement and water (beyond a w/c ratio of about 0.5, the quality of concrete degrades in relation to residual porosity after drying), the proportion of occluded air, the duration of curing before use of the concrete (a minimum of one month is recommended to achieve a certain constancy of the strength value), the granulometry of aggregate, the surface of the aggregate particles. Parametric studies (varying the % of sand, water, etc.) published in the literature generally show a much smaller dispersion of values than that presented by the authors of this study for their data. Also the measured strength values (between 3 and 10 MPa) for mixtures with a majority of concrete (VCM>60%) are clearly lower than the values commonly given in the literature (from 20 to 50 MPa after one month of curing).

At this stage I can only speculate on the origin of such a dispersion in between measurements on the same mixture, or between mixtures with nearby characteristics. Considering the strong variations of density and considering that the average density here is lower than that of a well-made concrete (density from 2.2 to 2.4), I imagine that occluded air is a dominant issue here, and that from one mixture to another the quantities of trapped air have strongly varied. It is possible that strong variations of the water/cement ratio are also at the origin of small bubbles and strong variations of density. In the first case in particular, the presence of large bubbles that vary from one sample to another could explain the very variable resistances. But it is also possible that the curing times were not respected, which poses a problem for example if a specimen and a particle were prepared jointly but were passed to the press and in the drum several days or weeks apart.

Another source of error seems to be related to the preparation of the specimens for the uniaxial press. It is fundamental to obtain quality measurements to have specimens with smooth and parallel top and bottom surfaces. If the specimens have been passed as they were when they came out of the mould, I can see that this can create an additional source of dispersion and also explain why the loading curves in the press are difficult to use and not very suitable for calculating a Young's modulus.

Perhaps the authors have taken all these "difficulties" into consideration, but if so, they should specify this and be able to explain why the density or strength data are so scattered, and why the press measurements are of poor quality. If not, I strongly urge the authors to repeat their experiments, making sure to produce a bubble-free concrete, to respect the optimal or constant proportions of water vs. cement, to let their concretes undergo a minimum of one month of curing before using them, and to rectify their specimens before doing strength test or runs within the drum. Having a reproducible preparation protocol seems to me to be necessary to be able to answer the question asked without bias and without approximation.

Problems of estimation and taking into account the errors of σ_U and A_b

In an attempt to overcome the large dispersions in strength, the authors replace the measured values with those from a linear regression passing through the middle of the points. This approach is not appropriate for three reasons:

1) If the specimens and particles are prepared with the same mixture (with the same water/cement ratio, or the same quantities of occluded air) then the abrasion resistance of the particles should be related to the mechanical resistance measured in the press, and not to a value interpolated from other mixtures with distinct preparation biases.

2) If the dispersion is due to a variable curing time, then depending on the time of preparation, the order of passage of the particles in the press and in the drum, the biases may not be corrected by the choice of an average value.

3) by considering a linear regression, the authors implicitly consider that a linear relationship exists between the VCM and the ultimate strength. However, the mortar experiments I could find in the literature (Singh et al., 2015; Bu, Tian, Zheng et al., 2017) show that strength (both compressive and tensile) is not a monotonic function of the sand proportion. Bu et al. indicate that for reduced content in sand (<66%) the strength increase with the sand content, whereas Singh et al. describe an opposite trend for sand content >75%. Similarly but for concrete mixed with aggregate, Stock et al (1979) also describe non-linear trends. In other words, wanting to pass a straight line has no experimental or necessarily physical reality. Wanting to pass a linear relationship will not reduce the noise on the data, it could instead add error and systematic bias.

This issue is important: the authors insist that the mass loss parameter becomes independent on the mechanical strength for the most resistant particle. However, the basic observation is that the mass loss parameter becomes independent on the %VCM, and authors' conclusion is fully dependent on the assumed increasing relation between %VCM and strength. If this increasing relation is not verified for VCM > 50% (and graphically an increasing trend is not really observed) then this whole authors' conclusion becomes pointless.

In any case, it is fundamental for the figures including σ_U and A_b to add the error bars and to take them properly into account to calculate a regression coefficient.

Fragmentation is poorly defined

The very notion of fragmentation is defined in this paper in an indirect way. As presented, all we know is that particles with a high % of sand show a more irregular abrasion pattern. But is this necessarily the result of fragmentation? One can imagine that these weakly cohesive particles give essentially sand and that the variations are the fact that the particle falls on an angle, a face or a vertex. The video put online on Youtube (https://www.youtube.com/watch?v=UsW8TMxfiqI) seems to me to show during the impacts essentially the production of sand, which seems to be confirmed by the authors page 14 (lines 10 to 14).

In the introduction, the authors mention the Hertz contact for chipping. The notion of Hertz contact zone is defined for an elastic medium. But here we can imagine that for low resistance particles, during the contact, the whole contact zone will be plastically deformed, fragmented and the important residual kinetic energy will induce a widening of this contact zone and an extension of the deformation/abrasion, until the kinetic energy is completely absorbed. In other words, a very strong abrasion can be localized around the contact zone, leading to a wide surface that is rougher and less round than before (and thus explaining that the shape cannot converge to a sphere) and without fracturing or fragmenting along a fracture that crosses the sample as is usually conceived for fragmentation in natural pebbles.

It seems to me therefore necessary for the authors to present some pictures of the products of what they call fragmentation in their experiments, to present the fragment size distributions, to demonstrate if it is the case that fragmentation in its classical definition (splitting in two or more large fragments, and not a shower of sandy particles) is indeed occurring, and finally to discuss the relevance or not of the behavior of their material to account for the fragmentation process in natural pebbles.

The discussion is not mature enough

The discussion needs to be reworked and deepened. Experiments and their results are proposed, a relation (fig. 13) is deduced graphically, but nothing is really said about the transposition of these results to natural cases.

- Can the results of experiments done in the open air be transposed to collisions occurring in water?

- Are the impact velocities realistic? It is not specified but from my calculations, I assume that the impact velocity is 3m/s, which is higher than most river environments;

- are the abrasion phenomena transposable to river environments? In the drum, the particles fall almost at right angles on a smooth surface (steel), while in nature most impacts will be made with a significant obliquity and the roughness of the impacted surface will lead to scratching which is not reproduced in the experiments.

- Is the fragmentation process invoked in this paper representative of what occurs in most rocks? It seems to me that here the defects that lead to fragmentation are related to punctual defects, mostly related to the presence of air bubbles or heterogeneities of the binder between grains during drying, while in natural rocks the fragmentation will result from the distribution of essentially planar defects (fracture, schistosity, layering..)

- Are the attrition rates representative of most lithologies? It is difficult to translate jumps into essentially horizontal travel distance. However, if we assume that these vertical jumps of ~40cm correspond to a succession of horizontal hops, a number of experimental jumps ranging from 25 to 4400 correspond to transport distances of 10m to 1.8km. Roughly speaking, the particles can barely travel a 1km slope before being totally reduced. What are we looking at in the end? Erosion on the slopes, in the colluvial parts? or do the authors think they are reporting fluvial processes? Another way of highlighting this mismatch is to look at the equivalent abrasion rates of these experiments which would be of the order of 75%/km to 25000%/km (considering the number of jumps/distance correspondence mentioned above). The rates of grain size reduction are therefore 3 to 6 orders of magnitude higher than the experimental rates measured on most natural rocks.

Summarizing all the points mentioned above, it seems that the experiments explore conditions quite distant from those of natural pebbles and river environments. It is therefore legitimate and necessary to ask the question of the transposition, or even the usefulness, of the results of this study to natural systems.

More concretely, even if the proposed formalism (Fig. 13 and link with Ab) is potentially interesting and could represent a first step, it has first the disadvantage of being dependent on the experimental set-up (the characteristics of the impacts are directly linked to the experimental set-up). In addition, it is still far from taking into account all of the results found in previous work. For example Kodama (1994) show that andesite and flint do not behave in the same way depending on the size considered. And also that the chert which presents a resistance in compression higher than the andesite will fragment whereas it is not the case of the andesite. What would be the elements to be taken into account in the relation (vs Ab) proposed by the authors to account for the results of Kodama?-

Lack of rigor

The basic mathematical rigor is absent: in the legend of figure 8, it is written that $k = A_b.C_1$ and on the other hand $k = 0.026A_b$, so that anyone would propose C_1=0.026, but here the authors conclude instead that C_1= 1/0.026 ! Also the estimated coefficient for figure 6c is wrong. Even if it could be a matter of carelessness, one can unfortunately then doubt the whole treatment of the results in all the calculations which are not explained.

How can the authors contrast their results with those of Sklar and Dietrich knowing that S&D use the tensile failure threshold, while they use a rough approximation of the compressive strength? How can they be surprised (and without being able to explain it!) by a factor of 1000 on the value of C_1 and that of Miller and Jerolmack knowing that A_b is not defined in the same way in this paper? Considering a factor ~10 between σ_C and σ_t for mortar (e.g. Bu et al., 2017; Singh et al., 2015; Chen et al., 2013), and the fact that σ_C is overestimated by σ_U (ultimate strength instead of the elastic/plastic transition) and Y is underestimated (slope is greater than sigmaU/strain value), there is no difficulty in explaining the 3 orders of magnitude observed for C1 between these two studies.

The authors insist on the one hand that they observe a transition between two distinct domains dominated by chipping and fragmentation respectively. But on the other hand, they try to fit their data with a single law (fig.8a, 8b, fig.11). This is paradoxical: if they can explain their data with one and the same law, there is continuity of processes and not a transition. If there are two distinct domains, then two distinct relations must be adjusted.

Comments on the form.

- Several symbols or calculations are only presented in figure captions (k_{cm}, C_1 calculation, etc). Some of them (k_{cm}) are not discussed nor used further in the text, despite the fact that a relation should be proposed between graphical derivation of k_{cm} and "k" estimation. Nowhere, the authors indicate the value of the velocity v_i that was used to compute "k" …
- It would be necessary to add in sup info a table with all the data.
- Why talk about rotation when it corresponds to a series of more or less identical drops at each rotation ending with an impact on a steel plate? It would be necessary to give the characteristics of this drop and then more adequate to speak about "number of impacts" rather than "number of rotation".

Other comments:

- P2-L5 (=page 1 on line 5): "most models implicitly assume … governed by fragmentation". I am not sure which models the authors have in mind, but I would say it is the contrary. Most models whatever they consider long river size evolution (Sternberg; Parker; Attal and co-authors; Sklar and co-authors), Landscape evolution model (Carretier and co-authors) or theoretical models on shape evolution (Domokos and co-authors) consider progressive and continuous wear of the pebble, i.e. implicitly chipping rather fragmentation.

- P2-L8 and figure 2 caption: I am not sure to follow the logic behind this statement. Hertzian cones will produce fractures that are expected to be at ~40° from the surface of contact. How would it be explaining fractures that are parallel to the surface?
- P3-L11: as far as I remember, I don't think that Attal and Lave (2009) are dealing with particle shape in their study _ remove that reference or replace by Kuenen or Krumbein. In contrast, they proposed some transition based on pebble velocity or size between dominant abrasion and dominant fragmentation, so that this reference would more adequate on line 15.
- P3-L16: "This study …" is ambiguous. Does it mean Novak-Szabo study? But in that case this study does not utilize laboratory experiments. Does it mean the present study? But in that case this sentence is out of place: it would sound like a sentence at the end of an introductive section to announce what will be done in the paper. But similar sentence is proposed again on page 5 (line 18 and further).
- P3-L34: this transition seems quite odd. What is the relation between the shape evolution and the controversy on the origin of fining by attrition vs sorting?
- P4-L4: "this mas loss is proportional …" I would rather say " … is presumed to be proportional…".
- P4-L23: "Ab" as defined in Miller and Jerolmack, or implicitly proposed by Sklar and Dietrich involves the tensile strength, not an arbitrary yield strength (that could be in flexure, compression, traction, etc). This ambiguity is largely responsible for the observed difference between the value of C1 estimated in this study and that in the Miller and Jerolmack study.
- P4-L29 to 35: This type of deformation discussed in this digression (already documented/discussed in Miller and Jerolmack) is no longer discussed in the rest of the paper. Therefore, I do not see its usefulness. I suggest that it be deleted.
- P6-L17: "… every rotation…": from what I observed on the youtube video, rotation is not a fundamental variable. The pertinent one is the number of free fall at each rotation. Similarly, rotation speed is of limited interest. In contrast it would be necessary to document the height of fall and consequently the estimated velocity of terminal impact (probably around 3m/s if the height of fall is around 40-50cm according to the device).
- P6-L27: which model of Instron UT system?
- P6-L28: I don't understand this sentence. It must be clarified. Did the authors put cubic specimen (what they call "particles") for the compressive test? Why this choice? Why didn't they make cylinders? In any case, I think that there are tables of correspondence in the literature to transform a yield stress obtain on a cube toward classical cylinder used for UCS measurements. It must be clarified also if the particles for the drum and the one for the strength tests were prepared from the same mixture (i.e. involving the same amount of water/cement ratio), or in two different batches.
- P7-L5: Given that the measured parameter is not a Young modulus (the true Young modulus measured from the slope of the stress/strain curve in the elastic deformation domain should always be larger than the parameter measured here, because the slope is larger than the stress/strain ratio between 0 and the ultimate strength), I would suggest naming it by a different name and a different symbol (Y* for example)
- P8-L5-6: what is the convention in e-surf? 0e6 or 3.0×10^6 ?
- P8-L18: 2700 kg/m3 for the 3rd mixture is not in the range +20%
- P8-L20: what kind of heresy is this? The samples (probably both the specimen for the strength test and the particles introduced in the drum) display large dispersions, there is not theoretical model to justify a linear fit (so that replacing the data by a value derived from this linear fit introduced an extra uncertainty), and this simplistic procedure would reduce the errors? It makes no sense.
- P8-L28: I don't see on fig.6 that mass is reduced more rapidly at the beginning of each experiment. To the contrary, the average curve (black line) do not appear to depart from Sternberg's law.
- P9-L5 to 15: this section should be rewritten. Sklar and Dietrich's relation is rejected on the basis of fig.8 , but fig. 8 involves the variable "k", which is defined later in this

section, so that the arguments of that section are not very clear.

- P9-L6: "…ultimate strength proposed by Sklar and Dietrich…". No! S&D propose to use the tensile strength (estimated through Brazilian test), not the compressive strength. As far I remember, they even claim that they tried correlations with tensile ad compressive strengths and observed a much better correlation with tensile than with compressive strength. See also general major comments.
- P9-L12-13: "we anticipate…" . I don't follow the rationale behind this statement. I don't find a linear relation so consistent with the data drawn on fig.8b. First the propose red line does not go through the origin; second the line is hand-drawn (a classical linear regression would be steeper); third a fit by a square root relationship would fit much better the two points at Ab=0.25 and 0.5; forth a graph with the logarithmic scale for the x-axis would show that the linear fit is a really poor predictor of the mass loss parameter for Ab<0.2.
- P9-L15: "mass loss is independent of material strength". Or maybe the estimate of the strength based on a linear fit is just erroneous for the strongest materials!
- P12-L2: the phrasing seems to me ambiguous. A limit on what? Do the authors mean that there is a limit on strength (or on Ab) beyond which particles achieve similar circularity?
- P14-L10&11: "particle composition"? Do the authors mean the lithology? Even if it is the case, Sklar and Dietrich did not explore shape evolution in their paper. I do not understand both the sentence and the reference. What does mean the "high composition of sand"? Do the authors mean the "high proportion of sand"?

- P14-L12: I do not see any bimodal distribution on fig.7. They seem to me unimodal on this graph (???).
- P14-L16: I disagree with this conclusion "a relevant material grouping". In log scale, for Ab<0.2, i.e. for 70% of the tested material this linear fit is a really poor predictor of the mass loss. On fig.8b , the linear relation is in fact based graphically on a single point (Ab=2, on the right part of the graph).
- P14-L18: "…not entirely understood": see general comments
- P15-L2: this conclusion should be discussed in a more extended way, particularly by checking if results of former studies verify or not this limit. Fragmentation of cherts [Kodama , 1994] or of limestone [Attal and Lave, 2009] seem to occur for Ab values much lower than 0.014s2m-2 . So I would tend to say that this relationship deduced from artificial particles does not apply to natural pebbles.

- Figure 6: along y-axis, fractional mass loss must be replaced by mass fraction. For the x-axis, it would be much better to display the true number of rotation rather than the normalised value. What is the "total number of rotation"? Are the 5 particle ran in the same experiment or in five different ones? In the second case, I don't understand what is the "total number of rotation". Normalization does not help in comparison at all. This normalization does not make more sense to derive the relation M=exp(-kCm*Nr). How applicable is this relation if we don't know the total number of rotation? In addition the proposed value of kCm is erroneous for the figure 6c.

- Figure 7: It would be better to propose two graphs: one with linear scale and one with log-scale instead of this figure with two domains of distinct unit size. This would be in particular a more neutral presentation to highlight or not the presence of a transition.
- Figure 8_ Caption: the C1 value must be corrected
- Figure 7, 8, 11: given the large uncertainties on the ultimate strength, on the “Young” modulus and consequently on the parameter “Ab”, it would be necessary to add error bars on these variables on those figures, and to include them in fitting and R^2 calculations.