

Associate editor

I have received two reviewer reports from Dr. Neely and an anonymous reviewer. Both have reviewed previous versions of the manuscript and find it significantly improved, but both also have remaining doubts and concerns. From reading the reports myself, it seems the concerns mainly lie in the clarity of the writing and overall presentation of this work. Please review both reviewer reports very carefully and address each individual comment as you edit your manuscript. Dr. Neely also recommends including a subsection on experimental limitations, which I agree would benefit the manuscript. The authors should make a concerted effort to refine their writing for clarity and to ensure they are presenting sufficient information for readers and reviewers to thoroughly understand their work.

Thank you for your feedback. We are pleased that both reviewers found the manuscript improved. We have carefully addressed all their comments and made significant efforts to improve the clarity of our writing throughout the manuscript. Regarding the experimental limitations, we would like to clarify that a dedicated section has been present in the Discussion since the first version. Following Dr. Neely's suggestion, we have now mentioned some key limitations earlier in the manuscript, while the full limitations section remains in the Discussion. All changes are detailed in our point-by-point response.

Reviewer #3

This version of the manuscript is much improved from the previous version, although I still find the broad readability of the manuscript to be challenging. Many paragraphs now begin with a helpful topic sentence, but other sections (e.g. 4.2 and 4.3) are still challenging to parse as a more “casual” geomorphological reader (one who is not looking to conduct experiments or is keenly interested in the nuances of rock fracture). At present, the manuscript is a thorough and thoughtful consideration of the experimental design and results, but the Introduction and Discussion still fail to help draw in a broader audience for this work.

R#3-1: We have revised the manuscript to improve its readability by adding clearer topic sentences at the beginning of each paragraph and simplifying the presentation where possible. We have ensured that the discussion is clear for geomorphologists who are not specialists in experiments or rock fracture.

I understand it may be personal preference when crafting an Introduction, but one downside to having one big “Introduction” section rather than breaking out the knowledge gap and hypothesis tests from a “Background” section is that readers must work through quite a bit of background before getting to the “why” of this particular paper. I know in my previous review I highlighted that information in the introduction was often not relevant to the science question(s) being addressed in this work. I think this version has done a better job of presenting mostly relevant background info here, but I would still argue for a more focused statement of the knowledge gap and hypotheses within the first two paragraphs. At present the “why/how” of the paper is spread sporadically through the first 120 lines of the paper. The main knowledge gaps or “whys” of the paper are stated in lines 98-101, followed by a nice statement of “how.” I recommend letting those two sections shine earlier in the Introduction.

R#3-2: We have rearranged the Introduction to state the knowledge gaps earlier and reorganized several sentences and paragraphs for improved clarity. We believe this revision addresses the reviewer's concern while maintaining the necessary background context and what we found to be a logical flow.

I also think the Discussion section would benefit from a synthesis figure, which might be a cartoon or annotated, selected experimental results to help walk the reader through the thoughtful discussion and textural synthesis in Section 4. (Table 1 does not accomplish this). It would be wonderful if the authors can tie any of the specific mechanisms to which they refer in the Discussion to field photos in this figure. Right now it is challenging to parse the results figures for any real-world application.

R#3-3: We agree with this comment and have added a synthetic figure (Figure 8 in the revised version) that we use in the discussion of the paper. However, as requested in the previous review (Dr. Neely, comment #2, page 4), we are now careful not to directly apply our experimental results to field settings. This is clearly stated in the discussion (section 4.5).

As a microcosm of this problem, the remarkable erosion for the 157° fracture dip sum experiment is now a little better explained in the text, but the text as is fails to explain why this outcome is significant beyond its quirkiness. Right now, (1) it is not immediately apparent what “sum of fracture dip” means when parsing the figures (here is where a cartoon or schematic would help), and (2) the authors do not connect this “optimal” fracture dip configuration in their experiment to any real-world scenario (are there fracture “sweet spots” in nature? What might those look like? Can field geomorphologists seek these configurations out?).

R#3-4: We thank the reviewer for this comment.

(1) To improve clarity, we have revised the presentation of the results related to fracture dip angle: the notion of "sum of fracture dips" has been removed from the paper, as it was indeed confusing. Each fracture set is now described explicitly, and we have added a 3D projection of the fracture network beneath the 3D perspective views in the newly revised Figures 4a and 5a. This 3D view provides a clearer visualization of the spatial relationship between the two fracture sets and makes the geometric configurations more immediately apparent to readers.

(2) Here again, we do not want to apply our experimental results, obtained from a limited number of experiments to specific field settings.

Again, I will suggest the authors consider some recent relevant experimental work when discussing dip angle:

<https://pubs.geoscienceworld.org/gsa/geology/article/53/9/780/659310/Dip-angle-controls-on-plucking-susceptibility-and>

R#3-5: We thank the reviewer for this suggestion. We have added the suggested reference to the discussion (Line 568).

A. Neely

Review of revised draft: New experiments to probe the role of fractures in bedrock on river erosion rate and process

I have read the revised manuscript draft and author responses. The authors have put considerable effort to revise their manuscript and consider the scope of their results. The limitations, definitions, and study design is more clearly presented, but there are still some minor clarifications to make in light of the last round of revisions. I am hopeful these can get sorted out.

This is a very challenging problem that the paper is trying to address, and I appreciate the persistence of the authors and their flexibility in making revisions.

My definition of plucking and macroabrasion was mostly gleaned from the author's definition of these two processes in the current and former draft. I also misunderstood that there is an implied 3rd horizontal joint set, which is the contact between the blocks and the bottom of the flume? Correct?

Yes, that is correct.

The disk simply rests on top of the flume bottom? Or is the disk adhered to the bottom of the flume? Please clarify how the horizontal discontinuity surface is represented in the experimental set up. This was clearly stated as a question in my last review and I still do not understand. For example, line 154 states "fixed at the bottom of the Plexiglas column" – if the concrete disk is "adhered" as in glued ect... then there is not necessarily a pre-existing horizontal joint set? My understanding is that water cannot flow through the BVOH fractures – correct? (if so, this was an important detail that I do not remember before) but can water flow underneath the blocks along this contact at the bottom of the cylinder? Unfortunately, the word choice is important here. If there is no flow of water around the blocks, the experiment relies on the condition that impacts propagate fractures, and there is no preexisting weakness for cracks to grow on – i.e. the dashed line in Whipple et al., 2000 figure 4). The cracks must form within the concrete at depths shallower than the total disk depth.

R#1-1: We have clarified the text to avoid any misunderstanding: the concrete disk is maintained by a central screw and is not glued or adhered to the bottom of the Plexiglas column (see changes e.g. lines 126, 160, legend of Fig. 1). This ensures the disk remains properly positioned during the experiments while preventing any water flow underneath or around it. Therefore, water cannot flow through the BVOH fractures, nor along the contact at the bottom of the cylinder as already mentioned line 403. Grain impact propagates cracks (ultimately, fractures) within the concrete itself, and the cracks indeed develop at depths shallower than the total disk thickness as already mentioned (e.g. lines 168, 258, 441, 452).

The difference between abrasion, macroabrasion, and plucking is "semantics", but it is important in this case, because these terms convey how the experimental results translate to natural rivers. Some aspects of natural rivers are captured by the experimental design, but some are not. This is ok and the authors more clearly state limitations in the revised version.

If I am understanding correctly:

1) The fractures do not create apertures or voids where water or sediment can circulate – I did not know this before.

Yes

2) There is still some bond between the blocks with an unknown strength. The BVOH follows different weakening patterns when adhered to the concrete. I did not know the specifics of this before.

Yes

3) It is possible there is no pre-existing horizontal plane of weakness. I still don't know how this is represented.

R#1-2: The bottom of the disk (the contact between the concrete disk and the Plexiglas column floor) represents a major mechanical discontinuity. However, within the 2 cm thick disk itself, we do not create any pre-existing horizontal plane of weakness. Cracks propagate from the surface downward into the concrete due to grain impacts, as explained in our previous answer (R#1-1).

4) The size of the impactors is greater than the width of the fractures. It was mentioned before, and the revised text acknowledges this limitation.

Yes

5) The flow dynamics that may promote plucking (hydraulic jumps, constrictions) are not controlled by the flume design. The revised text acknowledges this limitation.

Yes

R#1-3: We thank the reviewer for highlighting that our manuscript is now clearer.

Most of these things are different from the diagram of Whipple et al., 2000 or 2022. These do not really change the significance of the experiments – non-uniform strength properties following a fracture network are an interesting experimental control, and this experiment is covering a portion of that parameters space. This is really cool! But one could still ask, is this approach really capturing all aspects of the plucking phenomena? If not, what is it covering?

R#1-4: We respectfully disagree with this comment. When comparing our setup to Whipple et al. (2000, 2022), we note several important similarities: (1) the size of the clast is also larger than the fracture aperture in their conceptual diagrams (see Figure 4A of Whipple et al. 2022 and Figure 4 of Whipple et al. 2000), (2) there is no horizontal pre-existing plane of weakness in their models either, and (3) there is no explicit mention of hydraulic jumps or flow constriction. We acknowledge that our experiments do not include hydraulic clast wedging or pressure fluctuations in the fractures, which are suggested by Whipple et al. (2000, 2022) as potential contributors to the plucking process. This limitation is already stated in the manuscript (lines 108 and 559).

The authors have done a pretty good job re-aligning their discussions with their experiments, but it might be best practice to state up front (and perhaps in the abstract) the limitations of the experimental design.

R#1-5: We have added the main experimental limitations to the abstract (line 33).

A reasonable place to emphasize differences between natural plucking processes and the experimental design is near the end of paragraph 2.

“In this experiment, we adhere to this second definition of plucking, and explore a particular scenario where (state how the experiment will differ from natural conditions)”.

R#1-6: We have emphasized the main differences between our experimental setup and natural conditions at line 108. We did not add this at the end of paragraph 2, as suggested, because the experimental setup has not yet been introduced at that stage of the introduction.

I am still slightly confused on the difference between macroabrasion and plucking in this definition, all rock is fractured at some scale (this is semantics and partially an opinion -- sorry). I am also not up to date on the macroabrasion literature. Perhaps plucking produces A & B axes that match joint spacing, macroabrasion does not?

R#1-7: We agree there is a confusion, not in our paper, but in the literature concerning the definitions of macroabrasion and plucking. This is a point we have highlighted in our previous review answer (see pages 1 to 4). We clearly state the definitions of these two processes in the introduction (lines 64-82) and discuss about it in the discussion (section 4.2).

I have more line by line comments – some are very minor grammar changes and others are suggestions to change parts of the manuscript that were unclear to me:

Main text

Line 21: if stating some limitations, here might be the place. This way anyone reading the abstract would know that it is clear that the conclusions correspond to the experiment and not necessarily all cases in natural rivers. Overall, I think the abstract is written well though.

R#1-8: We have stated experimental limitations at line 33 in the abstract, where other limitations are already mentioned.

Line 24 “s” after fracture [done]

Line 34 could add in the parentheses “3D fracture patterns” – there are still more interesting cases that could be tested – perhaps you can do these experiments in the future (The review process will be less painful!) [done]

Line 49: “rock” instead of “rocks” [done]

Line 56: “breaking of bedrock into blocks by the impact of large clasts” – this is where a clearer distinction of macroabrasion vs plucking would be useful. I see that macroabrasion can refer to substrate with no-preexisting, but if there are pre-existing discontinuities, you can have macroabrasion and plucking according to these definitions. Ok, but what distinguishes the two?

R#1-9: See our response R#1-7, where we already address the macroabrasion vs. plucking distinction.

Line 85: “dip” instead of “dipping” “orientation” instead of “orientations” [done]

Line 120: could specify here how the concrete disk is related to the Plexiglas column “positioned” is not specific enough when “fixed” is used later. “Fixed” implies “adhered” or “glued”. Sorry if this was a misunderstanding.

R#1-10: See our response R#1-1.

Lines 157-159: Please clarify with respect to lines 384-386. “extremely fragile and can easily break along fracture plans (planes) even before the BVOH is further softened by immersion in

the tank” – contradicts “Although the tensile strength of the intact BVOH is high and generally depends on orientation”.

R#1-11: Thank you for pointing out this apparent contradiction. This was a language issue on our part. We have now replaced "intact" with "dry" (line 404) to make this distinction clearer. Previous lines 384-386 refer to the dry BVOH before being placed in contact with concrete, whereas lines 157-159 describe the BVOH after immersion in the concrete, when it becomes fragile.

Figure 1 caption: “fixed” – again this could imply that the concrete disk is adhered to the bottom of the cylinder. This might or might not be the case and needs to be clarified.

R#1-12: Again, see our response R#1-1.

Line 202: “with one (core?) point” ? – my guess is the point cloud has many more points with closer point spacing than 1 mm. the core points are spaced at 1 mm? (I might be getting the terminology wrong from Lague et al., 2013)

Line 204: “25 points from each cloud used to compute vertical difference” – something is wrong here. You have much higher resolution according to your figures and results. Average of 25 points per core point from both clouds?

R#1-13: We thank the reviewer for these comments. We agree that the formulation was unclear and could lead to confusion and have revised these lines accordingly to clarify these aspects (see lines 206 to 210).

Line 225: please state the limit here for how you define a plucked block. It is stated later in the manuscript “15 mm²” – apparently these are not necessarily the same size as the pre-existing discontinuity spacing, so please specify in the definition how this relates to macroabrasion vs plucking and your interpretation of the two.

R#1-14: As clarified in the Introduction (lines 115-118) and emphasized at line 231, we use the term plucking for any localized high-intensity erosion event, which includes macroabrasion since we lack a reliable method to separately document this process in our experiments. Concerning the specific threshold value (15 mm²), we added the values in the method section as suggested.

Line 369: “gravel” instead of gravels [done]

Line 370: the other limitation mentioned in earlier reviews is that the flow dynamics that enhance plucking are uncontrolled in this flume set up.

R#1-15: The focus of our paper is not on the controls of flow dynamics on plucking occurrence as did experiments working on specific channel width geometries (Dubinski and Wohl, 2013). In our paper we therefore refrain from discussing these flow controls.

Dubinski, I. M. and Wohl, E.: Relationships between block quarrying, bed shear stress, and stream power: A physical model of block quarrying of a jointed bedrock channel, *Geomorphology*, 180–181, 66–81, <https://doi.org/10.1016/j.geomorph.2012.09.007>, 2013.

Line 389-392: these points should be brought up earlier in limitations (I recommend during the introduction after defining plucking)

R#1-16: Thank you for this suggestion. The first sentence has been moved to the Introduction, as recommended (line 108). The second sentence, however, relates more directly to the discussion of results and has therefore been kept in the Discussion section.

Line 397-398: I disagree. Other experiments mentioned in the first review did do this by arranging already separated blocks in a flume. Aside.... I'm not sure if it would be "suitable for publication purposes" – more curiosity research -- but you could also start with an intact concrete disk, mount it in the flume, and fracture it in situ (a gentle hammering). I'd be interested in how these sorts of conditions compare to your experiments here.

R#1-17: We thank the reviewer for these suggestions. While in situ fracturing of an intact disk could indeed be an interesting approach, it is beyond the scope of the current study and our experimental setup. Indeed, in our experimental setup, it is not possible to create open fractures or detached blocks because the protocol requires the column to be emptied of sediment and water in order to measure weight loss and topographic evolution. If the fractures were open or the blocks already detached, the material would collapse during this emptying phase.

Nonetheless, we have rephrased the sentence to nuance it and make it clear that it refers to the context of our experiments (line 414).

Lines 406-409: A new methodological problem is mentioned here with no description of its significance. I'm not sure what to make of this and readers won't know as well with the way it is written. It seems like with your calibration between mass-loss and the photogrammetry differencing approach that the 3D reconstructions are accurate, so maybe you don't need to mention the need for the point cloud cleaning discussion.

R#1-18: We thank the reviewer for the suggestion and have removed these sentences from the discussion.

Lines 416-418: put this in the methods section to clarify how you are distinguishing between the modes of erosion

R#1-19: As addressed in our response R#1-14, we have added the threshold values in the Methods section as suggested. Nevertheless, since these thresholds are derived from the data analysis, the justification for their choice is still presented in Sections 3.2 and 3.3. We have added a clarifying statement at line 233.

Line 420-422: This is a good addition.

Line 426: because there is still some connection between the blocks by the plastic? Or because no water or sediment can circulate through the fractures?

R#1-20: This is an empirical result. We cannot fully determine the reasons or the causes of this result.

Line 465: what is the size of macroabrasion debris?

R#1-21: The size of the macroabrasion debris varies between the different fracture networks, however, as stated in the discussion section 4.2 (line 461), macroabrasion generates cm-scale erosion patches detected near fractures for the largest fracture spacing experiments (Fig. 4a and 4c, 50 m⁻¹ and 120 m⁻¹, and some 100 m⁻¹ experiments).

Line 468: Suggest -- “Yet, abrasion is systematically the dominant process in our experiments”
[done]

Line 486 “grain” instead of “grains” [done]

Line 491: “tends” instead of “tend [done]

Line 493: “sticks out” – in what direction? This is not clear.

R#1-22: We added “at the disk surface” to clarify (line 508).

Line 494-507: This section is difficult to follow. It sounds like what is meant is that the fracture patterns are interpreted to alter the morphodynamics of the erosional surface that results – which is interpreted to hinder erosion slightly compared to a uniform substrate.

R#1-23: Section 4.3, to which these lines belong, has been entirely rewritten to clarify. We are confident that it is now easier to follow. Indeed, we suggest that the fracture network alter the grain trajectories so that abrasion is lower with respect to experiments without any fracture.

Is it possible that the BVOH softens over time and is a stronger material in the early phase of the experiment? It seems like the mechanical behavior of the BVOH when it is adhered to the concrete is a bit different than expected.

R#1-24: We cannot constrain the mechanical behavior of BVOH, as it loses its dissolution properties when placed in concrete. However, no notable differences in terms of mechanical behaviour were observed after immersing a fractured disk in water for 24 h. Given that the experiments are relatively short, any changes in material properties are unlikely to have a significant effect.

Line 553: “not initially detached” – again, does this mean that the blocks are adhered (glued in some way?) to the bottom of the flume?

R#1-25: Again, see our answer R#1-1.

Lines 553-555: I would remove this sentence.

R#1-26: We respectfully disagree with this suggestion. This sentence is important because it highlights a key distinction between our work and previous experimental studies on plucking.

Line 559: what is the kinetic energy of the grain impacts? I did not see this anywhere in the results.

R#1-27: The kinetic energy of the grain impacts was not measured in our experiments. The statement in the manuscript is intended as a suggestion to explain the observed relationship, rather than a quantified result.

Line 560: “fractures” instead of “fracture” – either might work [done]

Line 563: “not related to a change in rock resistance” – but doesn’t the BVOH weaken with time? How can you rule this out as a driving factor?

R#1-28: See our previous answer R#1-24

Line 568: suggest removing “utterly” – usually “ly” words are not needed [done]

Line 576” “due to tectonic deformation” is too narrow of a definition of the processes that can fracture rock in this context.

- Thermal fracturing (Collins et al., 2018, their movie is interesting <https://doi.org/10.1038/s41467-017-02728-1>)

- Topographic stress (Slim et al., 2015 – <https://doi.org/10.1002/esp.3646>)

- Freeze thaw (Hallet et al., 1991 - <https://doi.org/10.1002/ppp.3430020404>)

- Expansive minerals – (swelling clays, anhydrite, biotite) (Jarzyna et al., 2023, <https://doi.org/10.1016/j.geomorph.2023.108667>)

- Bioturbation – probably not so relevant in a river, but maybe

A review paper is also fine to cite.

R#1-29: We have revised the text to include additional rock-fracturing processes – thermal fracturing, topographic stress, freeze–thaw, and mineral expansion – with the suggested references, providing a broader view beyond tectonic deformation (lines 597-599).

Line 581: remove “certainly” [done]

Line 586: “depend” not “depends” [done]

Line 587: “factor 2” is a bit confusing here. “with a 2-fold contrast in fracture spacing” – skeptical of presenting this this way given that the fractures in the experimental set up do not have apertures.

R#1-30: This part of the discussion has been rewritten and this formulation no longer appears in the revised manuscript.

Line 593-594: agreed.

Line 594-595: “not fully connected in 3D” this is really significant. If they are not connected in 3D, then a new fracture MUST form by abrasion given your set up. The propagation of those fractures would also maybe depend on the boundary condition between the flume and the blocks (depending on how the two are fastened together?) In natural channels fractures could be generated by any of the other mechanisms stated in the commen related to line 576 – in addition to tectonic fracturing. The blocks in that case are preprepared for plucking largely independent of fluvial processes (other than the river cutting topography, buffering temperature changes, or hydrating swelling minerals). This is a difference in comparing/generalizing the results to field settings and is still unclear with the description of the experiment set up.

R#1-31: We are sorry about this sentence which brought a confusion. In our experiments the mechanical discontinuities are 3D connected (2 set of vertical printed fractures and the horizontal disk bottom). So, a new fracture MUST NOT, but CAN form by abrasion, and indeed this what we generally observe. Yet, our point with this sentence was to discuss the relevance of our results to natural systems. And we now suggest with our results that “a natural river with a bedrock, characterized by a high density of 2D mechanical discontinuities that might not be fully connected in 3D, does not necessarily erode much faster than the same intact rock when bedload transport and abrasion occur”. To prevent this confusion, we have modified this sentence by refer to natural rivers (as it was already our intention; line 611). We also agree that fracture can be generated by processes other than grain impacts, as we mention in our paper (Lines 597), but that the proportion of each remains an open topic.

Supplement:

S2: Addition of photographs -- thank you for doing this! – my earlier comment was just assuming that you had photos (you need photos to do photogrammetry!) why not show us how the experiments were progressing? It would be helpful to label what is void space vs BVOH
R#1-32: We have added clarification to the Figure S2 legend. There is no void space in our experiments: all fractures are completely filled with BVOH material.

Fig. S2 caption; “began” “stopped” [done]

Line 71; “weighing” [done]

Figure S4: Three things --

I suggest plotting fracture spacing instead of fracture density, because it will be easier to compare the linear dimensions. [done]

There also seem to be two colorbars and it is not clear which corresponds to which panel. [done]
Is each dot a separate fragment? Yes

Please let me know if you have questions about aspects of this review. I am sorry if there are misunderstandings, but I need to know what you did methodologically to understand and properly review the paper. This is now the 3rd review, and I think key methodological details are still surfacing. I doubt that this is intentional, but please recognize that this is time consuming for me, and I am trying to help.