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 gleamed 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? **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 pre-existing 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.

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*.
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*.
3) It is possible there is no pre-exisiting horizontal plane of weakness. *I still don't know how this is represented*.
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.
5) The flow dynamics that may promote plucking (hydraulic jumps, constrictions) are not controlled by the flume design. The revised text acknowledges this limitation.

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?

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.

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)".

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?

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.

Line 24 "s" after fracture

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!)

Line 49: "rock" instead of "rocks"

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?

Line 85: "dip" instead of "dipping" "orientation" instead of "orientations"

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.

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".

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.

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?*

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.

Line 369: “gravel” instead of gravels

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

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

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.

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.

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

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?

Line 465: what is the size of macroabrasion debris?

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

Line 486 “grain” instead of “grains”

Line 491: “tends” instead of “tend

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

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.

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.

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

Lines 553-555: I would remove this sentence.

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

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

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?

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

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.

Line 581: remove “certainly”

Line 586: “depend” not “depends”

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.

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 comment related to line 576 – in addition to tectonic fracturing. The blocks in that case are pre-prepared 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.

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

Fig. S2 caption; “began” “stopped”

Line 71; “weighing”

Figure S4: Three things --

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

There also seem to be two colorbars and it is not clear which corresponds to which panel.

Is each dot a separate fragment?

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.