

Review round 2:

I have reviewed the revised submission and authors responses to the first round of reviews. There were some significant original suggestions from the first round of reviews that did not receive much clarification. Specifically, the material properties of the plastic material and the significance of a 3rd joint set or the experimental design with respect to plucking in natural channels.

I understand that these are experiments that were already conducted, and certain aspects of the experimental design cannot be changed. Also, I know that this is a project with limited duration. Science benefits most from patience and persistence. Hang in there!

I encourage the authors to address the points below or restructure the presentation of their results to clearly state limitations when extending these results to natural river channels. I am not an experimentalist or plucking expert, but the conditions that produce plucking are not controlled or present in these experiments. To me, it seems like the experiments are simulating macro-abrasion processes more than plucking? It might be a simple difference in wording, but this could confuse readers, myself included.

In my opinion, the current form of the manuscript still overextends the interpretation of the experimental results to field settings that are not comparable to the experimental design. This was brought up by both reviewers. The experiments were performed rigorously, and using novel techniques, so there is still much insight gained from the results and the experimental work. But, as it is presented now, the paper is describing flume experimental conditions that do not really reflect plucking that occurs in natural channels.

Specifically:

- 1) There is still not much clarity on the properties of the plastic material that was used to create heterogeneity in the erodibility of the substrate along the fracture planes. The authors suggest that this is difficult to quantify and do not provide further information. Because no information is given, I am providing suggestions, although I am not familiar with working with the material firsthand. So if these are impractical, I apologize, but this is a limitation of the study that is significant, and I was hoping the authors could provide further information or simple experimental results.
 - a. Can't observations of whether the plastic is remaining or gone throughout the experiment be useful for determining the relative material properties?
 - b. I would expect that the solubility of the BOVC is constrained to some degree or could have been constrained by submerging the material at different temperatures and durations. This should be compared to the length of time for each experiment

and the amount of water that is circulating through the flume (does the water reach saturation?)

2) No pictures of the flume runs are provided. Topographic scans are different from photographs, which could show development of fractures in the concrete or differences in erodibility between the plastic and the concrete, which apparently is not straightforward and could evolve during the experiment (relating to 1a).

a. Why are these pictures still not provided? This makes me somewhat skeptical.

3) There is no 3D connectivity of the fracture network. Is this really a fully developed fracture network that is representative of fractured bedrock?

a. Most prior studies of plucking use already detached blocks, because most channels are presumably eroding through already detached blocks (at least at some depth scale, the fractures connect or there is a weakness in the 3rd dimension (i.e. bedding plane) to generate a fracture. As I mentioned before, it is very hard to see fractures underneath a riverbed. They are usually buried if present! But at least some cliffs/spires/waterfalls where plucking is active show subhorizontal discontinuities which would create a 3d network of fractures. There is no discontinuity in this experiment to replicate this condition. **Qualitative** --- but I have visited the channel in this study of plucking by Snyder et al., 2003, and there are detached blocks near small waterfall lips throughout the channel, and the blocks have geometries matching the thickness of bedding planes.

Snyder, N.P., Whipple, K.X., Tucker, G.E. and Merritts, D.J., 2003. Importance of a stochastic distribution of floods and erosion thresholds in the bedrock river incision problem. *Journal of Geophysical Research: Solid Earth*, 108(B2). <https://doi.org/10.1029/2001JB001655>

b. Still, the experiment is looking at the influence of spatially heterogenous bedrock properties on the spatial pattern of erosion, which is an important problem, but to me, the experiment is quantifying a process that is more similar to macroabrasion than plucking? This is shown by the continuum of “block” sizes in figure 7 (maybe this could also be presented as a histogram of plucked areas?). If plucking was really occurring along fracture planes, I would think there would not be a continuum of block sizes. There would be a higher frequency of particles detached that match that of the fracture spacing. Macroabrasion is still an important erosion process! but it is not clear to me that these experiments can be equated with plucking? which could be confusing to many readers. Also still, it is misleading to state that plucking does not significantly increase erosional efficiency, implying that this is true everywhere (i.e. without a statement

clarifying that this is just in the experiments, which do not necessarily have conditions that cause or perpetuate plucking?).

I am trying to provide these comments to be constructive (although critical)!

Line by line comments (line number refers to line in revised manuscript):

Line 66: “*two main steps*” – the first step may be completed prior to any interaction with bed sediment?

Line 88: “*However, at long time scale, the landscape should equilibrate with the tectonic forces so that these variations in erosion rates might only be transient*”

But the point is that landscapes would then show a fluvial relief (or hillslope/colluvial channel relief) that is lower, like the analyses of DiBiase et al., (2018) – Figure 3c) and then Neely & DiBiase, (2023) – Figure 3, Figure 5a), which look at interaction between the fracture density and fluvial, colluvial, and hillslope relief.

Also, these landscapes might have a different response timescale to upstream propagation of baselevel signals i.e. an acceleration in uplift at a range bounding fault?

To me, a change in river erosion for a given channel geometry due to a process such as plucking is pretty significant in these contexts. River profiles are often used to interpret tectonic histories with models that do not consider differences in erodibility due to changes in erosion processes (i.e. Fox et al., 2014 – and similar studies) – and then the timing or occurrence of a change in tectonics is misinterpreted – (these inversion models can still be very useful starting hypotheses to identify more complex interactions)

Fox, M., Goren, L., May, D.A. and Willett, S.D., 2014. Inversion of fluvial channels for paleorock uplift rates in Taiwan. *Journal of Geophysical Research: Earth Surface*, 119(9), pp.1853-1875.
<https://doi.org/10.1002/2014JF003196>

Neely, A.B. and DiBiase, R.A., 2023. Sediment controls on the transition from debris flow to fluvial channels in steep mountain ranges. *Earth Surface Processes and Landforms*, 48(7), pp.1342-1361.
<https://doi.org/10.1002/esp.5553>

Line 101: In most cases? What cases? This line seems to contradict the concerns that both reviewers brought up about not having a 3D network of connected joints?

Line 105: Hydraulic jacking as mentioned in the past review involves smaller clasts that wedge into fracture planes or plug flowpaths under the clast, creating lift.

Line 107: This definition seems a bit too general. The same definition could describe abrasion, which is a completely different process from plucking?

Line 148: “joint” – this spelling correction occurs a few times throughout the manuscript

Line 507: “*constant sediment input*”? Over time more sediment is added? Or there is sediment added in the beginning of the experiment? Sediment is also produced from the eroded concrete. Minor, but this is a bit confusing to understand.

Line 508: “*identical hydraulic conditions over time*” – maybe the forcing is constant from the propellor, but the bed surface topography and fluid that is circulating is changing due to erosion?

Line 517: “*at the scale of our experimental setup*”

I think this kind of cautionary language and limiting the scope of the results to the experiment is needed in much of the discussion of the results. Extending these results to natural landscapes is challenging due to points raised in the review.

Line 519-521: but the point is exactly this! The slope of the channel or the width of the channel may be different because the plucking process is effectively removing material to balance the tectonic uplift rate (or sediment supply rate) with a different channel geometry.

There is no citation in that point to any field studies suggesting that plucking does NOT enhance erosional efficiency, so I do not see a basis for this argument or extending the observations to field settings?

Line 531-532: Minor detail, but this is not always true. Neely & DiBiase 2023 (Figure 5b) show that channels have similar width, or slightly wider channels with sparser fracture spacing. Why? I do not know (perhaps jamming effects due to large clasts sizes relative to narrow channels?). But that is what was there.

Lines 543-549: This paragraph illustrates the challenges of applying the results from the annular flume to a river. I think readers will be confused if they are left with this point. The experiments do not capture these feedbacks, but these feedbacks are thought to be important for sustaining plucking in the field?