Comment on esurf-2021-54
Anonymous Referee #1

Referee comment on "The imprint of erosion by glacial lake outburst floods in the
topography of central Himalayan rivers" by Maxwell P. Dahlquist and A. Joshua West,
Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-54-RC1, 2021

I like the ideas and goals behind this paper - to test for the importance of GLOFs in the
incision of Himalayan Rivers. I suspect the authors are right that this is an under-
appreciated process, and they do a good job of motivating the analysis. That said, there
are some issues with the analysis as presented that I think need to be addressed before
this paper can be published. These include:

- Normalized steepness index. In order to find a reference concavity, you need to
  restrict the analysis to settings where \((U/k)^{(1/n)}\) is constant in space, otherwise you
  are trying to fit a power law through data with multiple steepness indices. In the case
  of the Himalayas, it’s well known that the trunk streams have huge convexities in them
  (Seeber and Gornitz, 1983; Lavé and Avouac, 2001). This is for good reason as the
  rivers traversing the Himalayas cross massive changes in rock erodibility and rock uplift
  rate. I *think* that your very, very low concavities could be a reflection of this issue.
  In other words, you are fitting concavities through many rivers that, as a whole, are
  not concave. In addition, although I applaud the use of discharge as a more realistic
  metric, this actually further obscures the issue because it’s not clear how much of the
  atypical ksn scaling is driven by your integration of runoff. For these reason, I would
  advocate one of two approaches in your revision

a) Assume a more typical reference concavity (e.g., 0.45) and redo the analysis with
  drainage area instead of discharge. In this way, you’d be assuming that the scaling here
  is the same as other locations (and indeed countless papers in the Himalayas have found
  concavities in this range).

b) Use your discharge, width, and slope measurements to just calculate mean unit stream
  power for the rivers \((\rho g Q S/W)\). Then you can just look at patterns in river power
directly, rather through the ksn scaling.

- **Width scaling analysis.** This is a cool result, but one that could be an artifact of separating glaciated and non-glaciated rivers but not for the reasons assumed in the analysis (i.e. repeated GLOFs). Tongues of ice could extend well below the 4200 m LGM ELA. Indeed, today in the eastern syntaxis, which is the wettest area of the Himalaya, there are many glaciers that you can see in Google Earth that terminate at ~3,000 m in or near river canyons. How do you know that the signal you are seeing is not being driven (at least in part) by the fact that you are looking at areas that were influenced directly by ice? I know you’ve tried to avoid u-shaped valleys, but this is subjective. Are there reconstructions of valley glaciers that you can appeal to to help have more confidence in this? Even if there is no evidence for glaciers in these valleys, rivers that are downstream of glaciers are typically profoundly different than rivers that are not fed by glaciers. In particular, rivers that are (or were) fed by glaciers are commonly braided (or were) and hence have wider valleys. In addition, glaciated rivers convey much more coarse sediment, and are therefore more prone to aggradation. How can you be sure that the signal you are seeing is not simply an artifact of the difference in sediment supply between glaciated and non-glaciated basins? Here, some photos from satellite imagery with interpretations could help to make your case.

- **Hanging valley analysis.** As pointed out by Crosby et al. (2007) and Wobus et al. (2006), this is an instability that is common in unglaciated places too. The difference in hanging valleys between your populations is not remarkable. A check on this analysis could be to look at the height of the hang and estimate what it implies about rates of LGM and later river incision? Is this plausible? I suspect that on a 10 m DEM, the signature of GLOF-triggered knickpoints would be tough to see.

Again, I love the goal of this paper. However, I am not convinced that the authors have isolated signals in the topography here that are actually diagnostic of the processes they are trying to study.

A few minor edits below:

44- Should be “among” rather than “between”

66- no comma needed

127 - change “of” to “in”

139 - “stalemate” is a little colloquial here. Consider “steady-state”
205- This is expected from hydraulic geometry scaling and hence you should cite Leopold and Maddock (1953) here