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## Comment on esurf-2021-54

John Jansen (Referee)

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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-RC2>, 2021

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The abrupt draining of lakes dammed by ice or glacial moraines can trigger high magnitude glacial outburst floods (GLOFs) with an extreme capacity to entrain large boulders and erode bedrock, not to mention pose a hazard to life. The authors ask whether these extreme proglacial/postglacial events perform an under recognised role in the long term evolution of mountain topography — in addition to the well studied effects of glacial and periglacial processes. Previous work frames two opposing tool and cover mechanisms operating within river networks draining the rim of the Tibetan Plateau:

(1) headward fluvial incision promoted by the supply of glacigenic debris tools and glacial erosion versus

(2) coarse-grained, valley-damming sedimentary cover such as landslides and glacial moraines that impede headward fluvial incision.

It is astonishing just how little bedrock one sees exposed in Himalayan river channels (at least the few I've seen). How the hell do these rivers deepen their valleys? The authors contend that the cover effect imposed by coarse-grained deposits is overcome by the extreme transport capacity of GLOFs, which enhance fluvial incision of bedrock and leave a widespread topographic signature across the Nepal Himalaya. Perhaps most significant is the suggestion of a 'top-down' GLOF-driven model of valley incision augmenting the more widely appreciated (modelled and field-measured) incision responses to base level fall.

In my view, this is a well constructed argument that likely confirms the regional extent of what previous studies show on a more limited spatial scale. Its provocative edge is in showing that the geomorphically most-effective floods may be tuned to glacial cycles. Accepting this, the authors are making new demands on the canonical fluvial magnitude-frequency concept of Wolman and Miller (1960), which states that floods of moderate

magnitude and frequency are geomorphically most-effective, whereas extreme floods are too rare to drive landscape evolution. While the role of extreme floods in shaping fluvial landscapes has been accepted for half a century (e.g. Baker 1973, GSA SP 144), Dahlquist and West propose that GLOFs govern trends in valley slope, valley floor width, and trunk-tributary relationships on the scale of hundreds of km along the Nepal Himalaya. If this turns out to be correct, then the implication is that a topographic signature of GLOFs may exist in many of the world's glaciated mountain belts—in the form of a top-down to bottom-up physiographic transition. Indeed, one would have to ask, do moderate-sized floods (<100 yr recurrence) achieve much of anything along these mountain valleys?

#### *General comments*

(1) Overtopping and collapse of landslide dams also causes gigantic floods (e.g. Fan et al. 2020, ESR 203). There is no obvious reason why such floods would be any smaller than GLOFs and certainly landslides are more common than moraines in these landscapes. Are landslide dam outbursts also more frequent? The authors do make brief mention of this other variety of flood (I.59-62) but perhaps there is more to be said. Where do 'LDOFs' fit in the proposed top-down valley incision model?

I raise this point because landsliding is considered the dominant agent of denudation/sediment flux in tectonically-active mountain belts. By undercutting hillslopes, GLOFs are likely to trigger landslides in a similar way to how we understand fluvial incision propagates from base level fall. I would guess that sudden GLOF-flushing of a valley fill may affect friction thresholds in hillslopes in ways even more likely to trigger failure. We know that lithology exerts a strong control on landsliding, and if the physiographic transition corresponds both to i) the chief contact between strong High Himalayan rocks and weaker Lesser Himalayan rocks, and ii) the transition in dominance of top-down vs bottom-up processes, then should we not expect that lithology is playing a role here too?

(2) The authors link alluviation to wider valley floors (higher  $k^*wn$ ) and narrowing to bedrock exposure. Fair enough. KPs are found to be more common in tributaries to rivers with glaciated headwaters, and this is interpreted as the propagation of base level fall triggered by GLOF incision along trunk channels.

Mountain rivers characteristically undergo vertical fluctuations (of tens of metres or more) in valley floor elevation owing to downstream controls such as landslide dams or upstream sediment supply (see for instance Munack et al. 2016, QSR 149). Rivers with glaciated headwaters are subject to especially large fluctuations in sediment supply during a glacial cycle. While valleys floors were likely to have been alluviated during the last termination, the interglacial conditions of today typically promote incision along trunk streams (producing fill terraces) in response to the fall in paraglacial sediment supply. This drop in sediment supply is essentially a top-down process and the associated base level fall propagates up the tributaries as KPs incising old valley fills. Are the KPs shown in Fig. 4 cutting bedrock or valley fills? I recognise this may be difficult to determine in every case but it seems a key difference.

(3) I am still a bit unclear on why a threshold drainage area should apply to GLOF generation. Big GLOFs require big/tall moraines and glacigenic sediment volumes reflect glacial dynamics and supraglacial sediment loading (landsliding again!), neither of which is closely tied to area. What else could be responsible for this ~10 km<sup>2</sup> threshold? A signal of debris flows and their runouts? The authors are up-front about this potential complication (l.254-261) and they are in a good position to appreciate the issue given they have studied debris flows in the region.

Nevertheless, the 0.48 km<sup>2</sup> drainage area cutoff used in this study (from Roback et al. 2018) takes the analysis into channels that may experience a lot of debris flow activity. Judging by the slope-area data for central Nepal (Roback et al. 2018), many of these channels are well in excess of the  $S > 0.03$  widely regarded to be dominated by debris flow incision (e.g. Stock et al. 2005, GSAB 117). Perhaps the point just needs some additional bolstering (l.260-261).

(4) Gigantic immobile blocks emplaced in the channel via landslides are presumably subject to partial breakdown by plucking and abrasion during high magnitude monsoonal floods. It seems likely that some fraction of the less gigantic blocks will be mobilised thanks to this in situ destruction, and if the intervals between GLOFs is long then it may be significant. Given the troubling lack of nineteenth century references in this MS, in situ breakdown of clasts may be an opportunity to cite Sternberg (1875, Z f Bauwesen 25); see also Dingle et al. (2017, Nature 544).

#### *Specific points (keyed to line #)*

23 - Perhaps acknowledge the process of in situ breakdown of blocks over time, as noted above.

24-26 - Please clarify this a little. Do you mean the transport capacity increases with the passage of the flood bore? This is likely to vary depending on the nature of the dam failure. Perhaps spell out the general properties: geomorphic work done depends on the bed shear stress, the viscosity of the flow, the resistance to erosion (critical shear stress for entrainment) and the duration of the flood.

31 - Scherler et al. (2014) also point out that glacial dams are likely to reform after each failure thereby causing multiple possibly annual or decadal GLOF events. Perhaps under some circumstances GLOFs are rather frequent? This may be worth a mention.

35 - How representative of long term bed condition are today's observations? As noted above, valley floor elevations can fluctuate over tens of metres (or more) in these settings and this vertical instability is likely to vary over different timescales in response to: (1) the mag-freq of rainfall-runoff floods, (2) stochastic inputs of sediment to the valley floor via

landslides, much of that material being paraglacial in origin, (3) GLOFs, and (4) the interglacial-glacial cycles.

Chronometric evidence (OSL and Be-10) of fluctuating valley floor elevations is emerging too, e.g. Dosseto et al. (2018, QSR 197).

39 - Top down proglacial/paraglacial sediment load is already recognised as important for bedrock fluvial incision. Is 'glacially driven' erosion the right term here? How about glacially conditioned?

43 - Are these opportunities really unique?

56-57 - Are there other factors worth mentioning that affect the frequency of GLOFs? e.g. relief (~sed source), valley width and valley steepness (~lake volume), lithology, seismicity?

67-69 - What aspect of mass balance? Please clarify.

73-74 - Other mechanism exist too; I read this more in terms of a hypothesis that is to be tested here.

76-77 - Perhaps add that base level exists on a number of spatial scales. Base level can be local, e.g. landslide dams impose a local base level on the reach upstream which aggrades in response, a fault bound mountain front, or sea level.

97 - Does a best fit reference concavity value apply equally well across fluvial and formerly glaciated valleys? I expect valley troughs that have hosted glaciers for a good fraction of the Pleistocene might be less steep.

Fig. 2d - How is this a steady state ksn pattern? KPs are retreating upstream, hence the river profile is transient at the relevant timescale.

101-106 - As noted above, perhaps be more specific here: entrainment/erosion depends on bed shear stress, a function of bed slope and flow depth.

109 - All other factors being equal? What are these factors and what should we expect if they are not equal? Perhaps just rephrase.

110 - 'GLOF-influenced rivers will require lower ksn for the same erosion rate'. Is that the best way to express it? Is it more the case that GLOF-influenced rivers will tend to have lower ksn for a given erosion rate?

119-20 - If these KPs are solely GLOF induced, where are the base level induced KPs? Base level KPs are also known to cluster upstream of trib junctions due to the step down in drainage area (Crosby and Whipple 2006, Geomorph 82). Can the two KP types be differentiated? Presumably they amalgamate where they meet strong rock units.

139 - Stalemate is a good word but perhaps not the best term for topographic steady state. The processes are still active, it's just the external forms that remain invariant.

151-2 - The concavity parameter is defined under conditions of spatially uniform uplift and erodibility, but this landscape contains sizeable KPs (>20m) that must therefore represent transient conditions. How to reconcile with the choice of reference concavity?

208 - Or, rather it is unit stream power that increases with channel narrowing (seeing as W is the denominator).

210 - Rather than framing the width-area relation in terms of the slope-area equation, I suggest this power law W-Q relation (among the other hydraulic geometry relations) should be attributed to Leopold and Maddock (1953, USGS PP 252).

229 - This may be largely true, but I expect that valley fill alluviation behind a landslide, for instance, could elevate the thalweg in a matter of a few years only.

Good style to cite how the curves were fitted, e.g. Fig. 5 caption. Others should follow this example.

Having got to the end of the MS I unexpectedly found myself thanked in the acknowledgements! I think this may have more to do with Dr West's good manners than anything I could have contributed at the time!

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