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David Egholm
Associate Editor, Earth Surface Dynamics

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Dear Dr. Egholm,

Thank you for considering our manuscript “Glacial buzzcutting limits the height of tropical mountains.” We really appreciate the detailed and engaging reviews provided by both you and Peter van der Beek, and we thank you and Earth Surface Dynamics for your guidance in revising our work.

We hereby submit the revised manuscript, now entitled "Glacial limitation of tropical mountain height”, for your consideration. The critical reviews motivated us to refocus our core arguments, to augment our methods of analysis, and to expand their scope; as a result, the manuscript has undergone substantial modification. We estimate the order of the changes to the text and figures to be around 40%, including the creation of new sections and figures, the rewriting of several existing sections, and a reorganization of the paper structure; for these reasons, it was not feasible to generate a track-changed document. Instead, the changes are listed on pp. 2–3 below, followed by a detailed response to reviewers on pp. 4–15 (in which *the reviewer comments are in italics* and our responses are in plain text). We believe that by working carefully to address the comments and issues raised by the reviews we have made significant improvements to the manuscript.

The main changes are as follows. We now emphasize the concept of glacial limitation of mountain height (rather than the "glacial buzzsaw") and its relationship with a perched erosional base-level at the ELA. A major addition is the introduction of a modified form of hypsometric analysis—which we call “progressive hypsometry”—and its application in the tropics. We use this tool to evaluate the hypothesis that glacial base-levels are widespread in tropical mountains, and we tie results from this analysis to the original study of Costa Rica and Taiwan. We note that our conclusions are broadly similar to those presented in the previously submitted manuscript, but are now supported by a much broader and stronger base of data and analysis.

We hope this revision meets with your approval.

Sincerely,

Max Cunningham

Changes to text

OS = old section (in original submission)
NS = new section (in revised submission)

1) Title has been modified.
2) Abstract has been entirely rewritten.
3) Introduction section 1 has been expanded. Some of the text has been transferred to a new section NS2 "Evidence for glacial limitation: a review". A new subsection NS1.3 detailing the paper structure has been added.
4) OS2 "Tectonic and geomorphic setting" has been transferred to new subsections NS3.2.
5) "Methods" section OS3 has been moved to new section NS5.
6) A wholly new section NS3 has been added. This new section has a description of a study areas taken from across the tropics that are analyzed with both traditional and progressive hypsometry (the latter a methodological innovation presented for the first time in this paper).
7) A new subsection NS3.1.2 details issues regarding the ELA in the tropics.
8) OS3.2 describing surface exposure-age dating has been moved to NS5.3.2.
9) OS3.3 describing scarp mapping has been moved to NS5.3.1.
10) A short new section NS4 "Data" has been added to summarize DEM, imagery and exposure age data sources.
11) The old section OS4 "Results" has been moved to NS6.
12) The revised NS5 "Methods" includes new subsections on DEM processing (NS5.1), a complete revision of OS3.1 into NS5.2 "Hypsometry", a new subsubsection NS5.2.1 on "Range-scale hypsometry" (the traditional approach), a new subsubsection NS5.2.2 describing the new tool of progressive hypsometry, and a new subsection NS5.3 "Focus sites" which draws text from OS3.3 and OS4.1
13) The new results section NS6 includes additions covering results from range-scale (traditional) hypsometry (NS6.1) and progressive hypsometry (NS6.2), together with a reorganized description of results from studies of the two focus sites (NS6.3: Costa Rica; NS6.4: Taiwan). The comparison subsection NS6.5 is a largely unmodified OS4.3.
14) The new discussions section NS7 is a complete revision of OS5.
15) The revised conclusions section NS8 is a complete revision of OS6.

Changes to Figures

OF = old figure (in original submission)
NF = new figure (in revised submission)
NT = new table (in revised submission)

1) NF1 documenting rationale for the work has been moved from the supplement to the main text and annotated.
2) NF2 has been introduced to support expanded section 1 and NS2.
3) NF3 supports NS3, which is a description of study areas (both new and old) from across the tropics.
4) NF4 and NF5 demonstrate how "progressive hypsometry" works.
5) OF5 demonstrating scarp encroachment has been moved to NF6, and now includes field documentation of scarps.
6) NF7: field photos of ^{10}Be sample sites, has been moved from the supplement.
7) NF8 includes entirely new results from new hypsometric analyses.
8) OF1 has been moved to NF9, and now includes a glacial geomorphic map and field photos.
9) NF10: a satellite image, glacial geomorphic map, DEM, and field photos of Nanhudashan.
10) OF2 has been moved to NF11. Minimal changes. OF3, of "hypsometry at different scales" has been removed, and is now conceptually addressed in NF8.
11) OF5 to Fig. 12: No changes.
12) NF13: a schematic explaining how to interpret the progressive hypsometry plots.
13) NT1: provides a breakdown of the evidence for glacial limitation at each of the study sites.
14) OF6 to NF14: minimal changes.

AE Comments

As associate editor, I would first like to thank you (authors) for sending the manuscript to ESurf and Peter van der Beek for providing a thorough review. It has unfortunately not been possible for me to secure additional reviews, but the review by Peter van der Beek provides a number of relevant points and constructive ideas. I encourage you to use all the reviewer comments to revise the manuscript including adding better and more detailed documentation to support the hypotheses presented.

We appreciate the very thorough and detailed reviews provided by both and you and Peter van der Beek, and for steering our manuscript through a very helpful review process. We have considered all comments carefully, and include revisions to address the ideas and suggestions provided by you and Peter van der Beek.

Reviewer 1

Cunningham et al. report morphometric analyses of high-elevation mountainous massifs in Costa Rica (Cerro Chirripo) and Taiwan (Nanhudashan) to argue that these show hypsometric maxima at the elevation of the ELA during glacial advances, a characteristic of peak erosion by the "glacial buzzsaw". They also report 9 new cosmogenic 10Be ages from Cerro Chirripo to show that glacial moraines and sculpted bedrock are roughly contemporaneous with the LGM.

This is a controversial topic. Numerous authors have enthusiastically adopted the "glacial buzzsaw" concept but others have provided critical assessments of some of the observations put forward to support it. The authors provide a fairly balanced representation of the argument in their introduction. Nevertheless, pushing the idea to encompass tropical mountains is a bold step. It appears to me that this manuscript has been in the reviewing circuit for a while now and I believe it deserves to be published, if only to have the idea out and open to critical discussion. However, I feel that the authors could, and should, back up their arguments with much better and more detailed documentation.

We thank Peter van der Beek for the comments, and for encouraging the publication of our work. Below we provide a detailed response to all comments.

For the Cerro Chirripo, the problem is that the morphometric observations alone cannot discriminate between the "glacial buzzsaw" interpretation and the more conservative interpretation (put forward by Morell et al., 2012) that the high-elevation low-relief landscape represents a "relict" landscape, preserved and passively uplifted since the onset of Cocos Ridge subduction ~3 My ago.

We address this criticism on multiple levels.

First, we articulate the concept of glacial limitation much more thoroughly (p. 5, Sec. 1.1). Our goal has been to assess whether glacial erosion limits the height of tropical mountains. In the revised manuscript we present hypsometric analysis of ten different tropical mountain ranges, including the Talamanca Range, and show that none of them have been able to push rock mass through the ELA without being subject to the introduction of a glacial base-level. An ensemble of ten mountain ranges is the central evidence that glacial erosion is effective at reducing relief above the cpELA. In any particular mountain range, the uplift of low-relief topography that is glacially eroded as it passes through the ELA does not disqualify the potential for glacial limitation.

We use the Talamanca Range of Costa Rica as a type example of a marginally glaciated mountain range, and ask: has glacial erosion been effective there? We now demonstrate more clearly that it has (Fig. 9). These and similar observations in Taiwan support our broader claim that substantial glacial erosion takes place as soon as topography reaches the cpELA, and that it continues to shape landscapes as they rises through the ELA (Fig. 8).

Second, in the Talamanca Range, we identify glacial landscapes above 3000 m and centered around an estimated ELA of 3500 m. In a zone between 3000 m and 3500 m there is often a pronounced slope break that separates steep fluvial landscapes from lower-sloping glacial landscapes. Morell et al. (2012) identified knickpoints at ~2200 m ± 300 m, far below the glacial landscapes in Costa Rica. The presence of these knickpoints and other lines of evidence have supported the argument that a rapid increase in the rate of crustal deformation starting after 3 Ma drove the uplift of a landscape of 1-1.5 km of relief, with the lower slopes of this relict landscape preserved as a low-relief surface at around 2200 m. Morell et al. (2012) explicitly refer to low-relief topography above ~2000 m but *below* 3000 m as evidence of this uplifted landscape, with “high, isolated peaks above 3000 m.” The glacial landscapes we describe are all above 3000 m, and cannot be linked to the inferred base of the relict landscape at ~2200 m. The low-relief topography we observe near the ELA in Costa Rica is thus not presently explained by a “more conservative interpretation.”

The authors argue that the coincidence in elevation, both between the two studied examples in Costa Rica and Taiwan and with the elevation of the glacial-maximum ELA, supports the glacial buzzsaw interpretation. However, there is no way for the reader to assess this argument, as the ELA elevation for Costa Rica is only cited from (partly grey) literature, and is not given at all for Taiwan. We would like to see a detailed geomorphic map for the Cerro Chirripo, showing the elevations of the different glacial features discussed, as well as field photos showing some of these features. There are some in the Supplementary Information (and actually some more convincing ones on the first author’s website blog) but these should be part of the main paper.

A glacial-maximum ELA estimate of 3500 m seems on the low end for a site at <10 °N; for instance, glacial ELA estimates for the Mérida Andes in Venezuela, at approximately the same latitude, vary between ∼3600-4000 m (Stansell et al., 2007; although some estimates on the wet SE side of that mountain range descend to <3500 m). So again, more discussion and justification of these numbers seems important. A similar discussion is required for Nanhudashan; this site is at 24°N in a different geographic and climatic setting, so why should we expect a similar ELA elevation?

We have added substantial discussion about the typical cold-phase ELA around the tropics (p. 8, Sec. 3.1.2) and cite literature that brackets the tropical cold-phase ELA to between 3400-4000 m (this is actually the range that Stansell et al. (2007) cite for the Mérida Range). The 3400-4000 m ELA range is often observed within single, wide mountain ranges (e.g., Mérida Range, Merauke Range) but narrower mountain ranges (Talamanca Range, Finisterre Range) around the tropics always have a cold-phase ELA in this range.

We then describe the methods used to estimate the ELA at Cerro Chirripó (p. 11, l. 12-22) and throughout Taiwan (Sec. 3.2.2, p. 12, l. 5-31). We do cite work from the "grey" literature, so as to be scholarly and thorough, and to give credit to the long history of research at Cerro Chirripó and Nanhudashan, but we rely most heavily on sources published in journals such as *Quaternary Science Reviews* (Siame et al., 2007; Hebenstreit et al., 2011), *Quaternary Research* (Orvis and Horn, 2000) and *GSA Bulletin* (Lachniet and Seltzer, 2002). We also provide a glacial geomorphic map for Cerro Chirripó (Fig. 9) and for Nanhudashan (Fig. 10).

We use LGM ELA estimates for Cerro Chirripó and Nanhudashan that are presented in the literature, and it is beyond the scope of our work to assess the climatic reasons that the ELA is similar between these two places despite being separated by 13° latitude.

In summary, we have included the following elements in the revised manuscript:

1) more detailed geomorphic maps of both focus sites in Costa Rica and Taiwan;
2) discussion on the procedures for estimating the ELA in Taiwan;
3) discussion of other ELA estimates from around the tropics.

Likewise, it is not very clear what was sampled for cosmogenic isotope analysis and why. Showing the sample sites on a geomorphic map would help significantly, as would moving some of the field photos from the Supplementary material to the main text.

Field photos of all samples for ^{10}Be analysis are now presented in Fig. 7, and are labeled on the glacial geomorphic map of Cerro Chirripó on Fig. 9.

In the model proposed by the authors, glacial "buzz-cutting" during cold periods competes with scarp encroachment during interglacial times (as illustrated in cartoon style in fig. 6). However, it is not clear what would drive continued scarp encroachment in this model? As the fluvial landscape below the knickpoints has a typical concave form, any lowering of the glacial landscape during "buzz-cutting" would tend to lower the slopes below the knickpoints, which does not favour scarp retreat. The authors argue for "outward spreading" of the perched glacial landscapes but, in the absence of significant deposition, it is not clear how that would work. This appears like a weak point in their argument, as these knickpoints are more directly explained in the "remnant landscape" model.

We now make clear from the very beginning of the manuscript that glacial erosion introduces a perched base level near the ELA (p. 3, Sec. 1.1). Glacial erosion effectively "disconnects" fluvial landscapes in the following way: (p. 3, l. 20-24):

"Below the ELA, ice flow spreads laterally, ablates, and slows, driving sub-glacial erosion rates to zero. The (near-) ELA acts as an erosional base-level, above which ice-driven erosion pushes headward into the landscape (Fig. 2b). Glacial erosion ultimately disconnects these landscapes from fluvial base-level by blocking channel incision above the glacier terminus—an elevation where glacial erosion is also least effective."

The scarp encroachment we propose arises from the disconnection of glacial landscapes from fluvial landscapes. As we describe throughout the manuscript (p.3-4, Sec. 1.2; p. 20, l.11-18) this disconnection happens at high elevations in the tropics because of the high ELA. Whatever lowering takes place by glacial erosion is thus not sufficient to weaken the effect of scarp encroachment.

"Outward spreading" is poor phrasing on our part, and this line has been removed from the manuscript, as has this entire section. We now discuss the growth of terrain near the ELA base level, and support this claim with our results from "progressive hypsometry" (Fig. 8).

Finally, the knickpoints that mark the break between glacial and fluvial landscapes around 3000 m are not the same knickpoints as those thought to represent the break between the uplifted relict landscape and steeper topography at ~2200 m.

One could envisage the authors' model in case of continuous rapid uplift and fluvial downcutting, which is the case in Taiwan (I do not know the Costa Rica case sufficiently well to comment on this). But in this case, the scarp retreat would be independent of the glacial "buzz-cutting" and would happen anyway (which it does; pretty much every hill slope in the Taiwan Central Range is affected by landsliding). In that case, the glacially affected high-elevation low-relief parts of the landscape are just transients that are rapidly erased and one can question their significance for overall long-term landscape development. This part of the model clearly requires some more elaboration.

We have addressed this comment thoroughly at various points throughout the revised manuscript. For example, we are now more explicit about our choosing to focus on the Talamanca Range and Central Range of Taiwan because glacial erosion there has left a very marginal imprint, particularly compared to mountain ranges such as the Merauke Range and the Mérida Range.

In the context of ten tropical mountain ranges, we call attention to the fact that both the Talamanca Range and the Central Range of Taiwan (as well as the Finisterre Range, Owen Stanley Range, and Crocker Range) are all capped by the cold-phase, despite being only marginally glaciated. We pose three possibilities for this coincidence, which are discussed on p. 22, Sec. 7.2, and illustrated in Fig. 2.

The scenario described in the comment above (*the glacially affected high-elevation low-relief parts of the landscape are just transients that are rapidly erased and one can question their significance for overall long-term landscape development*) is actually closest to the scenario we propose to be the most likely. The crux of how "significant" a role glacial erosion has played in the long-term evolution of a mountain range such as Taiwan is a function of 1) how close it is to a fluvially-driven steady state elevation in the absence of glacial erosion, and 2) whether it is likely that it has been glaciated more than once. These possibilities are discussed in detail in Sec. 7.2.

More specific comments, tied to page/line number:

p. 2 / l. 15 (and elsewhere): some of the wording in the manuscript ("we add a new spin to the story . . .") makes it sound like the objective here is to "push" a "nice story" instead of seeking truth, which is what science is (should be) about. This is probably not the authors' intention and the writing is simply a bit too colloquial in places, but you should really try to avoid such phrasing.

Our objective is certainly not to push a nice story. We have reworded colloquial phrasing throughout the manuscript.

p. 3/ l. 20-24. The authors should be aware of a recent re-analysis (Schildgen et al., in press) that has shown the Herman et al. (2013) results to be flawed by a "spatial correlation bias", in which spatial variations in exhumation rates are translated into temporal increases by their model. Therefore, the thermochronometric record can no longer be used as support for increased erosion rates during Quaternary glaciations. Also, note that the Shuster et al. (2011) study argued for rapid glacial-valley incision (i.e. analogous to what Valla et al. (2011) argued for in the Western Alps) and does not pertain to glacial "buzz-cutting".

Thank you for pointing us to this reference. We have rewritten this section of the introduction, and now reference this paper (p. 7, l. 5-11).

p. 5 / l. 24: "narrative" – see comment on p. 2/l. 15 above.

p. 6/ l. 1-7: this needs to be backed up by field photos and a geomorphic map.

Field photos and a geomorphic map are now included in Fig. 9.

p. 7 / l. 1-5: similarly, a map of the Nanhudashan area showing the occurrence of these glacial forms would be useful.

This is now included in Fig. 10.

p. 7 / l. 20: Shuster et al. (2011) focused on glacial valley incision, not on cirque retreat.

We have removed this reference from the discussion of cirque retreat.

p. 8 / l. 27: "our conclusions are not affected by the choice of production rate or scaling"; without any justification, this is a rather empty statement. I would suggest to either delete it or to provide supporting data.

The supporting data are included in the supplementary file, and simply show that different scaling regimes alter calculated exposure ages by <2 kyr. Since our central conclusion is that glacial valleys at Chirripó were subject to LGM erosion, we conclude this relatively small range of variability does not affect our overall conclusion. We ultimately removed this line from the revised manuscript.

p. 9 / l. 10-12: a slope map would help to demonstrate and justify the location of these erosional scarps.

We did not find room for a stand-alone slope map, but we do give a sense of the presence of these scarps with the binary slope map superposed on the DEM in Fig. 9C and Fig. 10C as well as with field photos in Fig. 6 and Fig. 10.

p. 10 / l. 18-20: can you elaborate on what this statement is based on?

Elaboration is now provided on p. 17, l. 5-10.

We sought a way to quantify the effect of scarp encroachment into glacial catchments, specifically, what segment of glaciated valleys have been affected by scarp encroachment. To do so, we required some reference point for individual catchment outlet elevations, which we chose to be 3000 m. We chose this elevation on the basis that the lowest moraines observed extend to about 3000 m elevation.

p. 11 / l. 12: "unrealistically" appears as a strange word choice for assessing data. What you probably mean is that this age, which is significantly younger than the LGM, implies that the surface must have been buried. Nothing unrealistic about that . . .

We have reworded this sentence (p. 19, l. 4-6).

p. 12 / l. 3: the glacial ELA elevation in Taiwan has not been demonstrated or even discussed at this point.

Discussed above.

p. 12 / l. 25-26: this statement requires justification.

This line has been removed in the revised manuscript, but we have now documented more clearly that the highest landscapes in both Taiwan and Costa Rica are close to the cpELA, and upon careful examination of several such landscapes, we see clear evidence of glacial erosion.

p. 14 / l. 6: what do you mean by "tile-scale"?

1°x1° SRTM DEM tiles. Egholm et al. (2009) used these tiles in their global analysis.

p. 14 / l. 10-12: I don't think this statement has been demonstrated. One could just as easily argue, even within the context of this model, that the mountain belt elevation hovers around an elevation that is set by the relative efficiency of tectonic uplift versus (glacial or fluvial) erosion – it is *lowered a bit during glacial times and uplifted during the transient post-glacial period of scarp encroachment.*

See comment about three landscape evolution scenarios above.

p. 14 / l. 24-25: this is a fairly bold statement that extrapolates the findings and interpretations from Nanhudashan to all of the Taiwan Central Range. To do this, you would at a minimum need to show that the rest of the Central Range is equally affected by glacial erosion of the highest peaks and shows similar morphometry. In my understanding, glacial features in Taiwan have only been described from Nanhudashan.

This sentence has been removed by a similar case is made in Sec. 7.2.

First, to clarify, LGM glacial features in Taiwan are best preserved at Nanhudashan, and for this reason we focused our analysis there. More ambiguous LGM glacial features have also been reported at Hsuehshan (Cui et al., 2002) and Yushan (e.g., Hebenstreit et al., 2011). We now discuss this on p. 12, l. 1-17.

We have proposed that an explanation for the relatively constant elevation of peaks throughout Taiwan—many of them close to the ELA—can be explained by a glacial erosion acting at different, isolated peaks throughout the Pleistocene. Wherever glacial erosion does occur, a low-sloping, transient glacial landscape is left behind and eventually wiped from the landscape by fluvially-driven scarp propagation.

We respectfully disagree with the statement that "at a minimum" we would need to show that "the rest of the Central Range is equally affected by glacial erosion." In our model context, glacial erosion stops the highest parts of the landscape at the ELA, and glacial landscapes are then removed by the flanking fluvial network by scarp encroachment, reducing their preservation potential. To infer glacio-fluvial height limitation in Taiwan we rely on similar evidence from comparable mountain ranges (such as the Talamnaca Range, Finisterre Range, Owen Stanley Range, and Crocker Range). This statement is thus not premised on our observations from Taiwan alone.

p. 14 / l. 31-32: how would the glacial "buzz-cutting" "prime" the landscape for rapid horizontal scarp encroachment? See general comment above.

See answer above with regard to scarp encroachment.

Fig. 1: it would be nice to have an uncluttered DEM image with an elevation scale (as well as a horizontal scale and indications of latitude and longitude). The glacial extent and the location of the scarps could be moved to the satellite image of fig. 1a (or better, could be part of a geomorphological map). The inset location map is close to unreadable.

We have made updates to the Costa Rica Fig. 9 to improve readability.

Reviewer #2

General comments:
Most previous studies of mountain range height and glacial erosion have used correlations between ELA and max topography/hypsometric maxima along climatic gradients caused by temperature or precipitation to infer that glacial erosion influences mountain range height. To me such spatial correlations provide a stronger argument than the two isolated cases presented here. We know from global compilations of topography and ELA that many exceptions to the overall trend exist for numerous reasons. I therefore encourage you to expand your study and collect data from more tropical ranges. Do any of the tropical ranges stand high above the ELA? Or do the two cases documented here indeed represent a general pattern? That two selected ranges have heights that match the estimated ELA can easily be a coincidence. Even worse: Were the ranges selected for this study because they happen to have heights that match the ELA? You need to show us more data to answer such questions and to support the general points made.

To be clear, the decision to focus on the Talamanca Range and the Central Range did not begin with their ELA-height match. Rather, we were initially struck by the following: even though global scale observations (e.g., Egholm et al., 2009, Fig. 1C, adapted and presented in the revised manuscript as Fig. 1) of the ELA-height match include the tropics, glacial erosion has *not been proposed as a mechanism for limiting tropical mountain height*. We now make this point clearly in Sec. 1.2 (p. 3-4).

The core revisions address this comment. We begin with a review of the evolution of thought on glacial limitation (Sec. 2.2, p. 5-7), and emphasize that glacial limitation is not generally accepted as a viable mechanism in the tropics (Sec. 2.3, p. 7). To reassess this claim, we now present analysis of ten tropical mountain ranges. We originally considered all high tropical mountain ranges, but ultimately excluded some mountain ranges which we thought would reduce the clarity of the analysis (Sec. 3.1.1, p. 7). The list of tropical mountains now includes:

1) Leuser Range, Aceh, Indonesia
2) Central Range, Taiwan
3) Talamanca Range, Costa Rica
4) Crocker Range, Borneo
5) Finisterre Range, Papua New Guinea
6) Owen Stanley Range, Papua New Guinea
7) Merauke Range, Papua
8) Mérida Range, Venezuela
9) Sierra Nevada de Santa Marta, Colombia
10) Rwenzori, East Africa

We present hypsometric analysis of all of these mountain ranges (Fig. 8), the details of which we discuss in response to a separate comment. We ultimately choose to focus on the Talamanca Range in Costa Rica and the Central Range of Taiwan because glaciation there has been particularly marginal (Sec. 3.2, p. 9-10).

Regarding the topographical analysis you compute the hypsometry for individual catchments (focused hypsometric analysis) instead of simply computing the hypsometry of a large area (the full range, or anything above a certain elevation). While this may open for more detailed insights, it also has disadvantages when it comes to hypsometric maxima, because a catchment defined by flow routing should always have a hypsometric maxima somewhere in between the max and min elevations in the catchment. Hypsometry of a catchment may therefore differ from the hypsometry of a mountain range, which can have a hypsometric max close to base-level. Your use of catchments at different scales only partly address this issue, and to me mountain range hypsometry is just a simpler metric to understand and use.

We have engaged with this comment a great deal, and address it on several levels. First, we describe the ways that hypsometry has traditionally been used to assess glacial limitation (p. 5, l. 21 – p. 6, l. 15; p. 13 Secs. 5.2). We then employ traditional (mountain range-scale) hypsometric analysis on the ten targeted mountain ranges.

A central question is whether the absence of a hypsometric maximum at the mountain range-scale is indicative of the absence of (significant) glacial erosion. When the ELA is a relatively high elevation, fluvial catchments must be large (in elevation range) for glacial erosion to take place at all. Thus the absence of a hypsometric maximum at the ELA on the large scale does not indicate the absence of glacial erosion, or even glacial limitation (or glacio-fluvial limitation for that matter).

We thus are left with the following problem: what scale of analysis is appropriate to assess the role of glacial erosion in limiting mountain height? We argue that different scales of analysis are needed to assess the overall significance of glacial landscapes in environments like those found in the tropics.

We introduce a new method of hypsometric analysis that we call "progressive hypsometry" (PH) in Sec. 5.2.2, at which point we describe the algorithm in detail. We then implement PH in the ten selected tropical mountain belts. We pose this method as a solution to the problem of scale in hypsometric analysis. Rather than choosing one scale, either large or small, and checking for a hypsometric maximum at the ELA, we find the hypsometric maximum of catchments at virtually all scales in a targeted mountain range.

To summarize, progressive hypsometry can characterize the fine scale topographic patterns of entire mountain ranges, and can reveal features that go missed in tile hypsometry (Fig. 8a2-j2). In all of the mountain ranges we analyze, mountain range-scale hypsometry shows very little area near the ELA. Yet, nine of ten show evidence of glacial erosion at high elevations, and in some cases particularly strong glacial erosion. We discuss these results extensively in Sec. 7.1, p. 19-22.

Alternatively, you could also compare with focused hypsometries of catchments where there are no signs of glacial erosion. Do they have the same type of maximum or are they notably different?

Progressive hypsometry allows us to compare the hypsometry of virtually all catchments in each mountain range.

It would be useful to also see longitudinal profiles of valleys with and without evidence of glacial erosion.

We have looked at the longitudinal profiles of glaciated and unglaciated valleys in Costa Rica and Taiwan, but, in light of the new data provided in the revised manuscript, we consider hypsometry to be the most helpful to contextualize glacial influence (e.g., Fig. 8).

I recommend that you also address the height of the ridges above the ELA. The ridges on the plateau are rather low and I would expect them to be higher, if glacial erosion around LGM was the main erosion mechanism at high elevation. Pedersen et al. (Geomorphology 122, p. 129-139, 2010) showed how ridge height above ELA seemingly depends on the rate of tectonic uplift. Tectonic uplift rates are high in both these ranges, so what keeps the ridges down to few hundred meters above the estimated ELA? Could it be periglacial slope processes, and would they have enough time to operate in the Holocene?

Thank you for reminding us about this important reference, which is highly relevant to our work.

Unfortunately, this comment is difficult to address in a straightforward manner. As we describe in Sec. 7.1, p. 21, l. 3-23, we propose that the growth of tropical glacial landscapes is governed by the following three factors:

(i) the volume and pattern of rock uplift through the cpELA;
(ii) the efficacy of glacial erosion; and
(iii) fluvially-driven destruction of glaciated terrain.

Pedersen et al. (2010) invoke a steady state balance between rock uplift and glacial erosion to explain the correlation between ridge height and uplift rate in glacially eroded landscapes at the mid-latitudes (p. 136). In the mid-latitudes, it is easier to envision how this relationship develops. Our revised manuscript highlights that tropical glacial landscapes evolve in a rather different way, so this particular model is not necessarily applicable to the mountains we analyze.

More specific questions:

Page 3 Line 29: I do not see how it can be a provocative statement that glacial erosion limits the height of mountains – erosion does that. Please rephrase to explain the provocative part.

Thank you for bringing this to our attention. We now specify on p. 6, l. 26-27 that it is not universally accepted that *ice-driven* erosion, specifically, limits mountain height. We expand on this idea on p. 6 l. 26- p. 7 l. 8 in the revised manuscript.

Page 4 line 5-10: This paragraph unfortunately repeats a misunderstanding that I think started with Hall & Kleman (2014): The glacial buzzsaw mechanism does not rely on horizontal erosion, and I do not think that any of the computational landscape models that you cite (e.g. Anderson, 2006; Egholm et al., 2009; MacGregor et al., 2009) even have horizontal erosion. The link between ELA and hypsometry arises because (vertical) glacial erosion is downwards limited by the mass balance of the glaciers (Egholm et al., 2009). Small glaciers do not erode deeper than the ELA because they cannot exist there. Larger glaciers can, however, because the ice flux into them keeps them alive well below the ELA. That larger glaciers cut deeper and faster than cirque glaciers is therefore not surprising, and not at all in conflict with models for the glacial buzzsaw. These two elements of a glacial landscape go hand in hand.

This paragraph was in reference to Valla et al. (2011) who claimed that evidence of rapid glacial incision below the ELA "contradicted" the concept of the glacial buzzsaw, but your comment demonstrates clearly that their findings are not necessarily such a contradiction. We have rewritten this section entirely.

We now focus on the concept of a glacial base-level, which Egholm et al., 2009 reference in a similar way to the comment above. This idea is developed in Sec. 1.1 and Sec. 2.2.

Page 6, line 10: It would be good to have an uncertainty estimate for the ELA. It is important here because the differences in hypsometric maxima are rather small.

We ultimately chose to restrict the ELA to a single benchmark value in this segment of the analysis because we were interested in whether variability in the modal elevation of glacial catchments could be explained by scarp erosion. These landscapes are not large enough for substantial variation in the ELA, and we propose that the variability we do see is driven by post-glacial erosion. Our findings support this claim.

Page 7, line 21: Why not record the aggregate of many valleys? Sounds good to me.

Hypsometric maxima at the "aggregate" scale are effectively recorded in progressive hypsometry.

Page 9, line 31: This is where the uncertainty on the ELA becomes relevant.

See comment above.

Page 11, line 4: I do not think that you are constraining the timing of glacial erosion here. Your (few) boulder samples may constrain timing of deglaciation, but the (even fewer) bedrock samples do not show any clear pattern.

This line has been rephrased.

Page 14, line 30 and many other places including the title: Why not just write "erosion" instead of "buzzcutting"? I don't think we really need more "buzzwords" than we already have.

We have removed all mention of “buzzcutting” and stick with “glacial limitation”. This terminology is presently used in the literature, such as in Egholm et al. (2009) “Glacial effects limiting mountain height”.