Response to Reviewer #1

We thank Reviewer 1 (Peter van der Beek) for his thorough review, specifically for the helpful suggestions for additional figures, for highlighting weak points in our discussion, and for drawing our attention to some important references that were omitted. The suggested figures will eminently improve the clarity and quality of the paper, as will an updated discussion about general evidence for (and controversy surrounding) the existence of the "glacial buzzsaw."

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.

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.

The submitted manuscript focuses on the Talamanca Range of Costa Rica, where we identify low-relief 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 "more conservative interpretation."

In our early unpublished (but public) work (e.g., conference abstracts, blogs) we posed glacial erosion as an alternative to the uplift of a relict landscape as an explanation for the origin of high elevation, low-relief topography, but we now recognize that these are not mutually exclusive mechanisms. The discussion has evolved substantially since then, and in the submitted manuscript we are careful not to suggest that the glacial landscapes are related to the base of the apparent uplifted, relict landscape between 2000 and 3000 m.

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.

LGM ELA estimates produced from geomorphic mapping and ice surface reconstruction from around the tropics are generally between 3400-3800 m (e.g., Hastenrath, 2009; Mark et al., 2005) and this range of variability is consistent on a wide geographic scale. To demonstrate the widespread consistency, we list a small number of mountain ranges with paleo-ELA estimates within the cited range: New Guinea highlands (Prentice et al., 2005), eastern Africa (Kaser and Osmaston, 2002; Kelly et al., 2014) and the northern Andes (Stansell et al., 2007). There are some tropical ranges with a paleo-ELA estimate quite above this span of elevations, such as the Cordillera Blanca of Peru (4300 m, Smith et al., 2008), but in such places there are usually local circumstances that promote a higher ELA, such as a relatively dry climate associated with strong precipitation gradient in the Peruvian Andes.

The climate of Costa Rica and Taiwan is reasonably similar to other places from around the tropics whose LGM ELA estimate is between 3400-3800 m. Even before a detailed analysis of regional ELA estimates in Costa Rica and Taiwan, it is safe to assume that their LGM ELA was within this range.

We agree that the inferred position of the ELA in both landscapes must be presented in more detail, including visually in figures. In addition, more discussion is needed to demonstrate our level of confidence in the estimates provided. However, we emphasize that the LGM ELA estimates provided in the submitted manuscript are well within the expected range of tropical LGM ELA variability.

The LGM ELA of 3500 m cited from the literature for Costa Rica is, in our opinion, trustworthy. This estimate was replicated by two independent groups, who published their findings in *Quaternary Research* (Orvis and Horn, 2000) and *GSA Bulletin* (Lachniet and Seltzer, 2002). We do cite some examples from grey literature, so as to be thorough and give credit to the long history of research at Cerro Chirripó. The older work we cite is certainly worth considering, given that, e.g., Weyl (1955) and Hastenrath (1973) were serious workers, even though they lacked various luxuries afforded to modern scientists.

The later work, including Orvis and Horn (2000) and Lachniet and Seltzer (2002) is thorough glacial geomorphology, and uses standard techniques to estimate the ELA from preserved glacial landforms. Our work added surface exposure age dates to some of the landforms, and we found that they are broadly LGM.

The estimated for the ELA in Taiwan is given on p.7/l.5; however, more information is clearly needed.

ELA estimation in Taiwan is admittedly more difficult than in Costa Rica for multiple reasons. As we argue in Sect. 5.3, scarp encroachment into glaciated landscapes in Taiwan is far more severe than in Costa Rica, and we propose that for this reason the maximum extent of glaciation in Taiwan is difficult to ascertain. Previous work has indicated the presence of a glacial diamict at 2250 m in a valley flanking Nanhudashan, far below unambiguous glacial valleys (Hebenstreit and Böse, 2006). Landforms above this elevation are preserved better.

Another problem is that the timing of glaciation in Taiwan is ambiguous. The most unambiguous dated landforms are somewhat younger than global LGM (e.g., Hebenstreit et al., 2011; Siame et al., 2012). Other ages from a neighboring glacial valley in Hsuehshan (20 km west of Nanhudashan) are much older than the global LGM (Cui et al., 2002).

Finally, high rates of rock uplift complicate the estimation of the paleo-ELA in Taiwan (and elsewhere, for that matter, but most severely in Taiwan) since ELA estimation is based on reconstructions of preserved glacial remnants. We address this complexity with regard to our proposed model in the submitted manuscript.

Using the present configuration of glaciated valleys, Hebenstreit (2006) estimated an ELA of 3355 at Nanhudashan, specifically employing the terminal to summit altitudinal method (TSAM). Hebenstreit (2006) also used TSAM to estimate an undated ELA of 3400 m at Yushan, a third glaciated massif in southwest Taiwan. Other work has used the maximum vertical extent of lateral moraines in both Hsuehshan (Cui et al., 2002) and at Yushan (Böse, 2004) to propose an ELA of ~3400 m. The age associated with these ELA estimates remains difficult to pin down, and may be more closely associated with the Late Glacial than with the global LGM.

An appropriate LGM ELA estimate for Taiwan is therefore certainly below 3500 m. A reasonable low estimate might be 3200 m. Hebenstreit (2006) suggested the lowest possible limit of the LGM ELA to be 2775 m, but this is certainly an extreme lower limit.

To address this comment, we will add:

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.

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.

This is an excellent suggestion, and we agree that such figures would greatly improve the quality and clarity of the manuscript. We will introduce these figures in the revised manuscript.

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?

To address these concerns more discussion of the ELA is clearly needed, and we will provide this in the revised manuscript.

There are climatic differences between Costa Rica and Taiwan, but these are probably not as extreme as might be indicated by their difference in latitude. Costa Rica has weak thermal seasons (diurnal air temperature fluctuation exceeds the annual variability) and the mean annual air temperature at high elevations (3475 m) is 7.6°C (Lachniet and Seltzer, 2002). Thermal seasonality in Taiwan is greater, and monthly annual air temperature at Nanhudashan today ranges from -2.6°C in winter to 8.2°C in summer (Klose, 2006).

The question of why the LGM ELA in Costa Rica and Taiwan was similar is somewhat beyond the scope of this paper, as we simply rely on geomorphic evidence that indicates its position in both places. However, if paleo-glaciers in Taiwan responded mostly to summer temperature, and glaciers in Costa Rica were more strongly controlled by annual mean temperature, as is common in the deep tropics (e.g., Kaser and Osmaston, 2002), then the temperature that is most important for determining the position of the ELA is similar in both places today (8.2°C in Taiwan, 7.6°C in Costa Rica).

There is substantially more complexity in the pattern of the position of the LGM ELA in Mérida Range than is plausible in Costa Rica or Taiwan. Stansell et al. (2007) argue that a combination of local variability in precipitation, cloud cover, and aspect drove a change in the LGM ELA of ~600 m over the 25 km width of the massif. The width of glacial landscapes in the massifs we present (~5 km) are a small fraction of this width, and we propose that there is no important change in the position of the ELA at either Cerro Chirripó or at Nanhudashan. That said, the range of ELA proposed by Stansell et al. (2007) for the Mérida Range encompasses the ELA estimates we present for Costa Rica and Taiwan.

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.

We agree. We will move these field photos to the main text in the revised manuscript.

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.*

The scarp encroachment we propose arises from differential erosion between glacial landscapes and the fluvial landscapes flanking them. The ELA is an effective base level for glaciated landscapes, and glacial incision and headward erosion creates low-sloping valleys near this elevation. Low-slopes in the vicinity of the ELA promote slow erosion during ice-free periods—an effect that is not felt in the flanking fluvial network. We propose that the slope break between glacial and fluvial landscapes grows as erosion in the flanking fluvial network drives on during warm periods, and eventually grows into the escarpments we observe.

"Outward spreading" is poor phrasing on our part, and we rewrite this sentence in the revised manuscript. This phrasing was intended to reference to the concept of cirque backcutting, detailed, for example, in Oskin and Burbank (2005). We envision cirque glaciers eroding headward, leaving behind relatively low-sloping topography near the ELA, and generating erosion fronts (knickzones) *in situ* below the glacial limit.

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 below it 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.

Glacial landscapes in these mountain ranges do appear to be transient features, and their "significance in the overall, long-term landscape development" should be questioned, although it never has been before. Our central point has been that if glacial landscapes are transient features, then glaciation has either happened once, at the LGM, in isolated patches in both ranges, and never anywhere else or at any prior time in either range. The alternative we propose is that glaciation has happened there repeatedly.

From this and other comments, it is now clear to us that more discussion is needed on possible landscape evolution scenarios that could produce the topographic patterns we observe today, which we categorize into three broad groups:

1) **LGM glacial erosion alone**
    a. Both Taiwan and Costa Rica have been rising throughout the Pleistocene and during the last 20 kyr have reached elevations necessary for glaciation
    b. Steady state has not been achieved yet in either place, and *true* peak elevations have yet to be attained (i.e., both ranges will reach elevation >>cold-phase ELA)
    c. Glaciation has happened only once in both places, and has been spatially limited to Chirripó in Costa Rica and Nanhudashan, Yushan, and Hsuehshan in Taiwan

2) **Glacial decoration**
    a. Fluvial incision has kept pace with rock uplift and has reached topographic and flux steady state
    b. This elevation happens to put enough rock mass above the cold-phase ELA for periodic glaciation
    c. Glacial erosion happens in short bursts but is insufficient to lower the landscape below the fluvially-achieved steady state
    d. Glacial erosion also does little to affect the fluvial erosion below the glacial limit, and thus has virtually no effect on the steady state elevation, and the ambient erosion rate is largely invariant between glaciated and non-glaciated catchments

3) **Glacio-fluvial limitation (GFL)**
    a. During the Pleistocene rock mass has repeatedly crossed the cold-phase ELA
    b. Glacial erosion occurs once sufficient rock mass pushes through the ELA
    c. Periodic glaciation has been sufficient to reduce catchments above the ELA down to the ELA
    d. Glacially eroded catchments are disconnected from flanking fluvial network
    e. Fluvial escarpments propagate headward, remove glacial landscapes

Again, we have introduced Scenario 3 in the submitted manuscript, and we acknowledge that Scenario 1 and Scenario 2 are also possible. We recommend that the likelihood of Scenario 3 would increase if similar patterns were to be observed in more ranges than those presented. To this end, we extend our analysis to more mountains ranges in the revised manuscript (the details of this approach are outlined in our response to David Egholm).

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 rephrase colloquial statements in the revised 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 will rewrite this section of the introduction.

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.

We agree. These will be included in a revised manuscript.

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

We agree. We include a geomorphic map of glacial features at Nanhudashan in the revised manuscript.

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

Thank you for pointing this out. We will rewrite this sentence.

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 were 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.

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

We will separate the slope map from the DEM in Fig. 1, and add a new figure of a stand-alone slope map.

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

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 . . .

Fair point. We will reword in a revised manuscript.

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.

Cerro Chirripó is the highest peak in Costa Rica, and Nanhudashan is the highest peak in N.E. Taiwan. We looked at the catchments that share these peaks and found that they have a hypsometric maximum at the LGM ELA, a tell-tale sign of significant 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.

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).

The peaks of all of massifs in Taiwan are within ~500 m of the estimated ELA, and on the scale of individual valleys there is very little area above the ELA at all. 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, glacial erosion stops the highest parts of the landscape at the ELA, and glacial landscapes are then "reworked" into 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 (in the submitted manuscript only the Talamanca Range, but as we detail in our response to David Egholm, we will add substantially to the discussion of evidence of glacio-fluvial height limitation throughout the tropics). 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 agree. We will include updated figures in a revised manuscript.

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