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Interactive comment on "Glacial buzzcutting limits the height of tropical mountains" *by* Maxwell T. Cunningham et al.

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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 rep-

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resentation 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 $\sim$3 My ago. 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 buzz-saw 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 $\sim$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? Note also that final glacial retreat in Nanhutashan appears to have occurred much later, in the Holocene (Carcaillet et al., 2007; Siame et al., 2009).

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.

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. 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.

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.

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".

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.

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

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

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

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

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 . . .

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

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

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

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.

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.

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

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.

References (other than those cited in the manuscript): Carcaillet, J., L. L. Siame, H. T. Chu, D. L. Bourlès, W. C. Lu, J. Angelier, and P. Dussouillez (2007), First cosmic ray exposure dating (in situ produced 10Be) of the late pleistocene and holocene glaciation in the Nanhutashan Mountains (Taiwan), Terra Nova, 19(5), 331–336, doi:10.1111/j.1365-3121.2007.00756.x. Schildgen, T.F., P.A. van der Beek, H.D. Sinclair, and R.C. Thieder (2018), Spatial correlation bias in late-Cenozoic erosion histories derived from thermochronology, Nature, in press. Stansell, N. D., P. J. Polissar, and M. B. Abbott

(2007), Last glacial maximum equilibrium-line altitude and paleo-temperature reconstructions for the Cordillera de Mérida, Venezuelan Andes, Quat. Res., 67(1), 115–127, doi:10.1016/j.yqres.2006.07.005.

14/06/2018 Peter van der Beek

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