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Earth Surface Dynamics Discussions

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

D. L. Egholm (Editor)

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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.

In addition to the reviewer comments I list below some additional reflections of my own:

General comments:

Most previous studies of mountain range height and glacial erosion have used correla-

tions 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.

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

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?

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.

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.

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.

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

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

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.

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.

Please consider the review comments carefully and submit point-by-point responses if you are willing to and interested in revising the manuscript.

Best regards David L Egholm Associate editor

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