

Interactive comment on “The periglacial engine of mountain erosion – Part 2: Modelling large-scale landscape evolution” by D. L. Egholm et al.

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The manuscript "The periglacial engine of mountain erosion - Part 2: Modelling large-scale landscape evolution" by Egholm et al. inserts a new numerical model for frost cracking and frost creep, developed in a companion manuscript, into a broader landscape evolution model. The result is an elegant model for the development of glaciated landscapes. This contribution highlights the significance of periglacial processes in the development of high elevation, low relief surfaces, in particular emphasising that such surfaces need not form at lower elevations before being raised by tectonics, yet also may have been formed prior to the Quaternary.

I have a couple of minor concerns regarding the experimental design, though I doubt that addressing these would substantially alter the manuscript's conclusions. In Exper-

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iment 1, the initial condition (Fig. 3a) is a small area of highly dissected topography; to me it looks to be dissected at too fine a scale to be realistic. Meanwhile in Experiment 3 it looks a little odd that glaciers are allowed to develop as climate cools, but that rivers weren't present in the valley bottoms prior to that. As a general comment, it would be helpful to have an idea of the kinds of "real" landscapes that the authors have in mind when setting up these models.

Some minor comments:

Abstract. It would be helpful to re-write the third sentence ("However, to which degree...").

p. 329, l. 13. "switching between glacial and fluvial erosion".

Section 3.2. Why is it important to introduce "realistic" climate at this stage, but it wasn't important for the previous model?

p. 343, l. 10. This is a lot of overdeepening for relatively short glaciers! (See comment above regarding which landscapes the authors have in mind.)

p. 344, l. 9. Could the authors clarify that this is less erosion (i.e., removal of material) as opposed to less net lowering of the surface elevation (because of the isostatic response to valley incision)? What is the net change in surface elevation on the summit flats?

p. 345, l. 11. Are the parameters in the model sufficiently well constrained to make a quantitative statement like this?

Section 4.1.1. See comments in my review of the companion manuscript; perhaps some of this discussion of fine-grained sediment could be added/moved there?

p. 347, l. 18. Delete "operates".

p. 348, l. 8-11. Do wind direction and insolation influence periglacial processes purely through their impact on water availability, or more broadly? Perhaps the authors could

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develop this theme.

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