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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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In this reply we comment on all remarks given by the reviewer and present the associated changes to the manuscript. The comments from each review have been copied into this document in grey and are marked with C for comment and a sequential number. The corresponding response is marked with R.

Reviewer 3: S. Brocklehurst

C-3.1: 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 Experiment 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.

R-3.1: We thank the reviewer for the supportive comments and constructive suggestions. Regarding the setup of experiments, it is important to note that they are designed to answer simple questions such as: "what is the isolated role of frost given this initial landscape and this time scale?" and "what happens if periglacial and glacial processes cooperate?". In experiment 3, we opted to study the interaction between only two processes of erosion: frost cracking and subglacial erosion, and this is why we ignored the influence of rivers. We believe that this kind of experimentation can yield useful and transparent insights into how processes operate and interact. Although such experiments rarely tell the full story about any particular landscape, our goal is to derive generic insights into landscape mechanics. The challenge is thus to build upon the information provided by the experiments to uncover the genesis of the landscapes we see in nature. Accordingly, we have strengthened section 3 in which we motivate the experimental setup. Although we do not include fluvial erosion in the experiments, we now explicitly discuss in section 4.3 how we expect fluvial erosion to influence results from experiment 1-3. Finally, regarding the drainage density of the initial landscape in experiment 1, we find the spacing ratio of the model drainage network (half-width of range divided by outlet spacing) to be 2.6, which falls within a realistic range for mountain belts (Hovius 1996, Basin Research, 8, 29-44). Also, we note that the fine-grained relief is an important property of the initial landscape, as it must stand in contrast to the smooth surface of the final landscape. It is the gradual transition from rugged to smooth that demonstrates the effects of frost activity in experiment 1. We have now noted this latter point in the motivation for experiment 1.

C-3.2: 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.

R-3.2: We agree with the reviewer that this could strengthen the paper significantly. We have added a photo (Fig. 1) of a summit flat in Greenland (motivating experiment 1) and a photo of a larger-scale fjord landscape in Norway (motivating experiment 3).

Some minor comments:

C-3.3: Abstract. It would be helpful to re-write the third sentence ("However, to which degree...").

R-3.3: This part of the abstract has been rewritten.

C-3.4: p. 329, l. 13. "switching between glacial and fluvial erosion".

R-3.4: The text has been changed in accordance with the reviewer’s suggestion.

C-3.5: Section 3.2. Why is it important to introduce "realistic" climate at this stage, but it wasn’t important for the previous model?

R-3.5: This is another good question that relates to how the experimental design is presented. We deliberately wanted to have a stepwise increase in complexity throughout the paper, with experiment 1 being the simplest with constant climate, experiment 2 more complex with a varying climate but still only periglacial processes, and then experiment 3 combining a varying climate with multiple processes. The basic idea is that insights obtained in the first experiments can be used to interpret the results of the final ones. We now explain this strategy explicitly in the beginning of section 3.

C-3.6: 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.)

R-3.6: Ice covers the entire grid during the periods in which the most intense glacial erosion occurs. The glaciers extend outside the grid during these periods, and they are therefore longer than they appear. The landscape that we have in mind here is a fjord-type landscape, and in the new Fig. 1, we have included a photo from such a region. We think that the reviewer’s suggestion to link the models to specific examples is a good one. See also response to comment C-3.2 above.

C-3.7: 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 re- sponse to valley incision)? What is the net change in surface elevation on the summit flats?

R-3.7: We have now clarified this by explicitly stating the amount of: 1) erosion, 2) isostatic rock uplift, and 3) surface uplift.

C-3.8: p. 345, l. 11. Are the parameters in the model sufficiently well constrained to make a quantitative statement like this?

R-3.8: This estimated upper limit to long-term erosion rates is based on the assumption of transport-limited erosion. While rates of frost creep are better constrained by physics than are rates of frost cracking, the modeled efficiency of creep depends on the parameter beta, which defines the grain-size and porosity dependent frost-heave. We have added this information to the paragraph, and we now also refer to the influence of grain-size, which is further discussed in section 4 and in the companion paper. We have furthermore relaxed the statement to say “of order 10 m Ma-1” as opposed to “<10 m Ma-1”

C-3.9: 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?

R-3.9: We have significantly strengthened the discussion of grain size effects in the companion manuscript, and we have subsequently shortened and simplified the treatment here.

C-3.10: p. 347, l. 18. Delete “operates”.

R-3.10: Done

C-3.11: 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 develop this theme.

R-3.11: We have expanded the discussion of aspect ratio and we now refer more explicitly to some of the results from Anderson et al. (2012). We do not include such effects in our model experiments; we may develop these themes in subsequent refinements.

Interactive comment on Earth Surf. Dynam. Discuss., 3, 327, 2015.