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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 2: J. Roering

This manuscript follows nicely from the process model in the companion paper and applies it to the problem of broad, low-relief, high-elevation surfaces (or summit flats). The context for this work is remarkably well presented as it draws upon some very exciting recent studies and applies the process model to landforms that have long been enigmatic. In particular, the notion that summit flats could be polygenetic is rather compelling as presented here and this view provides an alternative to some long-held assumptions about inheritance and uplift history of these settings. Perhaps the biggest challenge in porting the periglacial models from Andersen et al. is determining the functional relationship between frost cracking intensity (FCI) for example and the rate of bedrock-regolith conversion. Here, the authors wisely opt for a simple implementation. In the case of frost cracking, they use a linear relationship for weathering rate as a function of FCI, but as pointed out in the previous paper, FCI values are highly dependent on regolith depth in most cases. As such, this model incorporates an appealing level of complexity with feedbacks between soil thickness and weathering rate. Previously, depth-dependent soil production models were connected to empirical functions (e.g., exponential or humped), but this contribution is a significant leap forward by linking with a mechanistic formulation.

C-2.1: I have minimal field experience with these low-relief surfaces, so I will defer to my esteemed colleague B. Anderson who has tackled their evolution with insight and aplomb through some very nice papers. That said, I think this contribution could more clearly define how the boundary condition is specified. In most soil-mantled hillslope evolution studies, the hillslope-channel interface evolves according to a valley incision rate such that hillslope form and soil thickness adjusts accordingly. In this case, my understanding of the small-scale model (experiment 1) is that the boundaries of the hillslope are maintained in a bare bedrock state such that the boundary lowering rate is equal to the weathering rate for zero soil thickness (p 335: line 28). This is a very interesting implementation because it implies that hillslope form (e.g., curvature and slope) evolve in response to the vigor of frost cracking (pg. 339: line 4), which suggests that climate variations will drive variability in this rate. Whether this variability translates into obvious morphologic transients is another story (apparently it doesn't because the humped frost cracking production function allows for the same erosion rate for bare soil and a finite soil depth). In this way, the hillslope morphology (e.g., curvature and relief) is not dependent on valley forming rates, but rather has a more direct linkage

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with climate through this boundary condition. This is a clever and compelling avenue that merits testing. I wonder if these marginal areas are also governed by slope stability limitations, such as toppling and rockfall, or perhaps those processes occur with less vigor than frost-driven weathering. Overall, the expansion of periglacial landscapes during the Late Cenozoic is a highly compelling notion and the authors have crafted a rich, sophisticated, yet accessible model to explore a range of scenarios. The text is very well-written and easy to follow. Clever geochemists now have a roadmap to help guide their fieldwork in these settings.

R-2.1: We thank the reviewer for the supportive comments and the insights concerning the boundary conditions to experiment 1. It is correct to note that sediment is removed along the boundaries, which therefore erode at rates corresponding to frost-cracking on bare bedrock. This also means that the evolution of the small-scale surface is fully decoupled from the development of valleys at lower elevation (and outside the grid). The experiment is designed to explore what happens when such an isolated "island" in the landscape is exposed to (only) frost cracking and frost creep for a very long time. We have made two changes to the presentation of experiment 1. First, we have included a new figure (photos from a summit flat in Greenland), which serves to motivate the experiment and the boundary condition used (see also response to comment C-3.2 by S. Brocklehurst). Second, following the input given here and the suggestions of R. S. Anderson we have filled out the discussion of the influence of the boundary condition (section 3.1).

Specific comments for consideration:

C-2.2: Pg 328: line 21: the linkage with periglacial and glacial modification is a bit vague here. This sentence wasn't digestible for me until after reading the manuscript. Perhaps a more explicit statement?

R-2.2: We have rephrased this sentence. We now refer explicitly to how glaciers may accelerate frost activity by steepening slopes and stripping the sediments from the landscape.

C-2.3: Pg. 329: line 8: from the thermochron and cosmo world some new evidence for enhanced erosion around 800kya to 1Ma is very compelling and would be worth considering here. See Valla et al in Nature Geoscience (2011?) and Haeuselmann et al., Geology 2007, for examples.

R-2.3: We have expanded this section and included more references including those suggested. See also response to comment C-5.1 by C.B. Phillips.

C-2.4: Pg. 331: line 8-10: the 'peneplain' literature is vast, a good review is by Widdowson, 1997, Geol Soc London, Spec Pub 120.

R-2.4: Thanks. We have now included reference to Widdowson.

C-2.5: Pg. 332: line 6-8: can you be more explicit with the scales? It's a bit vague as stated.

R-2.5: We now give the precise grid dimensions in order to make the sentence more explicit.

C-2.6: Pg. 339: line 4-7: I think this arises because the boundary lowering rate for bare rock is imposed by the frost cracking rate, so the soil thickness must then equal that for which production rates are the same.

R-2.6: We agree, and we thank the reviewer for pointing this out. We have strengthened this section following the suggestion from R. S. Anderson.

Pg. 341: line 5-8: this is a very important insight!

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