Earth Surf. Dynam. Discuss., 3, C259–C263, 2015
www.earth-surf-dynam-discuss.net/3/C259/2015/

Earth Surface Dynamics
Discussions
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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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Received and published: 14 August 2015

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 1: R.S. Anderson

General comments. It should come as no surprise that I like this manuscript. It does a beautiful job of amplifying results presented a decade ago, and applying them in an imaginative and robust manner to issues of mountainous landscape evolution. It is clearly written and beautifully illustrated. The three numerical experiments taken together serve to illustrate the importance of summit flats in these environments. The experiments reinforce the suggestion that summit flats are an emergent property of the long-term operation of periglacial processes (frost cracking as a means of producing mobile regolith, and frost creep as a means of transporting the mobile regolith). The authors argue that this natural emergence of summit flats is a strong alternative to the notion that these features require formation near sealevel (and hence require significant rock uplift since their formation). They argue that their numerical experiments, especially the long-term third experiment in which late Cenozoic cooling is simulated, are consistent with the apparent stagnation of these surfaces once they are produced, supported by very low exhumation rates deduced from both cosmogenic and thermochronologic data. I agree. That the increased coverage of Earth's landscape with a periglacial cloak in the late Cenozoic can explain the enhanced erosion rates documented in sedimentary basins is an intriguing suggestion. I am more enamored of the suggestion that the coupling of glacial and periglacial processes can explain the enhanced long-term erosion rates when the climate is allowed to swing strongly between glacial and interglacial modes in the Quaternary. As periglacial activity is enhanced when slopes are steep and when mobile regolith is thinned (to absent) allows glaciers, which both steepen head and side walls, and strip sediment from valley floors, a yet stronger role in mountain erosion by enhancing erosion directly in both glacial times and indirectly in interglacial times.

C-1.1: In section 3.1 in the discussion of the evolution of summit flats, the authors find that very smooth (uniform convexity) surfaces arise with "remarkably uniform" sediment thickness of 2-3m. It is not surprising that this is the case, in that a steady solution requires that the rate of rock conversion to mobile regolith (call is soil) be uniform with distance from the divide. As the soil production, frost cracking algorithm is a strong function of soil thickness for an given thermal swing, this requires that the soil thickness be uniform. The question that remains is why the particular thickness of 2-3m arises. In my own 1-D modeling of such surfaces, I found that a robust steady solution arose on summit surfaces only when the rate of soil production beneath finite soil equaled that of bare bedrock. The authors have chosen to implement a boundary condition on their calculation that at the edges of the surfaces the soil thickness is taken to be zero. Therefore the steady solution they have found matches that I found, and that soil conversion beneath 2-3m thickness of soil equals that they impose on bare bedrock. In many places we have explored in the Laramide ranges of western North America, the soil thickness on these surfaces is on the order of 1 m; we can therefore be assured that the frost cracking conditions there demand that the rate of bedrock conversion on bare bedrock equals that beneath 1m of bedrock. We therefore know two points on the weathering rate – soil thickness curve from such landscapes. These thicknesses, bare rock and some specific measurable uniform soil thickness, serve as attractors in these landscapes.

R-1.1: We thank the reviewer for this very useful input. We have used the points raised here to further discuss the results of the first experiment, and in particular to explain the steady-state sediment thickness and the influence of the bare bedrock boundary condition along the model edges (section 3.1). Furthermore, in section 4.2 we now discuss how measured sediment thicknesses may be used to calibrate and test the relations between soil production and soil thickness.

C-1.2: Let me make one other point: the choice of zero soil thickness is indeed appropriate in these settings of high surfaces bounded by glacial troughs. It is here that the very strong convexity arising from the boundary between low curvature high surface and near-vertical glacial wall will strip soil from that position due to the strong divergence in soil transport. As the authors echo, this is the essence of tors, or bedrock knobs in the landscape: sites of initial or eventual strong curvature in the landscape are maintained as bare bedrock, and they occur dominantly at ridge crests and at the edges of the surfaces.

R-1.2: Again we thank the reviewer for valuable input. We have used this to further motivate the boundary condition used in experiment 1 (section 3.1).

C-1.3: In section 3.1.2 the authors discuss the relationship between rock conversion rate and curvature of the landscape. Let me flesh this out a little. This is yet more cleanly seen in the solution for a steady state parabolic hillslope in which the lowering rate is everywhere uniform. This demands that the rate of rock conversion (here governed by the prduct of ke and FCI), equal the product of landscape diffusivity kappa with local curvature, C. Therefore C ke*FCI/kappa. As the generation of soil by frost cracking increases, the curvature must increase so as to generate the slopes required to pass the soil. On the other hand, as the efficiency of transport increases, the slopes required to pass the soil diminish and the curvature is commensurately smaller.

R-1.3: We have clarified the results shown in Figure 7.

C-1.4: In section 3.3 in the description of the glacier model, it would be good to tell the audience more about the algorithm behind the snow accumulation pattern. It sounds from p343 lines 13-15 that the snow is not allowed to accumulate (drifts off of?) high convex surfaces, thereby limiting glacial ice growth there. While I agree with this – it certainly matches what we see in western North American high summits surfaces – it would be good to see at least brief explanation of the algorithm used (without having to turn to Egholm 2012).

R-1.4: Snow accumulation in the model is governed by simple penalty functions that prevent accumulation on steep and convex slope. We have included a full account in the updated version of the manuscript.

I very much liked the descriptions of the model experiments, their results and implications in sections 4.2 and 4.3. They serve to formalize hypotheses about the generation of summit flats and what they mean, and of how late Cenozoic cooling leading to major swings in Quaternary climate could potentially influence global sediment production histories.

C-1.5: Technical details. This manuscript is very cleanly written. I encountered only a very few and minor editorial points, which I enumerate with page:line number be-low. I denote comments or questions in square brackets []. 328:6 cooling remains an unanswered 328:9 processes: frost 329:12 situation in which landscapes 332:16 equate with mobile regolith 332:20 edot is the soil production rate 332:21 (average) bulk density [I would also suggest subscripting the x and y in qx and qy throughout the manuscript so that it is clear these are components of the mobile regolith discharge q.] 336:6 debris accumulates in 337:20 inefficient whereas frost cracking remains active 338:15 freeze- thaw events due to growth of segregation ice lenses, and rates 340:21 average change in sediment thickness [At least I hope this is the case, as soil thickness itself does not incite an uplift due to isostasy.] 345:7 nearly stalls 347:6 hand, shows little 347:14 temperature in limiting 348:20 process (frost cracking. . .accession) the 351:2 The landscape evolution models presented here integrate 351:18 that have experienced 351:22 topographic smoothing

R-1.5: The text has been corrected according to the suggestions above. Regarding the comments in square brackets: 332:21: We would also prefer to have the x and y in qx and qy as subscripts, but unfortunately subscripts cause problems for Eqn. 7 where they are used for denoting grid point positions (and superscript is used to indicate timing). 340:21 Yes, we meant the average change in sediment thickness – thanks for spotting this.

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