

Interactive
Comment

Interactive comment on “The periglacial engine of mountain erosion – Part 1: Rates of frost cracking and frost creep” by J. L. Andersen et al.

J. L. Andersen et al.

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We thank the reviewer for the many constructive and supportive comments. We find that the insightfull remarks have helped us to strengthen the manuscript.

In this reply we comment on all remarks given by the reviewer and present the associated changes to the manuscript. The comments 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

This is a very well conceived and very well executed paper. The authors have taken the concept of frost-related processes and both summarized the state of the art, and

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taken it a step further. I was very impressed by the introductory material, which itself is a service to the community. The work builds nicely upon recently published papers in which geomorphologists have attempted to cast the periglacial problems in physics-based rule sets, one for the breakdown of rock, the other for the transport of the material once it is free to move on a slope. The authors have pushed the physics on each of these fronts with the target of assessing the rates of frost cracking and frost heave as functions of climatic variables governing the thermal state of the rock and overlying soil. In the second article it appears the authors will apply these rules to the evolution of mountainous landscapes

specific comments I like the advance made in section 3.2 in which the authors propose getting a little more “real” about the availability of water by integrating the available water along a path. This issue with water availability, and the likely variability of soil and rock moisture the is a prerequisite to frost heave and frost cracking, remains at the core of where our algorithms for long term geomorphic work accomplished in periglacial environments are in most need of further work. This is a next step. As the authors note in their discussion section, there is more work to do yet.

C-1.1: The frost creep section also advances upon past formulations (largely that of Anderson, 2002) by pushing on the availability of water both at the start and through the frost heave event. I would like to see here a discussion of the expected role of soil thickness, so that the audience may anticipate the role of soil thickness in modulating transport. It is suggested in past work that the rate of transport should taper smoothly to zero as the soil thickness declines to zero, which is a physics – based reason that soil discharge should vanish when there is no soil to transport (!). As this is something many landscape modelers in the past have had to hard-wire with an “if” statement (f H=0, Q=0), it is worth pointing out that these approaches allow a much more natural soil transport function. This is captured in Figure 8b.

The results section is very clean, with hard-hitting bulleted results.

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R-1.1: The comment on the influence of soil thickness refers to section 3.3. Based on the reviewer's suggestion, we have added a brief discussion of the potential influence of sediment thickness to the section.

C-1.2: The effects of snow cover are yet another place where we can do yet more in the future. Here the authors reduce the amplitude of the thermal swing at the soil surface by imposing a snow thickness. What is the rationale for the choice of the damping of the amplitude (presumably one could employ a simple exponential dependence of amplitude on thickness, tied to the thermal diffusivity of the snowpack). But the problem is more complex than this, as if the snow falls after the soil has had time to cool down in the Fall and early Winter, the role of the snow is to trap in the cold. (see Bartlett et al., 2004, on this effect of when the snow falls). In this manuscript I would not expect further discussion of the snow cover; my comment merely serves to emphasize that more work remains to be done.

R-1.2: The comment refers to section 4.3.2. We agree that the role of a snow cover can be more complex than captured by our very simplistic approach. We have added a few sentences to this section of the manuscript in order to underline the limitations of the chosen approach to the readers. Also, we have included a reference to Bartlett et al., and we thank the reviewer for leading us to this. However, we also find that a more detailed treatment, including modelling of the thermal properties of the snow pack, is beyond the scope of the paper.

C-1.3: Frost creep and depth-dependent transport. I would suggest the authors could amplify their statement by saying that this kind of approach allows the community to craft not only slope and depth dependent rules for soil transport, but attaches the transport efficiency to specific attributes of the climate that allow us to approach not only spatial differences in soil transport in the modern climate, but acknowledgement of the role of climate swings over the long-term evolution of mountainous landscapes.

R-1.3: The comment refers to section 5.3. We have followed the reviewer's suggestion

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and highlighted the possibility to study effects of climate.

C-1.4: Technical corrections: There are very few flaws in the writing. I will note the few I caught here by page: line number 286:7 prevail in some 304:23 we explore the influence 305:7 perhaps not surprisingly 307:4 this result relies on 307:6 bedrock, as suggested 307:13 likely to result in crossing or switching between these cases.

R-1.4: The text has been corrected according to the suggestions.

Interactive comment on Earth Surf. Dynam. Discuss., 3, 285, 2015.

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