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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.

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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 3: T. Hales

This paper describes a numerical experiment that introduces a new method for calculating frost cracking potential and the movement of soil downslope via frost heave. In short, the paper is presented well, it is clearly written and well organized. I enjoyed reading it. The conclusions are clear and the introductory material explains the problem and the gap very nicely. I think the idea that frost-susceptible landscapes have a soil production function that varies as a function of temperature conditions and soil thickness is interesting. This paper has the potential to be a very useful advance, particularly as it carves a clear path to producing a periglacial landscape evolution model, something we sorely lack. However, I would like to see the authors justify/work on some of the important details and assumptions within their model, as I am not sure that all of the conclusions are consistent with the physics of segregation ice growth in rocks and soils. The theory presented extends previous work by Bernard Hallet, Bob Anderson, myself and others. The key assumptions, both explicit and implicit are as follows: The authors have taken the approach of Hales and Anderson and applied segregation ice theory without any physics to explain how to break rock, implicitly assuming that the rock mass is behaving like a soil and wherever the temperature conditions are suitable, rather than requiring any rock fracture process. Again, following Hales and Anderson, water is not limited, i.e. is always available everywhere. When rock turns into soil, it is frost susceptible (i.e. about silt sized). Conduction dominates heat transport. While some of these are reasonable, justified, and necessary for this formulation, others have significant consequences on the results, particularly when you include soil into the formulation There are two areas that need special attention: (1) The authors present a new conceptualization of the "penalty" introduced by Anderson 2012. In part, this alleviated some counterintuitive results from the Hales model, particularly the peak in FCI at low MAT's. In essence, the penalty exists based on the argument that water migrates slowly through a frozen fringe, so thicker fringes would mean water would migrate more slowly (or not at all), resulting in a lower likelihood of segregation ice growth.

C-3.1: a. The addition of a penalty, as demonstrated by the authors, significantly changes the predictions of the MAT and Ta conditions required to promote frost cracking. Hence I believe it requires a thorough and detailed examination by the authors. In particular, I think the authors need to address, based on segregation ice theory, why

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their formulation of the penalty is the best method.

R-3.1: The addition of a penalty function to restrict water flow is not based on ice segregation theory, but was introduced in order to mitigate effects of previous models, that allowed water to be transported over indefinite distances, provided that the temperature was steadily increasing. This results in a very abrupt transition in FCI when crossing MAT = 0 deg C in the non-penalized model (also seen in fig. 11a in the discussion paper). We therefore liked the penalty of Anderson et al, where the FCI was exponentially dampened with increasing distance to water, thereby setting an upper limit to flow distance. However, since our model includes both sediment and bedrock, we thought it reasonable to include different penalty length-scales for different materials, based on the permeability of the rock or sediment that water has to migrate through in order to reach the site of ice segregation growth. The penalty can be understood in terms of a likelihood of maintaining a hydraulic connection through a medium from the water to the site of ice segregation growth and frost cracking. The lower the permeability, the shorter the average "route" (or hydraulically connected porespace) is likely to be, and therefore the less likely the ice lenses are to receive water (from the fewer continuous flow routes). However, in acceptance of the weak links to physics, we included also in the first version of the manuscript a sensitivity analysis of the penalty function, so that readers could clearly see the effects of the weakly constrained penalty. We have now more clearly outlined the thoughts above into the manuscript, in the discussion of water availability (Section 5.4). We have also strengthened the discussion regarding penalties and water availability in general and have highlighted that the form of the penalty functions, and the length scales associated, represent components of the model that are only weakly constrained. See also reply to comment C-2.2 by J. Roering.

C-3.2: b. Currently the authors propose a formulation that introduces a new penalty function (P10 L19-P11 L10) that is driven by a flow resistance function. The authors argue that the flow resistance varies with different temperature conditions, so suggest that there should actually be 4 different values. There was little justification as to how

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those values have been chosen nor why theory would suggest this is physically reasonable. It seems reasonable to think that the temperature dependent permeability (or flow resistance) should only be dependent on temperature, and grain size.

R-3.2: Our decision to use four parameters for flow resistance is based on the idea that permeability is dependent on temperature and grain size, as is also suggested by the reviewer in this comment. The values were chosen such that flow in bedrock is twice as difficult as that in sediment and that flow in cold materials is harder than flow in warm materials (also by a factor of two). We acknowledge that these values are not empirically constrained. For this reason we included a figure that shows the result from having these parameters set to the same value, as well as from removing the penalty, partly and entirely, (fig 11). In the new version of the manuscript we have further included our standard model in this figure for easier comparison. We have also clarified and extended the discussion around this figure and the penalty function in general. We hope that we thereby have succeeded in emphasising to the reader that this is a less known aspect of frost processes and an uncertainty in the present model.

C-3.3: c. It seems that the length-scale that is chosen is particularly important. As such, it would be useful to see how sensitive your results are to a reasonable range of parameter combinations.

R-3.3: We agree that extending the sensitivity analysis of the penalty function would be useful, and we have therefore included a new supplementary figure showing the FC patterns using different values for penalty length scales.

C-3.4: d. The introduction of a penalty results in a result that is not consistent with empirical measurements of long-term frost cracking. In the few places on the planet that we know both the MAT, Ta and frost cracking or solifluction rate (by Matsuoka, Harris, Ballantyne, and others) high rates of sediment production/transport tend to occur in areas of seasonal permafrost, or in warm permafrost. Figure 9 shows the results of your model that suggest that cold permafrost regions drive frost cracking. For exam-

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ple, scree production rates calculated by Rapp in Svalbard, are much lower than those calculated by Sass in the Alps, or Hales in the Southern Alps. Such a counterintuitive result only reinforces the necessity to discuss and examine the theoretical basis of the penalty in more detail.

R-3.4: We do not fully agree with this comment. Our model has the most intense frost cracking occurring for positive MATs in areas with little to no sediment or snow cover (which are conditions that apply to scree production). We would therefore expect exactly the relation between MAT and scree production described in this comment. This is particularly clear from fig. 6c, line 6, where the frost cracking rates for bare bedrock are shown. We also note that frost-cracking under meters-thick regolith, as suggested by our model in permafrost areas, is for practical reasons more difficult to constrain by empirical methods, and that this type of frost-cracking is unlikely to lead to extensive scree production. When that is said, we also note that it has not been our aim to push forward one particular frost-cracking model, but rather to explore the effect of different model assumptions and parameter settings. We agree that empirical measurements are key to the evaluation of this (and any) model and it is our hope that this will be pursued more in the future. So as to further clarify our argument, we have strengthened the links between our model results and empirical studies of scree production, and we thank the reviewer for emphasising this aspect. Finally, we realise that the sensitivity analysis presented in Fig. 9 was performed with a snow cover that dampens near-surface frost cracking. As there is no particular reason to include snow cover here, we have repeated the sensitivity analysis without it and redone the figure. The near-surface frost cracking is therefore now more visible and in better agreement with Figs. 5 and 6.

C-3.5: The second area that needs particular attention is the role of sediment in the model. Here is where the assumptions frost susceptibility and water content become particularly important. a. Solifluction, or sediment transport by frost heave, has been shown experimentally to be a function of the magnitude of heave (Harris papers 2007-

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2012). The magnitude of heave at any point on a landscape is going to depend on how frost susceptible a particular soil is, as if the soil is too porous and/or permeable segregated ice lenses cannot form, and if it is not permeable enough (e.g. clays) water cannot be drawn towards the growing ice lens. The range of soil grain sizes that are frost susceptible are very small, basically silts and clays, with some susceptibility in fine sand. For example, it is unlikely that the plain shown in Fig 1 is moving downslope by frost heave. Secondly, within the grain sizes chosen there is a wide range of possible amounts of heave as a function of grain size, i.e. a highly non-linear diffusivity as a function of grainsize. As a result, when trying to scale this up to a diffusion-type model, you would expect diffusivity to be strongly grain-size dependent. Currently, the diffusivity is only dependent on water content, my understanding of the model is that soils with greater porosity would result in higher frostheaves in the model. This is physically incorrect, as high porosity soils would not form ice lenses as they are unlikely to be frost susceptible.

R-3.5: We agree with the reviewer that the grain-size distribution of soils is an important aspect of frost heave. However, in the present model, frost heave is scaled by the parameter β, and not by water content or porosity as suggested by the reviewer, we therefore disagree that the model is 'physically incorrect'. In fact, a higher porosity would reduce frost heave in our model, although only for reasons associated with transient thermal effects and in particular latent heat. We note that the water fraction in Eqn. 23 (discussion paper) varies between 0 and 1 independently of grain size and porosity. So, all the effects scaling the magnitude of frost heave in the sediment goes into the parameter β, which we hold at a constant value of 0.05 throughout (the value is adopted from the Anderson-2002-paper cited in the article). This parameter explicitly defines the magnitude of volumetric expansion of sediment during frost-heave. Based on the reviewer's comment, we have strengthened the description of β, and we now discuss how grain size and porosity may influence its value. In the present model β is set independent of the porosity, for which we also use a single value. We acknowledge that our model is limited to frost-susceptible sediments. However, it is our experience

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from field sites in Norway that fine-grained and frost-susceptible sediments often underlie blocky summit mantles, such as the one shown in Fig. 1. This observation is underpinned by the fact that many of these surfaces display widespread patches of patterned ground, a phenomenon that is traditionally attributed to periglacial processes, in turn indicating that the underlying soils must be frost susceptible. We note that similar grain-size distributions have been reported from other high flat summit areas in Scandinavia (e.g. Strømsøe Paasche, Goodfellow et al.). We take this as an indication that the breakdown of particles to frost-susceptible grain sizes on these summits is at present able to keep pace with the generation of bigger blocks by frost cracking and that these blocks often more or less passively ”ride along” on top of the fine-grained materials. However, we acknowledge that this might not always be the case. In order to clarify this aspect of the model, we have highlighted the grain-size limitation further by underlining the role of β in the model description. In the discussion we have also added a reference to the discussion on grain-size in section 4.1.1 of the companion paper (see also R-3.6 below).

C-3.6: b. Given the issue of grain size, it is in soils where the saturated condition becomes most important. In essence the conclusion that a sediment of a particular thickness would contain a greater “store of water” that would promote frost action in rocks assumes that all soils “store” the water. In fact, this is going to be highly dependent on permeability, so that you would expect that this conclusion may be true for frost susceptible soils, but would not be the case for very permeable soils like the ones shown in figure 1. Again, as your formulation only depends on the porosity of the rock, and does not account for permeability, then you may end up with a physically unreasonable result that the soil in figure 1 is “storing” a lot of water and promoting frost cracking.

R-3.6: We agree that our approach breaks down for sediments with a very high, air-filled, porosity. However, we do not vary the porosity of the sediment within this modelling study; it is kept at 0.3 throughout, which should be within a reasonable range

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for fine-grained sediments. Furthermore, we would like to emphasise that our model incorporates a limit to the amount of water that contributes to frost cracking (the critical water volume, Vcw). This means that a water 'volume' in excess of 4 cm per square meter, does not lead to additional frost cracking as the process is then considered water saturated. We have added a sentence about this in the end of the results section and refer the reader to the more detailed discussion in the companion paper. (See also comment C-4.6 by S. Brocklehurst).

C-3.7: c. Finally, it is likely that the "diffusion" by frost creep is likely to be strongly non-linear. Experimental data, by Harris and others, and field observations, suggest that creep processes such as solifluction transition to mass movements (gelifluction, active layer detachments) at low slopes (15-20 degrees). As such, you would expect this process to be non-linear if you are dealing with landscapes with steeper slopes than this. Again the paper is well written, clear and could potentially provide a nice theoretical advance. My review was written without looking at the other two reviews, however, a number of the ideas I present come from discussions that I have had with Josh Roering, Jill Marshall, and Alan Rempel.

R-3.7: We agree with the reviewer that other transport mechanisms than those modelled here may result in a strongly non-linear relation between slope and sediment flux. We have emphasised this in the text (section 5.3).

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