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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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Received and published: 9 June 2015

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

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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. a. The addition of a penalty, as demonstrated by the authors, significantly changes the predictions of the MAT and  $T_a$  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 their formulation of the penalty is the best method. 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 those values have been chosen nor why theory would suggest

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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. 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. 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,  $T_a$  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 example, 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. (2) 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 2007-2012). 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 frost

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heaves in the model. This is physically incorrect, as high porosity soils would not form ice lenses as they are unlikely to be frost susceptible. 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. 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.

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

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