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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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This paper presents a numerical study that explains the development of flat summits that are common in areas with significant periglacial action. This is a significant gap in the literature and I found the approach to be novel. The paper is well constructed, and I particularly liked the introduction that explains the state of knowledge well. In short, I really like the theory.

There are two areas that I think could be improved. The first I have discussed in the review of the companion paper. The key thing that was difficult to evaluate in this paper was how sensitive the model result was to the key assumptions. In particular, how sensitive the model result was to the parameter rich calculation of FCI and the assumption that any sediment that is produced is frost susceptible. There is some

discussion of the frost susceptibility problem in this paper, but it is quite vague and to my mind is not particularly convincing.

If we ignore the details of the model construction, I think there are a number of very interesting hypotheses about the construction of summit flats explored in this paper. Given the empirical nature of most periglacial geomorphology at the moment (and therefore much of the community that would like to digest your results), I think the authors could strengthen the paper considerably by attempting to place their model results in the real world. My suggestion would be to test the final model results against topographic data. As someone who has stood on summit flats without measuring their topography, I had the following questions: are these flat summits really parabolic? Is the scale of the relief and slopes that you calculate consistent with the development of these surfaces? I really like the warm and cool results, can you compare these to summit flats along a latitudinal transect, possibly through Scandinavia from Svalbard to Denmark or something similar? Because I think that the theory proposed here could be very influential in this field, and appealing to the empiricists and field geomorphologists could help to broaden the scope of the conclusions.

It maybe outside the scope of this paper, but while I think summit flats are interesting, but the theory could be strengthened if the explanation could be extended to why periglacial processes aren't more efficient at mowing down peaks in areas of higher uplift rates (e.g. the "Teflon peaks" of the Chugach-St Elias Mountains and elsewhere). You do this a little in experiment 2, but could you crank up uplift rate and see what happens?

Some specific comments are as follows:

P7 L5: How do you determine the magnitude of the free scaling parameter. This seems really important, but difficult to constrain. P9 L25: Why not vary temperature across the surface? I assume it is because at 200m of relief it represents only 1.2 degrees of temperature difference in the model. P10 L3: Why do you set ke to that value? P12 L15: Yes, but not with the frost susceptibility of the soil. P12 L19: ke is not know, nor is the length scale of the penalty, nor the “flow resistance” parameters that you have introduced. P20: This is the section that to me is the most unconvincing in terms of physics. (1) Beyond the Chamberlain paper there is a large literature that defines the grain size conditions required to produce frost heave, again I suggest the Harris papers as a good place to look at how even small differences in grain size, temperature, and slope can have large changes in the rate of downslope sediment transport. (2) The argument gets lost a little here. You state that frost cracking is unlikely to produce fine grained sediment. So then you introduce other processes (that you have not modelled) that may be acting just as fast on these slopes. These processes are somehow create a fine grained matrix below the open blocks of the felsenmeer. (3) Then you state that the fine grained matrix of the felsenmeer is frost susceptible, however do not provide any reference to either the grainsize distribution or whether frost creep rates have been measured here. I would rethink this discussion more carefully to make it more convincing. P22 L5: Lowering of the rates won’t affect the conclusions, but they will affect the timescales of development of these landscapes. As you are running this across a specific timescale, how much does a lowering of the diffusivity affect this conclusion?

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