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Earth Surface Dynamics Discussions

Interactive comment on "Arctic-alpine blockfields in northern Sweden: Quaternary not Neogene" *by* B. W. Goodfellow et al.

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Editorial comments on 'Arctic-alpine blockfields in northern Sweden: Quaternary not Neogene'

Two referees have so far commented on the paper by Goodfellow and coauthors. I agree with both referees that this is an interesting study that provides us with valuable new insights into the history of the Scandinavian blockfields. The authors convincingly argue that the existing blockfields developed during the Quaternary in a cooling climate, and that their presence does not imply surface stability since the Neogene. The late Quaternary weathering rates must however have been low (6-16 mm/ky) to account for the measured cosmogene nuclide concentrations measured in samples from two summits.

The referees highlight several strengths of the study, including the substantial data set used and the detailed studies of mineralogy and granulometry. I agree with this; it is a well-written paper that improves our fundamental understanding of some fascinating landforms. I suggest that the paper should be published after revision based on the constructive referee comments.

Regarding the latter, it is worth highlighting the comment by referee 2 on the use of the ice sheet modeling. Also, both referees comment on the discount of glacial and periglacial "buzzsaws" in the discussion section. I suggest that it is specified more clearly what is precisely meant by the action of the "buzzsaws" and how much erosion these mechanisms imply for the surfaces. Clearly, several hundred meters of erosion in the Quaternary is beyond what is possible. On the other hand, given that the highest parts of the mountains may have experienced alpine style glaciers and small ice caps for >10 Myrs (e.g. Thiede et al. Quad. sci. res., 1998), it does not seem completely impossible to me that glaciers and frost from before the late Quaternary have contributed to the present form and distribution of the high surfaces. This early style of glaciation could perhaps even be more efficient in shaping the high surfaces than the Pleistocene glaciations, or can we really rule this out?

A few additional comments:

It is stated several times in the manuscript (lines 28, 193, 745) that the high surfaces can be used as markers against which to determine glacial erosion, provided that the surfaces have been stable during the late Quaternary. Yet, I guess that, besides surface stability, this also involves assumptions regarding the pre-glacial relief. What if the glaciers simply amplified an existing (but subdued) relief?

Regarding the CN analysis, I was confused by the corrections made due to snow cover. The apparent exposure ages are not corrected, but the total exposure ages are, right? Please clarify why this is.

I agree with referee 2 that the ice sheet model section could be improved, and that a

figure would help. Also, still repeating the comment of referee 2, I understand why grid resolution is immaterial for flexural isostasy, but for the burial age of a summit this is likely different.

line 621: “This offers suggestive evidence that. . .” -> “This implies that. . .” or something similar.

line 749: Would the likelihood of regolith forming on plucked or abraded surfaces not depend on the time available?

line 755: What are the erosion rates required by the glacial or periglacial “buzzsaws”?

Fig. 1: The black labels on the maps are difficult to read. Larger fonts or a different color might help.

Fig. 3: Should the labels of the two axes be swapped?

Fig. 7: This is a very long caption. I think that it can easily be shortened because some of it is repetition of the main text.

I wish the authors the best of luck when revising the manuscript, and I look forward to reading it again. I hope that the referee comments and my own will be of use.

David L Egholm, April 29 2014.

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