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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.

B. W. Goodfellow et al.

brad.goodfellow@natgeo.su.se

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Responses to comments by journal editor David Egholm on "Arctic-alpine blockfields in northern Sweden: Quaternary not Neogene."

Responses by Bradley Goodfellow (on behalf of all contributing authors).

We thank the two referees and the editor for their insightful comments. We have modified the manuscript accordingly and our responses to the editor are outlined below.

David Egholm editorial comments:

A) 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

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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?

This comment is addressed in our response to the anonymous reviewer. We have modified a number of paragraphs in the Discussion to account for these criticisms of the original manuscript, which we consider to be valid.

A few additional comments:

A) 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?

A good point. We have modified text in the Introduction and Discussion to account for this criticism. We concede that the utility of presently blockfield-mantled surfaces as markers by which to quantify Quaternary glacial erosion remains uncertain. We do though argue that if extensive erosion Plio-Pleistocene erosion of these surfaces has occurred, it might have more likely been through periglacial, rather than, glacial processes.

B) 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,

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right? Please clarify why this is.

This is correct. The apparent exposure ages are minimums for these two summits. Because of high uncertainty regarding surface burial by snow, we incorporated this into the calculations of regolith residence times, which also include surface burial by ice sheets and effects on nuclide production rates through bedrock isostatic response to glacial loading and unloading (which also have high uncertainties). We have modified the manuscript to explain our reasoning on this point.

C) 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.

We agree with this criticism. Please refer to our response to the anonymous reviewer.

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

Done!

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

Perhaps, but over time scales that may exceed the sum of ice free periods during the Quaternary. We now offer some discussion of this point in the manuscript. We also provide reasons why we think that establishing blockfields on glacially scoured bedrock might be very difficult.

F) line 755: What are the erosion rates required by the glacial or periglacial “buzz-saws”?

We have rewritten a large part of the Discussion to address pervious criticisms that our data not answer the question of whether or not a ‘buzz-saw’ may have impacted these

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surface during a period prior to the late-Quaternary. We have avoided putting a number on what erosion rates are required for a glacial or periglacial buzz-saw as our data do not address this. Our data only highlight low erosion rates during the late Quaternary.

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

Done!

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

Yes, they should be, and the error was corrected in the version that was available on ESurf Discussions

I) 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.

The caption has been rewritten sand shortened in accordance with this criticism.

Please also note the supplement to this comment:
http://www.earth-surf-dynam-discuss.net/2/C138/2014/esurfd-2-C138-2014-supplement.pdf

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Interactive comment on Earth Surf. Dynam. Discuss., 2, 47, 2014.