Responses to comments from Henriette Linge, an anonymous referee, and 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 are outlined below (in red).

1). Henriette Linge Review

Specific comments:
p. 67, l. 8-9: rephrasing is suggested for this sentence: 'Because nuclides have likely accumulated in surface regolith at a faster rate than provided for in our model and nuclide decay has likely been less.' Rates for accumulation and decay cannot vary, but the duration of exposure and burial can?

To clarify this issue the text has been rewritten as:

> The key consequences of this for our subsequent analysis of regolith residence durations are that the lengths of the ice-free periods during which cosmogenic nuclides accumulate are likely underestimated, whereas nuclide decay periods during ice sheet burial are likely overestimated. If nuclides have accumulated in surface regolith more quickly than provided for in our model and nuclide decay has been less, inferred maximum erosion rates will be underestimated and regolith residence durations, for a given erosion rate, will be overestimated in our analyses.

p. 67, l. 26: consider rephrasing, 2x offers/offered.

Done!

p. 70: Clarification of what implications that can be inferred from the presence of gibbsite is important and welcomed.

The presence of gibbsite in alpine regoliths has been a highly misleading indicator of the climate under which an alpine regolith may have originated and, subsequently, its age. Because of this and the reviewer comment we have added additional text to further clarify why limited gibbsite quantities commonly form in alpine regoliths. Furthermore, we added a figure showing thermostability relationships between feldspars, weathering solutions, and gibbsite, and also the relationship between gibbsite solubility and pH.

p. 71-72, evidence against the glacial 'buzz-saw': Early glacial erosion of the uplands in Norway was suggested by Reusch in 1910 (Effects of glacial erosion in Norway, 11th international geology congress) as an explanation for the existence of flat upland surfaces. The modelling study of Pedersen & Egholm (2013, Nature 493, 206-210) also suggests that glacial erosion formed the flat upland surfaces in alpine settings prior to the mid-Pleistocene transition (950 ka). Except for the predicted (and logically explained) resistance to formation of blockfields on glacially-eroded surfaces, it seems that there is not necessarily any conflict between the results of Pedersen & Egholm (2013) and the modelled total surface histories (p. 67) 'suggesting evidence that the late Quaternary has offered sufficient time for the present regolith mantles: : :to gain their respective 10Be inventories'. I think it is important to clarify why the glacial/periglacial buzz-saw model is not compatible with the findings from northern Sweden, despite the apparent agreement in timing.

This is a good point. We have modified our text to firstly highlight that the residence times and geochemical features of present blockfield regolith are compatible with the model of Pedersen and

Egholm but that we consider possible early Quaternary erosion of now blockfield-mantled surfaces to have more likely been through periglacial, rather than glacial, processes.

2). Anonymous Review

Specific comments:
As the age determination for the blockfields is dependent to a significant degree on the cosmogenic dating it would be useful to see a photograph of the two sample sites. Also a more detailed description of the sample locations would be useful i.e. it is noted as almost an afterthought in the Duoptecohkka site description that the samples were taken from summit crests.

The relevant paragraph has been rewritten in accordance with this criticism. We have also included a new figure of the sampling sites.

Section 4.3 discusses the presence, in minor quantities, of vermiculite, gibbsite, oxyhydroxides and kaolinite with the more advanced weathering products confined to concave water retaining locations. However, SEM investigations indicate an almost complete lack of evidence of chemical etching of grains. Given that there is clearly some chemical weathering occurring why is there a lack of evidence from the SEM analyses?

We have rewritten the relevant paragraph to better explain this. SEM investigates particles larger than clay-sized and while some of these grains are chemically weathered, they are rare. This is compatible with the mineralogy of the clay-sized regolith, which is dominated by primary minerals but also reveals minor quantities of secondary minerals.

Ice sheet modelling - the ice sheet modelling section is unclear. Firstly the spatial grid resolution needs to be clarified. It is run at 40 km resolution but it appears that the effect on mean elevation is little impacted compared with a 20 km grid or indeed a 50 m DEM? But what is the main aim of the modelling. If it is to investigate the impact of isostatic rebound on the cosmogenic nuclide production rates then this model seems suitable. However, in terms of the duration of ice cover this seems to be very coarse. What is the potential for smaller ice masses to exist on the summits which is not captured in the topographic smoothing resulting from the grid resolution? Could these have a significant impact on the exposure history? The model is run for approximately the last 1 Ma of the Quaternary. Is there any erosion component in the model as it would seem likely that there is valley incision over this timescale and this will therefore have implications on the isostatic rebound and cosmogenic production rates.

This is a very good point. We use the model for estimating both the impact of isostasy on nuclide production rates and for estimating ice sheet burial durations. We concede that our simple model has limitations and have rewritten our text to more thoroughly highlight these in response to this criticism. As the reviewer correctly points out, there is potential for smaller ice masses to have existed on the sampled summits. However, our sample locations experience very high winds, which limit snow accumulation (at least in the present day) and would perhaps restrict ice cap formation in the past. Furthermore, the sample locations are on ridge crests, which are perhaps unlikely places for small ice caps to persist following retreat of large ice sheets. Also, ice sheet models with resolutions required to capture these small ice caps are not run over such long timescales (>~1 Ma), which limits our analyses to a coarser resolution. Our model lacks an erosion component but erosional unloading may be of less importance in this landscape of selective linear erosion than in other, lower latitude, alpine areas. For each of the above reasons, our model may underestimate regolith residence times. Conversely, we also outline reasons why our model may over-estimate ice volumes, which could produce over-estimates of regolith residence times. We consider our model to have error margins of ~20% and consider our approach to be reasonable in answering an order-of-magnitude question: Do the regoliths have residence times confined to the Quaternary or could they extend back in time into the Neogene?

"the total surface histories of Duoptecohkka and Tarfalatjårro become asymptotic above cut-off values of ~290 ka and ~390 ka before present, respectively" does this refer to the burial and isostacy model? If so it seems from Figure 7 that these should be higher for both sites?

Higher values have now been provided in accordance with this observation. Even if we add an additional tens to hundreds of thousands of years to each quoted age, it does not affect our key argument that the residence times of the present blockfield regoliths are constrained to the late Quaternary.

The penultimate paragraph of the discussion concludes that the low erosion rates and regolith residence times provide evidence against the operation of a glacial buzz-saw and also a periglacial buzz-saw. However, the modelled total surface ages do not extend back beyond the middle Quaternary and it is earlier started that the "While average erosion rates of blockfield-mantled summits are low, they are of sufficient magnitude to remove shallow (1–2 m thick) regolith profiles within a late Quaternary timeframe". Therefore it is not clear how this demonstrates the conclusion? It is not clear exactly when then blockfield mantles are interpreted to have formed, are they assumed in equilibrium i.e. production rates _ erosion rates? There needs to be a much clearer discussion of the data presented and how it leads to the conclusions regarding age of the blockfields, erosion versus formation and implications for the glacial and periglacial buzz-saws.

Again, a really good point that highlights some logical shortcomings in the original manuscript. We have rewritten portions of the Discussion to address these points. We now highlight that we consider the regoliths to be in equilibrium (i.e., production rates equal erosion rates) and that while our data rule out the operation of a buzz-saw in the late-Quaternary, they do not discount a buzz-saw operating earlier on these surfaces. Indeed, given that geochemical evidence of Neogene regoliths is entirely absent, our data are compatible with extensive erosion earlier in the Quaternary. We do though argue that if this erosion occurred, it was more likely through periglacial, rather than glacial, processes.

Technical Corrections:
L158 1997; Bireman

The correct spelling is actually Bierman.

Blockfield structure – the first paragraph could be rewritten to be clearer

Upon re-reading we agreed and this paragraph has been re-written accordingly

L491 embedded in gravel

Done!

L675 over-all

Done!

3). David Egholm editorial 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?

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:
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.

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.

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.

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.

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

Done!

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.

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

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 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.

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

Done!

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

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.