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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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Responses to comments from an anonymous referee 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 anonymous referee are outlined below.

Specific comments:

A) 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.

B) 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.

C) 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?

D) "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.

E) 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:

A) L158 1997; Bireman

The correct spelling is actually Bierman.

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

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

C) L491 embedded in gravel

Done!

D) L675 over-all

Done!

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