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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.

Anonymous Referee #2

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General comments

This paper presents a comprehensive attempt to determine the origin and significance of summit-top autochthonous and allochthonous blockfields using a substantial data set collected from the mountains of northern Sweden. The paper is very well written and takes a combined approach in order to determine the age and origin of these enigmatic landforms. Field data comes from a significant number of pits excavated to depth in the blockfields where samples were collected for granulometry, mineralogy and visual (SEM) investigations in order to determine the weathering origins. Surface exposure ages are determined from two samples using 10Be and 26Al ratios to determine simple and complex burial histories. The complex burial histories are supported by ice sheet modelling which provides an indication of potential periods of ice coverage

and thus cosmogenic shielding. Based on these analyses the authors conclude that the blockfields are likely the result of both physical and chemical weathering occurring during periods when the summits are exposed subaerially but are not pre-Quaternary in age. Low erosion rates, calculated from cosmogenic exposure ages are used to argue that the blockfields preclude the glacial buzz-saw as glacially eroded surfaces would favour water shedding which is required for regolith formation. The low erosion rates are also used to discount the periglacial buzz-saw.

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 Duoptečohkka site description that the samples were taken from summit crests.

Section 4.3 discusses the presence, in minor quantities, of vermiculite, gibbsite, oxy-hydroxides 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?

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

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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.

"the total surface histories of Duoptečohkka and 619 Tarfalatjårro become asymptotic above cut-off values of ∼290 ka and ∼390 ka before 620 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?

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.

Technical Corrections:

L158 1997; Bireman

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

L491 embedded in gravel

L675 over-all

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