Review of

Graham et al.

In Situ 10Be modelling and terrain analysis constrain quarrying and abrasion at Jakboshavn Isbrae, Greenland.

This is a great study, and very important to finally quantify what has been for > 100 yrs regarded as the main subglacial erosion mechanisms. The study design is sound, the results convincing and generally well presented, and limitations and uncertainties well discussed. I am not familiar with the details of cosmogenic dating methods as such, and I trust that another reviewer will comment on this.

This Manuscript should be published. As always, however, a manuscript can always be improved, so here goes. Some minor re-organisation is needed, some extra background information in Setting, and some other minor improvements are suggested.

General comments

1. It took me while that we're really here looking at a mini-glaciation, or a advance-retreat cycle of c. 200 yr duration. This should be more clearly mentioned in the Abstract.
2. Reporting of Results. The figures of eroded volume are really quite meaningless, as this is only valid for a specific area. It would be much clearer and meaningful to report erosion depth (and erosion rate, as is done already). Thus: "depth of X mm by abrasion and Y mm of quarrying, averaged over a study area of 1.33 kmsq".
3. The only section that is weak is the **Study Area**. This needs a fair bit of work. Extra info, which can help to place the Results in context, are missing.
    a. The whole section is unclear to me – not being familiar with the Jakobshavn setting. (And given the general importance of this MS, hopefully many other readers are similar). It needs to be made much clear that this concerns a 'mini-glaciation', or a 220 yr advance-retreat cycle in historic times, not the main Last Glacial Maximum.
    b. The provenance or justification of the advance date should be dealt with here, not in the Discussion. Why not 1750 CE, or 1700 CE or 1600CE? It is also a bit odd to have a poorly constrained start date '(~1790) is then used to duration with a narrowly defined error (220 +- 5 yr).
    c. The rock type should be mentioned here. It's not mudstone or sandstone, right? (It looks like some granodioritie / tonalitic gneiss (?), but surely there's a bedrock geology map that can be consulted? (See also Comment Line 262 below).
    d. Surely something is known about the (maximum) thickness of the ice at the study site, if only from earlier topographic maps or early satellite imagery?
    e. Equality, is something known from satellite or other observations in the 1980s to 2000s about the ice velocity?
    f. Line 75-77 should move to Study Setting, not in Methods.
4. Some geomorphological results are only mentioned in the Discussion (line 306-309). These are Results, and should move to that section – not presented so late in the Manuscript.

Specific Comments

Lines

47-48. I guess would be fair to emphasise that that the sediment flux measurements are *indirect* proxies, and not direct measurements. (And maybe later in the discussion that such measurements can only work for small ice catchments, as in large ice sheets the grains size gets smaller with longer subglacial fluvial transport distances & would over-estimate the Abrasion component……).

98. Mention the distances from the quarried step/face. ".. two from the quarried floor, 1 and 2 m from the quarried step/edge, and one from a polished surface more distal (4m) from the quarried step/edge..".

208. 'four nearby results'. Do these occur on Figure 6? If so, their position should be plotted on that figure. Or on Figure 1. Also, we get no idea of the variability of the total of five abrasion depth values: this should be added – best as a separate, small table.

210-213. If reporting only averaged abrasion depth and not volume (see General Comment 2), these lines can go, but a calculation of the average abrasion depth should be written instead. That would be more informative, and less awkward.

238. Lee face orientation is a poor constraint on ice-flow direction, as it is commonly heavily predicated or influenced by the orientation of the pre-existing fractures (joints), and can thus deviate by up to 40° from the ice-flow direction; see study of Hooyer, et al. (2012). *Control of glacial quarrying by bedrock joints*. Geomorphology, 153, 91-101. I strongly suggest to take this out.

273. "… processes related to the formation of crescentic gouges (Gilbert 1905)…".

1. Should be Gilbert 1906??
2. Although Gilbert (1906) is a great paper, well ahead of its time, there are quite a few more modern relevant references: Harris, 1943; Embleton & King, 1975; Ficker et al., 1980; Wintges, 1985; Prest 1983; Drewry, 1986; Glasser and Bennett, 2004, and Krabbendam et al. 2009. The latter gives a reasonably good overview of the terminology, too (although dealing with a special case)
3. The phrasing here is very vague. I think it would be good and informative to mention the *actual* process, namely exertion high clast-bed contact forces, exerted by large boulders embedded in basal ice pressing onto the bed. (see also Comment on Line 339).

297-298. It is still unclear to me what the justification for 1790 CE is for the start date of the historic glaciation – and not 1750 or 1700… A short sentence, summarising the previous work would be helpful here.

262. hard crystalline rock, fracture spacing .. AAAH! This should be described in the Setting!

339. This is the first mention of clast-bed forces, which reads very odd to people not familiar with crescentic gouges. This is why this should be explained near Line 273.

Minor text issues (mainly to clarify things).

<u>Underline</u> means suggested addition to text.

Title: why not mention 'rates'. "In Situ 10Be modelling and terrain analysis constrain quarrying and abrasion ***<u>rates</u>*** at Jakboshavn Isbrae, Greenland". Just to set it apart from the more general geomorphological papers around….

Lines

36. "… rock fragments are entrained <u>in basal ice</u> and pressed..". Clarity..

51. ".. a well constrained, <u>short-duration (c. 200 yr)</u> advance-retreat cycle…"

78. possibly mention surface roughness? Mm-scale roughness, or similar. " noted rock surface texture and roughness to distinguish abraded from quarried surfaces…" or similar.

83. ".. divot…" this seems to be mainly a rather obscure term originating from golf….? Would "hollow" or similar not be better..? I feel this either needs changing throughout, or explained at first use. Throughout the manuscript…

84. ".. a sharp transition from rough fractured to smooth abraded surface texture.."

86. ".. to estimate the chape of the quarried volume (single… "

89-90. This is awkward phrased: try to improve.

93-95. Bit of repetition here. Most of Line 93-94 are repeated in next sentence. Why not: "At location A, we sampled and measured 10Be concentrations in five samples…."

111. "….(1: freshly fractured ~~exposed~~ surfaces…."

112: "…. that the fresh-appearing fracture surfaces …"

114. first use of gouges can be deleted.

200. "..erosion took place recently."

222-224. Great! I like this!

223. "… prior main glaciation" (or some other word).

225. ".. a mixture of rough fractured and smooth abraded lee surfaces."

247. "… for establishing the relative contribution.." (Suggestion only)

248. ".. on computational models or proxy inferences made from ….". For emphasis…

266-269. I like this!

300. "… but the ratio of abrasion to quarrying and the total depth of glacial erosion over the last historic glaciation would be unaffected."

Good luck with this – I truly hope this gets published. You guys actually scooped us with these methods - hohum, but such is life.

Maarten Krabbendam, Edinburgh, 25 May 2023