

Clim. Past Discuss., author comment AC3
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Reply on CC1

Tamara Pico et al.

Author comment on "Was there a glacial outburst flood in the Torngat Mountains during Marine Isotope Stage 3?" by Tamara Pico et al., Clim. Past Discuss.,
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Community Comment # 1

Pico et al present an interesting manuscript that tackles two outstanding questions in Quaternary glacial geology and paleoclimatology: what were the dimensions of the Laurentide Ice Sheet during Marine Isotope Stage (MIS) 3 and what triggered Heinrich events? Pico et al suggest that the formation and sudden discharge of a proglacial ice-dammed lake in the Torngat Mountains (Labrador, Canada) sometime during MIS 3 could have served as a triggering mechanism for a MIS 3 Heinrich event(s). Indeed, the authors present an impressive reconstruction of the region's paleotopography and potential paleolake dimensions and volume that hypothetically acted as a trigger for a MIS 3 Heinrich event. This argument relies heavily on 1) field evidence for the existence of a paleo ice-dammed lake with wave action eroding bedrock platforms, and 2) the age of the lake dating to MIS 3 based on interpretation of two ^{10}Be ages from a bedrock surface near the proposed shoreline. Based on our experience measuring ^{10}Be in glaciated landscapes exhibiting weathering zones and spatial patterns of ice-sheet erosion, we think there are more likely, alternative interpretations of an ice-margin history that would result in the apparent ^{10}Be ages reported by Pico et al.

We thank the authors of this comment for their positive comments on our manuscript and for their constructive feedback.

^{10}Be measurements in bedrock

The authors report two statistically indistinguishable ^{10}Be ages from bedrock surfaces of 35.9 ± 1.1 ka and 36.0 ± 1.1 ka, although it is not clear whether the samples are from the arrows in Fig. 4 or elsewhere. The authors recognize that this landscape was likely covered by ice during the Last Glacial Maximum/MIS 2 and assume that the most recent episode of deglaciation occurred ca. 10 ka. The authors suggest that the apparent ^{10}Be ages represent ~ 10 ka of recent surface exposure leading up to the year of sample collection, and ~ 25 ka of exposure (or exposure equivalent) occurred prior to the last deglaciation but fail to evaluate the range of possibilities for the exposure interval(s) represented by the residual ^{10}Be inventory.

To make sense of the excess ^{10}Be in these samples and to consider the sample locations'

full exposure/burial history, the authors assume that ice advanced over the sample sites at 30 ka and remained over these sites until 10 ka (20 kyr of ice cover). Therefore, there is an additional 20 ka of ice-cover history that is not reflected in the apparent ^{10}Be ages (i.e., no nuclide production under ice). The authors use this 20-kyr interval of burial to suggest that the likely exposure age is 56 ka (36 ka measured + 20 ka assumed burial), which also implies the excess ^{10}Be accumulated between 56 ka and 30 ka. In turn this "likely" age of 56 ka is used to date the drainage of an ice-dammed lake and further suggest that this region underwent an episode of deglaciation at this time.

We believe that alternative explanations are equally, if not more plausible.

The excess ^{10}Be in the surfaces (the ~25 kyr worth of ^{10}Be) could have accumulated in the samples just about any time in the last million or more years. Without ^{26}Al data, we are essentially "blind" to when it may have accumulated. With ^{26}Al data demonstrating no isotopic disequilibrium, one could then only reduce the time during which the ^{10}Be could have accumulated to the last ~100-200 kyr. We also cannot know if the excess ^{10}Be accumulated during a single episode of pre-LGM surface exposure, multiple episodes of pre-LGM surface exposure, single episode of subsurface isotope accumulation, or multiple episodes of subsurface isotope accumulation. Among all of these options, none of which are uniquely constrainable with available data, what support is there for the interpretation of a single period of MIS 3 exposure that the authors put forward? For the chosen interpretation to be supported, a fuller discussion and ruling out of alternative interpretations is required.

We appreciate the perspectives raised in this comment and respond below to how we will incorporate such alternate interpretations in the revision of the manuscript.

We offer two other possibilities that are more likely:

1) At 890 m asl in a landscape classified as "intermediate weathering," studies show that bedrock surfaces almost always contain inheritance, usually 10s of kyr of inheritance in age equivalent terms. We did not see any text confronting – and ruling out – a likely scenario that wave action from a lake, or meltwater flow, during the last deglaciation stripped surficial debris off of the sampled outcrops leaving planar bedrock surfaces with inherited ^{10}Be that lacks a unique temporal explanation. We think this is a likely explanation for the measured inventories.

The authors of this comment are correct to note that we cannot rule out the possibility of till cover. This cover would make the ages older since the till would shield the bedrock surface.

We will add the following the text to the manuscript to note the possibility of till cover:

"Till cover would cause the age assigned to the glacial lake shoreline to be older"

2) We are unaware of studies where an apparent ^{10}Be age that is *known to be influenced by isotopic inheritance* (as Pico et al show here) is interpreted literally because, again, there is no way to determine when exactly inherited ^{10}Be accumulates in a rock sample (see above). Here, the authors are prescribing that the inherited ^{10}Be accumulated between 56 ka and 30 ka. In fact, if the excess ^{10}Be accumulated at any other time(s), then the paleo ice-dammed lake likely cannot be MIS 3 in age. There are, however, some reasonable exposure/burial histories in this region that would result in measurable amounts of isotopic inheritance in bedrock. An alternative scenario, in addition to the one we mention above, is that the currently unglaciated terrain where these bedrock samples were collected was also ice free during the Last Interglacial (MIS 5e) when temperatures likely exceeded those of the Holocene. In this scenario, bedrock exposure during MIS 5e

would result in the accumulation of ^{10}Be and during post-MIS 5e glaciation across the region, the Laurentide Ice Sheet did not erode through the required 2-3 m of bedrock needed to reset the cosmogenic clock. We think this is particularly likely in a region that was susceptible to non- or minimally erosive cold-based to polythermal ice; 20+ years of ^{10}Be measurements in Canadian Arctic and sub-Arctic have revealed that bedrock is often influenced by isotopic inheritance. By Pico et al's own conclusion, this region likely experienced times of cold-based ice and they invoke this phenomenon to explain the preservation of paleoshorelines. Thus, the measured ^{10}Be inventory presented by the authors could simply reflect 10 ka of recent exposure combined with ^{10}Be accumulated during MIS 5e or perhaps even leftover ^{10}Be from even earlier interglacials (e.g., MIS 11), minus the amount of ^{10}Be that was stripped from the bedrock as a result of the depth of subglacial erosion that occurred during post MIS 5e ice cover. Slightly more subglacial erosion (but not exceeding 2-3 m) during the interval of ice cover would result in younger apparent ^{10}Be ages, while slightly less erosion would result in older apparent ^{10}Be ages, but both end members would yield apparent ^{10}Be ages influenced by isotopic inheritance.

It may be tempting to consider the statistically indistinguishable apparent ^{10}Be ages presented here as evidence of a true exposure signal that are not influenced by erosion. To fully evaluate this possibility, however, full descriptions of the sample locations are needed; it is presently unclear where these bedrock samples are from (e.g., listed sample coordinates plot closer to Ungava Bay than the site shown in the accompanying figure, and only at 140 m asl). For example, if these samples are close to one another then the two bedrock surfaces almost certainly experienced the same long-term exposure-burial histories and therefore the similar apparent ^{10}Be ages could be a function of residing under the same amount of till cover that was later stripped away (shielding; ^{10}Be production at depth). Additionally, it is not unreasonable to suspect that similar ^{10}Be ages are a function of the same magnitude of subglacial erosion through the ^{10}Be production-depth profile. In this latter scenario, the full exposure-burial history of the sample locations would be greater than ~ 36 ka, but the excess ^{10}Be that could potentially explain this full history has been eroded away.

The claim that this region, a suspected LIS inception point that therefore spends most of its history under ice, deglaciated during MIS 3 is a fairly extraordinary suggestion. We suggest this claim requires firm chronological constraints. Because it is highly likely this region, like today, was ice free during MIS 5e, we think the authors should provide compelling evidence for why the inherited ^{10}Be is not simply a relic from MIS 5e (or earlier interglacials) that has been preserved under a minimally erosive Laurentide Ice Sheet.

We appreciate the comment author's careful consideration of our manuscript and ideas for alternate hypotheses. We agree that there is considerable age uncertainty on the data presented in our study, and we hope that we have been clear that we view this dataset as preliminary evidence for future detailed fieldwork. Alternate hypotheses about the age of samples are certainly plausible. Such findings will be compelling in their own right since there is little data on pre-LGM glacial lakes in this region.

To note the possibility of alternate ages, we will include the following text on line 291:

Should an improved understanding of the ice burial history suggest a longer period of ice sheet cover, this glacial lake shoreline may be assigned older ages than those presented here. For example, depending on the history of ice cover and erosion, the lake shoreline may correspond to earlier periods of retracted ice such as MIS 5a or MIS 5e. Nevertheless, given the preservation of this lake shoreline feature, increasingly older ages are less likely.

Geomorphic evidence for a paleo ice-dammed lake

Considering the importance of this suspected paleo ice-dammed lake, the geomorphic evidence pointing to this lake's existence needs to be better described. Pico et al provide one figure (Fig. 4) with two pictures meant to document paleo-shorelines attributed to this ice-dammed lake. We do not see anything definitive in these pictures that suggests the presence of a large ice-dammed lake nor clear evidence of wave-cut shorelines. In particular, in Figure 4b, there appears to be extensive till and/or regolith cover with a few bedrock surfaces nearby. Without further information, this appears to be till and/or regolith cover that was washed aside as meltwater flowed across the landscape during a period of deglaciation, not necessarily associated with an ice-dammed lake. To be clear, it is possible that a large ice-dammed lake at ~890 m asl existed, but the evidence for this needs to be more thoroughly documented.

The noted shoreline is not in line with any fracture plane, and because it is eroded into bedrock it cannot be a solifluction or gelifluction lobe. For other glacial erosion processes we would not expect to see such a planar surface. Therefore, we interpret this geomorphic feature as a shoreline platform.

Lastly, if there was a large ice-dammed lake and the ^{10}Be samples are from wave-cut benches, the interpretation of the apparent ^{10}Be ages and lake chronology presented by Pico et al would require that wave action on the edge of the lake remove 2-3 m of bedrock to reset the cosmogenic clock. This magnitude of wave-based erosion might be possible at the outlet channel hosting a catastrophic outburst flood, but 2-3 m of wave-cut erosion by an ephemeral ice-dammed lake into crystalline granitic terrain is unlikely. Moreover, if wave action only removed a few 10s of cm or ~1 meter, instead of 2-3 m, then the measured ^{10}Be inventories reflect a component of ^{10}Be produced at depth and the full exposure history of the bedrock surface is more, but this evidence has been eroded away. We note that the text speculates that there was outburst flooding, but the evidence provided for this is also consistent with a variety of smaller scale glaciofluvial processes. Rather than a large ice-dammed lake that suddenly drained, we wonder why this cannot be a case of terrain that was ice-covered during the LGM and during landscape deglaciation, the likely significant amounts of meltwater runoff simply washed away till and/or regolith exposing bare bedrock surfaces that contain ^{10}Be from a previous period of exposure. Note this would be consistent with the alternative interpretation of the ^{10}Be inventories that we mention above.

Wave-cut benches in granite do exist, for example, on Sharp Island, Hong Kong (Berry & Ruxton, 1957).

Although the bedrock in our study region is gneiss, it is highly fractured rock. Frost action and ice wedging acts on preexisting pervasive fractures, resulting in a more easily weathered rock. Studies based on glaciated gneiss terrains suggest that such fractured rock results in plucking-dominated erosion, which is likely for our study area (Krabbendam & Bradwell, 2014)

- *Berry, B. P. Ruxton, Structure and Form of Cheung Chau Island, Hong Kong. Geol. Mag. **XCIV** (1957).*
- *Krabbendam, T. Bradwell, Quaternary evolution of glaciated gneiss terrains : pre-glacial weathering vs . glacial erosion. Quat. Sci. Rev. **95**, 20–42 (2014).*

We accept that it is possible, although unlikely, that these bedrock surfaces were exposed during MIS 3. To support their interpretation, the authors would need to provide strong evidence as to why they prefer that inherited ^{10}Be in their samples was from MIS 3 exposure instead of any number of alternative scenarios, including but not limited to some of those we discuss above.

Our study relies on legacy data from a 2003 field expedition, and we do not have data

from additional shoreline sites. We nevertheless pushed forward with publishing these data to draw attention to this work, and to highlight the exciting potential for several facets of Quaternary work, including constraining the configuration of pre-LGM ice sheets, understanding glacial inception, as well as quantifying the timing and amount of freshwater release into the North Atlantic. We have stressed in our current study that attaining such information will be the focus of future field studies.

We appreciate the ideas raised in this comment. In response, we will edit the manuscript to include discussion of some of the alternate hypotheses raised by the authors of this comment.