

Geochronology Discuss., referee comment RC1  
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## **Comment on gchron-2021-21**

Anonymous Referee #1

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Referee comment on "Cosmogenic nuclide exposure age scatter records glacial history and processes in McMurdo Sound, Antarctica" by Andrew J. Christ et al., Geochronology Discuss., <https://doi.org/10.5194/gchron-2021-21-RC1>, 2021

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### **General Comments:**

In this paper, Christ et al. present a new surface-exposure dataset from the McMurdo Sound region of the Ross Sea in Antarctica. Although the high prevalence of inherited cosmogenic nuclides in local sediments makes surface-exposure dating in the region a challenge, here the authors use a nearby radiocarbon chronology to benchmark their data and to enable direct comparison of their apparent exposure ages with the timing of the local Last Glacial Maximum (LGM). They also recalculate exposure ages from previously published studies to enable a synoptic view of regional exposure ages and inheritance.

Their results indicate that although inheritance is indeed pervasive in the sampled glacial sediments, the ultimate pattern of exposure-age scatter is in part dictated by lithology and associated transport history. For example, clasts derived from subglacial sources appear to best reflect the timing of local deglaciation. In contrast, clasts sourced from areas above glacial trimlines produce exposure ages suggestive of possible nuclide inheritance. Following this analysis, Christ et al. assess potential longer-term (pre-LGM) patterns of glaciation in McMurdo Sound. They suggest that the pre-LGM Discovery drift unit was deposited during MIS8, highlighting the utility of surface-exposure dating to investigate surface processes and landscape evolution through time.

This paper illustrates an excellent application of larger exposure-age datasets. It is well written and well presented, and I appreciate their thoughtful discussion of how different sediment sources with unique histories may impact surface-exposure chronologies. There are a few areas where I think the authors need to add additional detail or justification for their methods and interpretations. I also have one larger comment centred on their discussion of  $^3\text{He}$  ages from local dolerite. I detail these comments below, as well as a few technical comments/corrections.

## Specific Comments:

The authors note that Ross Sea drift pyroxene  $^3\text{He}$  ages predate nearby quartz  $^{10}\text{Be}$  ages by 14-32 kyr (line 384). They assign this offset to differing mechanisms of clast transport and deposition, and suggest that the age offset may be explained by 'inherited' cosmogenic  $^3\text{He}$ . Previous studies show that Ferrar Dolerite pyroxenes contain non-zero amounts of non-cosmogenic  $^3\text{He}$  (see Ackert, 2000; Margerison et al., 2005; Kaplan et al., 2017; Balter-Kennedy et al., 2020). This amount is generally around  $5-7 \times 10^6$  at/g, and so significant over the timescales of interest here. In particular, this amount could account for some or all of the apparent offset between the Ross Sea drift  $^{10}\text{Be}$  and  $^3\text{He}$  ages. This could lessen the need for a depositional mechanism in this case. If the authors have a 'shielded' piece of Ferrar Dolerite on hand from their field site they could measure this non-cosmogenic amount directly. Alternatively, they could assume a non-cosmogenic concentration roughly in line with that measured elsewhere. Or if they have reason to think that non-cosmogenic  $^3\text{He}$  is not present within their samples they should make that clear within their discussion and interpretations. In any case, I would encourage the authors to discuss this point within their text.

The authors use the "LSDn" scaling scheme for their exposure age calculations. While I see no problem with this they should include a few lines to justify this choice. Why is "LSDn" preferable for this location or time period versus an alternative scheme? If the authors chose an alternative scheme, would their interpretations change? For example, would samples still fall within the proposed MIS8 window using an alternative scaling scheme such as "St"? Or would younger samples still correlate with radiocarbon ages of Ross Sea drift? As the "LSDn" scheme can produce higher production rates relative to alternative schemes such as "St" or "Lm", justifying their choice of scheme here is key.

Related to the above, although the authors note their chosen scaling scheme I was unable to find any discussion of the nuclide production rates used for exposure age calculations. As they use the online calculator [[hess.ess.washington.edu](http://hess.ess.washington.edu)] I assume this means that they utilise the standard/default 'global' production rates provided, but this should be clarified.

In addition, what atmospheric model is used for exposure-age calculation? I presume they used the Antarctic 'ANT' standard of Stone (2000), but it is best to list all calculation parameters to ensure reproducibility.

Figure 5 is an excellent visual synopsis of the data, but would it be possible to indicate which samples come from each lithology? Perhaps using additional colours or shapes? As lithology is such a central component of the overall discussion I think including this element would be very useful for the reader.

Figure 5 also highlights my concern with the second major argument of the paper, that the Discovery drift unit dates to MIS8. As the authors note, while there is no existing

evidence which contradicts this hypothesis, there is also no geomorphic or geologic data elsewhere which directly supports it (beyond their three new dates). As presented, there are three <sup>3</sup>He pyroxene samples that cluster in age near the end of MIS8. A fourth pyroxene sample is roughly 100 kyr too 'old', and the authors disregard the 'young' age of an eroding granite boulder. As the authors note, drifts often incorporate clasts with ages apparently 'old', but there is no argument made as to why these three ages should be taken as 'correct'.

To be clear, I am not suggesting that the Discovery drift is *not* MIS8 in age, but I do not think the authors have enough evidence to make the claim quite as they do. I would suggest that the authors soften their language on this point, and perhaps highlight the very exciting implications - and potential future research avenues - engendered by this possibility. The way they phrase these ideas in the conclusions section is a bit less definite, and is I think more appropriate in tone.

In lines 418-19 the authors note that increased accumulation due to atmospheric warming coupled with reduced ocean forcing may provide an explanation for more extensive glaciation during MIS8. It would bolster their *mechanistic* argument to highlight potential parallels between this scenario and similar proposed for the LGM (such as by Hall et al., 2015, which they cite elsewhere).

### **Technical Corrections:**

In the methods section the authors note that all ages were calculated using "...Version 3 of the online exposure age calculator hosted by the University of Washington (<http://hess.ess.washington.edu>) (Balco et al., 2008)...". However in certain data table captions the authors state that ages were calculated using "...the CRONUS online calculator v3 with LSDn scaling scheme...". Either is fine, but these should be consistent.

The second sentence of the caption for Figure 2 appears to be missing words?

Lines 174 and 188 - A missing word: "Updated exposure ages for all samples were calculated version 3 of the online..."

### **References:**

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