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Reply on RC1

Andrew J. Christ et al.

Author 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-AC1, 2021

Author responses are recorded in italics below reviewer comments.

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 3He ages from local dolerite. I detail these comments below, as well as a few technical comments/corrections.

Response to general comments: *Thank you very much for your well-stated summary of work and thoughtful comments. We will address your general comments about our methods and interpretations, and specific comments about* $^{3}He_{pyroxene}$ *ages below.*

Specific Comments:

The authors note that Ross Sea drift pyroxene 3He ages predate nearby quartz 10Be 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 3He. Previous studies show that Ferrar Dolerite pyroxenes contain non-zero amounts of non-cosmogenic 3He (see Ackert, 2000; Margerison et al., 2005; Kaplan et al., 2017; Balter-Kennedy et al., 2020). This amount is generally around 5-7 x 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 10Be and 3He 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 3He 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.

Response: *Thank you for pointing out the need for a nucleogenic $^{3}He_{pyx}$ correction. In the submitted version of the manuscript we did not apply this correction. Unfortunately, we did not collect "shielded" Ferrar Dolerite samples to directly measure the non-cosmogenic ^{3}He contribution at our field site. We recalculated the dolerite exposure ages using the correction of 3.3E+06 atoms/g reported by Balter-Kennedy et al. (2020), as well as the 5E6 to 7E6 at/g correction that you suggest. These corrections decrease the $^{3}He_{pyx}$ exposure ages of dolerite samples in Ross Sea drift by ~12.6 kyr (3.3E6 at/g correction), 19 kyr (5E6 at/g correction), and ~26 kyr (7E6 at/g correction). See the table below for a comparison of the non-correct and corrected ages (using the LSDn scaling scheme) below. Regardless of the correction, nearly all of the exposure ages of dolerite in Ross Sea drift are older than the timing of the local LGM, indicating that our original observation about inherited nuclide inventories in dolerite clasts remains valid. This sensitivity test suggests that the 7E6 at/g correction is likely too much for these samples, as it produces an apparent exposure age that appears modern (ACX-13-08: 161 yrs). This would be the only sample in the entire dataset (regardless of lithology or nuclide) to generate such a young age. The 5E6 at/g correction produces an apparent exposure age for this sample that is plausible but still too young (7.7 ka). The 3.3E6 at/g correction produces an exposure age (14.3 ka) that corresponds to the timing of the local LGM in McMurdo Sound. In the manuscript. We will report exposure ages using the 3.3E6 at/g nucleogenic correction reported by Balter-Kennedy et al., 2020 as this is the most up-to-date value used in the Antarctic cosmogenic nuclide community and produces exposure ages that are more plausible than higher correction values.*

In the revised manuscript we will include the information about the $^{3}He_{pyx}$ nucleogenic correction in the methods section and cite the papers (Ackert, 2000; Balter-Kennedy et al., 2020; Kaplan et al., 2017) you have kindly supplied.

Table AC1.1: Sensitivity testing of $^{3}He_{pyx}$ nucleogenic corrections on exposure ages of Ferrar Dolerite

Sample name	*No nucleogenic correction*	*3.3E6 at/g nucleogenic correction*		*5E6 at/g nucleogenic correction*		*7E6 at/g nucleogenic correction*	
	Age (yr)	*Age (yr)*	*Difference (yr)*	*Age (yr)*	*Difference (yr)*	*Age (yr)*	*Difference (yr)*
ACX_13_008	*26,978*	*14,300*	*-12,678*	*7,752*	*-19,226*	*161*	*-26,817*
ACX_13_009	*44,503*	*31,938*	*-12,565*	*25,407*	*-19,096*	*17,722*	*-26,781*

ACX_13_012	*42,320*	*29,565*	*-12,755*	*23,014*	*-19,306*	*15,307*	*-27,013*
ACX_13_048	*255,752*	*245,901*	*-9,851*	*240,827*	*-14,925*	*234,857*	*-20,895*
ACX_13_052	*239,440*	*229,235*	*-10,205*	*224,133*	*-15,307*	*218,130*	*-21,310*
ACX_13_061	*357,598*	*347,931*	*-9,667*	*341,487*	*-16,111*	*335,043*	*-22,555*
ACX_13_068	*242,780*	*232,358*	*-10,422*	*226,990*	*-15,790*	*220,674*	*-22,106*
ACX_14_005	*376,717*	*361,628*	*-15,089*	*353,854*	*-22,863*	*344,709*	*-32,008*
ACX_14_015	*52,310*	*35,999*	*-16,311*	*27,519*	*-24,791*	*17,543*	*-34,767*

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.

Response: *Thanks for bringing attention to this, we recognize that we should have clarified our decision about the scaling scheme. We employed the LSDn scaling scheme, which is time dependent, because the compiled dataset spans a wide timescale over the past 500 kyr. As you have suggested, we applied the LSDn, Lm ,and St scaling schemes for sensitivity testing on the exposure age dataset. Regardless of the scaling scheme applied, we still observe the same trends according to nuclide and lithology. The LSDn scheme indeed produces younger exposure ages than St or Lm, but the difference is usually less than 1 kyr for samples with exposure ages <50 ka. None of the samples younger than 20 ka in McMurdo Sound have differences greater than 880 yr; this means our interpretations about the exposure age scatter relative to the radiocarbon constrained timing of the local LGM are not affected. The exposure age difference between scaling schemes becomes greater for older samples, but again does not affect our interpretations even for samples from Mount Discovery that correspond to MIS 8. As we revise the paper, we will include these details about the scaling scheme sensitivity testing.*

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] I assume this means that they utilise the standard/default 'global' production rates provided, but this should be clarified.

Response: *You are correct that we mistakenly omitted explaining the production rate used in our calculations. We used the global production rates supplied by the online calculator. We will clarify this in the revised manuscript.*

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.

Response: *Yes – thanks for calling attention to this. We did use the ANT standard of Stone 2000. We will include this detail in the revised manuscript.*

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.

Response: *Great suggestion – we will change the symbology to different shapes to show different lithologies. In Figure 5 we have used the colors of the glacial deposits (yellow for Ross Sea Drift and orange for older glacial deposits) shown in our geologic map on Figure 4.*

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 3He 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.

Response: *We agree that we can soften our tone about the MIS 8 age of the Upper Discovery Deposit given the small number of ages (n=3) that correspond to this glacial period and scatter of the other ages. We also agree with your suggestion to reframe this section of the discussion to highlight the implications and future research avenues that this data presents.*

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).

Response: *Good suggestion – we will include this citation in this portion of the discussion.*

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.

Response: *Agreed – easy to fix.*

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

Response: *Good catch – we will fix this.*

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

Response: *Good catch. We will revise to "Updated exposure ages for all samples were calculated using version 3 of the online…*

References:

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Thanks for your thoughtful and constructive comments – we appreciate it!