GENERAL COMMENTS

This manuscript presents in situ 14C measurements from an altitude transect of bedrock and erratic samples from the Grove Mountains in East Antarctica that had previously been measured for in situ 10Be and 26Al by Lilly et al. (2010). That earlier study found large inherited inventories of the longer-lived nuclides. In situ 14C provides a means of seeing through that inherited signal to try to discern evidence of deglaciation since the Last Glacial Maximum (LGM) at ca. 21 ka. The results could indicate whether ice sheet models that predict coastal thickening and inland thinning of the East Antarctic Ice Sheet at the LGM are consistent with empirical evidence. This study constrains ice thickness history at a location significantly inland from the East Antarctic coast, but in my view requires significant additional data and discussion in two main areas before publication would be appropriate.

Most importantly, in my view the manuscript suffers from sloppiness and inconsistencies in the descriptions of the procedures used, as well as in presentation of the data and assumptions made in their interpretation. Clarity and transparency in those areas is critical to allow comparison with other work. It's clear that procedures have changed since the original Goehring et al. (2019) Tulane laboratory study; however, I could not find supporting data for those changes in the literature (see Specific Comments below). Much of the interpretation (e.g., production rates, saturation concentrations, etc.) is based on measurements from the Goehring et al. (2019) study. Without detailing potential effects of the subsequently modified procedures on measured 14C concentrations from more recent replicate analyses of CRONUS-A or other intercomparison materials (or otherwise documenting that there are none), this leads me to question the implied robustness of the results presented here. It's not enough just to state that there are no effects from the procedural changes – that conclusion should be documented. Neglecting to discuss potential additional uncertainties (if any) arising from these procedural modifications and calculator dependencies (see Specific Comments) gives the impression of in situ 14C ages that are more robust than they actually are in my view – particularly for pre-Holocene ages. I'm happy to be proven wrong about this impression but the effects of procedural changes on measurements and interpretations should be well documented.

Additional CRONUS-A measurements have been performed at the TUCNL – we will incorporate these new data into our production-rate calculations and recalibrate all the ^{14}C ages of our samples. The code used to calculate our ages is the same as that from the online UW calculator, run offline. Given how production rates are determined by this calculator, adding a small number of additional CRONUS-A measurements is unlikely to significantly alter the calculated ages. Further, any adjustments to ages are unlikely to change the overall narrative that has been developed. We will add an additional table to the supplement documenting the measurements used to determine the production rate for our samples.

Second, I think that the study would have benefited from at least one bedrock-erratic pair being analyzed from the original paper by Lilly et al. (2010), with more justification for sample selection (if there was no sample material left for paired analyses, fine, but that should be stated explicitly). Only three bedrock samples were analyzed, and not in conjunction with their respective erratics from the earlier Lilly et al. (2010) study. Analyses in that original

study were split between 16 bedrock and 10 erratic samples, with several bedrock-erratic pairs – most contained significant inherited 10Be/26Al signals relative to the 14C results here. Given the analytical focus here on erratics and the fact that the Grove Mountains comprise an isolated small group of nunataks with varying compositions (GeoMap - https://data.gns.cri.nz/ata_geomap/index.html) subject to W-NW ice flow toward the Lambert Glacier and Amery Ice Shelf (e.g., Mouginot et al., 2019, Geophysical Research Letters, 46(16), 9710–9718), a more detailed discussion of the site setting in terms of ice flow and broadly up-gradient lithologies would be useful. In particular, a discussion of rock types comprising the local nunatak group shown in Fig 1b would be very helpful to place the erratic lithologies in context to assess the possibility of exposure prior to deposition – particularly for those nunataks broadly up-gradient from the sampled nunatak (see Specific Comments). In my opinion, the authors need to demonstrate to the extent possible that the erratics are indeed truly erratic, and not locally derived lithologies that may have been exposed supraglacially during transport or recently on hillslopes prior to deposition on bedrock at the sampled locations, given the clear long-term inherited signals. For example, were there striations or other evidence of subglacial transport associated with the cobbles? If there is none, that's fine, but then that should be stated and clarified that the authors are then simply assuming that they are true erratics. Descriptions of each sample (with photos if possible) in the supplement would be useful information in this context. Analysis of bedrock-erratic pairs from the same locations would have gone a long way to support the authors' arguments. Given the remoteness of the study area and 20-20 hindsight from a decade and a half ago, I'm willing to cut the authors some slack on these points, but I believe that at least some non-trivial discussion of ice flow and local bedrock lithology from the local nunataks and the potential for prior erratic exposure over the late Pleistocene/Holocene is warranted.

This is a good point, and we will make several edits in light of it. Additional data, especially bedrock-erratic pairs, would be very helpful here. The ten samples presented in this study were, however, the only samples that had sufficient material remaining to analyze. The erratic samples measured in this study had already been partially processed prior to the beginning of this study, and had already been crushed, preventing us from identifying any signs of subglacial transport (if there were any). We will add language to the manuscript acknowledging the gaps and uncertainties inherent in this dataset. We will alter language implying a definite erratic origin of these samples, instead noting that the samples did not originate from the nunatak on which they were collected. If the samples were derived from a nunatak up-glacier of Nunatak 1921 in the Grove Mountains, they could have traveled a limited distance before being deposited. In this case, any inherited ^{14}C in our samples would imply that the ice-sheet thickening (not revealed by the ^{10}Be and ^{26}Al in our samples) was larger or more recent than assuming that the ^{14}C concentrations of the samples were reset prior to their most recent exposure would. In the absence of evidence which could confirm their origin, we have made the more conservative assumption regarding the magnitude of ice-sheet thinning since the LGM.

Once these significant issues (and Specific Comments below) are addressed, I look forward to reviewing this paper again.

SPECIFIC COMMENTS

Line 1: Thicker and thinner are relative to something. Suggest changing the title to 'A thicker than present East Antarctic Ice Sheet plateau during the Last Glacial Maximum'

We agree and will change the title to "A thicker, opposed to thinner-than-present, East Antarctic Ice Sheet plateau during the Last Glacial Maximum".

Line 9: in situ is not hyphenated as it is Latin – should just be italicized throughout

We acknowledge this point but prefer to keep "*in-situ*" hyphenated here because it forms a compound adjective.

Line 14: On the other hand, 'ice sheet' SHOULD be hyphenated, as it modifies the word 'interior.' The rule is that compound words that act as a noun such as 'ice sheet' should not be hyphenated, but should be hyphenated when they modify another noun (serving as an adjective), like 'ice-sheet interior', 'ice-sheet history', 'ice-sheet model', etc. Be consistent throughout.

We agree and will changed all instances that do not form part of a proper-noun phrase.

Line 20: 'Saturated' is colloquial - better to avoid colloquial phrasing without first defining. Better to say "Samples with 14C concentrations at a secular equilibrium between production and decay (saturation)..."

We will make the suggested changes to first define 'saturation'.

Line 43: Should use Oxford commas throughout in lists – 'radar, ice-sheet, and geological…'. Suggest replacing 'sparse', with 'rare' since 'sparsity' is used later in the sentence

We will insert the Oxford comma. While changing "sparse" here to avoid duplication is a good suggestion, we are attempting specifically to draw attention to how geographically dispersed this data is, not the lack of data itself, so we feel that "rare" risks causing some readers to slightly misinterpret our meaning here. Other phrases that communicate the same thing as "sparse" lack the clarity and word economy of "sparse" here, so we prefer to retain this phrasing.

Line 44: Active voice is almost always better in writing: ''often disagree' instead of 'are often in disagreement…'

While active voice *is* almost-always better in writing, in this case, the data (unlike, e.g., the researchers who collected them) are themselves inanimate records incapable of actively disagreeing with one another. We thus consider passive voice more appropriate here.

Line 51: Replace 'indicate with 'suggest'. Less definitive since an interpolated value is referenced

We will make these changes.

Line 58: Replace ‘Yet’ with ‘However,’.

We will change the line to “Existing cosmogenic-nuclide data from regions of cold-based non-erosive ice, however,”.

Line 60: Replace ‘nuclides’ with ‘nuclide inventories’. More precise wording

We will make the change.

Line 62: Reference should just be to Goehring et al. (2019) – no Balco. If there are multiple Goehring et al. (2019) citations for different papers, then specify 2019a, 2019b, etc.

We will add numeric codes to the citations to differentiate them.

Line 65: Fig 1c appears to have the Interior and Coastline labels reversed, based on the caption

We will make this correction.

Line 73: Shading looks more pink than red

The text describing the figure color will be altered to “pink”.

Line 77: Fig 1b: Would be useful to indicate the general direction of ice flow on this image - what nunataks are upstream of the sampled nunatak, if any

We agree and will add an arrow to the figure, with text added to the caption: “Ice at this site flows slowly (blue arrow; Rignot et al., 2011) northwest, towards the Amery Ice Sheet, though flow speeds are low and directions are strongly influenced by topography in the vicinity of nunataks (Lilly, 2008).”

Line 89: Instead of ‘be saturated with’ it would be more accurate and succinct to say here 'have concentrations of in situ 14C at secular equilibrium between production and decay (saturation) - a state that requires at ca. 5 half-lives of continuous exposure (Dunai, 2010). Need to make clear that it's the concentrations that indicate secular equilibrium.

We agree and will make the suggested change.

Line 90: Delete ‘exposed’

We will delete this word.

Line 91: Replace ‘covered at some time since the LGM’ with ‘likely covered for some duration post-LGM’. More precise wording.

We agree and will make this change.

Line 107: This is the situation I referenced in my General Comments. Do the rock types of the cobbles occur in the Grove Mountains (or is bedrock all just orthogneiss throughout)? The GeoMAP site, (https://data.gns.cri.nz/ata_geomap/index.html) indicates felsic plutonic rock types are actually quite common in the vicinity of the sampled nunatak. While that map is at quite a large scale, this suggests that at least some of the cobbles could well be locally derived erratics and thus have the potential for some subaerial exposure either on the land surface (e.g., rockfall and downslope transport) and/or on the ice surface (e.g., rockfalls onto the ice) before being deposited at the sampled locations. I'm not convinced by the single declaration that they are not locally derived, without any other discussion. Are the erratics striated or otherwise have evidence of subglacial transport? The rock types in Table 1 indicate orthogneiss for the bedrock, but all the erratics could totally be associated with felsic plutonics in the vicinity, lacking any more detailed description of the rocks. If the quartzites are sedimentary, state that as that is evidence of a true erratic. Metamorphic quartzites could potentially be associated with the gneiss. My point here is that uncertainty is fine but one needs to be up front about it. Statements of certainty when in fact significant uncertainty exists is a common theme I find in this manuscript.

We agree that some of this wording could be clarified, and will change "locally derived" to "derived from Nunatak 1921".

We are unfortunately limited by what information is available for the samples and study site. Orthogneiss is the only bedrock type known to us for these nunataks (e.g., Lilly, 2008; Lilly *et al.*, 2010). Unfortunately, all of the erratic samples had already been crushed and sieved by the time we accessed them, preventing us from identifying surface features that could support subglacial transport. Sample notes do not mention striae, etc. on the erratic samples, but we cannot rule out such erosional characteristics. We are unable to make any association between the quartzites and the local orthogneiss. While the felsic metamorphic samples could indeed be related to nearby (e.g., charnockite) exposures, we lack sufficient evidence to tie them to any known outcrop, as well as evidence indicating that they were sourced from exposed rock rather than a buried occurrence of similar rock either nearby or further afield in the East Antarctic interior. While ice flow through the Grove Mountains is not in simple, the general flow direction to the north and west narrows the range of nearby nunataks which could act as source regions for these samples. The Grove Mountains are not extremely large; a locally derived erratic could have travelled atop a glacier for <10 km at most before being deposited on Nunatak 1921, but the slow ice velocities and young ages in our lower-elevation samples mean that this could still contribute a portion of the measured nuclide inventory. We hope that specifying that we can say only that they are not derived from the Nunatak from which they were collected is a clear-enough explication of our level of certainty here. We will add additional discussion to the manuscript that explains that we cannot be certain about the provenance of the erratic samples. The main conclusion of our study, that the EAIS at this location was thicker than it is at present at the LGM, is unchanged either way – ice would have to have been thick enough to deposit the samples at their current location regardless of whether the samples were transported supra- or subglacially. We propose to note this uncertainty in our sample descriptions but not change our discussion of our results, as assuming any subaerial exposure during transport would necessarily involve claiming additional ice-sheet thinning since the LGM for which we would lack evidence – as we can

only claim with certainty that the samples are not from Nunatak 1921, assuming subglacial transport is the more-conservative assumption.

Line 114: Indicate the half-life for each nuclide considered so that the reader can assess the duration required for saturation for each.

We agree. The half-lives of these nuclides will be added earlier in the manuscript.

Line 123: Looks as though from Fig 2 that 4 of the erratics were part of bedrock-erratic pairs from the original paper? Why were the corresponding bedrock or erratic samples not analyzed here - at least one or two of them? In those cases, the higher altitude erratics generally indicated longer apparent exposures than the corresponding bedrock. It would have been very useful to have that perspective for this dataset as well. State the reason(s) both members of the pairs were not analyzed to clarify for the reader. See comment on Line 107.

While bedrock-erratic pairs would definitely have further strengthened our understanding of this site, unfortunately, only material from those ten samples were available for this study. We will acknowledge this in the text.

Line 128: Is the lithology actually 'Unknown' or just not recorded and no unprocessed sample remaining? If the latter it's better to just say that. If the latter is not the case then it should be possible to ascertain some sort of rock type for the sample.

Unfortunately, all the erratic samples measured in this study arrived in our possession already crushed. The crushed sample contained quartz, feldspar, and mafic minerals, but no lithology could be positively identified. The "Lithology" has been changed to "Unrecorded", and we will add a note to this table noting the sample mineralogies.

Line 141: 'Li-flux-containing' is awkward wording. Clarify to indicate that the flux had been previously fused and degassed of contaminants and cooled prior to loading.

Crucibles are typically round – these should be referred to as Pt combustion boats.

We will change the wording as suggested.

Line 142: Goehring et al. (2019, Nuclear Instruments and Methods in Physics Research B, 455, 284–292) indicate combustion for 1 hr at 500 C and extraction for 3 hr at 1100 C. However, Nichols and Goehring (2019, Geochronology, 1(1), 43–52) subsequently indicated a procedure of 30 min at 500 C followed by 3 hr at 1100 C – this change in combustion procedure that potentially has implications for the extracted 14C results (e.g., Lifton et al., 2023, Geochronology, 5, 361–375) was presented without supporting experiments. Analyses presented in Balco et al. (2023, The Cryosphere, 17(4), 1787–1801), coeval with the samples from this study, indicate combustions of 30 min at 500 C with extractions of 2 hr at 1100 C, again without experimental data supporting the procedural change. It's thus unclear to me what procedure was actually followed here in these analyses, as the authors cite only Goehring et al. (2019). Given that these procedural changes have the potential to affect the

measured 14C concentrations significantly in either direction, I think it's crucial to clarify what procedures were actually used in the analyses here, and to the extent that they deviate from the procedures in Goehring et al. (2019), to document that the modified procedures had no significant effect on the resulting measurements.

The procedure as laid out in the manuscript specifies 30 min of heating at 500 °C, followed by 3 h at 1,100 °C, consistent with that described in Nichols and Goehring (2019). We will therefore cite Nichols and Goehring (2019) in the methods, and will alter the associated wording.

Line 143: Specify what is meant by 'hot', and describe the quartz for the reader - single crystal, gravel, sand, etc. - provide citation

We will specify that the quartz used is chips at 850 °C, as per Goehring *et al.*, 2019, which we will cite.

Line 144: Specify to what equivalent mass of C the sample is typically diluted to.

The mass of C in the sample is diluted to ~110 µg, which we will add to the text.

Line 146: NOSAMS measures the 14/12 isotope ratios, not the concentrations. Concentrations are derived from those ratios - reference the data tables. Clarify.

We acknowledge that this wording is misleading and will clarify the description and supplementary table.

Line 148: As noted in Balco et al. (2023), and in Greg Balco's review comments, this is not necessarily representative of the system blanks at the time of extraction for this study's samples. The blank data included in Balco et al. (2023) show wide temporal variability - the 3110-atom uncertainty is not representative – the standard deviation of the data is about 10x that, as indicated by Balco's comments. Also indicated by Balco, if this is standard error, that is not appropriate for a non-Gaussian distribution such as that of the blanks. Comparing the sample numbers (TUCNL) from this manuscript's supplement with those from the Balco et al. (2023) supplement, it appears that these data were coeval with some of the Balco et al. data. In my opinion the most defensible approach is to utilize only the blanks from the time of these analyses. See my comments on the Supplemental data for more details.

We agree that the blank correction should be changed. On the advice of other reviewers, this sentence has been changed to "…of 58,000 ± 12,600 atoms was subtracted from the total measured atoms from each sample; this value represents the standard deviation of process blanks run at the TUCNL (Goehring et al., 2019) over the timespan within which samples for this study were measured (July 10, 2021-August 27, 2021)." We will update all sample concentrations and uncertainties in the text and supplement accordingly, but note that the changed values do not impact the conclusions of the paper.

Line 155: Again, see the Balco et al. (2023) 14C supplement. The measurements of CRONUS-A since those included in Goehring et al. (2019) scatter quite a bit more than what is quoted here and show a significant uptick in concentration in the most recent two values in that paper (but which are still well before these analyses). Given that the Goehring et al. (2010) CRONUS-A measurements are used as the basis for the default 14C production rate in the University of Washington v3 online calculator (UWv3 - Balco et al., 2008), and particularly in light of the last two higher concentrations in Balco et al. (2023), the authors should present any subsequent CRONUS-A measurements spanning the time that the Grove mountains samples were run to demonstrate that the production rates assumed are appropriate. If the high concentrations of more recent samples are more representative of results when these samples were run, then a production rate consistent with those should be used, with associated changes in ages and predicted saturation concentrations.

The authors need to be fully transparent about their data and underlying procedures/measurements since the production rates used (and subsequent exposure age implications) depend on the significantly (15-20%) lower measured concentrations of CRONUS-A in Goehring et al. (2019) as compared to most other labs (e.g., Lifton et al, 2023).

Also, the authors should post the code being used to calculate the ages - running the concentrations and site parameters through the UWv3 calculator gives quite different ages for the older ages, and larger uncertainties as well than what are presented here. Any conclusions based on ages thus need to be approached very cautiously as pre-Holocene ages appear less robust than the late Holocene ages.

We will incorporate the additional CRONUS-A measurements performed at the TUCNL and recalculate our ages using the code from the online UWv3 calculator, using the additional production-rate data, and clarify this in the methods section (line 158). Given how production rates are determined by this calculator, adding a small number of additional CRONUS-A measurements is unlikely to significantly alter the calculated ages, but we will redraft any conclusions put into doubt by the recalculated ages. The main point that stands to change based on the ages of the pre-Holocene unsaturated samples is in whether or not samples GR04 and GR18 conclusively postdate MWP-1a – our discussion of this point will change if the new ages reveal that these samples do not. In particular, we will remove lines 201-204 if these samples no longer bound the ice surface following MWP-1a. We will add an additional table to the supplement documenting the CRONUS-A measurements used to determine the production rate for our samples.

Line 156: It is not clear where this claimed 6% uncertainty comes from. The CRONUS-A concentrations in Balco et al. (2023) have a standard deviation of about 8-9% - if one includes the last two from that paper which are much higher than most of the others, it's over 10%. Also, the 6% uncertainty in 14C concentrations is almost certainly concentration-dependent - see Balco et al. (2023) for example with low-concentration samples' % reproducibility - this should be stated as such. CRONUS-A is a high concentration sample - some of these concentrations are high as well but many are much lower, so that uncertainty is likely a minimum value in my opinion. Clarify.

The 6% uncertainty comes from repeated measurements of CRONUS-A material on the TU-CEGS ("at the TUCNL") (see lines 155-157). These show reproducibilities of ~6%, therefore we use this value as the most appropriate error window for our measurements. We adopt this method following its use in other studies published on samples from this lab – see, for example, Nichols *et al.* (2019).

Line 160: A blank of 0.58 ± 0.31 atoms is inconsistent with the text – also see above comments. Make sure the units and values are correct and clearly stated, and reference supplemental data tables for complete information.

We agree and will correct the blank uncertainty to 0.13 in the text and supplementary table referenced in caption.

Line 161: What level of uncertainty is being cited here and throughout? 1 sigma, 2 sigma? An early statement as to what level all uncertainty values represent will take care of them for the whole paper, unless otherwise noted.

We will specify one sigma in the text.

Line 165: 0.02 ± 0.01 ka? 20 years? I don't believe it. Running the data through the UW v3 calculator yields ca. 200 yr. Proofread all numbers in tables and text.

We apologise for this error, and will correct all floating-point typos.

Line 169: Is there field evidence to support the claim of sediment or boulder cover that moved recently? Pretty speculative. Also, is there evidence of drifting snow in the sample vicinity currently? State clearly that this is speculative. Again, it's fine to speculate or have uncertainty just be up front about it clearly. Particularly when you already said there's no evidence of past snow or sediment cover.

We can unfortunately only be speculative as we have no field notes from the researchers who collected these samples. We will acknowledge that this is speculative in the text.

Line 174: Again, what significance (or sigma uncertainty) level is indicated by the error bars? 1 sigma? 2 sigma?

Our uncertainties are presented at one sigma, as above. The levels of the ^{10}Be and ^{26}Al uncertainties from the original studies are not reported, so we assume they are 1 sigma uncertainties.

'Approximately' is a more precise term than 'roughly'

We agree, and will change the wording.

Line 176: As implemented in this figure, the highlighting makes the numbers harder to read. It would be better to make the sample numbers from this study in a different font, color, bold or italic, etc., to set them apart more clearly. You could also make the photo larger and

more legible if you move the legend into panel d, for example, or otherwise rearrange the legend.

The samples measured in this study will be instead be marked with outlined yellow boxes, in larger, bolded font; the photo will be enlarged and legend shortened and widened to accommodate the change.

Line 178: Again, what significance (or sigma uncertainty) level is indicated by the error envelope? 1 sigma? 2 sigma? Based on what production rate? Specify

The error envelope here represents the uncertainty on repeated CRONUS-A measurements at the TUCNL (5.6%, rounded to 6% for reporting in text). We will specify this in the text.

Line 194: 'Lengthily' is not a word. Suggest rephrasing to something like '...but not thick enough to override the summits for a long enough duration to allow the 14C to decay below saturation'

"Lengthily" is a word, and we use it correctly here, but acknowledge that it may nevertheless cause confusion for some readers. We will change the wording to use more-common language.

Line 201: 14.9 ± 1.0 is inconsistent with what is listed in Table 2. And as noted previously, pre-Holocene ages appear to be calculator-dependent, so any correlations with well-defined events such as MWP-1a should be appropriately couched in language emphasizing the uncertainties.

We agree and will change the wording to more-clearly emphasize the uncertainties in the dataset.

Line 214: Delete 'do not show saturation' and instead reference that many of the samples from the original Lilly et al. (2010) paper show evidence of complex exposure over long time frames (and then state the range of minimum exposure durations consistent with each sample)

As suggested, we will refer to the original samples and delete that saturation wording. The oldest sample (GR15) has a minimum $^{10}Be/^{26}Al$ exposure duration greater than the maximum possible for, for example, GR01; thus, no range of minimum exposure durations is consistent with *each* sample. We thus think it most appropriate to only note the evidence long, complex exposure histories for our samples and then reference Table S4, which lists the ^{10}Be and ^{26}Al ages of these sample, rather than listing them in the main text.

Line 216: Suggest 'Our 14C data indicate that this site was covered by ice > ca. 10 m thick for long enough to allow in situ 14C concentrations in the samples to decay.' To what level, though? Measurement background – what is that for this case? Specify. It depends on how long the shielding lasted, and starting from what concentration? What are you assuming here - justify that.

We will add details specifying the assumptions that samples were ^{14}C-saturated prior to cover and that cover was deep and long enough for concentrations to decay to near-background levels; although the degree and magnitude of cover will have varied by elevation (see Fig. 3).

Line 218: What do you mean specifically by 'covered briefly'? Put a value on this - if the summit was covered by over 10 m of ice, how long could it have been covered during the LGM or subsequently and still yield saturated concentrations today?

We will specify the value in the text ($\lesssim$1 kyr, as per Fig. 3). However, note that, the further in the past that cover occurred, the longer it could have lasted while still allowing summit samples to exhibit saturation today.

Line 219: Suggest '… nunataks were progressively re-exposed through the late Holocene.' More succinct.

We agree and will make this change.

Line 223: What is the starting point for these calculations - do you assume all samples were saturated before burial? What effect does starting from a non-saturated concentration have on the predictions here? Describe for the reader.

On the advice of other reviewers, this figure will be revised. The caption will specify that it assumes initial exposure 50 ka, and that assuming saturation at this time does not alter our conclusions. The minimum burial age resulting in saturation decreases by up to 5 kyr in some regions of the graph space, but more than 1 kyr of burial in the last 30 ka still results in unsaturated summit samples.

Line 230: Again, the 14.9 ka age is calculator-dependent. Provide specifics and discuss effects of different calculated ages for that sample (a different production rate and calculation scheme would also likely affect this plot overall)

The actual age is here irrelevant to the question of whether it is possible for an exposure history to plot in this region of the graph space. A sample buried 30 ka can have been buried for at most 30 consecutive kyr; we will remove the age from the text. The figure and its caption will be altered accordingly.

There appear to be three or four slightly different shades of gray on the graph, 'gray-shaded' could be more specific. Might be more obvious for the reader if colors were also used instead of just grayscale.

On the advice of other reviewers, the colours in this figure will be updated, consisting of only one shade of grey for clarity.

Line 231: '…being unsaturated with 14C' is awkwardly worded. Suggest 'having a concentration below saturation for 14C'

We agree and will change the wording as suggested.

'The unshaded portion of the graph…': As above, define where this is - it's not clear from the figure in the PDF - does everything have some degree of shading except for concentrations >7.3e5? Suggest modifying the shading (colors or something more obvious to the reader than really light grays - bigger steps between grayscale values would help). Maybe hatchures instead of black in forbidden region. Describe what is meant by 'uncertainty window' - it should reflect Fig 2b, but does not appear to with this shading scheme. Are you indicating any concentration > 6e5, per Fig 2? Make sure all figures and discussion/descriptions are internally consistent.

Yes, the line between the white and grey regions of the graph now represents the 7.3e5 atom g^{-1} contour – we will state this in the Fig.3 caption.

Line 238/39: Be skeptical of all 10/26 ages in cold-based regimes such as many places in Antarctica. It obviously would be good to have 14C from these datasets if possible now as well to confirm there's no significant inherited inventory (not 10s-100s of ka, obviously, but perhaps a few ka worth). Be up front as to the potential pitfalls of relying on long-lived nuclide chronologies in these environments - it may be that the ages are fine once all recalculated using the same methods/assumptions (e.g., UWv3 calculator, LSDn), but to me there is always going to be some uncertainty in 10/26 ages for LGM and younger time frames in Antarctica, without 14C confirmation. Future 14C work, yes, but point out the potential for even low levels of Be inheritance.

This is a very good point. We will add text noting the potential for inheritance in these regimes: "Direct constraints from cosmogenic 10Be and 26Al show evidence of the ice being thicker near the Antarctic coast (though note the potential for inheritance even here". We will also note the usefulness of ^{14}C in checking for it (see later comment).

The authors should also provide code for calculations through GitHub/Zenodo or similar, as noted previously, so that interested readers can work to reproduce all calculations and results here.

Line 243/44: See above comment - rephrase this paragraph to clarify the possibility of even low levels of inheritance in longer lived nuclides.

We will add a note here that acknowledges this point: "The potential for low levels of Al and Be inheritance in cold, arid regions highlights the usefulness of 14C as a tool for improving ice histories derived from long-lived nuclides.".

Line 248: Prefer 'geomorphic' to 'geomorphological'

We will make this change.

Line 257: Fig 4: 'Present-day ice surface' should be located along the actual line for clarity - above the LGM surfaces on the right side would work. Alternatively, have a legend on the

figure identifying each dashed line separately from the lines themselves. If the present-day ice surface continues below the previous LGM surfaces, then it should continue into the gray area at the bottom of the figure.

We will move the line labels to right of the figure, and extend the line of the present-day ice-surface into the grey area at figure bottom.

Line 261: Is the ice surface slope upstream of the hinge zone constrained by the Prince Charles Mountains data as you have drawn this, or could it just be subparallel to the previous LGM surface upstream of the hinge? Clarify in the caption and discuss further in the text.

We will adjust the ice-surface slope upstream of the hinge zone as suggested, as this would indeed be more realistic. We will also mention in the caption that neither distances nor angles in this figure are to scale.

Line 268: Reword - this is a confusing sentence. Suggest '...EAIS being thicker than previously suggested at the LGM is that...' Any leads and lags should be evaluated with 14C in coast and interior locations to reduce the possibility of minor inheritance in longer-lived nuclides. At least you should qualify any discussion with that possibility so the reader is clear on that.

We will change the wording as suggested. We will also note in the paragraph that we compare ^{14}C data from the Grove Mountains to sites that lack ^{14}C data.

Line 271: Clarify which White et al. (2011) you're citing in each case - should be 2011a and 2011b to differentiate. The authors cite two.

We will clarify these citations.

Line 274: As noted earlier - be careful tying this 15 ka and other 14C ages to other events as especially the older ones are calculator dependent. Make sure all previously published ages in this paper (14C, 10Be, 26Al, etc.) are re-calculated using the same underlying assumptions and algorithms, and make sure to state that that is what has been done. The UW v3 calculator and ICE-D Antarctica is quite useful for that. And specify which production rate datasets you're using for each nuclide.

We agree and will recalculate the ^{10}Be ages presented for the sites mentioned in the text. We will also add a note stating how these were recalculated (using the ICE-D calculation framework built on UW v3 [Balco et al., 2020]).

Line 276: Again, my take on these 10Be ages is that they should be viewed with caution as they can easily skew a bit old - hence the importance of 14C. Any comparison between 14C results and 10/26 results from other sites should be appropriately qualified in the discussion - that there is the potential for 10Be/26Al ages (even post LGM deglaciation ages) to skew older due to an inherited component. I don't think you can get away from that possibility. And

especially since the 14C ages and uncertainties are calculator-dependent to some extent, the authors need to dial back strong correlations.

We agree that 10Be ages should be viewed with caution and tried to reflect that in our wording. For example, the phrasing "…elevation was reached by…" refers to conservative estimates, allowing for the possibility of inheritance increasing the apparent exposure age. We will modify the wording regarding our discussion of the lag time between deglaciation of the Prince Charles Mountains and Grove Mountains in order to avoid implying certainty where there exists uncertainty. We think that future ^{14}C deglaciation ages in the Prince Charles Mountains are unlikely to overturn our conclusion that deglaciation began earliest near the Lambert Glacier grounding zone and propagated up-glacier, however; thus, we will add a sentence to this paragraph explaining that the longer-lived-nuclide ages may decrease as additional data becomes available.

Line 277: Clarify which White et al. (2011) citation – there are 2 such papers cited.

We will clarify.

Line 286: Replace 'thinned' with 'thinned more'

We will replace "subsequent thinned" with "subsequently thinned more".

Line 288: Replace 'thicker-than-at-present' with 'thicker-than-present'

The beginning of this sentence will be changed to "Ice in East Antarctica being thicker at the LGM than at present only within…".

Replace 'hundreds of' with 'hundred'

We will change this wording as suggested.

Overall this sentence is the sort of thing I'm talking about in my earlier comments - yes using the calculator you are employing gives something close to MWP-1A, but UWv3 gives a significantly older age (although overlapping with much larger uncertainty). Just be transparent and up-front about the limitations of the data. This is also why it's good to provide the code so anyone can see what's happening.

We will add text noting the large uncertainties of our dataset. In this case, older ages would still imply ice loss prior to MWP-1a, however, so we suggest retaining the text comparing our results to published literature suggesting only modest East Antarctic contributions to MWP-1a.

Line 298: Suggest starting with 'Our new in situ 14C results provide improved constraints…'

We will modify this sentence similar to that suggested: “Our new in-situ 14C results provide improved constraints on past East Antarctic Ice Sheet thickness at a site ~400 km inland from the present-day coast”.

Line 300: Again, ‘thicker’ and ‘thinner’ are relative terms - say relative to what. Suggest just saying 'thicker than present at the LGM'

We will changed to “…thicker than at present at the LGM, but…”.

Line 302: ‘between thinner ice in the interior and thicker ice at the coast relative to today’ is confusing to me as worded - suggest rewording to clarify that the hinge zone separates the thinner-than-present LGM ice in the interior from the thicker-than-present LGM ice at the coast.

We agreed that this wording is confusing and will change the sentence to “…the ‘hinge zone’ separating the interior ice (which was thinner at the LGM than it is today) from the ice nearer the coast (which was thicker at the LGM than it is today) was…”.

Line 306: Please provide all relevant code used for the calculations in this paper.

SUPPLEMENT

Line 1: Split Table S1 into at least two tables: 14C measurement data and ages and 10/26 measurement data and ages - all ages should be recalibrated from the original paper using UWv3 or publicly available code from the authors.

We will split Table S1 into Tables S1-4, with all ages recalibrated.

For clarity I would suggest combining value and uncertainty columns (i.e., x.xxx±y.yyy) and have them at the same exponent and a common significant figure level (e.g., both should be 10^3 at/g or 10^4 at/g, not one at 10^3 and one at 10^4)

We will amend as suggested.

Use the symbol for permil for the units in the stable C column

We will make this change.

Quartz column: GR01 should read 0.6034. Have all values in this column at 4 decimal places.

We will set all values in this column to four decimal places.

All values in Carbon yield and diluted carbon columns should have one decimal place

We will change all Carbon yield values to one decimal place.

I would recommend 4 decimal places for all scientific notations, and combine value ± uncertainty in a single column with a single exponent common to the entire column. Pay attention to significant figures. Carry as many as possible through each column so the reader can arrive at the same value as the authors. But no need to carry extra, as in the 10/9 ratio column. No way we know those numbers better than the nearest 10-100e-15 values.

We have elected to report each scientific-notation value in the supplement to the number of decimal places that limits the largest value in its column to four digits. The Be and Al data come directly from previously published work, and we report it here exactly as it was originally reported, so that readers can arrive at the same value as the authors.

Line 3: 'Table of sample measurement details' should be "Notes" below the table.

We will adjust "Notes" below tables accordingly.

'0.58 ± 0.31' 1 sigma? 2 sigma? Standard deviation? Seems like something like standard deviation from the blank data in Balco et al. (2023). Present all blank data from the time of the extractions, or reference blanks presented in Balco et al (2023) for that period if that is a complete record of that time period. If that dataset is valid, the blank fluctuated by about a factor of 4 over the period covering the TUCNL numbers represented here.

Our blank subtraction represents the mean and standard deviation of the number of atoms in the blanks run concurrently to our samples. A note will be added to the main text clarifying this.

'Where the 1 sigma…': So, are all measurement uncertainties in this paper quoted as 1 sigma? At any rate the 6% value is on a concentration – that is not applicable to any of the other measurement columns. Clarify that. As noted earlier I'm also dubious about the 6% value since the CRONUS-A data in Balco et al. (2023) has a standard deviation of ca. 8.3%, and if you just look at the subset of measurements from Goehring et al. (2019), that value is ca. 8.7%. And the last two CRONUS-A measurements in Balco et al. (2023) are significantly higher than any of the previous values, and stop quite a bit earlier than the sample numbers here (60-100 samples later than the last of the ones listed in Balco et al.) – standard deviation of the whole dataset is over 10% if those are included. Include any additional CRONUS-A measurements from the time period spanning the measurements here to demonstrate either that the two high values are just scatter significantly outside the mean of the other samples, or that they represent a new mean if there was some sort of procedural change that happened to cause them to be higher from that point onward. In which case the default production rate in UWv3 is incorrect. If there was a procedural change at that time, that should also be clearly described and justified.

The square brackets surrounding the "^{14}C" in that column heading indicate that those values are concentrations. We adopt the 6% value to maintain continuity with previously published literature which used this value (e.g., Nichols *et al.*, 2019; Goehring *et al.*, 2019).

Line 12: If you need to break this 14C table across two pages you should just make a second table with 14C concentrations and ages, but also have a column on the left with the sample IDs for each page. Same for a separate table with 10Be and 26Al measurements – each should have the IDs.

We will split these tables as suggested.

Blanks for the system need to be presented for the time frame of the extractions here. The authors need to demonstrate that they are consistent with what they claim for the long-term blank, which was calculated from earlier data. As noted earlier, in Balco et al. (2023) there were periods in which the blanks deviated from that mean value by quite a bit.

We have chosen to use a blank derived from the mean and standard deviation of the blanks run concurrently to our samples (Table S2).

Define 'effective blank'. This should be listed next to the blank-corrected total 14C columns. Also consider having just a % uncertainty column as with 10Be and 26Al.

Columns will be renamed as with ^{10}Be and ^{26}Al.

Notes for this table should indicate how the ages were calculated, and which production rate dataset was used.

The production-rate dataset (that of Borchers *et al.*, 2016) will be specified.

Line 21: In general it is clearer in tables to have the units entirely below the column heading, in a different size or typeface (bold, italic, etc.), instead of running on to the end of the heading without any typographic differences.

We will separate units by line breaks, and bold column heading.

The 27 in 27Al should be a superscript

"27" will be superscripted.

What sigma level is represented by the uncertainties? Are they treated similarly to the 14C, with comparison to replicate CRONUS-A or another repeat measurement?

Uncertainties in this data are taken directly from the original publications, which use no sigma notation. We thus assume they are 1 sigma values. Be concentrations are measured with respect to NIST SRM-4325 and Al concentrations SRM PRIME-289-0221, respectively.

The [26Al] % uncertainty column does not reflect the uncertainties and measurements in the previous column of atoms/g

This is a typo, which will be fixed.