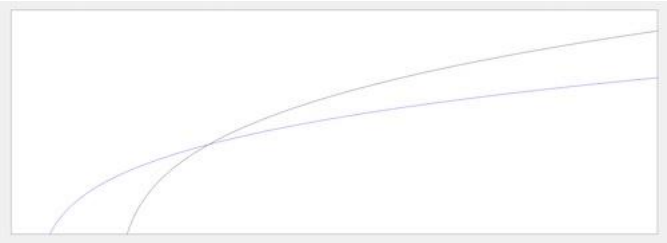
This is a valuable and fairly short and straightforward paper that should be published in approximately its present form. There are a lot of sites in interior Antarctica with Be-10/Al-26 data that are ambiguous as regards the LGM thickness, the only real way to fix this is with C-14 data, and that is what this paper does. The data here are useful and important.

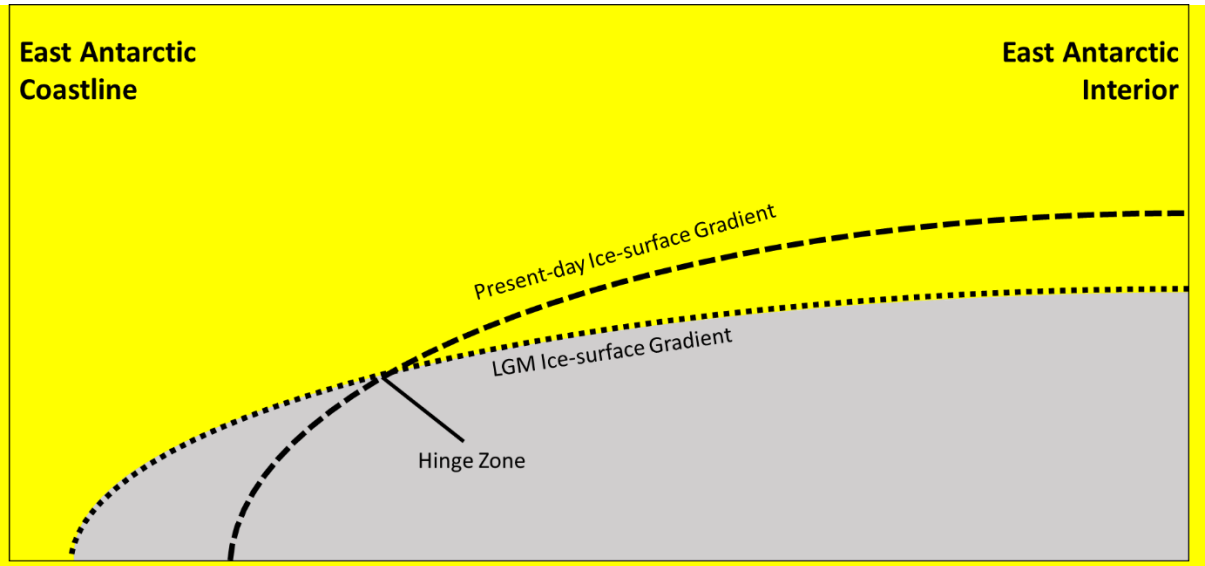
There are a few things that could be improved, as follows.

1. The diagrams in Figures 1 and 4 that are supposed to show ice surface slopes would (and should) be greatly improved by drawing them with parabolic ice sheet profiles. As drawn with straight lines, it doesn't represent the concept of an LGM ice sheet with an extended grounding line but a thinner interior. For example:



At the very least, the 'coastline' and 'interior' labels appear to be in the wrong place on Figure 1c and need to be corrected. But currently the figures are barely acceptable -- they should be redrawn with more realistic profiles so that readers can understand what they are talking about.

We agree with the comments, and will revise the figure using more realistic parabolic profiles as suggested. See as follows:



We will retain the straight lines in Fig. 4 will to aid visibility but will include the following in the caption of Fig. 4 to clarify this: “Ice-sheet profiles have here been depicted as straight lines to aid visibility; the true ice-sheet profiles would curve as in Fig. 1(c)”.

2. The discussion of the blank corrections needs some attention. First, the stated uncertainty of 3000 atoms is extremely small. Is this really the standard deviation of

many process blanks? Second, the statement '..continually updated mean of all process blanks run at TUCNL since 2016...' somewhat conflicts with data in other publications, for example the appendix in this paper:

https://tc.copernicus.org/articles/17/1787/2023/

In particular look at Figure 1 in this:

https://tc.copernicus.org/preprints/tc-2022-172/tc-2022-172-AC2-supplement.pdf

The Tulane blanks shown here may have a mean near 58,000, but they have a standard deviation that is substantially (like 10x) larger than 3000. Furthermore, they are not normally distributed, so it would be inappropriate to use the standard error instead of the standard deviation, or to divide by sqrt(n). This issue doesn't really matter very much for the present paper (except regarding the fairly minor point about whether nonzero cosmogenic C-14 was actually observed in GR15), so the authors can do whatever they want here, but they need to explain what they did in more detail -- as written, the description of the blank correction is not acceptable.

We thank the reviewer for this observation. We will change the supplementary tables to reflect a blank of the mean value of the process blanks run concurrently with our samples and a blank uncertainty of the standard deviation of said. We will also add Table S2 to the supplement, which will present the relevant blank data. We will update the main text of the manuscript with the corrected sample concentrations and uncertainties.

3. The discussion of 'stratigraphic order' in various places (e.g., caption to Fig. 2 near line 182) doesn't really make any sense and needs some work. First, there isn't really any 'stratigraphy' here; what is actually being talked about is just the geometric constraint that lower-elevation samples can't be deglaciated unless higher-elevation samples have already done so. Second, depending on how you define the correct 'order', you could say that GR15 is out of order (if the order is defined by GR04, GR03, GR13, and GR01), or you could say that GR13, GR01, and GR12 are out of order (if the order is defined by GR04, GR03, and GR15), or you could pick some other things to be 'out of order' if you wanted. What the authors are trying to say here is pretty simple -- if the general deglaciation trend is defined by all the samples that are not GR15, then GR15 is out of order -- but the discussion here needs some attention.

We will clarify our reasoning for considering these samples outliers as: “We consider samples GR12 and GR15 outliers, as the trend of decreasing elevation with decreasing age recorded by all of the erratic samples places these two samples out of order”.

4. In figure 3, the sample names should just be put on the figure contours, instead of complicatedly referenced in the caption. Also, labelling the upper right corner 'burial histories inconsistent with post-LGM exposure' is very confusing, because really these are just impossible burial histories: if burial began 15 ka, the duration of burial can't be any longer than 15 ka. It would be much clearer to just mark this 'impossible' or something of that nature, or (preferred) just make the figure border the correct shape to exclude this area entirely.. More seriously, though, it is not clear to me exactly what purpose this figure serves in the text. What point is intended to be made here? The reference to the figure in the text (line 219) suggests that it is supposed to indicate that you can't get anywhere near saturation concentrations unless the burial took place a

long time ago (lower left corner) or was very short (upper left corner). But nowhere is this really explained. In my opinion, this should be edited a bit to (i) move material from caption to text to make it clear what the point of this figure is; (ii) only include contours for the sample(s) that you are actually talking about, and (iii) generally clarify this discussion. Alternatively, this figure makes only a minor contribution to the discussion and can probably be removed entirely without significant loss of understanding.

Finally, I assume this figure assumes initial saturation at the time of burial -- that doesn't appear to be stated anywhere either.

We will update this figure – the figure itself will be simplified, removing the concentration contours and focusing on only the difference between the saturated and non-saturated concentrations. The upper-right corner will be labeled simply “Impossible burial histories”, and we will update the figure caption to the following: “Figure 3: Burial-history contour plot. Contours show 14C concentrations resulting from glacial histories assuming sample 14C concentrations had entirely decayed away by 50 ka as a result of ice cover during the last glacial period, followed by initial exposure 50 ka and one episode of burial under ice assumed to be sufficiently thick to reduce nuclide production in the sampled surface to negligible rates. The black-shaded part of the graph shows impossible histories (i.e., histories that require future burial). The grey-shaded part of the graph shows histories that would result in sample GR21 having a concentration below saturation for 14C. The unshaded portion of the graph shows the uncertainty window of a saturated sample at this latitude and elevation (7.3 x 105 atoms g-1, 72.9088°S, 1,912 m a.s.l.). Only the lesser end of the saturation window is consistent with any significant degree of burial under enough ice to effectively stop production (~10 m); thus, only samples that were buried a long time ago or for a very short duration could show concentrations approaching saturation. Sample GR21 plots off the bottom-left corner of this figure; its mean 14C concentration (7.81E+05 atoms g-1, see Table 1) is thus inconsistent with any episode of burial longer than 1 kyr in the last 50 ka, indicating constant exposure since the LGM. Assuming saturation 50 ka decreases the minimum burial age for saturation by up to 5 kyr but does not alter our conclusions.”.

Also a couple of minor comments:

Abstract, line 18. The 'relatively insensitive' here doesn't really make any sense. Be-10 and Al-26 are perfectly sensitive to short periods of exposure; the difference isn't the 'sensitivity', but the half-lives. It seems like what the authors are trying to say here is more like '...mostly come from surface exposure dating using cosmogenic nuclides with long half-lives, such as Be-10 and Al-26. These often record a cumulative exposure history extending over many glacial-interglacial cycles, rather than reflecting a single period of exposure after the most recent deglaciation....."

We will update the sentence to say “nuclides with long half-lives, which record ice-cover changes on timescales of tens of thousands of years and potentially multiple glacial cycles”.

Line 142. It would probably be helpful to mention that only the gas released at 1100 C is analysed; the 500 C step is for cleaning.

We agree and will added this suggested text to the method.

Line 150. "run mass" doesn't really make sense here. I see that 'run' is supposed to be a passive voice verb, but it reads like a noun. Simply 'sample mass' would be much better.

We will alter the phrasing as suggested.

Near line 204. The statement 'most post-LGM thinning is recorded during the Holocene' does not seem to be very meaningful, because most of the time since deglaciation is also in the Holocene (so this is kind of like saying that most of the numbers less than 10 are also less than 9). What is the point the authors are trying to make here? This could use some clarification.

We will add text clarifying that most of the thinning recorded since the LGM took place during the Holocene, after the earlier, late-Pleistocene stage of deglaciation, when the majority of Antarctic ice loss is recorded.