Author responses to referee comments on "Thickness of the divide and flank of the West Antarctic Ice Sheet through the last deglaciation"

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Referee comments and our responses are shown in black and blue, respectively.

Response to Anonymous Referee #1

Spector et al. report new data from the Weddell Sea sector and from the Ross SeaWeddell Sea ice divide in West Antarctica. The last glacial maximum (LGM) highstand is determined from the elevation of young (post-60 kyr BP) Be-10 and saturated C-14 nuclide concentrations, and modelled scenarios of past ice thickness change at the sites. The authors then interpret these respective ice thickness changes to record a combination of snowfall accumulation and dynamic thinning through the deglaciation. The paper is generally well-written with clear figures and adequate provision of supplementary data. The data are new with robust lab, measurement and calculation procedures, and the approach to determine ice thickness change from these data is novel. The data are worthy of publication and the conclusions would be of interest to the readers of The Cryosphere. However, I have some concerns that need to be addressed, which have potential implications for the reliability of the paper's conclusions.

Thank you for the careful review of the manuscript and helpful comments. We have incorporated them as described below.

Main comments:

1. Treatment of Be-10 and C-14 data

As the authors point out, C-14 has an advantage over Be-10 owing to the shorter decay time, which prevents most inheritance from pre-exposure from being recorded in the sample concentrations. At Pirrit Hills, the Be-10 ages are used to indicate when the local ice surface reached a highstand (e.g. before 18 kyr BP; page 6, lines 19- 21). Yet, it is noted for other studies that the use of C-14 has highlighted that approx. LGM Be-10 ages are not reliable indicators of the highstand (page 7, lines 3-5). The authors do not acknowledge the possibility that their Be-10 ages may similarly record minor inheritance, which would therefore make them minimum age estimates and less reliable highstand indicators.

To address this, we have added the following sentences to the end of the first paragraph of Section 4.1.

> *"Strictly, we cannot rule out the possibility that the youngest ages also record minor prior exposure. However, the similarity of these ages to the youngest ages from the nearby*

Heritage Range (discussed below) suggests that this effect is either absent or minor, amounting to less than 1-2 kyr."

Samples with C-14 concentrations that are indistinguishable from saturation are used to delimit the LGM highstand at Whitmore Mountains (e.g. page 7, lines 8-9). The Nichols et al. TCD paper (which shares authors with this paper) discusses how high C-14 concentrations can result from analytical and geomorphic issues – what is there to say that any of these saturated samples do not suffer from the same issues? Can we trust this approach for delimiting the LGM highstand?

The delimitation of the highstand has implications for the interpretation of ice thickness change (e.g. which modelled scenarios are ruled out).

This comment is similar to a comment by Referee #2. Both comments will be addressed below in the response to Referee #2.

2. Calculation of ice-cover and exposure history scenarios

There is little discussion about the production rate in the method (page 7, line 31 to page 8, line 24). Do these model scenarios account for sample-specific production rate differences? It appears that production is calculated for spallation only, without any mention of production from muons. Muonic production near the surface is important for C-14 (Lupker et al., 2015; Hippe, 2017) – is this accounted for in these calculations?

This is a good point. Yes, the calculation does include ^{14}C production by spallation as well as from muon interactions. During times when the samples are simulated as being buried by ice or firn, we assume that all production ceases. This is obviously not fully realistic. However, because the thickness of overlying ice or firn is unknown, it is not possible to make a realistic estimate of the ^{14}C production rate by muons during times of burial. The effect of this simplification is minor. As discussed in Section 4.2, cover by only a few meters of firn is sufficient to block the majority of the cosmic-ray flux. We have changed the text to indicate that the production rate, P, includes both spallation and muon interactions.

Minor comments:

Page 1, lines 10-11 (abstract): The final sentence would be better as a conclusion/implications. Perhaps something about how the model(s) calibrated with geological observations performed best.

This is also a good point. We have added the following two sentences:

"Most of the models we consider do not match the observed timing and/or magnitude of thickness change at these sites. However, one model performs well at both sites, which

> *may, in part, be due to the fact that it was calibrated with geological observations of ice thickness change from other sites in Antarctica."*

Page 1, lines 22-23: Got a suitable reference for this sentence?

We have added a citation to Cuffey & Patterson (2010).

Page 2, lines 2-4: Is there evidence of atmospheric temperatures being warm enough to induce thinning in this region of Antarctica?

Surface temperature, as reconstructed at the WAIS Divide ice-core site, increased primarily between ~20 and ~15 kyr B.P. (Cuffey et al., 2016). Therefore, thinning due to warming may have either (i) not yet commenced to a significant degree or (ii) begun sometime during the Holocene. The only sites we are aware of that provide information about this question are the nunataks near the margin of lower Reedy Glacier in the southern Transantarctic Mountains. As summarized in Section 5.1 of the manuscript,

> *"Exposure dating at these sites establishes that thinning coincided with deglaciation of a large portion of the Ross Sea 9-7 kyr B.P (Spector et al., 2017; Todd et al., 2010). By ~7-6 kyr B.P., most of the thinning was complete; the ice sheet stood within ~50 of the present-day surface, down from a highstand that was at least ~150 m above present and likely 200-250 m or higher based on the height of depositional limits farther upstream on Reedy Glacier (Todd et al., 2010)."*

The scenario in which thinning due to climatic warming began during the Holocene suggests that subsequent surface lowering would have occurred at these sites following ~7 kyr B.P. Such lowering is not observed. Exposure dating of erratics collected very near to the present-day ice level at these sites demonstrate that the ice surface has remained stable since ~7 kyr B.P. (Spector et al., 2017; Todd et al., 2010), which is consistent with the idea that thinning due to warming of the surface has not yet commenced in West Antarctica.

To clarify the manuscript, we have added the following sentence to the third paragraph of Section 1:

> *"Exposure ages from small nunataks on lower Reedy Glacier demonstrate that ice levels have remained stable since ~7 kyr B.P. (Spector et al., 2017; Todd et al., 2010), suggesting that thinning due to climatic warming following the LGM has not yet commenced to a significant degree in West Antarctica."*

Page 3, lines 19-20: Or, similar to what you mention later, the abundance of debris may relate to debris source (i.e. the ice sheet had access to more debris when near to its highstand).

Variations in debris abundance at different heights could, in part, be due to differences in debris source, as suggested by the reviewer; however, we think the simplest explanation is that debris *abundance varies as a function of the thinning rate and thus the amount of time the ice surface spends at a given height. At the Pirrit Hills, ice-sheet models predict a period of at least a few millennia of limited ice-thickness change when the WAIS is close to its highstand there, followed by more rapid thinning during the deglaciation (Figure 7). Given that thinning rates are generally expected to vary in this way, while there is no a priori expectation regarding the debris source, the most parsimonious explanation is that the abundance of debris is a function of the thinning rate.*

This is essentially analogous to the idea that moraines and grounding-zone wedges accumulate when a glacier snout or ice-sheet margin resides in one position for a period of time. The absence of such features up-valley or shoreward are typically interpreted as an indication of the retreat rate, rather than as evidence that the glacier or ice-sheet ran out of rocks to deposit.

In this case, we have opted to not change the manuscript text.

Page 3, line 33 to page 4, line 1: This is an important point, but the data to support it has not been published. This point should be removed or the data included in the Supplement with suitable description and analysis to reach this conclusion.

The sentence about measurements of long-lived cosmogenic-nuclides on the till has been removed. We have replaced it with the following sentence: "The simplest interpretation is that the patch of weathered till is a remnant of a more extensive ancient deposit that has been largely eroded away."

Page 4, lines 23-27: As above. Is it necessary to include the data if the data "are not relevant to this paper"?

The reference to unpublished data has been removed.

Page 6, lines 6-7: Confusing sentence. Is CRONUS-A from the Whitmore Mountains or the McMurdo Dry Valleys?

We have changed this sentence to read: "CRONUS-A was collected from a slowly-eroding site in the McMurdo Dry Valleys (elevation: 1679 m; distance from Whitmore Mountains: 1650 km) that remained ice free during the LGM."

Page 7, lines 11-14: Overly complex sentence which could be made simpler for the reader.

We appreciate the suggestion, but we think that this sentence is relatively straightforward and works well in the context of the preceding and subsequent sentences.

Note that, in this sentence, we replaced "Because we can rule out that …" with "Because it is very unlikely that…" This was due to the fact that the discussion of unpublished cosmogenic-nuclide data was removed from earlier sections of the manuscript.

Page 7, lines 16-17: Can you provide a reference for the typical West Antarctic snow density?

We have added a reference to Mayewski et al. (2005), who describe the shallow cores recovered as part of the ITASE project.

Page 9, lines 13-15: It is dangerous to use ice sheet models to support interpretations of data, and then use that data to assess the quality of the models. Reconsider using models to interpret the data.

This is a good recommendation. We have removed the references to ice-sheet models in Section 5.1.

Page 9, line 35: "ice" typo.
Fixed

Page 10, lines 9-10: Most of these cited studies do not record grounding-line changes.

These studies record ice thickness changes upstream of the grounding line that were interpreted by the original authors to be related to grounding-line changes. We have added the words "appear to have" to the sentence in question. It now reads "...while grounding-line changes in the Weddell Sea sector appear to have continued into the late Holocene…"

Page 11, lines 16-17: Yes, perhaps this suggests that the grounding line retreated too early, but, as you say previously, thinning is also dependent on changes in the accumulation rate.

This is a good point. We have changed the end of this paragraph to read:

> *"Because thinning at the Pirrit Hills is expected to have been primarily paced by the retreat of grounded ice in the southern Weddell Sea, this suggests that the grounding line retreats too early in all of the thermomechanical ice-sheet model simulations. Premature thinning in the models could also be caused by underestimating the magnitude and/or rapidity of the deglacial rise in snowfall, which as discussed by Hein et al. (2016) may have delayed the onset of thinning."*

Page 12, lines 1-3 and lines 15-16: Why not compare to other models calibrated with geological observations (e.g. Whitehouse et al., 2012; Briggs et al., 2014)? This would strengthen the conclusion that the best performing models are calibrated with geological observations.

This is also a good point. We have obtained model output for Whitehouse et al. (2012) for the Pirrit Hills and the Whitmore Mountains, and we have included this model in the evaluation.

Regarding the model of Briggs et al. (2014), we were only able to obtain model output of ice-surface elevation, not ice thickness, which is the variable we use in the analysis.

Figure 7: You should explain what the vertical dashed line represents in plot c.

The figure caption now explains what the vertical dashed line represents, along with the gray areas.

Response to Anonymous Referee #2

This well-written and well-considered manuscript contrasts LGM ice thickness changes from the centre of the West Antarctic Ice Sheet with those closer to its margins. The authors reveal new cosmogenic nuclide-derived constraints on ice sheet thickness at two unstudied locations (Pirrit Hills and Whitmore Mtns) derived through 10Be surface exposure dating and novel interpretations of in situ 14C data. The authors find support for a post-LGM highstand in the centre of the ice sheet as a likely consequence of increased precipitation during deglaciation, followed by dynamic thinning that propagated upstream from the ice sheet margin. The idea is not new, and there are other field studies from interior regions of the WAIS (Steig et al., 2001; Ackert et al., 1999), the Ross Sea (Todd et al., 2010; Hall et al., 2015) and Weddell Sea (Hein et al., 2016) and that find similar evidence for this balance between oceanic and atmospheric controls on ice sheet behaviour during deglaciation. The manuscript adds further support for this idea, obtained through novel application and interpretation of in situ 14C from a rare, interior WAIS location. The modelling of in situ 14C exposure histories is novel and will be of wide interest to those who work with cosmogenic nuclide data. The conclusions are supported by the data, and should be of wide interest to the readers of the Cryosphere. I recommend publication after addressing some minor points.

Thank you for the thorough review and constructive comments. We have addressed each comment as described below.

Specific comments:

- The mix of saturated and finite 14C ages on Mt. Seelig is perplexing and requires further discussion. The saturated 14C ages are used to “eliminate” the possibility that the WAIS was thicker than 190m (elevation of the lowest saturated age) during the LGM, without discussion on the reliability of those ages. It is worth discussing the reliability of those saturated ages given the recent study by Nichols et al. (2019), who repeated measurements of 14C in samples that were previously reported as saturated (Balco et al., 2016) and got finite ages. As the data come from the same lab, further discussion is warranted.

As the reviewer mentions, some replicate ^{14}C measurements on Antarctic samples prepared at Tulane University scatter significantly more than expected from their analytical uncertainty alone. This problem, however, does not appear to plague samples at random, but rather it has only been observed on glacial erratics collected from the Schmidt Hills (Nichols et al., 2019; see figure below) and on samples from Hatherton/Darwin Glaciers (unpublished; not shown in figure). Other replicate analyses on Antarctic samples prepared at Tulane University do not exhibit this problem (see figure). For example, ^{14}C concentrations measured on the Antarctic rock standard, CRONUS-A, scatter by 5.2% (n=13; not shown in figure; Goehring et al., 2019).

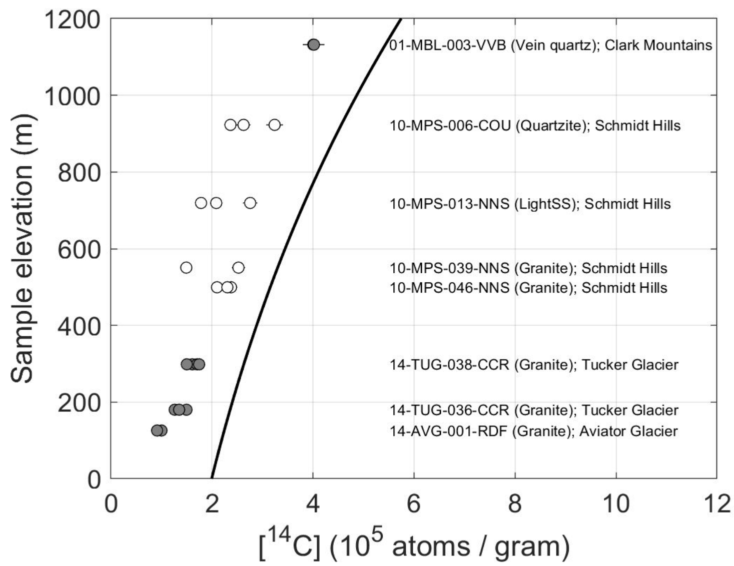

Figure 1. ^{14}C concentration plotted against sample elevation for samples prepared at Tulane U. on which replicates have been measured.The black line shows predicted ^{14}C saturation concentrations, which are a function of elevation. White circles represent glacial erratics from the Schmidt Hills; gray circles represent bedrock samples from various sites in Antarctica.

Because of the possibility of contamination from modern carbon, it is more likely to measure an erroneously high ^{14}C concentration than an erroneously low concentration. Experiments with different sample preparation procedures by Nichols & Goehring (2019) suggest that, in some cases, excess scatter is due to contamination by modern carbon. The excess scatter exhibited by replicate analyses on samples from Hatherton/Darwin Glacier (mentioned above) is probably due to insufficient removal of organic surfactants used for quartz purification (Nichols & Goehring, 2019). Although samples from the Whitmore Mountains were exposed to these same surfactants, Nichols & Goehring (2019) have shown that the acid-etching procedure we followed sufficiently removes organic contaminants. Although samples from the Schmidt Hills (figure) were not exposed to surfactants, the replicates that produced the lowest ^{14}C concentrations were measured on the most aggressively-etched quartz (similar to the treatment received by the Whitmore Mtns. samples). In some cases, contamination by modern carbon is expected to produce ^{14}C concentrations higher than predicted saturation concentrations. This is not observed for samples from the Whitmore Mountains. Rather, three samples spanning ~300 m of elevation have concentrations indistinguishable from saturation. To summarize, while we cannot *prove that the ^{14}C measurements from the Whitmore Mountains are accurate, for the reasons discussed above, we have no reason to believe that there is unrecognized analytical error.*

We feel that discussion of this issue is beyond the scope of the manuscript, and we rely on the open discussion format of this journal to disseminate this information.

References:
Nichols, K. A. and Goehring, B. M.: Isolation of quartz for cosmogenic in situ 14C analysis, Geochronology Discuss., https://doi.org/10.5194/gchron-2019-7, in review, 2019.

Nichols, K. A., Goehring, B. M., Balco, G., Johnson, J. S., Hein, A. A., and Todd, C.: New Last Glacial Maximum Ice Thickness constraints for the Weddell Sea sector, Antarctica, The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-64, in review, 2019.

The authors consider the proximity to local ice caps and snowfields to explain the finite 14C ages at higher altitudes, with distances of 5-20m reported. However, it is not discussed whether the saturated samples are located further away from the snowfields? Is there any sort of correlation here that could be used to support this argument?

We did not measure the distance from samples to the nearest snowfields while in the field, but from our photographs and notes there is no obvious relationship between snowfield proximity and whether a sample is ^{14}C saturated or not. To clarify the manuscript, we have added the following paragraph to the end of Section 4.2.

> *"While snow cover is the only simple explanation for ^{14}C concentrations below saturation in samples collected above 190 m, there is not an obvious relationship between proximity to present-day snowfields and whether a sample is ^{14}C saturated or not. The only exception to this is the highest sample from Mt. Seelig, which has a ^{14}C concentration below saturation and was collected from a very small outcrop (few square meters) that protrudes through the margin of the summit ice cap (Fig. 4c). All other samples from Mt. Seelig are estimated to have been collected within approximately 5-20 m of snowfields. How susceptible each sample site is to snow cover is likely related to local wind effects near the cliff edge, which are difficult to predict. Therefore the absence of a clear relationship between snowfield proximity and whether a sample is ^{14}C saturated is not surprising."*

- The authors cite unpublished Al/Be/Ne data to support their argument for low erosion rates and long exposure of bedrock surfaces in the Whitmore Mtns. The authors should either publish this data within this manuscript, or remove reference to them.

The reference to unpublished data has been removed.

- Comment on Section 5.1: the argument for simple monotonic post-LGM thinning of ice sheet margins despite a deglacial increase in snowfall is likely correct, but is overly simplified. In the Heritage Range, the competition between increased snowfall and dynamic thinning is argued to explain the delayed deglaciation at that site, which initiated in earnest only after ~10 ka when dynamic thinning began to outpace deglacial increases in snowfall (Hein et al., 2016). If correct, the influence of increased snowfall is more widespread than implied (i.e., it's influence extends beyond the divide), even though it may not have caused local thickening. Similar competition between processes has been suggested to explain ice histories in the Ross Sea sector (Hall et al., 2015; Todd et al., 2010).

At the Pirrit Hills, the WAIS does appear to have thinned monotonically. The highstand was reached by the time that the accumulation rate in West Antarctica, as recorded at the WAIS Divide ice core site, began to increase from it's ice-age low (see Figure 5a of manuscript). This implies that thickening did not occur at the Pirrit Hills in response the accumulation-rate rise. As suggested by the reviewer, it is certainly possible that increased snowfall rates delayed the onset of thinning at the Pirrit Hills. To account for this, we have added the following sentence to the first paragraph of Section 5.1: "As discussed by Hein et al. (2016) in regard to the Heritage Range, the onset of thinning at the Pirrit Hills (and other sites) may have been delayed by the deglacial snowfall increase." We have also added a sentence to this effect to the end of the third paragraph of Section 5.2.

Technical corrections:

- What is the distance of Pirrit Hills from the grounding line and the divide?

The Pirrit Hills are ~200 km from both the grounding line and from the divide. The first sentence of Section 2.1 has been modified to convey this.

-p3 line 30- statement that no glacially-transported cobbles found contrasts with next statement on discovery of indurated till. Rewrite to clarify.

We appreciate the suggestion, but the first sentence in question does not state that no glacially-transported cobbles were found. Rather, it states that "we found no glacially-transported cobbles or boulders perched on bedrock surfaces." This is an important difference. One of the goals of these sentences is to contrast debris "perched on bedrock surfaces" with "indurated and weathered till". We have opted to not change the manuscript text.

- P4L8 – replace "remove" with "minimize"

Changed.

-P4L15 – Perhaps include in Table the distance of each sample to the present snow/ice cap and reference here.

See response to previous comment on snowfield proximity.

-P4L19 –For clarity, state from where the ice margin elevation was determined at each site (given this can vary significantly around a nunatak).

We have changed the text to indicate that sample heights are measured relative to the blue-ice areas that are at the base of each ridge.

- P8L10 – can't use unpublished data to support your argument.

The reference to unpublished data has been removed.

- P9L35 – spelling of "ice"

Fixed.

Additional changes

In the abstract, we replaced the sentence "At the Pirrit Hills, ice reached a highstand ~320 m above present during the last glacial period." with "At the Pirrit Hills, evidence of glacial-stage ice cover extends ~320 m above the present ice surface." The purpose of this is because we cannot fully rule out the possibility that the highstand during the last glacial period was not somewhat higher than the highest deposits that we found.

Section 2.2 was accidentally labeled 2.1.1. This has been fixed.