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

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The Editors comments and our responses are shown in blue and balck, respectively. At the end of this document we also provide point-by-point documentation of additional, unsolicited changes we have made to improve the clarity of the manuscript.

Responses to the Editor

I would like to thank both reviewers for their detailed and constructive comments on this manuscript and also the authors for posting their response to the reviewers' comments.

Several issues are not fully resolved by the authors' response to the reviewers' comments. I note these below, along with a few additional points that the authors may want to consider as they prepare a revised version of their manuscript.

Reviewer 1 raises some concerns that have potential implications for the reliability of the paper's conclusions. I share some of these concerns, and in particular I feel that you do not always provide a full discussion of the uncertainties on the data or explore alternative explanations for the evidence. Some of the assumptions you make when interpreting the data have significant implications for the exposure/burial history that you subsequently infer. I encourage you to acknowledge the limitations of the data more thoroughly and discuss the viability of alternative scenarios where relevant.

In general, both reviews are positive, and they highlight the novelty and importance of this study. I therefore encourage you to submit a revised manuscript that addresses the points mentioned below and in the individual reviews.

Kind regards,

Pippa Whitehouse

We thank Pippa Whitehouse for a thorough review of the manuscript and for providing constructive comments.

Specific points

Both reviewers comment on the reliability of the saturated ages reported on page 7, lines 7-9. You provide an extensive discussion on this point in your response to the reviewers' comments, but you also state that you do not plan to include any additional information in the manuscript. Given that both reviewers commented on this, and given the importance of these ages in determining the ice history at Mt Seelig, I think it is important to include a brief discussion on the reliability of the saturated ages in the main text - if only for the benefit of those readers who are unfamiliar with the issues associated with analysing and interpreting such samples. As you note, the full details can remain in the review documents.

We agree that this issue should be mentioned in the manuscript. Previously we had found it challenging to figure out how and where to discuss it. We have now added a paragraph to Section 3.3 discussing this issue, and we refer readers to the interactive online discussion of this article for a more detailed discussion of the issue.

In two places Reviewer 2 mentions a number of earlier articles that discuss the competing roles of precipitation change and grounding line dynamics in controlling post-glacial Antarctic change. Of the articles mentioned by the reviewer, all are already cited in the original version of your manuscript except Hall et al. (2015, Nature Geoscience). I encourage you to consider including a reference to this highly relevant article in the revised version of your manuscript.

We have added reference to Hall et al. (2015) in Section 5.1.

Reviewer 1 queried whether there is evidence of atmospheric temperatures being warm enough to induce thinning across West Antarctica. Much of your response (and the text in the manuscript) appears to rely on the assumption that there is a delay of 10-30 kyr between atmospheric warming and ice thinning. You include a reference to an entire textbook, which makes it difficult to determine the precise basis for this assumption, but the text on line 4 (page 2) suggests that it relates to the time required for surface warming to have an impact on conditions at the base of the ice sheet. However, on lines 2-3 (page 2) you also mention the process by which an increase in ice temperature (at any depth) will change the rheology of the ice, thus allowing it to deform and flow more easily. The time lag for this second process is presumably much shorter, perhaps negating your assumption that there must be a delay of at least 10 kyr between warming and thinning? And in fact, I don't think the reviewer is even asking whether warming-induced thinning has commenced, but rather whether the deglacial increase in atmospheric temperature was sufficient to trigger thinning by one of the processes described above. Please address this second point.

We agree that referencing an entire textbook was not particularly useful. In each case we were referring to Chapter 11.4.2 of Cuffey & Patterson (2010), which discusses the response of ice sheets at the end of an ice age. We have changed these citations to include the specific chapter.

As you mention, warming the ice at any depth will change its rheology and allow it to flow faster. However, because the bulk of ice deformation occurs near the base of the ice sheet, most of the effect of surface warming on ice-sheet thickness does not occur until that warming has propagated to near the base of the ice sheet. As discussed in Cuffey & Patterson (2010, Ch. 11.4.2), this is expected to require roughly 10 to 30 kyr.

In the previous draft of the manuscript, we had mentioned this thinning mechanism in the introduction, but then not discussed it further in the paper. This is because, given the time delay of this mechanism, along with the fact that surface temperatures remained relatively low until ~15 kyr B.P. (Cuffey et al., 2016, PNAS), it appeared unlikely that this mechanism could have significantly contributed to thinning in West Antarctica. In the revised manuscript, we acknowledge the possibility that some thinning by this mechanism could have occurred in the late Holocene. Mention of this now appears in Section 1 and Section 5.1. This does not actually affect our conclusions. At the Pirrit Hills, any thinning by this mechanism would likely have occurred only after the majority of thinning to the modern ice level was complete. At the Whitmore Mountains, the possibility of late Holocene thinning by this mechanism does not change the fact that, as stated in the text, “thinning to the modern ice level at Mt. Seelig could not have occurred before 7 kyr ago (i.e. before modern ice levels were reached on lower Reedy Glacier)”.

Opening sentence of section 5.1: “…despite the deglacial increase in snowfall…” It is not clear what evidence you are drawing on to support this statement, but elsewhere in the manuscript I note that you refer to the WAIS Divide ice core when discussing accumulation change across West Antarctica. The Pirrit Hills are in a different catchment to the WAIS Divide ice core (figure 1 of your manuscript) and hence they may have experienced a different snowfall history to that at WAIS Divide (page 10, line 24 of your manuscript). The statement at the start of section 5.1 therefore requires additional justification if you wish to use accumulation change at WAIS Divide as a proxy for accumulation change at the Pirrit Hills. If you are drawing on alternative evidence to support the statement about accumulation change at the Pirrit Hills then please make this clear. In light of my comments, please also check the robustness of the statement in the conclusions that refers to accumulation rates at the Pirrit Hills.

We agree that our justification for this sentence was not obvious. We had discussed this issue regarding the Whitmore Mountains, and the reasoning is the same for the Pirrit Hills. For both sites, the magnitude of accumulation-rate changes may not have been the same as at WAIS Divide, but the timing of changes were probably similar because (i) all three sites are fed by storms originating in the Amundsen Sea low, and (ii) the accumulation rate increased considerably in both East and West Antarctica at this time. To address this issue, we have moved this discussion (for both sites) to the beginning of Section 5.1.

On page 9, you draw on evidence from sites across West Antarctica to support your inference that ice was previously thicker in the Whitmore Mountains. Considering the likely flowlines of the

ice sheet during the last deglacial period, it is not clear to me that ice thickness changes at Mt Waesche (page 9, line 16) should necessarily be similar to those at the Whitmore Mountains. Similarly, one could envisage a scenario whereby ice was thicker than present at Byrd Station during the LGM (page 9, line 19) but not at the divide upstream of this site. It would be useful if you could include a statement about the degree to which ice thickness changes at Mt Waesche and Byrd Station can be expected to co-vary with ice thickness at the Whitmore Mountains (as you do when discussing evidence from the Ohio Range).

We agree that ice-thickness changes at Mt. Waesche are difficult to connect to changes at the Whitmore Mountains, and we have removed reference to Mt. Waesche here. Reviewer 1 strongly suggested that we not discuss the behavior of ice-sheet models in this section because later in the paper we evaluate those same models. We agreed that this was a good point, and we complied with the recommendation. Therefore, with regard to the evidence of thickness changes from Byrd Station, it is difficult to provide a quantitative statement about the degree to which thickness changes are expected to co-vary at this site and at the Whitmore Mountains, as you recommended.

Page 9, line 33: “Thinning to the modern ice level at Mt. Seelig therefore could not have occurred before 7 kyr ago”. To improve the clarity of your argument, please be more explicit about which of the constraints mentioned in the previous paragraph you are drawing on to make this quantified statement.

We agree that this sentence was not clear. It now reads “Thinning to the modern ice level at Mt. Seelig therefore could not have occurred before 7 kyr ago (i.e., before modern ice levels were reached on lower Reedy Glacier).”

Page 10, line 10: could the ice have been thicker than present for a brief period during the LGM? i.e. could it be that the samples were not completely saturated at the beginning of the ~15ka burial period?

This is a good question. The model we use to investigate the possible histories of exposure and ice cover (equation 1) assumes that samples were initially ^{14}C saturated. Relaxing this assumption actually restricts the set of exposure and ice-cover histories that are permitted by the observations. So, if the two lowest elevation samples from Mt. Seelig were not initially saturated, then the onset and duration of ice cover would be later and more brief, respectively, than implied by Figure 6a. To give an example, if we assume that the samples began with C-14 concentrations 5% below saturation, it implies that the highstand was reached within the past ~13 kyr as opposed to the past ~15 kyr. This would only strengthen our findings that ice cover was relatively recent and brief.

In the previous draft of the manuscript, the effect of this assumption (i.e., initial saturation) on our findings was not explained well. We have rewritten parts of Section 4.3 because we did not think that our explanations of the C-14 constraints and of Figure 6a were sufficiently clear. In

addition, we have also added a paragraph discussing the assumption of initial C-14 saturation and what effect relaxing this assumption would have. This issue is also discussed some in Section 5.1.

Page 10, line 31: ICE-6G is not really a 'model of glacial isostatic adjustment'; it is an ice history model in the sense you are using it

We now refer to ICE-6G as a model of ice-sheet history.

Please include latitude and longitude labels on figure 1

This issue was commented on previously, and we did add labels to the figure at that time. The labels are purposefully subtle, so as to not distract from the rest of the figure, but we believe that they are sufficiently visible and legible.

Additional Changes

Section 1: It was previously somewhat vague whether the second paragraph of the Introduction was referring to ice sheets generally or to the WAIS specifically. We have reworded parts of this paragraph to make it clear that we are talking about the behavior of the WAIS at the end of the last ice age. Other changes to the Introduction are either (i) discussed above in our response to the Editor or (ii) largely stylistic.

Section 2.1: We made a few very minor stylistic changes to this section.

Section 3.3: We have made the following changes to this section:
Paragraph 1

- We clarified the description of our analytical methods for extracting Be from quartz.
- We expanded the discussion about sample contamination by beryl or other Be-bearing minerals. The text now explicitly states that we calculated ^{10}Be concentrations using ICP-OES determinations of total Be rather than the amount of Be added as carrier.
- We expanded the description of our ^{10}Be blanks and our blank corrections.

Paragraph 2

- We have provided a more full account of the scatter and bias in Be isotope ratio measurements from LLNL, and how we have increased the uncertainty of our sample Be isotope ratios to account for this.

Section 4.1: We made minor stylistic changes to the third paragraph of this section. In the fourth paragraph, we have changed the first and last sentences so that the paragraph now describes exposure-dating results in the Weddell Sea sector before drawing inferences about them.

Section 4.2: We made minor changes to this section to (i) make it easier to read and (ii) better explain the relationship between sample sites and present-day snowfields.

Section 4.3: Previously, the explanation of the model we use to interpret the ^{14}C results was somewhat confusing. We have rewritten much of Section 4.3, and we believe that it is now much clearer, and that it makes Figure 6a easier to understand as well.

Section 5.1: Various changes to this section are described above in our response to the Editor. Additionally, we have combined the last two paragraphs of this section, which makes it easier to read.

Section 5.2: We have made a minor stylistic change toward the end of this section.