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

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.
- 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.
- 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.

- 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.

- 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).

- 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.

- 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?

- 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

- Please include latitude and longitude labels on figure 1