

Reviewer 3

General comments

This is a great paper, and the authors are to be commended! They have written the first paper to convincingly demonstrate the utility of the RAID system. They have also provided a really impressive and careful analysis of the timescale for the Sherman Island site, which will prove useful as a climate record, and strongly supports the idea of drilling a complete "normal" ice core record at this site.

Thank you for your kind words and your assessment that both the RAID system is convincing and that the age scale for the Sherman Island RAID data is adequate. I am happy to hear that you agree Sherman Island would make a good future drill site!

Technical comments

I have a few criticisms that I suggest should be considered in a revised paper.

First, a few minor grammar/style things:

Please define abbreviations before using them. In the abstract, "LIG" is used, but not defined.

LIG abbreviation now defined in abstract, and additional missing definitions elsewhere in the text have been identified and defined

The terms SI:RAID and SI:CORE are used but not defined. At first, I thought SI might mean Supplementary Information!

Thank you for pointing this out, both terms (SI:RAID and SI:Core) have now been defined in the text when they are introduced. I will steer clear of "SI" on its own for this reason.

In the abstract, it is stated that the new record is the "oldest, continuous, ice-derived palaeoclimate records for the coastal Amundsen-Bellingshausen Sea sectors." This is a stretch. WAIS Divide gets its snow accumulation from the Amundsen Bellingshausen Seas, and probably reflects the Amundsen-Bellingshausen Sea sectors almost as well as the Sherman Island site does. Instead, I would say "oldest, continuous, ice-derived palaeoclimate records *IN* the coastal Amundsen-Bellingshausen Sea sector.

Thank you, you are right that is an important clarification. We have made the change you suggest.

On Page 1, where Joughin and Alley 2011 are cited, I suggest a more primary source, rather than a review paper.

*Thank you, we have provided a reference instead to Joughin et al, 2014
<https://doi.org/10.1126/science.1249055>*

Similarly, on page 2, I don't think Deconto and Pollard is the best citation for the statement that the WAIS may have collapsed in the past. It would be better to cite evidence for this from sea level records or other papers that provide data suggestive of this. Deconto and Pollard is a modeling paper that *assumes* WAIS has collapsed. It is not a source of evidence that it did.

Thank you, this is a good point. The Deconto and Pollard paper is useful for helping to

set the context of why investigating the WAIS during the LIG further using empirical (ice core) evidence is important, but it is not the correct citation for the statement here. I have instead referred to Dutton et al 2015. I have amended the subsequent sentence to "WACSWAIN... aims to use ice core records to investigate the WAIS during the LIG, to supplement existing modelling studies (e.g. Deconto and Pollard)"

Major comments

On page 4, the model of Martín et al. (2015) is briefly described. But I would like to understand the motivation for using this model, rather than simply models such as that of Dansgaard-Johnsen, which is commonly used. Furthermore, elsewhere in the paper other models (e.g. Liboutry) are used, so this is confusing. Also, it is stated that horizontal advection is neglected, but nothing is said about whether this is a reasonable assumption for this site.

Thank you for your helpful comment. As I explain below, we have some work to do with re-visiting the model simulations in general and improving the explanation of the modelling in the text, so thank you for pointing out this confusion specifically. We will address this and provide further description and explanation for the use of this model.

On page 5, I find the Caption for the graph to be confusing. It is stated that divide flow is shown in red, flank flow in blue, and that green shows the optimized model. This implies that the best model is inconsistent with both divide flow and flank flow. But in the text, it's clear that this is NOT the difference between the curves. The difference is that assumptions made about the snow accumulation rate were probably too simple. That's a nice result, but it is not at all obvious. As far as I can tell, the green curve actually uses divide flow, but allows for variable accumulation. (If I am wrong, then I am confused by the text, and some work is needed to make it clear what has been done). I strongly suggest renaming the curves, with something like "divide flow with Dome C-proportional accumulation", "flank flow with Dome C-proportional" and "divide flow (??) and optimized accumulation".

I assume this refers to Figure 5? I agree that the explanation in the text needs some more work to improve the clarity. We are currently working to improve the model simulations (partly as per your below suggestion and as a result of the other reviewers' comments and subsequent discussion), and will address this as part of the revised manuscript. Thank you very much for your clear description of what is confusing about the figure and the accompanying text.

I have one significant criticism. Why is Dome C used for accumulation rate in the first place? It is about as far from Sherman Island as possible. Why not use WAIS Divide? Even South Pole would be better! The authors show clearly that the assumption that the accumulation history is proportional to that at Dome C does not work -- if it did, then the "optimized" timescale would be very similar to the "divide" timescales. There is a missed opportunity here. The authors do not show what accumulation history goes with the optimized timescale (they should show that!). It would be very interesting to know whether that history agrees better with Dome C or with WAIS Divide or South Pole.

I would very much like to see the "divide" and "flank" timescales re-calculated, using WAIS Divide (and perhaps also South Pole), and some commentary on which one provides the closest fit to the optimized timescale.

Thank you for this assessment. I will address these two comments together as they are concerning the same criticism. Another reviewer also raised concerns about the use of the Dome C accumulation rate as the reference. We will do another set of model simulations using the WAIS-Divide accumulation rate record. We agree that this is an important comparison to make because the WAIS Divide and Dome C climates are so different. We don't expect that this will significantly alter the results, because at both sites for the last ~1000 years (the approximate age range covered by the Sherman Island RAID samples) there has not been a large change in accumulation rate, and furthermore the model smooths the accumulation record. However, this should also be made more clear in the text, and is a parameter which in turn we could also investigate further. You are correct that we need to be more clear in the text in our explanation of the model runs presented and we thank you for your constructive comments.