

Optimal site selection for a high resolution ice core record in East Antarctica.

T. Vance et al

Point-by-Point reply to Referee comments.

Anonymous Referee #1

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Review- Optimal site selection for a high resolution ice core record in East Antarctica

This is an interesting and beneficial reference paper for the ice core community. It utilizes a range of parameters to select the optimal location for ice core drilling. It's good to demonstrate how much research goes into selecting an ice core site and the importance of setting out clear objectives.

I have only a few minor comments. In the discussion regarding IPO I'm not sure I feel confident about the use of ERA-20C. I appreciate that you would like to use as long a record as possible, and it's good that you've included some caveats, but based on the two figures shown (Fig 6 and supplementary fig), I'm not sure that the two data sets are producing similar composites. Rather than introduce doubt would it not be better to stick to ERA-int and just add the caveat that the record is short? If you're trying to demonstrate the difference between positive and negative phases of IPO, and how this will influence the proposed ice core sites, then the supplementary figure looks more convincing to me.

We appreciate the referee's time to consider this data issue. The lack of data at southern high latitudes is something anyone interested in decadal variability in this part of the world laments regularly!

At the outset of this manuscript, we also thought it seemed best to present the ERA-Interim data as part of the main text, and to use the ERA 20C reanalysis for confirmation of the previous negative IPO cycle (and that we would put this analysis in the supplementary information). However, we had a couple of major problems with this, which we failed to discuss clearly in the manuscript. (They are now discussed, see below).

The first problem, is that if we used ERA-Interim we would only sample negative years that are at the beginning of the current negative phase. In contrast, by using the 20C reanalysis, while we lose a great amount of data prior to 1979, what we do gain is a full negative cycle. That is, we sample negative years that span the full range of the negative phase, either side of the minima. Its is possible that the phase change of IPO (either minima or maxima) matters more than whether years are arbitrarily below or above a threshold. Because of this, the ERA-Interim analysis probably only samples negative years from when the IPO is trending more negative (although it is hard to completely establish this due to end effects), and not from when it is still negative, but trending positive. The spatial pattern between these two 'types' of negative years may be quite different.

The second problem is that we have no way of knowing what the imprint of warming and high GHG's has on the current negative phase, and this may have quite an effect that is not evident, or not as evident from the earlier 20th century negative phase.

We would also like to note that there is no reason why two different negative phases should have exactly the same spatial pattern, similar to the fact that different El Niños rarely have the same anomaly patterns, so we would not necessarily expect the two composites presented to look exactly the same, but rather we would expect/hope that they would have similar characteristics. For the context of this manuscript, the three possible sites all sit close to the boundary between anomalies that flip sign from positive to negative in both composites – which is great for our purposes.

Because of this, we wanted to use ERA 20C in the main text but with the caveat that data sparsity is a problem, and present the same analysis using ERA-Interim as supplementary material. Furthermore, as we are interested only in broad spatial patterns at this stage, we feel that using ERA 20C was appropriate for the main text. We would like to keep this format, as we think it is important to not rely heavily on 1979-present when investigating processes that are decadal in nature. Our point is, that both reanalyses lack data for different reasons, and both should be treated with caution when investigating decadal anomalies.

We have re-written two sections and discussed this more clearly, and would prefer to keep the current format for the reasons outlined above. **The re-written sections are Section 3.5 (lines 205-219 of revised pdf file) and section 4.6 (lines 380-384 of revised pdf file).**

Minor comments:

Page 5076, Line 20 – I felt this needs an introductory sentence “Filling in a data sparse regions with new ice core records will contribute to our understanding of regional and global scale climate processes but the location of ice core sites requires careful site selection”. Then lead onto constraints.

Done, as suggested. New text now reads:

Filling in data sparse regions with new ice core records will contribute to our understanding of regional and global scale climate processes, but the location of ice core sites requires careful site selection. Unfortunately, ice core site selection is not governed solely by the local climate response and its preservation in the ice core record. Rather, site selection is primarily restricted by both glaciological and logistical constraints, with optimal site positioning from a climate processes perspective being constrained to this restricted domain. **Lines 38-43 of revised pdf**

Page 5077, line 12 – Sentence structure. “These include three sites from the WIAS.?”

Restructured. New text now reads:

“Three sites that do largely meet these criteria are the...” **Lines 55-61 of revised pdf**

Line 24 – add timescale eg . . .over the last 2000 years?

Added as suggested. **Line 67 of revised pdf**

Anonymous Referee #2

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This paper carefully goes through the different aspects to consider when planning to drill an ice core. As far as I know no such systematic study, including a literature review, has been done before so this is a very useful and valuable paper! In addition to presenting a systematic method for selection of new ice core sites the paper is really well-written and has a good structure! I suggest to accept this with only some very minor editorial changes.

Minor comments p. 5080 line 1. "a strong teleconnection. . ." should have capital "A"
[Corrected as suggested. Line 117 of revised pdf file](#)

p.5083. line 11. Please spell out "SH"- or introduce this abbreviation when the concept "Southern Hemisphere" is used for the first time in the text.
[Spelled out as suggested rather than abbreviated, as this occurs in a subsection title rather than body of text. Line 191 of revised pdf](#)

p.5090. line 1. "GPH" is not explained as far as I can see.
[Spelled out. Line 361 of revised pdf](#)

p. 5096, line 4. The abbreviation SAM for "Southern Annual mode " is used here for the first time it but should have been introduced when the concept "Southern Annual mode" is mentioned the first time in the paper
[This is actually the first mention of the Southern Annular Mode in the manuscript, and it is both spelled out and abbreviated here.](#)