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Climate of the Past Discussions

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Interactive Comment

Interactive comment on "The last deglaciation: timing the bipolar seesaw" *by* J. B. Pedro et al.

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Authors' Response to Review by Eric Wolff

We thank Eric Wolff for this perceptive and constructive review. The review raises some concerns about the statistical technique (SiZer) that we use to identify the timing of climate features in the ice core records and offers suggestions of how we might improve this aspect of our analysis. Following careful consideration of these concerns and suggestions we believe that we have improved the way we interpret the SiZer output. We also try to provide greater clarification of the strengths and limitations of the technique. In explaining these revisions it is helpful to first make several key points to which we will refer back in our response to individual comments. These points are also pertinent to our response to the review by Edward Brook and are copied there. A number of comments refer to Fig 3. For convenience, we copy a revised version of Fig

3 to the end of this response

1. **General comment about the SiZer technique**

   Any objective statistical method that seeks to identify the timing of warming and cooling trends in a time series is faced with the challenge of separating the influence of the very long term and the very short term variability from the millennial to sub-millennial features of interest, and related to this, accurately representing uncertainties. Both reviewers support the use of an objective statistical technique such as SiZer for this job.

   SiZer allows us to illustrate how the timing of climate features, specifically periods of significant warming and significant cooling, are sensitive to different smoothing filters. The vertical axis on the SiZer maps shows the smoothing filter width. At one extreme we have very long filter widths e.g. 3000 y (top of y axis fig. 3d), at this extreme the deglaciation is seen as a simple unbroken warming trend. At the other extreme we have very short filter widths e.g. 40 y (bottom of y axis fig. 3d), at this extreme the short term variability is large and no confident statement can be made about significant warming and cooling periods. The major millennial to sub-millennial scale features that the paper is interested in (e.g. the ACR) are expressed at filter widths between these two extremes.

   From the comments of both reviewers we acknowledge that we need to do a better job of explaining why we ultimately settle on particular filter widths and just how sensitive our results are to changes in that filter width.

2. **Why do we settle on particular (and different) filter widths for the Antarctic and Law Dome records and are our choices arbitrary?**

   For the Antarctic composite the narrowest filter at which deglaciation is seen, on the simplest level as a warming interrupted by the ACR, is at 200 y. As it is an individual core, the level of noise in the LD record is expected to be larger than that in the 5–core Antarctic composite and therefore a wider filter is appropriate. For the LD series the filter width must be increased to 250 y before the deglaciation is seen as a simple warming trend interrupted by the ACR. This is explained in Sect. 2.3 (paragraph 2 and 3) and was our basis in the previous manuscript for selecting 200 y and 250 y as the optimal filters from which to report significant changes in slope.

However, the comments of both reviewers have given us cause to revisit these criteria. Both reviewers note that at these filter widths the colours representing significant changes in trend sometimes 'slope in predictable directions' and in some cases revising the filter width by 50-100 y can result in changes in the timing of features that are large enough to affect one of our conclusions. Following Eric Wolff's suggestion we trialled SiZer on artificial data. The artificial data shows us that change point detection becomes robust when, moving to lower filter-widths, the timing of change points stabilises (e.g. generally below the 120 y smooth for the composite and below the 150 y smooth for LD). Accordingly, we have revised our analysis to detect change points at lower filter widths. We have also improved the text (copied below) in a way that we think explains our choices of filter and why they are not arbitrary:

2.3.1 Law Dome

For the LD record at filter widths of above 250 y the deglaciation is seen, on the simplest level, as a warming interrupted by the ACR (dashed horizontal line, Fig 3b). At filter widths much larger than this the timing of climate features is highly dependent on filter width, which suggests that detection is not robust. Moving to filter widths of $\leq$ 150 y the timing of features generally stabilises (below solid line, Fig 3b). Hence we select 150 y as the optimal filter width at which to detect climate features in the LD record.

2.3.2 Antarctic Composite

For the composite record, again at filter-widths of above 250 y the major millennial *scale features of the deglaciation are identified (dashed horizontal line, Fig 3d). Moving to narrower filters, a threshold is seen at a width of ≤ 100 y, at which multiple short term signals, that we regard as noise, are expressed. To stay above this noise threshold we select 120 y as the optimal filter width to detect climate features of interest to this work.*

3. C**an we improve the way we represent uncertainty in the timing of climate features?**

   Previously, as a means to estimate uncertainty we considered the effect on the timing of climate features of varying out selected filter width by ± 50 y. Both reviewers questioned this method of estimating timing uncertainty and why we use ± 50 y compared to some other value. We agree that our technique for estimating uncertainty can be improved. Looking at Fig 3b and 3d we see that the timing of most climate features is generally stable below the revised filter widths (solid horizontal lines) that we choose for the LD and Antarctic composite records, in these cases adjusting the filter width by - 50 y has little to no effect, but adjusting the filter width by + 50 y can move into a section of the curve that, as Eric Wolff points, the smoothing process has distorted the timing of the climate feature. For this reason we no longer vary the filter by some select amount in order to estimate timing uncertainty. Instead, we take a more descriptive approach; where the timing of a climate feature does not stabilise below the revised filter widths we mention this explicitly in the text. This is notably the case for the resumption of warming in the Antarctic composite following the ACR. We revise the text to make this clear as follows (Sect. 3, paragraph 6):

   '. . . *warming is first detected at the 120 y filter-width at 13.02 ± 0.20 ka (Fig 3d). This would place the resumption of warming coincident with GI-1b (ACP) or GI-1a in the north. However, it is not until 12.74 ± 0.20 ka that warming is finally seen on all timescales.'*

   The sensitivity of the onset of warming to the particular filter width leads us to

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withdraw our earlier conclusion that the onset of warming after the ACR is coincident with the start of the IACP in the north. In the abstract of the revised text we now describe the relative north-south timing of the resumption of warming as follows:

'Warming then resumes in Antarctica, potentially as early as the Intra-Allerød Cold Period, but with dating uncertainty that could place it as late as the onset of the Younger Dryas stadial.'

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Authors' response to individual comments

Individual comments by the reviewer are labelled R:, and the authors' response is labelled A:. Italic script is used to mark text that is being quoted from the manuscript.

R: Page 399, line 9 should be "complementary" (unless it is very polite!). line 18: closing bracket missing after Rasmussen et al 2008).

A: Corrected.

R: Page 401, line 10, and Fig 2: You clearly did not use the onset of the methane rise, as the triangle (and in Table 2) is at 16.09 ka, while the onset of the rise is about 17.5 ka. Do you mean you used the mid-point of the shallow rise? Please clarify/correct.

A: The tie is at 16.09 ka, which as the reviewer correctly points out is the mid-point rather than the onset of the slow deglacial rise in methane. We have corrected the text accordingly. Note that this tie is weaker (has larger dating uncertainty) than the sharp methane transition ties younger in the deglaciation (uncertainties are listed in Table 2).

R: Table 2: Importantly, you have some errors in the uncertainties here. The uncertainty in matching the methane rise at the start of Holocene cannot possibly be as high as 750 years as stated here. Indeed, I know it is not, because I saw an earlier version of this paper in which it was 75 years, and this would be consistent with the overall

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uncertainty of 90 years (not 900). Similarly at the start of GI-1e, I think the uncertainty is meant to be 30 years (not 300), as this has to be the case if the overall error is only 100 years. Thus you need to change 3 numbers, 11.63+/- 0.075, 11.78+/-0.09 and 14.64+/-0.03. Please check this carefully.

A: These were transcription errors only and have now been corrected. We also looked carefully over all other numbers reported in this Table and corrected several other very minor transcription errors.

R: I am not sure that you emphasise enough how critical the choice of smoothing interval under SIZER is. For example, you choose 250 years for LD but (apparently arbitrarily) 200 years for the stack. I guess you do this because with 250 years, within uncertainty, the start of warming for the stack becomes 19 ka instead of 18 ka. I think this has to be clearly pointed out. By choosing a smoothing that gives an 18 ka start you move to what the human eye perceives as the start, but this is subjective.

A: For the choice of smoothing filter and use of different widths for LD c.f. the composite see point 2 above. As the reviewer has noted, there are potentially two different dates that could be called the onset of deglacial warming. Cases may be made for 18.98 ka and 17.90 ka as the true 'onset of deglaciation' and the distinction depends on somewhat subjective criteria. Ultimately, from a dynamics perspective and in line with the reviewers' request it is important to describe what is actually happening in the data. Our solution is to spell out our criteria more clearly (Sect 2.3, paragraph 2):

'We define the onset of deglaciation as the time at which a significant warming is first observed on all timescales (i.e. regardless of the degree of smoothing)'

And, then describe what is happening at both 18.98 and 17.90 ka (Sect. 3 paragraph 4):

Significant deglacial warming begins in the south at 18.98 $\pm$ 0.39 ka on all timescales, and we therefore define this as the start of deglaciation. However, the warming trend

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then falters at 18.68 ka, before resuming strongly and again on all timescales at 17.90 ± 0.39 ka.

R: Even if the smoothing doesn't shift you to a different event, as in that case, there are also consequences of the choice of smoothing on the precise timing of events, because the edges of purple and blue lines slope in predictable directions. (As a specific example, take the example where y=1 from t=0 to t=1000, and then rises linearly from t=1000 onwards, in each case with no noise. Wouldn't SIZER show a purple/red boundary that was at 1000 years at a filter width of zero, but sloping to younger ages with longer filter widths? This would then be interpreted as a change with an uncertainty that did not encompass the actual change point of 1000 years.)

Specifically for this problem, when for example warming starts at 18.0 ka, this is specific to the 200 year smoothing/cutoff, and the warming on a shorter timebase (and in the data according to my eye) does not start until perhaps 17.8 ka. I accept you had to choose some criterion, but this potential issue (which can shift start dates by 200 years) should be discussed. I am not convinced that the use of the +/-50 year interval on the smoothing filter is really helpful in this regard, as illustrated by the schematic example in brackets. Anyway, I agree that SIZER is a good tool to use, but I think you need more explanation of how the filter width will affect the results.

A: This comment is dealt with in detail in points 2 and 3 above.

R: Page 405, lines 20-30. You mention the issue of uncertain delta-age at EDC, and the line-up of the (in the case of EDC) much-smoothed methane record needs also to be looked at. However you then discuss as though the difference between EDC and the composite (about 200 years) is significant: surely, at less than 10

A: Previously we did note that timescale uncertainties may explain the offset between the EDC and composite curves. Now we make this clearer. We preface the short discussion about possible reasons for the offset (Sect 3, paragraph 2) with the words *'If real, this delay may represent...'* We also include the statement *'Alternately, the*

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timing difference between the EDC and composite curves may simply reflect timescale uncertainties'.

R: Page 407, line 14-16. Here is where the uncertainties induced by your choice of SIZER smoothing interval are most important, because you conclude that the composite southern warming is at the onset of IACP and UNAMBIGUOUSLY earlier than the onset of the YD. However, although you get 13.18 ka (just after the IACP onset), this is really an artefact of the 200 year smoothing/cutoff, and the warming on a shorter timebase does not start until 13.0 ka, or even 12.8 ka if one went to 100 year smoothing.

A: See point 3 above. In brief, for this particular feature we agree that the timing is quite sensitive to changes in filter-width and that we cannot rule out warming coincident with the start of the YD. We have revised the text to make this explicit.

R: Thus only your choice of smoothing has driven you to the IACP rather than the YD. If you look at the data (Fig 5, black line) it is simply not true to say that the warming starts at the IACP; the smoothing filter has simply drawn the statistical start of warming backwards into a flat section of curve. Unless you can explain why I am wrong (and the data suggest I am not), you will need to pull back from the conclusions about the IACP – you cannot really discriminate between IACP and YD unfortunately (change also needed in abstract line 15).

A: Using the updated data files (see comments to review no. 1) and the improved interpretation method for SiZer results, we agree that we cannot discriminate between IACP and YD and as already explained above we alter the text in the abstract, results and conclusion to make this clear. However, we disagree that this was an artefact due to the choice of smoothing alone.

R: I don't like Figure 6 at all. This is an oversimplified version of Figure 5, and is not needed. If quoted out of context could lead to other people drawing misleading conclusions (especially in view of my comment about the IACP above). I would remove it.

A: We take this advice and remove the figure completely.

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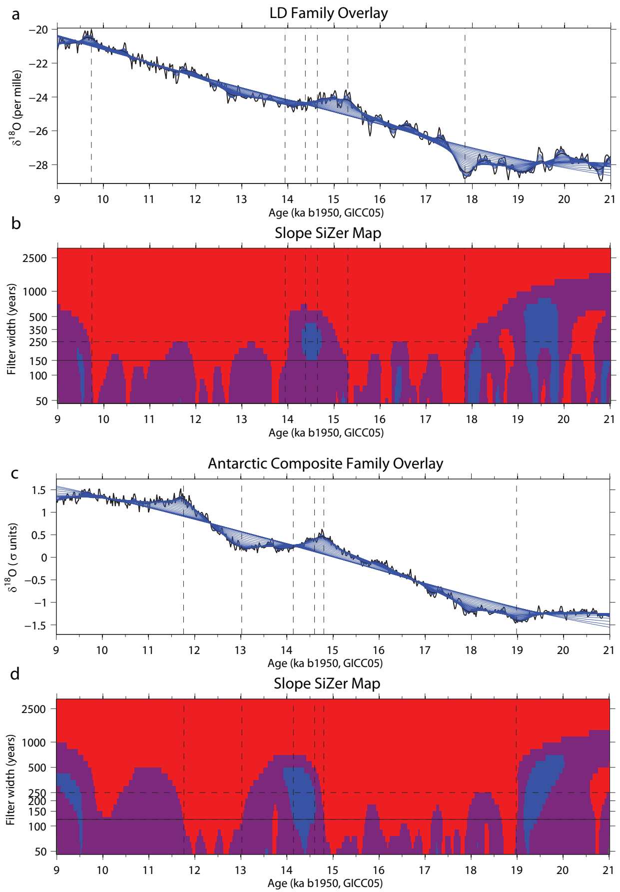

Fig. 1. Fig. 3. 'SiZer maps' of the significance of features in LD δ18Oice and the Antarctic composite through the deglaciation (see manuscript for full caption).