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Climate of the Past Discussions

CPD
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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 Edward Brook

We are grateful to Ed Brook for this detailed 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 have made some revisions to the way we interpret the SiZer output. 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 Eric Wolff 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 $\leq$ 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. **Can 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 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 5-10. It seems simplistic to set up atmospheric and oceanic teleconnections as alternative explanations for the see-saw, since the system is coupled.

A: We agree that the system is coupled and we do not intend to set up atmospheric and oceanic teleconnections as black and white alternatives. Nevertheless, these two mechanisms do involve different components of the climate system, so some distinction is warranted. To address the reviewers concern, we have revised the text to avoid any impression of unconnected mechanisms. We remove the phrase emph'These two processes, oceanic and atmospheric, differ fundamentally' and *'these different theories'*. The text (Sect 1, paragraph 2) now reads as follows:

The conventional explanation for these opposing climate trends is the bipolar ocean seesaw; it proposes that the two hemispheres are coupled via oscillations in the dominant direction of heat transport in the Atlantic Ocean due to perturbations in the meridional overturning circulation (Broecker, 1998). More recently, an alternate (though potentially complimentary) mechanism has been put forward that invokes atmospheric teleconnections in forcing the bipolar coupling (Anderson et al., 2009 and references therein). Sorting out the relative roles of oceanic and atmospheric processes is critical for understanding Earth's climate dynamics. A key role of the palaeoclimate record is *to provide firm observational constraints against which these dynamical mechanisms and their timescales can be tested.*

R: Page 401, Line 5-10. Transitions in the Law Dome methane record are dated by assuming that the fast methane transitions are synchronous with Greenland climate events. This is appropriate, and the estimates of delta age seem adequate. Once concern though, is that the method for identifying these methane transitions is, I believe, simply the visual identification of inflection points. While this is not objectionable, ultimately the timing of these events is being compared to the timing of inflection points in the Antarctic record identified by a different method, the SiZer method. Do these two methods behave the same way? I have some concerns about what the SiZer method does (below) so think this is a relevant question.

A: We use the mid-point of visually identified methane transitions to align timescales between LD and the Greenland records. This is the same practice as has been used for other synchronisations of Antarctic records with Greenland. The error in the LD transition was determined by picking the mid-age between the pre-transition methane sample and the post transition methane sample and specifying the uncertainty to extend to these pre- and post-transition sample ages (with appropriate delta-age corrections).

SiZer, by contrast, is designed for picking the timing of changes in curves from positive to negative slopes (or vice-versa). This is exactly what we need to do in determining when warming starts/ends in the LD. We are trying to use the best methods we know of for these different jobs.

R: Page 402-403, Section 2.3. Using an objective algorithm to identify inflection points is appropriate, particularly as it allows more objective estimates of uncertainty. But, the SiZer method requires smoothing filters. The choice of smoothing filter is somewhat arbitrary as is the 50 year range chosen to represent the uncertainty. Since this produces uncertainties of 200-400 years, which are big enough to be relevant to the conclusions of the study. The 200 year figure seems OK since it is somehow related to the scale

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Interactive Discussion

Discussion Paper

of variability that is of interest. The 50 year value used to estimate an uncertainty is harder to interpret. It is certainly relevant, but why use 50 years vs. some other value.

A: Refer to points 1 to 3 above. To summarise, the comments from both reviewers have lead us to reduce the filter width at which we interpret significant changes in slope. This is because at lower filter widths the timing of features in most cases stabilises. Where the timing of features does not stabilise (e.g. resumption of warming post-ACR) we state this explicitly in the text and we have modified our conclusions accordingly.

R: Also, it seems that there should be another way to approach this. Uncertainties were established for all of the chronologies used in the composite. Couldn't a Monte Carlo approach be used to create multiple realizations of the composite, with SiZer applied to all of them, thus producing an uncertainty estimate for the transition points in the composite? The jackknifing is a good approach, particularly to see if any one record deviates from the others substantially, but It does not deal with the chronological errors in the whole group of records.

A: As the timing determination using SiZer is not automated, we are not able to propagate uncertainty through the analysis in an automated way to allow Monte Carlo simulations. Automating the timing determination would involve development of automated feature extraction algorithms, a distinctive project in itself that is outside the scope of this work.

R: It is also not clear why the SiZer approach is applied to Law Dome, since the point of the composite is to get a better representation of Antarctic climate.

A: The LD record is a new data set that has not been published elsewhere. Hence we feel that it is reasonable that it has some emphasis. In the first two paragraphs of Sect. 3 we use the results of the SiZer analysis of the LD curve in discussing similarities and differences between LD and the composite and to emphasise the need for caution in interpreting the phasing of inter-hemispheric climate changes from single site records.

CPD

7, C700–C711, 2011

R: Section 3: In this section the composite record is compared with the NGRIP isotope record, specifically with the timing of inflection points in that record (Lowe et al.). Those points are the timings of transitions – that is, the mid points of transitions, not the onset of transitions, or the end. So what is being compared are the onsets of transitions in the Antarctic composite with the mid-point of transitions in the Greenland record. This is evident from Figure 5. One can see that this distinction probably matters by looking at the Bølling onset. The sharp rise in the NGRIP record at about 14.7 ka, to my eye, corresponds exactly to the SiZer derived timing of the start of ACR cooling, where as the shading in Figure 5 shows a lead of that cooling vs. the identified Greenland event. In the text the authors indicate that within error these are coincident features, which is true based on their analysis of the error, but I would maintain that there is an "apples and oranges" problem here. The same problem crops up at the end of the ACR/start of the YD. It appears to me that the onset of the YD coincides with the warming that begins at the end of the ACR.

A: Firstly, the values in Lowe et al (2008), for all the North Atlantic climate events between the onset of the Bølling and the onset of the Holocene, are the timing of onsets of transitions and not their mid-points (see Table 1 of Lowe et al., 2008). These values are typically determined using the deuterium excess values in the NGRIP ice core. In any case the difference between onset and midpoints often is no more than 20 y so there should not be a problem. This is now made clear in the text (Sect 3, paragraph 3.):

'the main climate transitions (the onsets of which are defined in the INTIMATE climate event stratigraphy of Lowe et al., (2008)) are already simultaneous between the Greenland records when studied at 20 y time resolution (Rasmussen et al., 2008).'

With regard to the timing of the Bølling onset vs the ACR; the onset of the Bølling transition is defined by Lowe et al., (2008) as 14.64 ka (BP 1950). Our objective technique using SiZer places the end of a significant warming trend at 14.8 and the start of a significant cooling trend at 14.60. Given the 200 y timescale uncertainty in the composite we stand by our comment that the events are synchronous within error bounds.

With regard to the timing of the YD vs the end of the ACR; as explained in point 3 we are now explicit about the sensitivity of this particular result to changes in filter width. We acknowledge that within dating errors the resumption of warming may, at the latest, coincide with the YD.

R: Why not apply the SiZer analysis to the Greenland record?

A: The onsets of Bølling, YD and the Holocene are already defined using statistical tools appropriate for the detection of transitions (Steffensen et al., 2008). We think that a new set of dates for the Greenland transitions, using a different tool that is more appropriate for detecting slope changes than abrupt onset, would not be helpful. We prefer to use the dates recommended by the INTIMATE group.

R: Another issue with the interpretation based on SiZer is that it appears, to my eye, to identify warmings slightly too early (18 and 13 ka), but pauses in warming at about the "right" time (for example, the end of the warming right before the ACR).

A: Refer to points 2 and 3 above.

R: On page 409 in this section, in the first paragraph (lines 1-13) different possible mechanisms of rapid signal transmission are discussed. These include oceanic and atmospheric mechanisms. This section is somewhat contradictory to the discussion in the introduction, which sets up a strong distinction between the two mechanisms.

A: As described in our response to the reviewer's earlier comment we have revised the relevant part of the introduction to better reflect the coupled system.

R: In reading over the data section I realized there is an important error in one of the data sets used by the authors. It is not their fault, but needs to be corrected in the manuscript. The Siple Dome data they used is erroneously labeled in the data file available from the NSIDC that the authors used. That column should have been labelled "Estimated Antarctic Temperature in Degrees C." The error was not noticed

CPD

7, C700–C711, 2011

because the Holocene estimated temperatures are close to the isotope value. I do not think that this will affect the timing of the transitions in the data, but it will probably change the composite somewhat. Since what is archived is dD, the analysis can either be done with dD or with dD/8, which should estimate d18O sufficiently for the purposes of the paper.

A: We are glad that the reviewer noticed this problem! We have now updated the composite using a scaled version of dD (i.e. (dD-10)/8), in place of the mislabelled 'd18O'. It would have been our preference to use the actual Siple d18O record, however there are presently some issues that preclude its use (Ed Brook, Jim White personal communication). As the reviewer points out, the scaled version of dD estimates d18O sufficiently for our purposes. We revised the text (and axis label for the Fig 1.) to make it clear that for the Siple record we are using scaled dD rather than d18O.

We also noticed a second issue with a data set used in the composite, in this case with the Taldice record. The online at NSIDC associated with Stenni et al., 2011 reports sample ages with respect to sample top depths. This is different to the conventions used in the published data from the other cores used in the composite (EDML, Byrd, Siple Dome, Law Dome) and in constructing the composite we wrongly interpreted that the Taldice ages were centre ages. This meant that the Taldice ages used in the composite were out by approximately 20 y. We have now corrected this mistake.

These two revisions (to Siple and Taldice) required two notable changes to the text. The first is of relatively minor importance and relates to the results of the jack-knife test. Previously all six versions of the composite (with individual records excluded) gave the same pre-ACR isotope maximum. Now we find a standard deviation of $\pm$ 20 y. The text is adjusted to reflect this as follows:

For instance, in the resulting 6 versions of the composite, the timing of the pre-ACR isotope maximum (mean $\pm$ 2σ) is stable at 14.76 $\pm$ 0.02 ka.

Secondly and more importantly, the timing of the resumption of Antarctic warming after

CPD

7, C700–C711, 2011

Interactive Comment

the ACR shifted 80 y younger i.e. closer to the onset of the YD in the north. The effect on the timing of all other climate features in the composite was in all cases $\leq$ 20 y. This result also contributed to our decision to back away from the conclusion that the resumption of Antarctic warming after the ACR was coincident with the IACP.

Interactive comment on Clim. Past Discuss., 7, 397, 2011.

CPD
7, C700–C711, 2011
Interactive Comment

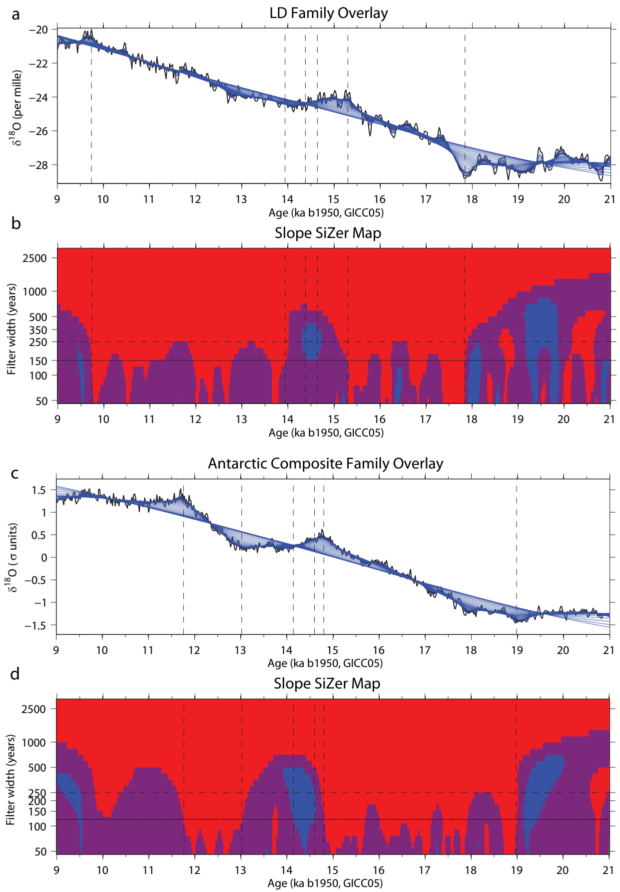

Fig. 1. Fig. 3. 'SiZer maps' of the significance of features in LD δ18Oice and the Antarctic composite through the deglaciation (refer to manuscript for complete caption).