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Climate of the Past Discussions

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

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More and more detailed data from the termination of the last deglaciation have become available in recent years, revealing the interplay between the termination and millennial scale climate variability. This paper takes advantage of the authors' own Law Dome record, of the ability (using methane) to synchronise several ice core records to a single age scale, and of good statistical methodology. It concentrates on coastal ice cores where (with the possible exception of EDML) the uncertainties in synchronisation are small enough to allow century scale commentary. It then takes a novel direction for this kind of ice core study: instead of comparing different ice core datasets around Antarctica (as done for example in the recent Talos Dome paper) it stacks the data to determine what is a robust Antarctic-wide signal. This is a procedure widely used in marine studies and in shallow ice core work, but until now it has not been possible for ice cores covering the transition. Of course one has to be very careful with this type

of stacking procedure: (a) one runs the risk of getting a biased result if the sites used do not really represent a good sampling of Antarctica, and (b) one must be careful not to hide and ignore important events through the stacking procedure. However, the authors seem aware of these issues and have used the results in an informative and sensible way.

The result is a very impressive ability to tie the times of Greenland and Antarctic events with important implications for mechanisms. I do have concerns about some of the more detailed conclusions, that I believe the data do not support, and I have also spotted an error (probably only in transposing data, and not in the resulting matches) in a table. However these comments are minor, and I have no hesitation in recommending this well-written paper for publication in CP after minor but important amendments.

Detailed and general points:

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

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 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.

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)

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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. 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.

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% of delta-age, this difference is well inside the uncertainty in the EDC ice age uncertainty at this depth? Be careful drawing conclusions here.

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 com-

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posite 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. 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).

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.

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