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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.

E. Brook (Referee)

brooke@geo.oregonstate.edu

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Creating a composite or stacked record for Antarctic ice cores, as done here, is a useful exercise, and well worth pursuing. The authors do this, and then they examine the timing of inflection points in the composite, relative to abrupt changes in Greenland, to try to say something about the mechanisms of the bi-polar seesaw. Although I think this paper is worthwhile, and the additional Law Dome data are very important, I feel there is room for improvement in the way the data are analyzed and interpreted. A primary concern is whether the conclusions about the timing of the IACP relative to the end of the ACR are valid, and in general whether the methods used to identify changes in the records are detecting the appropriate features. My detailed comments below elaborate on these and other issues.

Page 399, Line 5-10. It seems simplistic to set up atmospheric and oceanic telecon-

nections as alternative explanations for the see-saw, since the system is coupled.

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.

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 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.

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.

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.

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 tran3sitions 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. Why not apply the SiZer analysis to the Greenland record?

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).

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.

Appendix

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 labeled "Estimated Antarctic Temperature in Degrees C." The error was not noticed 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.

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