

Interactive comment on “Rapid coupling of Antarctic temperature and atmospheric CO₂ during deglaciation” by J. B. Pedro et al.

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Dr Parrenin states that we have made “a clear step forward” in constraining the phasing between deglacial Antarctic temperature and CO₂ while also asking for “some clarifications of our method” in order to strengthen the manuscript.

We will fully address the comments on the current manuscript alongside our responses to the two formal reviews. However, we wish to resolve some points here that the comment raises about our previous paper: “The last deglaciation: timing the bipolar seesaw”, Pedro et al., *Clim. Past*, 7, 671–683, 2011, www.clim-past.net/7/671/2011/). This is not the place for lengthy discussion or re-review of a published paper; however to avoid possible misunderstandings, we will briefly respond. Interested readers might also refer to the discussion phase of our earlier paper

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(<http://www.clim-past-discuss.net/7/397/2011/cpd-7-397-2011-discussion.html>), where similar concerns were already addressed.

The first point regards the early Holocene trend in Law Dome isotopes: “. . .there is a warming trend during the Early Holocene which cannot be seen in other ice cores. I am not convinced that this isotopic trend is representative of Antarctic climate, even at a regional scale. I am not even convinced it is a climatic trend, it could well be due to glaciological artefacts (changes of ice sheet thickness, etc.). Therefore, the article misses an argument as to why LD is really improving the stacked record.”

In response: (1) The early Holocene trend at Law Dome is already noted in our previous paper and we cite a paper which suggests that elevation changes may be partly involved (Delmotte et al., 1999). (2) Our previous paper demonstrates (using jack-knifing) that the timing results derived from analysis of the stack shows very little sensitivity to the removal of the Law Dome record, or other individual records. (3) The stacked Antarctic record spans only 21-10 ka BP, therefore the behaviour of Law Dome isotopes in the early Holocene is of little relevance to the results of our previous paper and certainly does not affect our current analysis of the temperature-CO₂ lag.

The second point raises a similar concern about the Siple Dome and Byrd records: “the Siple Dome isotopic record is quite different from the classical ‘East Antarctic plateau’ scenario, but this time the difference is more striking for the first part of the deglaciation (before the onset of the BA)” and the “Byrd isotopic record also presents a different scenario at the onset of TI which again may not be representative of Antarctic climate”.

In response: (1) As above, we agree, and state clearly in the paper, that trends in individual cores may represent “. . .local, non-climatic and/or sub-continental trends”. (2) The very point of constructing a composite record is to reduce these local signals and produce reconstructions that more robustly represent regional climate trends; this concept is backed by a strong body of previous work [e.g. Fisher et al., 1996; White et al., 1997; Andersen et al., 2006; Schneider et al., 2006]. (3) As above, the presence

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of a common signal is supported by jack-knife testing. (4) It should be recalled that the East Antarctic plateau cores (with their characteristic low accumulation rates) are much more weakly dated than the near-coastal cores, for example EDC has around an order of magnitude higher delta-age uncertainty than Law Dome. This makes the near-coastal (higher accumulation) cores more suitable for evaluating North-South phasing. (5) When considering climate changes in the high latitude southern ocean, the coastal records may be more appropriate targets than the plateau records.

The third major point concerns the Law Dome methane tie point at 16.09 ± 0.33 ka (the mid-point of the slow deglacial rise in methane through the interval ca. 17.5 to 15 ka BP). The comment contests our estimate of the uncertainty, stating "Personally I evaluate the 2σ uncertainty of such tie point as half the duration of the transition. In this case, this would give 1.5 kyr."

In response: (1) The slow rise in methane is noted in the paper as a lower quality tie than the fast methane transitions which occur later in the deglaciation, however in the absence of other markers capable of synchronising to the Greenland cores it does provide some constraint; shifting the Law Dome methane curve by more than 330 years leads to marked contrast with the GISP2. We acknowledge that this is a qualitative judgement, but would point out that it is conservative when compared to previous estimates; for example, Lemiux-Dudon et al., [2011] list an uncertainty of only 160 years for the corresponding tie in the EDML core. (3) In any case, the main conclusions of our previous paper centre on the relative timing of transitions in the North (Bølling-Allerød, Younger Dryas, Holocene) and trend changes in the South (ACR onset and termination). These are unaffected by debate over the precise uncertainty allocated to the slow rise in methane.

As mentioned at the start, we will provide full responses to Dr Parrenin's comments on the current manuscript when we submit our responses to the two formal reviews.

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