

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

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Review of Pedro et al. 2012

This paper further quantifies the phase relationship between Antarctic temperature and CO₂, using an approach that takes advantage of most of the relevant data that are available. It is a nice addition to the literature on this subject and seems to narrow the uncertainties. I don't find the result all that surprising, though that does not minimize its importance.

Page 624. Why not also use the EDC CO₂ record in this analysis? It can be placed on the same time scale as the others using methane and therefore the delta age problem for EDC is not relevant. The advantage of the EDC record is possibly better data quality.

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I say possibly because although the EDC record is smoothly varying and therefore looks visually very reliable, it may be that smoothing in the firm has reduced shorter-term variability in the data (this may actually be a reason not to use it).

Page 624, lines 0-10. A little confusing here. On the one hand the authors suggest that the Ahn et al. lag result may suffer from “from the fact that the Siple Dome deglacial isotope record contains abrupt changes not observed in other Antarctic records (Taylor et al., 2004; Brook et al., 2005), suggesting a local climate signal that would not be expected to correlate with CO₂ evolution.” and on the other hand they use the Siple Dome isotope record in the Antarctic composite, implying it is a regional climate record, I believe).

Page 625, line 14-16. The authors have interpolated the CO₂ record to a very fine sample spacing) then smoothed that interpolated record, and compared it to the isotope record, also smoothed, but sampled originally on this finer spacing. They explore the impact of the smoothing chosen on the results, but I would like to be sure that the interpolation does not affect the results. What happens if the isotope data are interpolated to the sample spacing of the CO₂ data? The critical issue is to understand the limitations placed on the result by the sampling interval for CO₂, which I do not think are necessarily addressed by the analysis done here.

Page 626, line 14-16. The correspondence of CO₂ and Antarctic temperature may support the idea that southern ocean processes control CO₂, but it is not an ironclad fingerprint. CO₂ is globally distributed and any change in source/sink balance could cause atmospheric levels to rise. It is not inconceivable that processes outside of the southern ocean that cause CO₂ to go up could correlate with Antarctic temperature.

Page 626, line 25. I think this should refer to Figure 1b not 2b.

Finally, it would probably be appropriate to mention and address the result from the Shakun et al. paper recently published in Nature that addresses the relationship between global temperature and CO₂ during the deglaciation.

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