

Interactive comment on “Leads and lags between Antarctic temperature and carbon dioxide during the last deglaciation” by Léa Gest et al.

Anonymous Referee #1

Received and published: 25 July 2017

General Comments

The manuscript reports on the time relationship between atmospheric CO₂ and Antarctic temperature during the last deglaciation. The authors make use of the WAIS CO₂ record, which is the highest resolution and best-dated of the Antarctic CO₂ records, and a new regional Antarctic temperature reconstruction from a stack of 4 Antarctic ice cores. Their central estimates of the temperature and CO₂ time lag does not substantially differ from Pedro et al., 2012 and Parrenin et al 2013. However, their good methodology and use of the best records currently available leads to lower uncertainties and likely more robust results than those previous works.

The Antarctic temperature and CO₂ time relationship is an important constraint for process studies of carbon cycle feedbacks during deglaciation so the new results are

valuable (even if there is little discussion of their significance in the manuscript itself). I believe the manuscript is well suited to publishing in *Climate of the Past* after revisions described below.

Specific Comments

The results should be presented more quantitatively in the abstract. For example the meaning of line 24 'synchronous within a range of 210 years' is unclear. The value at the onset of deglaciation is particularly important and must be reported in the abstract. Please report the central estimate of the lag for each breakpoint, the uncertainty, and whether these 1 sigma or 2 sigma uncertainties.

A mean lag over the deglaciation should also be given. This could be done using lag correlation analysis, as in Pedro et al., 2012. I'd suggest to exclude the onset of the Holocene from such an analysis since 1) the correlation between CO₂ and the temperature stack appears to deteriorate there (can the authors confirm or reject this?) and 2) there is evidence that different carbon cycle mechanisms operate in the early Holocene compared to earlier in the deglaciation [e.g. Schmitt, *Science*, 2012]. If the authors will not provide an estimate of the mean lag over the deglaciation then they should provide a convincing argument as to why not.

It would strengthen the manuscript if the phasing analysis was also carried out on a radiative forcing time series determined from the WAIS CO₂ record. Use of radiative forcing in place of the CO₂ data may affect the results (e.g. see Ganopolski and Roche et al., 2009). It should be straightforward for the authors to do this since it was done in Parrenin et al., 2013.

Section 2.1: The ATS2 should either be made available as a Supplement, or archived at a public database.

In general the manuscript would benefit from a proof reading and edit from one of the coauthors who's first language is English.

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Technical Comments

Line 12: ..phasing between CO2 and Antarctic temperature..

Line 28: "Future climate and carbon cycle modeling works should take into account this robust phasing constraint." Which of your constraints should the modelers use? A little more discussion would be appropriate.

Line 35: no space before colon.

Line 37: "..still a matter of debate". Agreed, but provide a citation.

Line 40: for completeness add Fischer et al., (1996) and Mudelsee, (2001).

Lines 41-46: This paragraph includes important information but is convoluted, please rewrite more clearly. Please cite the work supporting that the ACR and B-A are synchronous.

Line 55: you mean it represents global atmospheric CO2 variations?

Line 71: "roughly in phase".. be more precise.

Line 75: gas chronology

Line 98: Do you use the borehole calibrated WAIS temperature reconstruction of Cuffey et al., 2016? If so cite it, if not, why not?

Line 123: Explain why this is a reasonable error estimate.

Line 123: Roughly? Please be more precise.

Line 127: consist of

Line 129–131: Cite the original authors of the CO2 records. Note that Pedro et al., uses CO2 data from Byrd and Siple Dome, not Law Dome.

Line 136: 1 sigma or 2 sigma?

Line 141: Its unclear here if you are describing the Parrenin et al. (2013) method as LinearFit.

Line 143: "We opt instead for a nonlinear least squares.." this is confusing given you call your method 'LinearFit'.

Line 175: 1 sigma or 2 sigma uncertainties?

Line 187: to the importance

Line 219: "no CO2/temperature phasing"? Does not make sense, reformulate.

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Lines 220–222: The way this is written is hard to follow. Suggest report your significant results and then report the important differences with respect to other studies.

References

Fischer, H., Wahlen, M., Smith, J., Mastroianni, D., and Deck, B.: Ice core records of atmospheric CO₂ around the last three glacial terminations, *Science*, 283, 1712–1714, doi:10.1126/science.283.5408.1712, 1999.

Mudelsee, M.: The phase relations among atmospheric CO₂ content, temperature and global ice volume over the past 420 ka, *Quaternary Sci. Rev.*, 20, 583–589, doi:10.1016/S0277-3791(00)00167-0, 2001.

Schmitt, J., Schneider, R., Elsig, J., Leuenberger, D., Laurantou, A., Chappellaz, J., Köhler, P., Joos, F., Stocker, T. F., Leuenberger, M., and Fischer, H.: Carbon isotope constraints on the deglacial CO₂ rise from ice cores, *Science*, 336, 711–714, doi:10.1126/science.1217161, 2012.

Interactive comment on *Clim. Past Discuss.*, <https://doi.org/10.5194/cp-2017-71>, 2017.