Clim. Past Discuss.,
https://doi.org/10.5194/cp-2019-142-RC4, 2020

Climate of the Past Discussions

Interactive comment on "Millennial-scale atmospheric CO_2 variations during the Marine Isotope Stage 6 period (190–135 kyr BP)" *by* Jinhwa Shin et al.

Anonymous Referee #4

Received and published: 3 April 2020

Jinhwa Shin and colleagues present new measurements of CO2 trapped in bubbles of ice at the EPICA Dome C (EDC) site in Antarctica during the penultimate glaciation. They reconstruct a high-resolution record of atmospheric CO2 changes and compare its variations to climatic signals from Antarctica and the North Atlantic region. For the early part of their glacial record, atmospheric CO2 and CH4 display contrasting lags, shifting from hundreds to more than one thousand years. The authors interpret this shift in terms of a reorganization of the Atlantic meridional overturning circulation, and also conclude that the amplitude of CO2 variations may be influenced by the duration of AMOC perturbations.

The new data are welcome, nearly tripling the existing CO2 record from Vostok and nearly doubling the existing CH4 record from EDC, and the resulting discussions are worthwhile. This is a potentially valuable new contribution and can be considered for publication following revision that should include addressing the following points.

1) The new CO2 data are offset to lower values from previous data, and are not replicated. Is there an explanation for the first point and a justification for the latter?

2) The authors state that they "make use of contrasting boundary conditions during the last two glacial periods to gain insight into the co-occurring carbon cycle changes." They then note that those boundary conditions are only "slightly different", and in the end, never explain what those differences are or why they might be expected to matter. This undermines the rationale for proceeding with this study and should be much better explained in a revised manuscript.

3) It appears that a potentially significant conclusion of the manuscript derives from the observations associated with a single small millennial event. This hardly seems justified and should be bolstered either by theoretical arguments or indications of similar behavior in existing data from another time interval.

4) The division and labeling of sub-events is neither referenced nor adequately described, much less explained. Such division is understandable and can be helpful, but only if clearly delineated and consistently applied. Are the divisions related to marine oxygen isotopes, and should they be, or to something else? What is the justification for 6c, 6d, and 6e, when there is no 6a or 6b?

Smaller points to be considered and addressed:

Page 2 line 15 – "opposite behaviour"

Page 2 line 17 – Do the authors really infer that CO2 changes are "in response" to temperature?

Page 7 line 8 - The half cycle between minimum and maximum values is not the defi-

nition of an inflection point, nor is it any point at all.

Page 14-15 – Data do appear to be limited, although Helmke, 2003, Kandiano, 2003, Obrochta 2014, Mokkeddem 2016, and Barker 2015 come to mind.

Page 15, line 16 – “available”

Figure 3 – What are the “Six variations on millennial time scales. . .”?

Figure 5 – Golay should be capitalized in the legend.

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