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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 #1

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Review of manuscript cp-2019-142:

General Comments:

The manuscript by Shin and others presents new, high-resolution measurements of CO2, CH4, and δ15N in EDC ice core samples spanning the glacial period, MIS 6. The new data resolve millennial-scale variations in CO2 and CH4. The authors independently identified MIS 6 stadial durations in tree pollen % and planktonic δ18O in the Iberian Margin marine sediment core MD01-2444. The authors also revised the MIS 6 gas age chronology of the EDC ice core (previously AICC 2012) using new estimates of Δdepth from the δ15N data. The revised EDC age scale, along with the timing of

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climate variations observed in the sediment core, provides the authors with a temporal framework for understanding millennial-scale CO2 variations during the penultimate glacial period.

The authors specifically analyze the timing of the CO2 changes relative to changes in CH4, considered here a proxy for NH warming, identifying leads/ lags between the two records. They also discuss differences between the CO2 features in MIS 6 and analogous features that occurred in MIS 3. The authors also observe differences in the magnitudes of CO2 maxima during MIS 6. They identify a relationship between the amplitude of CO2 change and the duration of the preceding stadial event, offering the hypothesis that the amplitude of CO2 variations depends on the duration of AMOC perturbations. They also identify a shift in the lag of CO2 maxima from MIS 6e to MIS 6d and suggest that this may be due to a change in the organization of AMOC.

This manuscript is well written, organized, and clearly presented, the science is in my opinion sound, and the new datasets represent important contributions that will be of interest to others in the field. The work is appropriate for the journal Climate of the Past, and I recommend this paper for publication after minor revisions. Below I list specific comments that, if addressed, will aid in the clarity of the paper and hopefully strengthen the analyses therein. I also list technical corrections below.

Specific Comments:

INTRODUCTION – P3L9 – Can you provide a reference for the longer duration of stadials in early MIS 6?

– P3L15 – There are more pre-existing CO2 measurements from late MIS 6 besides those from Vostok (Lourantou et al., 2010; Schneider et al., 2013).

METHODS – P3L31 – Did the measured CO2 concentration depend on the amount of air injected? (Presumably, the pressure in the sample loop depleted across the 5 individual injections. Was there a linearity effect?)

– P4L18-19 – Was the amount of contamination in each chamber consistent from day to day? Did it depend on the length/ amount of crushing?

– Did you run replicate CO2 measurements on ice samples from the same depths? In my opinion, this would be a better estimate of the true system precision.

– P4L25 – Can you state briefly how the new, corrected CO2 record compares to the preexisting CO2 data?

– P4L32 – Can you state the precision of the CH4 measurements?

– P5L3 – What do you think are possible reasons for the systematic offset? Please describe briefly.

– P6L20 – Figure S4 in the SI does not have a label to distinguish blue from red.

– P6L22 – I do not follow how Figure 2 supports the claim that the previous method was "relatively unbiased but not entirely exact."

– P6L13 – In Figure 3 it appears that the midpoints in the transitions are somewhat ambiguously defined. Sometimes they fall between a local max and min for d18O, sometimes for pollen %. The markers are chosen as midpoints between local maxima/ minima, but sometimes it is unclear where those max/ min data points are. 6d.2, for example, could easily be shorter (i.e., it looks like the end marker at 174.2 ka could be defined at an older age). 6c.2 is a particularly ambiguously defined stadial – I do not see which maximum and minimum pair defines the older marker. Could you define the stadial durations more objectively? The ambiguity and subjectivity in picking the stadial transitions lead me to believe that they were defined while also considering the ice core data. That's not necessarily a bad thing, but perhaps you should just be forthright and show the gas data in Figure 3 along with the sediment core data.

RESULTS – P8L3 – You should mention the known phenomenon of CO2 offsets between different ice cores (e.g. WAIS versus Law Dome). The co-author Christoph could certainly comment on this.

– P25Fig5 – It is unclear how the blue CDM events were defined. Do they relate somehow to the stadial duration markers you defined previously? If not, please clarify how you identified them (or provide proper reference to SI).

– P26Fig6 – Shading or vertical lines would help to delineate the CDM's in Figure 6. Right now the text floats at the bottom and is unclear exactly what the labels refer to.

– One result that strikes me as interesting, and not discussed in the paper, is that the lowest CO2 and Antarctic temperature values occur in the early/ middle part of MIS 6, not the latest part (as in MIS 2). CH4, on the other hand, reaches the lowest values during late MIS 6, right before the termination, as does peak glaciation as inferred from the benthic d18O. This is unlike MIS 2, which is characterized by low CO2, low Antarctic temperature, low CH4, and peak glaciation occurring simultaneously. Can you speculate why CO2 is higher in late MIS 6 relative to earlier in MIS 6, despite full glacial extent?

– P28Fig8 - The authors compare the timing of CO2 maxima relative to the onset of NH warming. The CO2 measurements come from different ice cores with different age scales (to my knowledge at least, Byrd is not synchronized to the AICC 2012 as EDML, EDC, and TALDICE are). What is the bias or uncertainty in the analysis due to age offsets? Why not exclusively use the EPICA cores on a unified age scale for this analysis?

DISCUSSION – P11L26&31 – When you say that the terrestrial biosphere can "compensate" for the slow response of the deep ocean, do you mean in terms of its timing or in terms of the direction of CO2 change? Please clarify. "Compensate" may not be the best word to use in case it is confused with carbonate compensation.

– P13 – After the discussion of AMOC and deep ocean ventilation, I realized there was no discussion entertaining productivity fluctuations as a possible mechanism for millennial-scale CO2 variability (Ziegler et al., 2013; Gottschalk et al., 2016; Anderson et al., 2014; Martinez-Garcia et al., 2014).

– P13L13-18 – Need more references in this paragraph.

– P14 – After reading this section it strikes me that there is a large amount of discussion about AMOC changes without actually showing any AMOC data. The discussion is very "AMOC-centric." Indeed, we believe that AMOC changes are probably key to explaining the MIS 3 CO2 changes, but to assume the same mechanism operates in MIS 6 without data to suggest so, and then to make assertions about the AMOC based on the CO2 trends at least requires some qualification in my mind. It is okay to speculate, but please say explicitly that you are doing so and that it is based on extrapolation of the relationships observed in MIS 3.

CONCLUSIONS – P14L22 – "Unprecedented" strikes me as too strong of a word.

– I think the conclusion section should contain less about the AMOC. The primary contributions of the paper (in my mind) are the new data, the revisions to the EDC gas age scale, and perhaps the observations of leads/ lags relative to abrupt CH4 changes. The differences in the organization of AMOC between and within MIS 6 and MIS 3, as well as the relationships between stadial length and AMOC perturbation should be left out here. They are interesting hypotheses, but they are not supported by data. See also my note above about rewording the discussion to be more explicitly speculative.

Technical Corrections:

– Section 2.4 is titled "Ice age revision. . ." but the gas chronology, not the ice chronology, is what is actually revised. It might be confusing, so consider titling this section "Gas age revision. . ."

– In Figure 8 the authors show various CO2 maxima plotted against the lead/lag with respect to the onset of Northern Hemisphere warming. It would be helpful to clarify, for example, "CDM 12" corresponds to DO 12, etc.

– P2L10 – Capitalize "Hemisphere" in "Northern and Southern hemisphere, respectively."

– P2L15 – “opposite”

– P2L17 – I suggest leaving out “In response to the millennial temperature perturbations,”

– P2L32-33 – No need to repeat “MIS 3” and “MIS 6” in parentheses. Just state the age ranges.

– P2L32 – Why just “early MIS 6?” The data also span some of late MIS 6, younger than 160 kyr.

– P3L10 – I think the sentence about a shallower AMOC cell can be combined with the preceding discussion about weaker AMOC.

– P12L8-9 – You already said this in the previous sentence (NADW can be slowed down after freshwater forcing). I think it can be omitted.

– P25Fig5 – There is a typo in the legend. “Uncertainties of calculated from savitsky golay filtering.” I am not certain exactly what it is supposed to say.

– SI P7FigS7 – The caption says “Two boxes. . .” but there are five.

Anderson, R. F., Barker, S., Fleisher, M., Gersonde, R., Goldstein, S. L., Kuhn, G., Mortyn, P. G., Pahnke, K., and Sachs, J. P.: Biological response to millennial variability of dust and nutrient supply in the Subantarctic South Atlantic Ocean, Philosophical Transactions of the Royal Society a-Mathematical Physical and Engineering Sciences, 372, 17, 10.1098/rsta.2013.0054, 2014.

Gottschalk, J., Skinner, L. C., Lippold, J., Vogel, H., Frank, N., Jaccard, S. L., and Waelbroeck, C.: Biological and physical controls in the Southern Ocean on past millennial-scale atmospheric CO2 changes, Nat. Commun., 7, 11, 10.1038/ncomms11539, 2016.

Lourantou, A., Chappellaz, J., Barnola, J. M., Masson-Delmotte, V., and Raynaud, D.: Changes in atmospheric CO2 and its carbon isotopic ratio during the penultimate deglaciation, Quat. Sci. Rev., 29, 1983-1992, 10.1016/j.quascirev.2010.05.002, 2010.

Martinez-Garcia, A., Sigman, D. M., Ren, H. J., Anderson, R. F., Straub, M., Hodell, D. A., Jaccard, S. L., Eglinton, T. I., and Haug, G. H.: Iron Fertilization of the Subantarctic Ocean During the Last Ice Age, Science, 343, 1347-1350, 10.1126/science.1246848, 2014.

Schneider, R., Schmitt, J., Kohler, P., Joos, F., and Fischer, H.: A reconstruction of atmospheric carbon dioxide and its stable carbon isotopic composition from the penultimate glacial maximum to the last glacial inception, Climate of the Past, 9, 2507-2523, 10.5194/cp-9-2507-2013, 2013.

Ziegler, M., Diz, P., Hall, I. R., and Zahn, R.: Millennial-scale changes in atmospheric CO2 levels linked to the Southern Ocean carbon isotope gradient and dust flux, Nature Geoscience, 6, 457-461, 10.1038/ngeo1782, 2013.

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