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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 #2

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Shin et al. present new records of ice core CO2, CH4, and d15N from the EPICA dome C core during MIS 6. They show that the CO2 maxima tend to lag Antarctic d18O maxima, by an amount that increases thought the glacial period. The magnitude of CO2 increase scales with the duration of North Atlantic stadial period, suggesting a key role of AMOC variations in millennial-scale CO2 variability.

The data and analysis are obviously of interest to the broader paleoclimate community, and this paper should be published with only minor corrections. I believe that the manuscript can be clarified in some places. The main conclusion seems to be that MIS6 behaves very similarly to the last glacial period (as expected). Owing to this

similarity, the discussion is somewhat long and involves a lot of speculation – much of which has already been said in earlier work (for example Bereiter et al. 2012).

Throughout the paper the authors present speculative climatic mechanisms, or uncorroborated results from individual model simulations as established fact. One example: "Due to the reduction of Summer Monsoon intensity in East Asia, salinity at the surface of the Pacific Ocean is increased. Thus, AABW and North Pacific Deep Water (NPDW) transport is enhanced (Menviel et al., 2014). Enhanced NPDW transport ventilates deep Pacific carbon via the Southern Ocean which may lead to atmospheric CO2 increases." While this is not a bad description of Menviel 2014, I think this would be better presented with some caution because while possibly correct, this is in no way a consensus view.

Throughout the paper, the authors compare MIS 3 and MIS6. In several places the authors write that MIS3 and MIS6 had different "background conditions". I am not sure what is meant by that. In what way are they really different? Both periods represent a range of orbital conditions, sea ice volumes, ITCZ positions, Heinrich events etc. So there are many places where they are very similar. I would advise the authors remove this idea that these two glacial periods are somehow very different – I don't think they have made the case that they are (and their data surely suggest that the carbon cycle responds in a very similar manner).

Due to the historical convention the last ice age is actually MIS2-4, rather than just MIS3. So a more meaningful comparison would be MIS2-4 to MIS6. Also, the authors also include MIS5 in their analysis (Fig. 8). I think the paper would be a lot simpler if the authors just claim to be studying millennial-scale CO2 variability, rather than focus on Marine Isotope Stage distinctions that may not be relevant.

In all figures I would appreciate a more clear demarcation of the sub-sections. I am not very familiar with the MIS6a-6e definitions. Do they follow precession/Benthic sequences like in MIS5, and who has defined these? Could you please add the MIS5 and

MIS6 (and MIS7?) sub-stage numbering into figures 1, 4 and 5. Also, for consistency you should mark the H-events of stage 6 in Fig 1. Where does the event numbering 6.e1 etc. come from? I have seen alternative numberings elsewhere in the literature.

Could you please add the synthetic Greenland reconstruction from Barker et al. 2011 to Figure 3, to see how it compares?

The authors do not address the CO2 offset between the records enough. It is up to 10 ppm with Vostok, which is quite large. They explain this as due to the blank correction, which is only around 1.7 ppm and therefore insufficient. Such offsets are seen more often in comparing CO2 from different cores, and may actually be in the ice. Can you explain the EDC CO2 offset between this work and Lourantou?

Specific line-by-line comments:

P1L16: I don't think you can argue that the background conditions are different. That hasn't been established.

P2L11: Broecker does not talk about the bipolar seesaw, but a seesaw in deepwater formation. Other references to consider are Blunier & Brook (2001); Pedro et al. (2018).

P3L6: Normally a stronger monsoon is not associated with a weaker AMOC. How does this work?

P6L9: Add or replace with Etheridge et al (1992); this idea is much older.

P5L5: The "assumption" that the bipolar seesaw was present is a pretty obvious one, and I don't think it needs to be questioned. My personal choice would have been to use Antarctic isotopes to define the stadials and interstadials (see e.g. Kawamura et al., 2017), rather than NA sediments that have much poorer age control.

P7L31: the offsets persist in periods of stable CO2, suggesting there is more than chronological error going on. Please discuss offsets between the cores.

P10L19: again, the link between monsoon and AMOC does not make sense to me

P12L7: upwelling or ventilation /de-stratification?

P12L15-16: Anderson does not cover MIS6, plus those records lack the resolution to investigate short stadials.

P13L22: yet the CO2 variations of MIS5 are larger than those in MIS3?

P14L22: remove "unprecedented". Some ice core CO2 records have decadal precision.

P21: Could you add the H-events of Stage 6 also (or perhaps an IRD record that spans the full period)? Could you mark the MIS6a-6e substage numbering? (I am not familiar with this nomeclature).

P26: could you add the DO onsets you infer from CH4 as vertical bands?

P27: Why did you not add the Stage 5 events here?

References:

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