



EGUsphere, referee comment RC2  
<https://doi.org/10.5194/egusphere-2022-574-RC2>, 2022  
© Author(s) 2022. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## **Comment on egusphere-2022-574**

Anonymous Referee #2

---

Referee comment on "Predicting trends in atmospheric CO<sub>2</sub> across the Mid-Pleistocene Transition using existing climate archives" by Jordan R. W. Martin et al., EGU sphere, <https://doi.org/10.5194/egusphere-2022-574-RC2>, 2022

---

Review „Predicting trends in atmospheric CO<sub>2</sub> ...“ by Martin et al.

### **Overall assessment**

The paper by Martin et al. represents a very simple, statistical (further on loosely called regression) model to estimate past atmospheric CO<sub>2</sub> from the LR05 stack of benthic δ<sub>18</sub>O. As LR05 is a combined record of deep ocean temperature and ocean volume (not of CO<sub>2</sub>) the regression of CO<sub>2</sub> with LR05 is only statistical in nature and does not include a direct causal connection. Accordingly, a good predictive skill of LR05 to calculate CO<sub>2</sub> beyond its calibration period (the last 800 kyr) cannot be expected. Not surprisingly, the predicted CO<sub>2</sub> does not closely reflect the limited data we already have about CO<sub>2</sub> in the MPT from blue ice snap shots and CO<sub>2</sub> reconstructions based on δ<sub>11</sub>B in foraminifera.

Based on this disagreement, the authors conclude that the null hypothesis of "a common global climate - carbon cycle - cryosphere feedback across the MPT" must be rejected. This is correct in a purely statistical sense, however, without laying out what exactly the causal relationship is between the three Earth System components and why these could be imprinted in the LR05/CO<sub>2</sub> regression, the null hypothesis appears to be not well justified. Accordingly, I think the minimum the author have to do to their manuscript is to discuss this connection and to bolster the justification of the null hypothesis. Another point of criticism could be raised that also the existing CO<sub>2</sub> from blue ice and δ<sub>11</sub>B may contribute to the difference between observed and predicted CO<sub>2</sub>. For example, the very old ice from the bottom of blue ice areas may be subject to diffusional smoothing of CO<sub>2</sub>. This could explain that the minimum (glacial?) values found in the blue ice are higher than the true atmospheric values, however, it would not be in line with the (interglacial?) blue ice maxima in CO<sub>2</sub> being also higher than the prediction. Also the limits of the δ<sub>11</sub>B reconstructions have to be better laid out as they are strongly dependent on the input parameters that are used to calculate CO<sub>2</sub> from δ<sub>11</sub>B and also from the CO<sub>2</sub> saturation state at the marine drilling site in the past, as also illustrated by the relatively large uncertainty of the δ<sub>11</sub>B reconstructions compared to ice core records.

In summary, while the study by Martin et al represents an interesting exercise (as was the initial EPICA challenge published in a non-peer reviewed journal), the question remains, whether this contribution in its present form provides sufficient new insight to justify publication in CP.

**Specific comments:**

line 16 : "is to make"

line 17 and throughout the manuscript: Myr instead of myr

line 25: the authors state that the null hypothesis should be rejected, however, without laying out the causal relationship between the regression parameters and potential reasons why the regression may not hold back in time, this statement is not entirely satisfying.

line 58-59:  $\delta^{18}O$  is not just a sea level proxy but also influenced by deep ocean temperature. A process-based discussion of why LR05 is a viable input parameter to predict CO<sub>2</sub> is required.

line 66: please include also the record by Dyez et al., *Paleoceanography* 2018

line 68: The very old ice at Allan Hills is not really from the surface but from a shallow ice drilling of more than 100 m depth

Methods: the uncertainty in the regression connected to the independent age scales should be discussed

line 85: not clear what  $r(226)$  means, please explain. Did you allow for lag correlation? (see also comment on age scales above)

line 89: the limitations of blue ice CO<sub>2</sub> reconstructions and d11B reconstructions of CO<sub>2</sub> should be discussed as well