EGUsphere, referee comment RC2
https://doi.org/10.5194/egusphere-2022-574-RC2, 2022

Comment on egusphere-2022-574

Anonymous Referee #2

Referee comment on "Predicting trends in atmospheric CO_2 across the Mid-Pleistocene Transition using existing climate archives" by Jordan R. W. Martin et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-574-RC2, 2022

Review „Predicting trends in atmospheric CO2 ...” 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 CO2 from the LR05 stack of benthic d18O. As LR05 is a combined record of deep ocean temperature and ocean volume (not of CO2) the regression of CO2 with LR05 is only statistical in nature and does not include a direct causal connection. Accordingly, a good predictive skill of LR05 to calculate CO2 beyond its calibration period (the last 800 kyr) cannot be expected. Not surprisingly, the predicted CO2 does not closely reflect the limited data we already have about CO2 in the MPT from blue ice snap shots and CO2 reconstructions based on d11B 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/CO2 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 CO2 from blue ice and d11B may contribute to the difference between observed and predicted CO2. For example, the very old ice from the bottom of blue ice areas may be subject to diffusional smoothing of CO2. 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 CO2 being also higher than the prediction. Also the limits of the d11B reconstructions have to be better laid out as they are strongly dependent on the input parameters that are used to calculate CO2 from d11B and also from the CO2 saturation state at the marine drilling site in the past, as also illustrated by the relatively large uncertainty of the d11B 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 from 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: d18O 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 CO2 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 CO2 reconstructions and d11B reconstructions of CO2 should be discussed as well