



EGUsphere, referee comment RC1
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Comment on egusphere-2022-574: Very simple analysis

Anonymous Referee #1

Referee comment on "Predicting trends in atmospheric CO₂ 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-RC1>, 2022

This manuscript presents a null hypothesis prediction for CO₂ across the MPT based on generalized least squares regression between Late Pleistocene CO₂ records from Antarctic ice and the LR04 global benthic δ₁₈O stack, a proxy for changes in global ice volume and deep water temperature. The regression-based predictions are then compared with sparse MPT CO₂ estimates from blue ice (Higgins et al, 2015) and boron isotopes (Chalk et al, 2017) with respect to mean value and glacial-interglacial range and compared to trend in CO₂ from an intermediate complexity model run across the MPT (Willeit et al, 2019). The authors argue that misfit between pre-MPT CO₂ values and the regression-based predictions would be evidence for a change in climate-carbon cycle-cryosphere dynamics across the MPT.

The analysis appears to be performed well, and I have only a few concerns about the interpretation of the results. However, my main concern is that the work is too simple. I would encourage the authors to add more intellectual substance to the paper by exploring perhaps nonlinear regression between benthic δ₁₈O and CO₂ or discussing in more depth the underlying mechanisms relating benthic δ₁₈O and CO₂ to say more about the implications of potential misfit between CO₂ and the regression-based estimate.

Additional Specific Concerns

Abstract, line 18: I think the authors meant benthic foraminiferal stable isotope (δ₁₈O). The δ₁₈O data used is from foraminiferal calcite, not "water."

Line 118: It is not clear what the authors mean by "This trend is seen in our predicted record, and in the filtered BI-CO₂ and BOR-CO₂ data (Fig. 1C)." The previous sentence describes glacial stage CO₂ draw-down and the absence of an interglacial draw-down. In Fig. 1C, it appears that this description holds for the predicted CO₂ record (i.e., glacial draw-down but steady interglacial values). However, the BI-CO₂ and BOR-CO₂ data show a change in BOTH glacial and interglacial CO₂ compared to the post-MPT average. The

text should be revised to make clear which trends are similar between the predictions and observations and which are different.

Line 185-186: The authors need to explain why out-of-phase responses in northern and southern ice before the MPT (as proposed by Raymo et al., 2006) would lead them to expect "large discrepancies" between their regression-based CO₂ prediction and the realized data. This inference seems to rely on the assumption of a certain relationship between CO₂ and northern or southern ice sheets, but I'm not sure what relationship the authors are assuming. Section 4.2 overall is quite short and would benefit from a more in-depth, process-based discussion of implications of the anti-phased hemisphere hypothesis for pre-MPT CO₂ variability.