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Comment on egusphere-2022-574: Very simple analysis

Anonymous Referee #1

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-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 d18O 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 d18O and CO2 or discussing in more depth the underlying mechanisms relating benthic d18O 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 (d18O). The d18O 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 CO_2 prediction and the realized data. This inference seems to rely on the assumption of a certain relationship between CO_2 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 CO_2 variability.