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Climate of the Past
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Interactive comment on "Sequential changes in ocean circulation and biological export productivity during the last glacial cycle: a model-data study" *by* Cameron M. O'Neill et al.

Anonymous Referee #2

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SUMMARY

This paper uses the SCP-M model - an 11-box model of the ocean carbon cycle - to simulated changes in ocean circulation, marine productivity, and resulting changes in atmospheric carbon dioxide during distinct Marine Isotope Stages (5a-e, 4, 3, 2, and 1) over the past 130,000 years. The model is forced using reconstructed quantities for SST, salinity, ocean volume, sea ice cover, and reef carbonate productivity. The model outputs for atmospheric CO2, atmospheric d13C, D14C, oceanic d13C, and deep/abyssal carbonate ion concentrations were optimized against estimates for these parameters that were obtained from global reconstructions. Following optimization,

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optimized estimates of global and Atlantic meridional overturning rates and Southern Ocean biological productivity are presented. These results suggest: (a) global overturning rates responded early (MIS5d) in the glacial cycle; the largest response in the AMOC occurs between MIS5a and MIS4, with low rates also during MIS2; Southern Ocean biological productivity descrease early during glaciation (MIS5d) and recover gradually during each successive MIS, exceeding interglacial values during MIS3 and reaching peak values during MIS2. An interesting contribution of this paper is the use of previously compiled ocean tracers for the last glacial-interglacial cycle as a means of optimizing these box model results. In particular, the inclusion of sparse Indo-Pacific data in the optimization is an interesting and useful expansion from the predominantly Atlantic-centric perspective on glacial-interglacial global ocean circulation changes. This inclusion leads to the interesting result that early changes in the global overturning circulation is a possible strong contributor to early changes in carbon dioxide. The authors support their simulations by suggesting that previously hypothesized ideas such as early AABW expansion and weakened circumpolar deep water upwelling could result in reduced global overturning, which influenced the abyssal deep Pacific carbonate ion concentrations and d13C values. They suggest with their box model simulations that reduced GOC can influence d13C in the abyssal Indian and Pacific Ocean sectors without necessarily resulting in a strong Atlantic abyssal d13C response. Obviously, the use of box-model simulations has its limitations (i.e. the generalization of the whole ocean in a box model context, which necessarily brushes over many processes and regional variability). Furthermore, the authors base these conclusions of Indo-Pacific change in carbonate ion concentrations on a single core. That said, their proposal that an early glacial response in GOC could be responsible for early CO2 drawdown is an interesting new contribution to the field.

I provide detailed comments below. Of substantial concern is the qualitative treatment of data in the Methods section, which I think could be more rigourous given that the authors base some of their most important conclusions on optimizing their results to these data. First, the authors should filter their d13C results to include only species known to represent deep ocean d13C changes and classify what they mean by "abyssal" and "deep" in their data (comments 7-8); second a better quantification of their observed changes for the MIS periods is warranted (I suggest examination of probability density functions and whether differences are statistically significant) (see comments 9-10, 13). Second, I think a more thorough description of their model would be useful to readers who wish to understand, e.g., what drives biological productivity export in their model, and what their southern ocean box actually represents in terms of the real ocean (comment 6).

COMMENTS

1. The authors base their paper on a recently published carbon cycle box model (O'Neill et al. 2019). They provide a brief description of the model but I found that this manuscript would benefit better description of some of the key parameters that are quite important to this paper, such as the controls on Z (biological productivity). It was very unclear to me on first reading how values of Z were ascertained.

2. Figure 1 – this graphic, while nice and colourful, is challenging for reading the actual numbers and symbols (especially the white ones which do not show up at all on my colour print). Readability is more important than colour! I suggest making box numbers, symbols all BLACK using larger fonts so that they are readable.

3. Pg 5 lines 15-17. This sentence seems out of place: "Therefore, our modelling excludes the last glacial termination (∼11-18 ka)." Should it occur before the previous sentence?

4. Section 2.2.1 Model forcings: Although the authors ultimately conclude that sea ice cover – as a barrier mechanism constraining air-sea CO2 exchange – is not that important, they authors should emphasize limitations of their use of the ice core sea ice proxy. First, this proxy is non-linear, so their simulations probably over estimate early (MIS5d) sea ice cover and underestimate later (MIS4-2) sea ice cover. This point is made very clearly by Wolff et al. 2010 (and supports) the authors' assertion that the

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barrier effect of sea ice early in the glaciation is probably small.

5. Furthermore, it is worth pointing out somewhere in the discussion that this modeling exercise only examines the potential role of sea ice as a barrier to CO2 exchange, and not its synergistic (and likely more important) roles in influencing nutrient distributions, marine productivity, and a trigger for deep ocean circulation changes. The authors state this somewhat in their "Advantages and limitations" section, but I think that this point could be made more explicitly.

6. Another larger issue that the sea ice proxy highlights is the spatial heterogeneity of the Southern Ocean and how the model results are linked with reality: the sea ice proxy likely represents changes very close to the continent and early glacial changes in sea ice are not well reproduced in the few long sea ice records that are found near the APF. This not only suggests that a barrier effect of sea ice would be limited to only part of the Southern Ocean, it points to larger issues with treating the Southern Ocean as one box, with an unclear delineation of how much of the S. Oc. this box is presumed to cover. If the box is supposed to ONLY cover those areas close to the continent where AABW and Circumpolar Deepwater processes that influence GOC are most important, then the authors' main conclusion of increases in S. Oc. export production aren't well supported by paleoceanographic data which show reductions in export South of the APF for the majority of the glacial cycle between MIS5d and MIS2. Some discussion of what the Southern Ocean box actually represents - and this potential disconnect with paleoceanographic data - is warranted.

7. Throughout the paper the authors refer to "abyssal" and "deep" water masses for all basins, but I was never able to find the depth cut-offs that were used to distinguish these depths in the different basins. Please put them in the figure captions and text (not just supplemental information, if it is there.)

8. The authors discuss briefly that previous studies have only used the C. wuellerstorfi data to reconstruct deep ocean d13C (Peterson et al. study; Kohfeld and Chase

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study). Which data did these authors select from Oliver et al. (2010)? They mention only using "deep" and "abyssal" sites (again, depths undefined) on page 11, but they do not indicate whether they have filtered the data to only include C. wuellerstorfi (or even Cibicidoides spp), which they SHOULD be doing if they haven't. Otherwise, the changes in d13C described on page 12 are invalid as descriptions of deep ocean circulation changes in d13C.

9. On Page 12, the authors qualitatively describe the differences between "deep" and "abyssal" changes in d13C. Why leave this discussion qualitative, when the data are available and quantification would be hugely useful. These data in the Pacific that are described are the data that pin the authors' entire argument surrounding early changes in GOC. I think that this warrants a bit more quantification of these data (once species other than Cibicidoides are filtered out of the dataset). I would be interested to know if the differences between deep and abyssal d13C in the Indo-pacific are statistically significant, and I think plots of the probability distribution functions of these data would be very useful.

10. Some type of quantification would also be very useful for the authors' description of the "transient drop in abyssal Atlantic ocean CO3= at MIS5b" on page 14. I was not convinced that this transient drop exists from the figure presented.

11. Please note on the bottom of page 13 and top of page 14 that the authors mean to refer to Figure 7 (not 6) to describe carbonate ion concentration data.

12. Last sentence before Results section: Please cite the figures you are using to make these observations about changes in d13C and DD14C

13. Similar quantification would be useful in the comparison between the carbonate ion concentration model output and data in Figure 9 and in the discussion on page 16-17.

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