Sequential changes in ocean circulation and biological export productivity during the last glacial cycle: a model-data study.

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First, I appreciate the enormous amount of work that the lead author has done to address the comments of the three prior reviewers, evident by the additions to the methods section and the response to reviewers. In undertaking this review, I have therefore chosen not to focus on minor issues, nor comment on the length, writing or quality of figures, but will concentrate on whether the interpretations/lessons from the study are valid.

The authors use a simple box model of the ocean with attached atmospheric and terrestrial components to understand what happened to ocean circulation and Southern Ocean biological carbon export over the full glacial cycle, from MIS-5e to the Holocene. As far as I can tell, the model takes SST, global mean salinity, sea volume, sea ice cover, carbonate reef production, global overturning circulation rate, Atlantic meridional overturning rate and biological carbon export as inputs. Values of SST, global mean salinity, sea volume, sea ice cover, carbonate reef production and biological carbon export outside of the Southern Ocean are fixed for each experiment at each MIS. The authors chose to vary the remaining three inputs, global overturning, Atlantic overturning, and biological carbon export in the Southern Ocean in thousands of combinations at each MIS. They then compared simulated atmospheric pCO$_2$, $\delta^{13}C_{CO2}$ and $\Delta^{14}C_{CO2}$, as well as oceanic $\delta^{13}C$, $\Delta^{14}C$ and carbonate ion concentrations in deep and abyssal waters with the equivalent paleoproxy values, and fit a linear least-squares optimisation with using these 9,000 simulations to find the "optimal" values of global overturning, Atlantic overturning, and biological carbon export in the Southern Ocean at each MIS.

The authors then discuss the trends in atmospheric and oceanic proxy data and review the physical mechanisms that drove these trends, referencing prior work. This analysis lays the foundation for their quantitative work with the model.

Following a comprehensive introduction and description of their tools and palaeoproxy data, the authors make a brief description of their results, finding declines in both overturning rates (global and atlantic) at slightly different times during the glacial, and an increase in southern ocean carbon export at MISs 4 and 2, as the major changes needed to explain the proxy records. A large contribution of SST decline to CO_2 drawdown was also found. Other processes (salinity, ocean volume, reef calcification/dissolution, terrestrial carbon store) were of minor importance.

The approach is very interesting and insightful. Although it is not a truly transient simulation, it is a welcome addition to the field and deserves publication. It also is not hugely controversial, as the results echo other studies calling for a decline in overturning rate and increased southern ocean productivity during the glacial, something that the authors recognise and discuss.

However, one finding that is particularly interesting is how the slowdown of the GOC at MIS 5e-5d explains the first drop in CO_2 while showing little change in $\delta^{13}C$. As far as I am aware, this is an important finding that should be shared with the community so that more complex models can be used to further test this, as the authors allude to in their discussion. I have long wondered at the absence of change in $\delta^{13}C$ at the transition between MIS 5e-5d and thought that this must be explained because surely such a drop in CO_2 must involve changes in ocean circulation.

Overall, I strongly advocate for publication with minor revisions/clarifications. I disagree with reviewer 1's request to remove the $\delta^{13}C$ from the paper. Sure, the paper like any other has its shortcomings and limitations, but the results are in my opinion worth publishing. It would be a shame to bury them. Moreover, the substantial work done in the response to reviewer 1, particularly in regard to their concerns about MIS-averaging and the treatment of carbonate chemistry, clearly shows the legitimacy of the model, their approach and the findings.

I ask for the following revisions/clarifications:

1. It should be more clearly stated how the authors calculate their standard deviations in their optimisation approach. I would also appreciate a figure/table/paragraph that ranks the most important variables in the optimisation procedure per box or per MIS, whichever makes the most sense and doesn't add too much writing. Clearly, atmospheric CO_2 and SST, with small standard deviations, will likely be strong contributors to the optimisation, while CO_3 with a standard deviation of 15 uM and changes of < 30 uM must be small. But some more clarity on how the different variables contributed would be good.
2. I needed to read O'Neill et al (2019) GMD to find out that the SCP-M does not hold biological production (Z) constant in non-Southern Ocean boxes. What happens to these boxes in the simulations? Even after reviewer 2 asked for this, I still think that this should be made much clearer. Is it, for instance, dependent on phosphate?
   Logically, extra-SO carbon export should decline as the GOC declines because less nutrients would be supplied to surface waters in the lower latitudes. Surely, if you simulate phosphate concentrations explicitly then the carbon pump should respond? Is this so?
   I need more information on what is happening. The reason I need more information is because I would expect the slowdown in GOC to slow down carbon export. If this does not happen, then this could be the reason why purely physical mechanisms are responsible for a drawdown of 70 ppm, and biological effects are less important. If the biological pump were allowed to respond to the global slowdown in overturning, then the physical contribution would be weaker than presented because more carbon would escape via the tropical upwelling as your so rightly state with the reference to the Takahashi paper. However, if a more effective carbon pump was to develop in the low latitudes, for instance by increases in C:P ratios (Matsumoto et al., 2020) and/or via N_2 fixation (Buchanan et al., 2019), this would ensure that the influx of carbon into the ocean via physical mechanisms would be prevented from outgassing by a tighter biological lid. The two must ultimately work together.
3. As far as I can tell, the authors simulate an increase in Southern Ocean production during MIS 2 and MIS 4 without applying the increase in dust deposition that is seen during these periods. It is therefore noteworthy, and should be emphasised further, that the increase in Southern Ocean production at MIS2 and MIS4 emerged independently without needing to provide the dust record to the model, and yet aligns with it. I think that this is a striking finding that needs more emphasis. Although, it is important to say that the model does not (or does it?) alter carbon export outside of the Southern Ocean, which may also have been important for "tightening the lid" on the ocean carbon store (as I've discussed above).
4. I don't follow the logic of paragraph 4 in the Discussion. First talks about the model results compared with the analysis of Kohfeld & Chase (2017), then diverts to Stephen & Keeling (2000) Antarctic sea ice changes. I suggest making the narrative of your discussion clearer here.

5. It is important to mention that the contribution of SST is very likely overestimated, given your use of box model rather than the general circulation model. Box model atmospheric CO_2 is known to be more sensitive to the SST changes in the higher latitudes compared with general circulation models (Archer et al., 2000). This should be stated clearly in the discussion section as a caveat. I want to know how the results might change if the exercise were repeated with a GCM, as this may inspire others.
6. Page 5, line 8 – Don't you mean 40S-60N?

References

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Matsumoto, K., Tanioka, T., and Rickaby, R. (2020). Linkages Between Dynamic Phytoplankton C:N:P and the Ocean Carbon Cycle Under Climate Change. *Oceanography* 33. doi:10.5670/oceanog.2020.203.