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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.

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This paper describes the application of a carbon cycle box model on the last glacial cycle (last 130 kyr). The model has been described before, but is modified here for the application to paleo-timescales. A great part of the effort is the compilation of available paleo-data to which simulations results are compared.

For this effort steady-state results for mean values for each of the Marine Isotope Stages (MIS) from 5e-1 are evaluated, while periods with rapid changes (eg glacial inception, Termination I) are not investigated. An optimisation approach is then used to derive the parameter values of a few important processes, namely Global Ocean Circulation, Atlantic Meridional Ocean Circulation, and Southern Ocean biological pro-

ductivity.

The approach is in itself an interesting piece of work, that combines data analysis with modelling, but I have two major concerns, that need the attention of the authors: (1) the shortcomings of the steady-state approach, and (2) the δ^{13}C cycle.

Shortcomings of the steady-state approach

The chosen approach of steady-state analysis combined with optimization is a way, which certainly has benefits, but also shortcomings. I believe the benefits lie in the possibility to test a great number of parameter values, and this is certainly analysed with great effort and detail and worth publishing (but see my recommendation on shortenings of certain parts below). However, there is little learned on the potential shortcomings and pitfalls, which in my view need to be discussed more deeply. I believe where this approach is falling to short is the following: By analysing only steady-state the authors miss out the opportunities to judge the results based on the timing (when do processes change leading to what results).

I provide one example where the article nicely fails, producing a potenially right answer for very likely the wrong reason: One of the dominant features of atmospheric δ^{13}C during the last glacial cycle is a drop by about 0.5‰ during MIS4. The steady-state approach now leads to the evalution of a mean value of atmospheric δ^{13}C which does not really cover this decrease at all, it shows about a decline by about 0.2‰ from MIS5a to MIS4 (Fig 4). So, any explanation for this drop would be falling too short in the observed amplitude by 0.3‰. Note that this δ^{13}C feature is not rapid, it is an anomaly that has been detected from raw data by spline smoothing and is alltogether nearly 20 kyr long, however the decreasing flank falls in MIS4, the increasing flank in MIS3, thus the signal is largely smoothed out in the chosen MIS-centric analysis. The analysis of the results now comes to the conclusion that very likely changes in terrestrial carbon storage was responsible for a change in atmospheric δ^{13}Căof -0.2‰ (as said explaining

a too little amplitude), it is furthermore said that the drop is accompanied by a 30 ppm fall in CO_2 (page 12, lines 1-5), citing Hoogakker et al., 2016. I believe this is entirely wrong: The drop in CO_2 happens clearly a few kyr **before** the drop in atmospheric δ^{13}C, as seen in Fig. 4. Furthermore, since both CO_2 and δ^{13}C are meassured at the same samples and are both derived from gases in ice cores, this temporal offset between CO_2 and atmospheric δ^{13}C can not be explained by chronological issues. The anomalies in biosphere as documented by Hoogakker et al., 2016 all fall in line with the CO_2 changes, but not with the δ^{13}C changes, also note that Hoogakker et al., 2016 was published before the atmospheric δ^{13}C data set of Eggleston et al (2016). In that respect citations from Hoogakker on page 19 are also missing the correct timing: In Hoogakker NPP drops between around 70 ka (parallel to the drop in CO_2), while the δ^{13}C drop occurs 5 ka later. Also note, that in Eggleston et al. (2016) the authors of this atmospheric δ^{13}C record tried to make sense of it by focusing on the part in which δ^{13}C falls, but CO_2 rises again (Fig 2 in that paper) focusing on an opposite behaviour than described here.

The second most dramatic change in atmospheric δ^{13}C is a sharp drop by 0.2‰ during Termination I, a time window which has been chosen to be not be included in this steady-state analysis, again missing the opportunity to use ^{13}C to pin down responsible processes. Only the long-term trend in δ^{13}C of +0.2‰ from the penultimate interglacial to the Holocene seemed to be meaningful covered by the approach.

The δ^{13}C cycle

As already seen above the steady-state approach might not be the best way to tackle atmospheric δ^{13}C. Furthermore, for an evaluation of δ^{13}C in general in such steady-state experiments as performed here the fluxes (e.g. as mol C/yr) and δ^{13}C-signatures in/out of the simulated atmosphere/ocean carbon cycle are essential: atmosphere-land carbon fluxes, volcanic CO_2 outgassing, weathering, and burial of organic and inorganic carbon in the sediments. Little to non of those fluxes (and δ^{13}C-signatures)

are given in the text itself. If I dig into the python source code of the model (or the description of version 1 in O'Neill et al. (2019)) I find a few information, but the source code is difficult to interpret as a non-user and some information seemed to be either misleading or wrong. An examples:

Continental weathering consists of two different processes depending on the rock type that is weathered. In carbonate weathering 1 mol of $CaCO_3$ together with 1 mol of CO_2 from the atmosphere leads to the entry of 2 mol of HCO_3^- into the surface ocean. In silicate weathering 2 mol of atmospheric CO_2ăare necessary to weather 1 mol of $CaSiO_3$ leading again to the entry of 2 mol of HCO_3^- into the surface ocean. For details see, for example Lord et al. (2016). From the description of weathering in O'Neill et al. (2019) I have the impression that the carbonate weathering is not depicted correctly (no consumption of atmospheric CO_2). Furthermore, from the python code I learned that weathering (probably meaning carbonate weathering, since in silicate weathering all CO_2 comes from the atmosphere with its δ^{13}C-signature) has a δ^{13}C-signature of -6.9‰, similarly as volcanic CO_2. While the volcanic δ^{13}C seems to be in the expected range (although on the lower side) I believe the weathering δ^{13}C-signature is wrong, since carbonate rocks have a typical δ^{13}C-signature of about +1-2‰, see for example Sano and Williams (1996); Mook (1986).

I also do not understand how their approach with not explicitly considering terrestrial carbon change (terrestrial carbon to my understanding is covered as externally to the atmosphere/ocean system, fluxes in/out of it prescribed by optimization) covers changes in C3 vs C4 photosynthesis (which have a significantly different isotopic fractionation) on glacial/interglacial timescales (Collatz et al., 1998; Köhler and Fischer, 2004) which leads to differences in the mean terrestrial δ^{13}C and therefore also the changes in the δ^{13}C-cycle as a whole (Kaplan et al., 2002).

Overall recommendation

My recommendation therefore is, that the model in its present form might be a useful tool for evaluating marine processes, and might be well used together with the available marine data (apart from δ^{13}C), but fails to give meaningfull results for the δ^{13}C cycle. This includes atmospheric and marine δ^{13}C. I urge the authors to get those parts out of the manuscript. If they wish to further analyse the δ^{13}C-cycle I believe fundamental model improvements are necessary, that can not be obtained by a major revision, but by a revised model version. Besides this, shortcomings of the steady-state approach should be discussed in more detail and the unclear (wrong?) aspects of carbonate weathering and annual fluxes in/out of the simulated system (atmosphere/ocean) need to be clarified for each MIS, maybe in a table or a new figure.

Minor issues in chronological order:

1. Figure 1: It is not clear, how GOC (red arrows) is split up in the part upwelling in Atlantic and Indo-Pacific Ocean.
2. Figure 1: Does your approach focusing on changes in GOC, AMOC and export production inply, all other proceses (fluxes) stay constant in time?
3. Figure 12: x-axis is wrong, eg. MIS5e is between ∼114-122 ka, while it has been between ∼118-128 ka in other figures.
4. With respect to iron fertilisation you might check on Shaffer and Lambert (2018).
5. The fact that not one single process is needed to explain LGM-Holocene carbon cycle changes is long known, and has been called “the carbon stew” by some authors. You might want to check and discuss in more detail earlier modelling approaches on one glacial cycle (or longer), for example in Ganopolski and Brovkin (2017).

6. Figure 14: Changes in CO_2 caused by changes in terrestrial NPP and carbon stocks are missing in this figure. Please add.
7. Section 5.3. You might want to check on recent finding of terrestrial carbon storage from δ^{13}C in Jeltsch-Thömmes et al. (2019).

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