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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 #3

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O'Neil et al. try and attribute the different changes in pCO2 that occurred from the LIG to the Holocene to changes in oceanic circulation and Southern Ocean (SO) biology. For this, they perform a series of simulations of all the MIS 5 through 1 with a box model forced by changes in SST, salinity, and sea-ice as derived from proxy records. They further vary the values of the Atlantic and Global overturning circulation as well as SO biology over a wide range of possible values. They then chose the simulations,which best fit with paleo-records (D14C, d13C, CO3(2-)...). It is an interesting attempt, which can inform on carbon cycle changes from the LIG to the Holocene. Please find below some comments that should be addressed.

Major comments: 1) The "data analysis" section 3 presents the changes in atm. CO2, d13CO2, oceanic d13C, D14C and CO3(2-) as inferred from proxy records from the LIG to the LGM. This is obviously a huge task, but which I am afraid can give rise to approximations and simplifications. I would consider seriously amending this section. How can the "increase in d13C across the glacial cycle be attributed to the growth of tundra at high latitudes"? (p12, L. 2-3). p12, L. 11-14: How were the values for MIS3 DD14C in the Atlantic derived? From Fig. 6a, it looks like there is no data across MIS3. This is quite a shortcut to explain the deglacial D14C decrease, and maybe you want to check the references and include " increase in Southern Ocean ventilation" above anything else. P14, L. 5-6: This reads like speculation.

2) Fit with the data: 50 umol/L as an "arbitrary standard deviation' for [CO3] is huge and represents more than the [CO3] changes (0-30 umol/L) recorded across the G-IG cycles. How much was taken for the standard deviation for d13C adn D14C? It looks quite large. Figures 9-11 would gain in having a more appropriate range in the y axis. At the moment the ranges and std are large, so that it almost looks like there are no changes from MIS5 to MIS 2.

3) References: In general I find that only a few references are used over and over and sometimes not appropriately. A few additional references are included in this review. Please note the typo throughout the document in "Ridgwell".

Specific comments: 1) Abstract: The first line does not make sense. Please reformulate. L. 3 Please add "SO" in front of "biological productivity"

2) Introduction: - L.15-19: please be more specific. Instead of "Ocean biology" you might want to refer to "iron fertilisation and its impact on nutrient utilisation", or changes in remineralisation depth (e.g. Kwon et al. 2009). What do you mean by composite mechanisms? - It would be good to also introduce the numerous modelling studies that have been done on the topic of G-IG changes in pCO2, and notably transient simulations of the G-IG trying to understand the changes in pCO2 (e.g. Ganopolski &

Brovkin 2017, Menviel et al., 2012).

3) Methods: - Variables included in the model: surely the model includes Dissolved Inorganic Carbon. By “CO2”, do you mean atmospheric CO2? Does the model really includes “carbonate ions” as a prognostic tracer? - p4, L. 2: please refer to section 2.2.1 and Figure 2. - p7: I am very confused by the treatment of the terrestrial biosphere in the model and the paragraph L. 19-27. It reads like there is an interactive terrestrial module. But how can NPP be calculated with significance if there is no atm. Temperature or precipitation in the model? Why is “tundra”discussed with such emphasis in this paragraph? Tundra is not an “inert” carbon pool, and I don’t think “permafrost” is a vegetation type. What is “pre-carbon fertilisation” - p8: what is the point of Table 1 if all the values of GOC, AMOC, biology are the same? It would be interesting to mention the PI control values though. - p10-11: The ’depth issue” should also be discussed in 2.3.1 and 2.3.2.

4) Discussion: p20, L. 3-6: It is not what the simulations tell you, but the proxy data! p21, L. 1-2: This is wrong → you are forcing your model with SST, Sea-ice. . .. so all these factors contribute to the pCO2 decrease. The experiments show that changes in oceanic circulation and SO biological productivity also contribute to that pCO2 decrease. Please take into consideration that G-IG pCO2 changes have been previously successfully simulated with models of intermediate complexity (e.g. e.g. Ganopolski & Brovkin 2017, Menviel et al., 2012) and box models. p21, L. 3-4: I don’t understand the meaning p21, L. 7: Might want to check Piotrowski et al., 2008, Yu et al., 2016. (Nat. Geo). p21, L. 10 –p22, L. 5: This section really has to be discussed in light of all the work that has been done on the impact of iron fertilisation in the Southern Ocean. Some work on the topic: Watson et al., 2000, Nature; Jaccard et al., 2013, Science; Yamamoto et al., 2019, Climate of the Past; p22, L. 18: “sea-ice cover” p23, L. 1-12: Figure 13 is interesting but care has to be taken here given the large size of the “boxes”. This should at least be discussed in light of previous modelling studies on the subject (e.g. Menviel et al., 2015, GBC).

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