Reviewer #2

The authors present the biogeochemical cycle implemented in CLIMBER-X. This model is one of the few models of intermediate complexity to simulate Earth system changes over many thousands of years. The model includes modules to simulate marine biogeochemical tracers, ocean-sediment interactions, and weathering as well as representation of the land biosphere, methane, and wetlands. This publication is timely and will serve the group and the community as a reference. The manuscript is generally clear and the authors manage to present a rich set of model outcomes. I recommend publications after the following comments have been addressed.

We would like to thank the reviewer for the constructive comments and suggestions on our paper. Below we provide a point-by-point response to the individual comments.

Representing ventilation time scale reasonably well is a prerequisite for simulating biogeochemical tracers in the ocean. Natural and bomb-produced radiocarbon, and CFCs are often used to probe the ventilation time scales of the ocean. The authors should present how well these different age tracers are simulated by the model.

We followed the reviewer suggestion and expanded the discussion on ventilation ages, including also CFCs, as further outlined below.

a) Results for CFC-11 and CFC-12 are not shown and discussed though mentioned in the model evaluation section 4 on line 290. Please compare simulated versus measured CFC-11 and CFC-12 distributions and inventories in additional figures and text.

In the original paper version we did not included CFCs because they are not really part of the global carbon cycle. However, CFC-11 and CFC-12 tracers are included in the ocean model following the OMIP protocol and, as suggested by the reviewer, in the revised manuscript we added figures comparing CFCs distributions to observations.

b) The marine radiocarbon distribution is presented at the very end of the ocean section (Fig. 20). However, age biases are key to understanding the biases in the various biogeochemical tracers. Please show and discuss radiocarbon results before the discussion of the other tracers.

We followed the reviewer suggestion and now discuss age biases first.

c) Please present and discuss the distribution of bomb-produced and natural radiocarbon separately

We think that adding this additional analysis would not add substantial new information on the model performance, compared to existing analyses including CFCs.

d) The discussion of biases in biogeochemical tracers (DIC, ALK, P, O2, Si, d3C…) would benefit by linking these biases to the biases in the age tracers.

In the revised version of the paper we better link biases in the age tracers with the biases in other biogeochemical tracers.

I miss a table that provides the model parameters. It would be very helpful if model parameters and key equations were summarized in a table (could also be in an appendix or as SI)

We added an Appendix describing the model equations that were modified compared to the original description papers. We also summarized the relevant parameters in a Table.

Iron limitation is prominent in the model (Fig.7). The authors should provide more detail on the iron cycle implemented in the model. How is the ratio of aeolian input versus advection of Fe in the Southern Ocean and elsewhere? How do the different iron sources of the model ocean compare with each other and other estimates? What is the role of ligands? Which parameters have been applied? What fraction of deposited iron is bioavailable? How will the balance between aeolian versus marine sources affect the model's sensitivity to glacial-interglacial dust deposition? Part of this information could be nicely incorporated in table 1.

More details on the iron cycle are now given in the Appendix describing the key model equations.

The model applies a temperature-sensitive particle remineralization rate. However, viscosity and thus particle sinking are also influenced by temperature. These two factors have opposite impacts under changing temperatures. Why is the temperature dependence of viscosity not considered? Will this cause a too-large sensitivity of atmospheric CO2 to changes in ocean temperature in glacial-interglacial simulations?

A 10°C temperature increase roughly doubles the remineralization rate but changes water viscosity (and thus sinking velocity) by only 25%. We actually did test runs accounting for the viscosity effect on sinking velocity by multiplying it by a factor reference_viscosity/viscosity following equation 3 in Taucher et al. 2014 and we found that variable viscosity only caused an increase in atmospheric CO2 by about 2 ppm in LGM simulations, compared to expected >10 ppm CO2 decrease from the temperature-dependent remineralization rate (Ganopolski & Brovkin 2017). A further reason why we didn't include the viscosity effect is that the parameterization of the sinking speed is quite crude in the model. However, we are in the process of including the M4AGO particle sinking scheme (Maerz et al. 2020) in the model, which includes the viscosity effect on sinking speeds, and this may be included in a future release of CLIMBER-X.

Conclusions: It would be nice if the authors present an outlook on future, planned (?) model improvements. For example, the implementations of flexible stoichiometry instead of constant Redfield ratios or N2O in the land biosphere appear to be possible targets.

The main purpose of this paper is to present the current version of the CLIMBER-X model which is already used for various studies. In the future (as any other model) CLIMBER-X will be further developed. Some of possible future model developments could be:

- Organic carbon cycle with burial on land and in sediments, nutrient fluxes from land to ocean
- Include refined carbonate chemistry on shelfs, including corals
- Add nitrogen cycle on land: could be important for nitrogen limitation of photosynthesis and would allow to have interactive atmospheric N2O, because N2O fluxes from the ocean are already available from HAMOCC

Title: The model remains an Earth System Model of Intermediate Complexity and this should be reflected in the title. Please replace the term Earth system model with Earth System Model of Intermediate Complexity.

To be consistent with the title of the first part of CLIMBER-X description paper (Willeit et al. 2022) about the climate component, which is already published, we decided to leave the title as it is. We think that the first sentence in the abstract makes it sufficiently clear that it is an EMIC.

L115: Please be more specific about how this virtual flux approach is implemented. Is the global net surface flux set to zero? How is the dilution effect implemented during times of net global freshwater addition/removal during ice sheet melt and formation?

Two options are available in the model to implement the dilution effect on DIC and alkalinity in the case of the global net zero freshwater flux. The first one ensures that the net global surface tracer flux is zero by applying deviations from the global average freshwater flux to the global average surface tracer concentration. The second (default) option applies the actual local surface freshwater flux to compute a new virtual top ocean layer thickness and then dilutes the tracers accordingly. In this case, the conservation of tracer inventories is ensured by compensating disbalances over the global ocean.
Additionally, if the ocean volume is changing because of buildup or melt of land ice, concentrations of all tracers are globally adjusted while conserving tracer inventories. This is a reasonable simplification, considering that land ice volume changes occur on multi-millenial time scales, over which the ocean can be considered in approximate equilibrium.
This discussion has been added to the revised paper.

L123: Could you please explain how this integration works? I guess HAMOCC in the ESM has a time step of order 30 min and is resolving daily radiation changes. How does the scheme account for sub-daily fluctuations in radiation for computing photosynthesis?

The low-resolution version of the MPI-ESM model (MPI-ESM1.2-LR), for which HAMOCC was originally tuned, does not resolve the diurnal cycle of radiation in the ocean, but is tuned for daily mean radiation. This is therefore fully consistent with the HAMOCC implementation in CLIMBER-X. *"The MPI-ESM1.2-HR and MPI-ESM1.2-LR configurations differ in the atmosphere-ocean coupling frequency (section 2). Primarily, therefore, a different tuning of biological parameters in HAMOCC6 is required, as due to light limitation of plankton growth source-sink dynamics differ when using daily mean light versus considering a day-night cycle. The default tuning of the model assumes daily mean light." /Mauritsen et al. 2019/*

L148: In CLM4.5 photosynthesis is downregulated by nitrogen limitation on a daily basis. This misconception/flawed approach has been corrected in CLM5. Why are you following this approach? It is strongly recommended to update to a more realistic N-limitation or is the model here running without N-limitation?

The model is running without N-limitation, as there is no N cycle implemented. We simply use the formulation of CLM4.5 to compute the Vc,max, which involves a constant PFT-dependent value for the foliage nitrogen concentration.

L174: Are there pools for products (paper, wood for construction) fed by deforestation? Please explain.

The land use change scheme in the model is rather simple and does not include separate pools for products fed by deforestation. We have now added this clarification.

L223: d13C of fossil fuel has changed considerably over the industrial period. Assuming a constant value of -26 permil leads to a positive bias. Please prescribe d13C of fossil emissions using updated information, e.g., Andres et al.

As suggested by the reviewer, we have now introduced a variable d13C of fossil carbon emissions, prescribing it after Anders et al. 2016.

L 293: Please comment on whether changes in C3 and C4 crops are prescribed for d13C discrimination.

Following deforestation, the model will growth C3 or C4 grasses, depending on climate conditions.

Fig. 2c I am a bit surprised that the DIC inventory at time 0, at the beginning of the spin-up is somewhat lower than the best estimate from the GLODAP data (37400 PgC), despite prescribing the DIC distribution from GLODAP at the beginning of the spin-up. Is the ocean volume too small?

The discrepancy is a consequence of using a water density of 1000 kg/m3 when converting the GLODAP DIC concentrations in micro-mol/kg to kmol/m3, which is the unit for DIC in HAMOCC, during model initialisation. We have fixed this and now the actual 3D water density field is used instead.

Fig. 13: Perhaps specify the boundary of the SO and whether the SO section is included in the profiles of the Atl., Pac., and Indian.

The boundary of the Southern Ocean is set at 35°S and the Southern Ocean section is not included in the profiles of the Atlantic, Pacific and Indian ocean. We added this information to the figure caption.

Figs. 14, 15, 17, 18, 19, 20: Why is the Southern Ocean sector only shown for the Indian Ocean? It would be much more instructive to show the SO sector of the Atlantic and Pacific in the top rows.

We updated the figures as suggested.

How are weathering fluxes distributed in the ocean?

The weathering fluxes are transferred from the land to the ocean in the same way as water runoff, following the runoff routing scheme. We added this sentence to the paper.

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