

Response to Anonymous Referee #1

We would like to thank the reviewer for a very constructive review. In the following we will comment on all points raised and hope we are able to answer the outstanding questions adequately. Most of the points raised by this reviewer have already been implemented in the model or will be in the course of the revisions. We believe they will greatly improve the quality and completeness of the manuscript.

This manuscript describes a newly developed isopycnal ocean carbon cycle model, which is a useful complement to the current ocean carbon cycle models that are mostly based on horizontal/vertical coordinates. The construction of this model must have been a tremendous modeling work. The construction and evaluation of the model is quite extensively discussed. This manuscript fits well the scope of Geoscientific Model Development, but I think significant improvement is needed before it can be published in GMD.

Major comments

Model evaluation:

The newly developed isopycnal ocean carbon cycle was evaluated in terms of temperature, salinity, DIC, phosphate, oxygen, and uptake of anthropogenic CO₂. However, there is no discussion about how the model-simulations of radiocarbon (in terms of Delta 14C) and CFCs, two of the most important chemical tracers typically used to evaluate ocean models, are compared with observations. There is a long history in using the simulation of natural radiocarbon to evaluate modeled ocean mixing and circulation over the timescale of several thousands of years, and using the simulation of bomb radiocarbon and CFCs to evaluate modeled ocean ventilation over the timescale of several decades. The simulation of natural 14C, bomb 14C, and CFCs is an essential part of ocean model evaluation, which should be included and discussed (For example, the standard simulation protocols of radiocarbon and CFCs used to evaluate ocean carbon cycle models participating Ocean Carbon Cycle Model Intercomparison Project, OCMIP, <http://www.ipsl.jussieu.fr/OCMIP/>)

We have performed a CFC simulation following the OCMIP protocol and will add the results from this and a comparison to the GLODAP data to the manuscript.

Regarding the simulation of radiocarbon we would like to follow the suggestion of Anand Gnanadesikan (Referee #2) and omit this in this manuscript. HAMOCC does in principle simulate radiocarbon, but more work and an additional spin-up would be required to include this in the manuscript. The model in its present configuration runs 10-12 years/day and an additional long spin-up would delay a revised manuscript beyond the time scale reasonable for a revision process. We will, however, include radiocarbon in future simulations.

Sensitivity studies:

It looks to me that all of the sensitivity studies should be conducted with the model spinup to allow the model to reach equilibrium. As shown in section 5, the focus of the sensitivity studies is on nutrient distribution, I see no particular reason to perform these sensitivity studies under anthropogenic CO₂ emissions and NCEP forcing and only for 60-years.

As stated in the paper, we are aware of the problematic issues with only running the sensitivity studies for 60 years. However, since the model takes around a day for 10 years of model run, running the sensitivity studies to equilibrium is not a practical option. We still think that they provide useful insight into the factors controlling biological production in the model. We also use repeated passes of the NCEP Re-analyses during the spin-up and thus do not regard this choice of forcing as a problem. It also makes the results of the sensitivity studies comparable to those from the reference run used elsewhere in the paper.

We agree that since the studies are aimed at exploring the controls of biological production, the atmospheric CO₂ and air-sea flux time series are probably superfluous. We will therefore omit the associated figure and table in the revised manuscript and rework this part of the paper more towards a focus on biological production and nutrient distributions.

Specific comments:

Abstract

Page 1024 Lines 12-14 “The most significant adjustments of the biogeochemical code. . . . ocean biological production” Please rephrase this sentence. What is the biogeochemical code used here?

We rephrased the sentence to: “The most significant adjustments of the ocean biogeochemistry model HAMOCC for use with an isopycnic coordinate are in the representation of upper ocean biological production.”

1 Introduction:

Page 1025 Lines 5-8 “due to its relatively quick turnover time scale of 1000-2000 years . . .” In what context is the time scale of 1000-2000 years can be considered as “quick”? What is the comparison here? Please clarify. Also, if we consider the entire ocean, the turnover time is longer than 5000 years.

Following a comment from reviewer 3 we rephrased the statement as: “due to its centennial turnover time scale (Matsumoto et al., 2007)” which both reflects the content of the reference correctly and clarifies the why ocean circulation is important for the uptake of anthropogenic carbon emissions.

Page 1026, Line 2 Please clarify what you mean by “sinking oxygen water column levels”

We replaced “sinking” by “decreasing” and hope that this clarifies the point.

Page 1026, Line 9 Please clarify what you mean by “reverse weathering”

The conversion of deep-sea sediments to the composition of the upper crust (first of all clay minerals) is called reverse weathering. The "new upper crust" is after subduction and conversion again available for weathering on the continents' surface. Reverse weathering processes have been observed from in-situ data and also laboratory experiments.

Page 1027, Line 20 “induction” should be “introduction” ?

Yes, done.

Page 1027, Line 23 Please clarify what “uncertainty assessment” refers to.

At present, we need to consider different models (or ideally ensemble computations) in order to estimate the uncertainties in simulations of past, present, and future climate. We are still far away from a "perfect" ocean model. Taking different modelling approaches into consideration gives a more realistic uncertainty estimate for our ability to simulate climate processes. The isopycnic model is of special interest here, as the simulation of advection process conceptually differs from other models which are based on depth (or pressure) as vertical coordinate.

2. Model description

Page 1029 Please state what ‘M’ stands for in equation (1)

The Montgomery potential M is defined as:

$$M = \frac{p}{\rho_r} + gz$$

where p is pressure, ρ_r potential density, g the gravitational constant and z depth.

Page 1030 Based on what the value of background diffusivity C is set ?

The parameterization of background diffusivity is taken from Gargett (1984) and the diffusivity is tuned to give a reasonable poleward heat transport in coupled atmosphere-ocean-sea ice simulations (Otterå et al., 2009).

Gargett, A. E., 1984: Vertical eddy diffusivity in the ocean interior. J. Mar. Res., 42, 359–393.

Orre, S., J. N. Smith, V. Alfimov, and M. Bentsen, 2009: Simulating transport of ^{129}I and idealized tracers in the northern North Atlantic Ocean. Environ. Fluid Mech.. In press.

Otterå, O. H., M. Bentsen, I. Bethke, and N. G. Kvamstø, 2009: Simulated pre-industrial climate in Bergen Climate Model (version 2): model description and

large-scale circulation features. Geosci. Model Dev. Discuss., 2, 507–549.

Page 1034, Lines 16-18 Please rewrite the sentence “For computational efficiency ran the biogeochemical . . . time step”.

We rewrote the sentence as: “MICOM uses a leap-frog time stepping scheme. The biogeochemical tracers are only defined on one of the two time levels of the time stepping scheme. This increases computational efficiency since the inclusion of a large number of passive tracers significantly increases computation time especially in MICOM's advection and mixing routines.”

Page 1035, Line 13 “The model was spun up for 950 years” My modeling experience is that for dynamic ocean models, it usually needs more than several thousands of years of spinup for the entire ocean to reach equilibrium. It needs to be justified here why 950-year of simulation is sufficient for the model spinup. What are the criteria used here to determine whether the model equilibrium has reached?

We terminated the spin-up when successive passes of NCEP Reanalyses yielded virtually identical global CO₂ fluxes. For the last 2 passes of NCEP Reanalyses 1950-99, the rms difference between the air-sea CO₂ fluxes, which vary between -0.3 Pg C yr⁻¹ and 0.3 Pg C yr⁻¹, is 0.0245 Gt C yr⁻¹. This implies that the air-sea CO₂ fluxes are basically reproduced between the two runs. Volume-weighted rms differences for the mean 1990-1999 distributions of phosphate, oxygen and DIC are 0.8%, 0.9% and 0.06%, respectively. Since spatial DIC variations are only on about the order of 10% of the global mean, the real DIC error is probably more like 0.6%.

We added this information to the model set-up section.

3 Model results and evaluation

Page 1039, line 4 “relative strong AABW cell of 14Sv”, relative to what? Is there any observational evidence showing that 14Sv is ‘relatively strong’ for AABW cell?

Orsi et al. (1999) derive a northward AABW transport of 8-9.5 Sv which make 14 Sv in our model “relatively strong”.

Page 1039, line 7 “show a realistic North Atlantic overturning of 14Sv”, please give evidence/references here to show that 14Sv is realistic for North Atlantic overturning.

It is indeed difficult to obtain observational values of the AMOC. Models are generally tuned so that their AMOC coincides with a realistic northward heat transport that in turn can be measured more easily. Comparing our simulation with those in the CORE project (Griffies et al., JPO, 2009) shows that the AMOC strength in the MICOM simulation presented here lies at the

upper end of values found in the CORE models. Lumpkin et al. (2008) use hydrographic sections and an inverse box model to determine the overturning strength at 48N. They deduce a value of 16-18 Sv. We change the statement to a more detailed discussion in the revised manuscript.

Page 1040, lines 17-19 "According to the Taylor diagramme, oxygen, . . . Is the tracer that the model simulates best" As the authors stated, in addition to biological processes, oxygen distribution is affected by temperature. The fact that oxygen simulation is better than that of phosphate could be a result that the bias in simulated biology and simulated temperature offset each other, which should be discussed here.

This is a good point and we will amend the discussion on oxygen to reflect this.

Page 1041 lines 11 to 12 "POC export production at 12.4 . . ." The strength of POC export production could be very sensitive to the depth at which the export is defined. It should be clearly stated at which depth the modeled POC is defined here.

POC and other exports are defined at the base of the euphotic zone, i.e., 90 m. We added this information to the text.

4. Air-sea fluxes and the uptake of anthropogenic CO₂

Page 1043, line 25 I understand that climate change tends to act as a positive feedback for atmospheric CO₂, but please state more clearly what is the "carbon cycle-climate feedback of less than 15ppm"

We added the phrase, "the effect of global warming induced changes in the physical climate system on atmospheric CO₂ levels" to the sentence and hope this explains matters sufficiently.

5 Sensitivity studies

IRON: It is not clear to me how the IRON simulation differs from the default model simulation. It is stated in the model description section that "The model includes an ecosystem model . . . with nutrient co-limitations by phosphate, nitrate and iron (Aumont et al., 2003)" (Page 1032, Lines 21-23). Does the default model have no iron limitation? Also, please include a description of how the iron cycle is simulated in the model.

We have added a description of the iron cycle to the model description. The description of the IRON sensitivity states that the default model does have an iron limitation in which the ambient iron concentration is converted to phosphate units using a Fe/P ration of $3 \cdot 10^{-6} \text{C/P}$. In the IRON sensitivity study the iron limitation was enhanced by increasing the Fe/P ratio to $5 \cdot 10^{-6} \text{C/P}$, i.e., phytoplankton takes up more iron per unit phosphate which depletes the available iron faster.

We have amended the text and hope in combination with an improved

model description this will clarify the matter.

The last paragraph of this section. I don't think that the discussion of changes in simulated export production and atmospheric CO₂ from these sensitivity experiments is useful. What do these results mean? Do changes in export production and/or atmospheric CO₂ suggest improved model simulations as a result of changes in model physics and/or nutrient limitation? Again, these sensitivity simulations should be performed under constant atmospheric CO₂ concentrations with model simulations reaching equilibrium.

As stated in the response to your earlier comment and in the introduction to the section on the sensitivity studies, we are unable to run the sensitivity studies into equilibrium due to limited computer resources and time constraints. We are aware of this and actually this state in the text, but still think that the sensitivity studies provide useful information and will thus retain them in the manuscript.

However, we will focus the discussion more on the model improvement aspect and drop the discussion of atmospheric CO₂ levels.

6. Discussion

Page 1048, line 12 "After a thorough evaluation we conclude . . ." The ocean carbon cycle has not been evaluated using the simulations of radiocarbon and CFCs, two of the most important chemical tracers used to evaluate ocean models. This makes it hard to convince me (and probably the readers) that it is a 'thorough evaluation'.

We hope that we will be able to retain the sentence after adding CFCs and other analyses suggested by you and the other two reviewers to the manuscript.

Page 1048, line 16 What is "layered ocean models"?

Layered ocean models represent the ocean as a stack of shallow water models that represent the ocean as a set of interacting constant potential density layers.

Page 1048 line 17 "our attempt has been more successful" Please clarify to what aspects the attempt here are more successful.

We will revise and specify this comment at the end of the revisions.

Page 1049, line 24 "modeled iron concentrations agree with available observations" Please include a figure showing this. Also, please add a description of how the iron cycle is simulated in the model.

We have added a better description of the model's iron cycle to the model description and will add a figure showing surface iron concentrations to the manuscript in accordance with the suggestion by reviewer 2.

Figures:

Fig.2. Please give a short description of the Taylor diagram for readers who are not familiar with the construction and meaning of this diagram.

We have added this to the figure caption.