### Response to Referee #3

We thank the reviewer for a number of additional useful comments. A large proportion of these are directed towards quite significant alterations in the model to address inadequacies. Our manuscript gives an introduction to the model system and is necessarily a snapshot during the development process. Due to computational limitations – the model runs at about 10 years per day – we are not able to run full additional spin-up simulations including solutions to these inadequacies within the framework of this manuscript. However, we will add comments to the manuscript to indicate where we have since developed improvements to the model.

This submission and the earlier Tjiputra et al submission are companion papers. This submission elaborates in greater detail the ocean component, both physical and biogeochemical, of the earth system model described in Tjiputra. A major selling point here is the use of an isopycnal model based on MICOM. In general I am favorable impressed and would recommend publication if the following points can be addressed adequately in revision.

Perhaps the most significant point to be made is that the calibration and assessment of biogeochemical ocean model seem incomplete without simulating the two transient tracers that have come to prove so useful: CFCs and carbon-14. The omission of these two tracers from the Assmann submission is glaring. These two have in some sense become metrics to assess ocean carbon models, so any revision really should include new simulations of these tracers. CFCs are short time transient tracers, but C14 requires long spin ups (thousands of years). It would be useful for the community to know how long (clock time) such a simulation would take with this new Bergen model.

#### We will include results from a CFC simulation in the revised manuscript.

The model runs at a rate of 10 years/day which makes an additional simulation including 14C too time consuming in the framework of the revision process. We will therefore follow Anand Gnanadesikans suggestion and include CFCs but not 14C in the

manuscript. We will work on including radiocarbon in future model simulations.

Here are other points that I would like the authors to address in their revision: 1) P. 1025, line 7: deep ocean circulation does not have time scale of 1000-2000 years. It is shorter and centennial. Assmann should actually read the cited reference.

# You're right – sorry about that. In view of the comment of reviewer 1 there appear to be a large differences in the opinions on the time scales of deep ocean circulation. We will retain the reference but amended the text to reflect its content correctly.

2) P. 1029, line 10: I noted this in the Tjiputra submission as well, but there does not seem to be adequate justification given for referencing potential density to 2000 db. It seems important to get it right the near surface, where dynamics is more variable, than the deep, where things tend to be quieter. What is gained and what is sacrificed by changing the reference from 0 db in MICOM to 2000 db? If AAIW becomes so cold and indeed reaches 3000 m (p 1038, line 18) because of 2000 db referencing, I do not see the benefit of changing the reference point.

As already mentioned in the model description the selection of 2000 db as the reference pressure is motivated by the study of McDougall and Jackett (2005), who argue that these isopycnals are a reasonable compromise in approximating neutral surfaces throughout the global ocean. Potential density with respect to surface pressure ( $\sigma_0$ ) generally becomes significantly non-neutral in the deep ocean and has large-scale inversions in much of the ocean (e.g., Antarctic Bottom Water has a lower potential density with respect to surface pressure than North Atlantic Deep Water). However,  $\sigma_{2000}$  is monotonically increasing with depth, except in some weakly stratified high-latitude haloclines (McDougall and Jackett, 2005). As the vertical coordinate used by an ocean model must be a monotonic function of depth,  $\sigma_{2000}$  is now widely used as the vertical coordinate in isopycnal models (Sun et al., 1999).

3) P. 1033, line 3: again I noted this in Tjiputra submission, but should not silica be linked to P, N, and Fe? Is it possible for diatom to be produced in the model where there may be insufficient nutrients? Could this be contributing to the overestimated POC production in the Southern Ocean, for which the authors have looked into IRON, ABS, and DIAPYC (section 5)?

In HAMOCC bulk primary production of the single phytoplankton class is calculated taking into account light limitation and nutrient co-limitation by P, N and Fe. The POC export is derived from this bulk primary production. HAMOCC does not simulate diatom production per se, but only accounts for the export of opal shells. To this end the POC export is split into a part associated with diatoms and one associated with calcifiers. The proportion of the POC export that has opal shells is calculated using the ambient silicate concentration. Since opal export depends on the POC export as well as silicate concentration, it can never exceed the POC export and is thus also linked to P,N and Fe limitations.

As far as we can tell the factor that really improves the POC export in the Southern Ocean is an improved treatment of the absorption of SW radiation in the mixed layer as used in the ABS sensitivity study. We have included this in the next model version, but due to the long time it takes to spin the model up again, we unfortunately cannot include it in this publication. We will stress this point more in the revised version of the discussion.

4) P. 1034, line 5: does export production include advection of DOM (in addition to POC sinking)?

### No, it doesn't. We will clarify this in the text.

5) P. 1040, line 18: it is noted that temperature and oxygen are the best simulated tracers. Is it because the temperature is done well, and therefore the gas saturation is about right?

## That is a good point. We will use this comment and the comment on this issue by reviewer 1 to amend the discussion on oxygen.

6) P. 1040, line 21: why is sea ice too large? Have the authors tried to improve this?

The Antarctic summer sea ice extent is too large, because the warm core of the CDW has been eroded away by excessive mixing. We will add a note to clarify this in the text. The problem was addressed in a more recent model version by using in situ instead of potential density at 2000db to assess the stability of the water column. In addition we now use the mixed layer formulation by Oberhuber (1993) instead of Gaspar (1988) and have added mass field and isopycnal eddy mixing.

7) P. 1042, line 2: carbon uptake is 0.05 Gt/yr ; is this because the model is not quite at steady state? If the authors do a long spin up for C14, I would think that the carbon uptake would really approach 0 Gt/yr.

The mean ocean carbon uptake in our reference simulation is  $0.051 \pm 0.0058$  Pg C yr<sup>-1</sup>. There is a small decrease of  $-4.2 \times 10^{-5}$  Pg C yr<sup>-1</sup> during this time period which is significant

at the 99% level. So in principle you are probably right that the model may eventually get to zero. But we decided that the imbalance was small enough not to affect the main model results at the time scales that we are mainly intending to use the model at which is 1860-2007 (2100), i.e., a few centuries, especially when considering anthropogenic emissions. We amended the manuscript to reflect this assessment.

8) P. 1045, line 14: where do these stoichiometric ratios come from? They are a bit different than Anderson and Sarmiento for example.

The stoichiometric ratios used in HAMOCC are taken from Takahashi et al., JGR, 1985. Within error bars they agree with those by Anderson and Sarmiento (1994). We have added the reference to the manuscript.