Response to Reviewer 2 (Anand Gnandesikan)

We would like to thank Anand Gnandesikan for an extremely helpful review suggesting a number of useful additional analyses to the manuscript. Most of these will be added to the manuscript and will certainly make it a more complete presentation of our model and more useful as a comparison for other modeling studies.

This paper introduces a new ocean biogeochemical model embedded in an isopycnal ocean model. As someone who has been working on a similar effort to this one I appreciate the scope of what is reported here and felicitate the authors on their accomplishment. I strongly support publication of this paper after some revision. I have listed some suggestions below to improve the motivation, description and comparability of the model. Those suggestions which involve citing my own papers are to be taken as strictly optional!

1. Motivation:
Using an isopycnal model is motivated on p. 1027 in terms of being an "alternative formulation" of the primitive equations. Although there is some reference to avoiding "artificial mixing and advection" in the ocean interior, this statement is less well fleshed out than it could be. Our motivation at GFDL for using an isopycnal model is really based on three facts. First (as reported in Winton, Hallberg and Gnanadesikan, JPO, 1998) isopycnal models do a much better job at simulating overflows than level coordinate models, a fact that has clear implications for the the simulation of water masses. Second, using rotated diffusion tensors in level coordinate models to do isopycnal mixing results in up-gradient fluxes, which can lead to unphysical (negative) values of biogeochemical tracers that have sharp gradients in the vertical (Gnanadesikan, Ocean Modelling, 1999). Finally, in time-varying flows, advective overshoots and truncation can lead to spurious diffusion (Griffies, Pacanowski and Hallberg, MWR, 2001). Since the rate at which biological cycling within the ocean occurs is strongly determined by vertical diffusion (Gnanadesikan et al., GBC, 2004), limiting this spurious diffusion is potentially critical to getting the right rates of chemical cycling.

Thank you – we have added some of this to our introduction.

2. Description

A. The major limitation of the description is the lack of a description of the mixed layer and its interaction with the ocean interior. Given the importance of the mixed layer depth for determining light limitation and nutrient supply, a more detailed description needs to be provided here, preferably with a table of any parameters that determine this exchange. (Doing this would also help set up the DIAPYC run). In this regard, what is done with respect to penetrating shortwave radiation?

The following will be added to the manuscript:
The mixed layer depth is found by a turbulent kinetic energy balance of a

one-dimensional mixed-layer of the type described by Kraus and Turner (1967). In the version of MICOM used here the formulation by Gaspar (1988) is employed. The TKE balance is affected by wind stress, current sheer and surface buoyancy fluxes. Detrainment from and entrainment into the mixed layer are computed as in Bleck (1992). The penetrating shortwave radiation is absorbed using an exponential decay curve. It is assumed that all water is clear water (Jerlov water type 1). Currently we do not include the effect of phytoplankton on the absorption of shortwave radiation in the physical model.

B. Additionally, it would be good to give a mean value for the background diffusive coefficient in the 300-500m range in the tropics vs. the bottom values. Again this is to help make the models easily comparable without requiring someone to go and calculate N.

We will add these to the revised manuscript.

C. The iron cycles across models are not well described. I would like to see at least a contour plot of surface iron and values for the total iron flux to the ocean and total iron inventory in the ocean. Models currently being run differ by an order of magnitude in terms of the iron fluxes applied.

We have added this information to the manuscript.

3. Model validation and comparability

A. Using full-depth nutrient fields for the Taylor diagramme will somewhat overstate the goodness of fit. It would be useful to also add points for surface phosphate, surface nitrate, and surface silicate. The values reported here are pretty good. A comparison with Schneider et al. (Biogeosciences, 2008) would be a good idea (with the appropriate caveat that the models in Schneider are coupled). Also do a comparison with chl and log(chl). This will probably not be all that great, but serve as a comparison point for future studies. Alkalinity fields are also available from the GLODAP site.

We will add an additional Taylor diagramme for the surface distribution to the manuscript and add a comparison to Schneider et al. (2008) which we refer to elsewhere to the text. HAMOCC does not actually include Chl. We could however show phytoplankton concentrations if desired.

B. One of the challenges in comparing models is to understand the role of different limitations. I would like to see a calculation of the production-weighted limitation from light, phosphate and nitrate, and iron. This would be useful for comparing the IRON, ABS and DIAPYC runs as well.

We will add production weighted light and nutrient limitations to the manuscript. A table summarizing biological production and its limiting factors will be added to the section on sensitivity studies. Following the objection of reviewer 1 to the atmospheric CO2 and air-sea flux time series

and table, we are considering replacing the associated figure and table with distributions of nutrient limitations and a corresponding table.

C. We have recently published (Dunne, Sarmiento and Gnanadesikan, GBC, 2007) a synthesis of particulate export of POC, PIC and opal that you might find useful. I am not a big fan of the Laws et al. export fluxes, as they don't make oceanographic sense (depending on which flux product is used there is either no contrast between the southern subtropics and the Southern Ocean, or between the Southern subtropics and the equator). The other reviewer is correct that the exact depth of export is important (is it the depth of the euphotic zone?).

The synthesis in the paper you mention does look very useful. We will use it in our discussion on export and primary production. We have added the depth (90 m), where export production is calculated, to the text following the comment by reviewer 1.

D. A major question one would have with this model is whether the use of the isopycnal model changes the solution significantly compared to a level-coordinate model. If there is a comparable solution it would be useful to present it for a few fields, in particular would be the zonally averaged export and primary productivity. I think this would really strengthen the paper, but if no strictly comparable result exists it should not hold up publication.

Unfortunately not. We did consider doing a comparison of HAMOCC in MICOM and MPI-OM, but decided that the physical ocean models were too different apart from their vertical coordinate to really be able to tell which differences were due to the vertical coordinate and which ones simply due to differing numerial schemes and parametrisations.

E. Add correlation and regression coefficients to the anthropogenic uptake in Figure 13.

We will add these to the manuscript using the GLODAP data as a comparison.

Questions and comments

A. p. 1040: You state that sea ice is impermeable. Is this taken to be true if there is any sea ice? In the OCMIP2 models, for example fluxes were scaled down according to the sea ice concentration.

We treat sea ice the same way as in the OCMIP2 models and will amend the model description to clarify this since there are several places in the text where we refer to this.

B. p. 1045: It is stated that the "full incoming shortwave radiation" is applied to the mixed layer. Isn't there some decay over the layer? Shouldn't the *average* downwelling radiation be used?

HAMOCC uses the shortwave radiation at the upper interface of a layer to calculate biological production. We initially tried to keep the code as close to the original as possible and thus used the same in the isopycnal version. However, while the global values of primary and export production are realistic, this gives the excessive values of production in the Southern Ocean seen in our standard run. Introduction of a scheme that uses a more average value for short wave radiation that reaches the upper layers does improve this as shown in the ABS sensitivity study. We will point this out more clearly in the revised manuscript.

C. p. 1049: "a more sophisticated parameterization is hard to come by" In Galbraith et al. (Biogeosciences Discussion, 2009) we have a discussion of how to deal with luxury uptake. Though I would say that "a well validated parameterization" remains hard to come by!

We have amended the text.

Comments on other reviewer

A. CFCs and radiocarbon. I agree that CFCs should be simulated, as this involves a fairly short run. However, radiocarbon can take thousands of years to come to equilibrium. I don't think the paper should be held up for the second set of simulations.

Thank you for this comment. We will add results from a CFC simulation to the paper and will look more into simulating radiocarbon in future model experiments.

B. The turnover time for the entire ocean is not longer than 5000 years! Radiocarbon gives an underestimate of the turnover time, as it takes so long to equilibrate, and the average radiocarbon concentration of the deep ocean is -170 permil.

We have now used Matsumoto (2007) and his timescale of 300-900 years as a basis for amending the controversial statement in the introduction. The discrepancy between reviewer 1's and reviewer 3's opinion on ocean turnover time is interesting though and indicates that the matter is not entirely resolved.

C. Regarding spinup time scales. 1000 years may be sufficient for some parts of the simulation (surface nutrient fluxes, carbon fluxes). In fact in Galbraith et al. (2009) we find that after 400 years the surface nutrients are changing by less than 1% per century. That said, I agree with the reviewer that the criteria used should be stated.

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.