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Interactive comment on “Natural ocean carbon cycle sensitivity to parameterizations of the recycling in a climate model” *by* A. Romanou et al.

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Manuscript title “**Natural ocean carbon cycle sensitivity to parameterizations of the recycling in a climate model**” *by* A. Romanou et al.

[Reviewer comments are shown in italic and authors’ responses in regular font.]

Overall statements

The manuscript "Natural ocean carbon cycle sensitivity to parameterizations of the recycling in a climate model" by Romanou et al. tackles the relevant problem of model

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sensitivities to both physical and biological model uncertainties. Unfortunately the methods used are partly not transparently shown in the text. In other cases they appear to be not always appropriate.

In the following Response to Reviewer 1 we will try to address all points that they make and improve on the presentation and justification of the methods used.

- The different hydrodynamic model results must be shown in the manuscript, especially those which induce the differences in the biogeochemical results.

These results have been presented with great detail in a recent publication, which was in press at the time of the present review and it was probably difficult for the reviewers to access. It is now published: A. Romanou et al., 2013: Natural air-sea flux of CO₂ in simulations of the NASA-GISS climate model: Sensitivity to the physical ocean model formulation. *Ocean Model.*, 66, 26-44, doi:10.1016/j.ocemod.2013.01.008. There is very specific reference to figures and sections where the information is relevant to this analysis.

- The application of (very) different remineralisation rates represents an almost trivial exercise simulating two different situations: an efficient export system and a near surface recycling system. Concepts like the remineralisation length developed by others (Kriest and Oschlies, 2008) are not applied.

The NASA Ocean Biogeochemistry Module, that is used in the GISS ocean carbon cycle simulations presented here, models remineralization rate, which is not the same as remineralization length. Although the Kriest and Oschlies (2008) is a pioneering study on the effects of sinking POM parameterizations to the redistribution of POM in the mesopelagic ocean, it has somewhat idealized model setup and we feel that subsequent studies by the same authors are more relevant to our study here. These studies are discussed in the text.

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- *Necessary ingredients of biogeochemical models like preferential remineralisation of different elements in particulate organic matter or the dynamics of the carbonate system governing the partial pressure of CO₂ seem to be neglected or not discussed.* This is correct, in this study we isolate the effect of changing remineralization rate for carbon and nitrates. We plan to look at the combined effect of other remineraliation rate changes, such as for iron and silicate.

- *In many cases the figures are in bad condition (too small, lettering wrong).* The figures, titles and captions are now redone.

Detailed statements

The abstract should be enriched by some concrete results.

The second part of the abstract is now expanded to include all our key findings.

Page 11114 line 25: Specify "simpler". Which features induce a better skill for surface properties?

Simpler models refer here to those with fewer components as stated in the text Page 11114, line 1. We have added additional wording in this place as well.

P 11115 line 17: ".. the remineralisation rate within two different ocean models". You mean the remineralisation rate in the biogeochemical model?

Correct, the remineralization rate parameterization is the same formula in both models, but because the remineralization term depends on temperature, the remineralization process is different in the two ocean models.

P 11116 Model description: Even though the model setup might be described elsewhere, please give the relevant issues here: How long was the spin-up of the coupled model? Which correction factors were applied? Which years were simulated? Is the present-day situation shown? Was the meteorological component free running or did

you use reanalysed fields? Was the atmospheric pCO₂ fixed or was it in response to the ocean? How did you handle ice dynamics?

All these questions are addressed now in the text, in a new paragraph. Briefly, The integration run was 3000years to spin up the atmosphere-ocean-ice coupled model, and 300 years after that to spin up the atmopshere-ocean-ice-ocean carbon model. All fields are prognostic, there are no corrections applied, or nudging, or restoring or assimilation/reanalysis data used. These are free, fully coupled model runs. Preindustrial runs were studied here to assess the natural carbon cycle sensitivities. The present-day runs, as well as future scenarios (RCPs) will be addressed in subsequent papers. Atmospheric pCO₂ varies due to ocean carbon flux, but the global average is kept at 285.2 ppmv. Ice is also prognostic, with full thermodynamic and kinematic physics for its evolution.

P 11116 line 5: ". . the Russel or HYCOM oceans": avoid such slang. You mean the different simulated ocean dynamics, right?

Corrected.

P 11116 line 9: "Vertical coordinate is mass per unit area", please describe in detail.

Since mass is a formally conservative property in the Russell ocean model, while volume (and therefore fixed-z) is not, we use mass/area to express the vertical coordinate. This makes the introduction of freshwater fluxes in the water column particularly easy. Besides freshwater and tracer flux treatment, the vertical coordinate resembles a fixed-z coordinate. The normalization to area is done to ensure that when we compare two adjacent boxes we take into account that they might have different grid area. The paragraph is rewritten in the text to reflect this.

Page 11117 line 16: How does the correction factor work? Has it implications on the pump mechanism?

The flux form of the transport equation advects the product $N \times \delta p$ where N is tracer

concentration and δp is layer thickness. Thus, a division by δp is required at the end of each time step to recover N . This division is problematic at grid points where a finite amount of $N \times \delta p$ is advected into a grid cell whose thickness has just gone to zero (or near-zero). Accuracy problems that occasionally arise at such locations are currently eliminated by applying a global correction factor to each tracer field following each transport step. The correction is too small to be detectable in plots of tracer fields but assures that tracer is formally conserved in the model.

Page 11117 line 27: "carbon submodel": no carbonate system module included? Then all statements regarding CO₂ and air-sea exchange become very difficult because physical and biological variations change the distribution of the species (CO₂, HCO₃⁻, CO₃⁻) in the system and thus the partial pressure of CO₂.

There is very simple "carbonate" system included, in the sense that surface alkalinity is a constant function of surface salinity and therefore surface pCO₂ depends on alkalinity too. However, there is no carbonate pump model. This clarification will be added in the text.

Page 11118 line 22: In your model you assume detrital C:N to be constant. Discuss this.

Redfield ratio C:N is considered constant over the timescales that we simulate. We can use this property to reduce the number of equations since we need only one equation for particulate carbon and particulate nitrate.

P 11119 Results: Give details of the different hydrodynamic outcome of the models, especially those which induces differences in biogeochemical results which are discussed later.

We have pointed to Romanou et al. (2013) for more detailed comparison of the physical model comparisons. However, in various places within the new text we have added information and explicit reference to figures and sections of the Romanou et al. (2013)

paper in order to facilitate easier physical comparisons and how they affect the carbon pump.

P 11120 detritus equation ff: Do you use constant C:Chl ratio? Give the numbers used for all the constants and justify the choices.

We are using a fixed value of C:chl=50 g:g in NOBM, because it is straight forward to ensure mass conservation, which is very difficult when this ratio is variable. It would be difficult to estimate exactly how deep carbon export would change because it would depend on how the variable C:chl was achieved and how fast the sinking and remin rates are. We can assume C:chl would be minimum for living phytoplankton at depth (they would be maximizing chlorophyll to get the most light possible), but most detritus would originate from the surface layers. So here C:chl would be highest at the surface (say around 200 max but only for the brightest sun, the shallowest layers, and in the tropics) and lower as you go deeper, get clouds, and get toward the poles.

P 11121 line 1: Show the SST distributions.

SST climatologies are shown in Romanou et al. (2013) Fig. 2. This is added to the text as well.

P 11121 line 5: In Fig 2 plot a third column showing the differences between first and second column.

A third column was added in Fig. 2 and the discussion in the text has expanded to include it.

P 11121 line 9: You mean the subarctic front?

No we mean the Subantarctic Front, along approximately 50S, which is approximately the northern edge of the ACC. This is now clarified in the text.

P 11122 line 11: Write "detritus concentration is larger .."

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Done.

P 11121 eq 6 ff: Give the values of the constants used and justify your choice.

The values used here are the same as in Gregg and Casey, 2007.

C:N ratio = 106/16

nitrogen:chlorophyll ratio = 50/ (C:N ratio)

phytoplankton maximum growth rate for each species:

diatoms = 2.00 in units (perday) and at 20C

chlorophytes = diatoms growth rate *0.84

cyanobacteria = diatoms growth rate *0.670

coccolithophores (E. huxleyi only) = diatoms growth rate *0.755 This information is added in the text as well.

P 11123 line 21: The effective remineralisation rate is a normalised parameter which cannot be translated into absolute numbers of nitrate concentration.

This is correct, the remineralization term is normalized to 20C. Here we are stating that in warmer waters (as in GISSER) the source term that is associated with remineralization will be larger than for slightly colder water (as in GISSEH).

P11124 line 11: Your conclusion holds for the two models you have used. Be careful with generalisations.

True. We have added language to avoid generalization.

P 11125 line 7: Here you refer to some aspects of the physical models. These features must be discussed in section "Model description" or "Results (physical)".

This information is in the Model Description now where both model treatment of ice is given in detail.

P 11125 line 25: Why are diatoms less influenced by remineralisation rate changes?

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(Line 20) The changes are less pronounced in diatoms and more in other species because of the geographical distribution of diatoms. Their distribution is lower in the regions where remineralized nutrients are more readily available.

P 11126 line 21: Which other nutrients? As you have a fixed ratio of elements in detritus all nutrients should be remineralised in the same way.

The other nutrients in the model that have detritus pools are silica, ammonium and iron. Both silica and iron remineralize at different rates from nitrogen.

P 11126 line 24: The f ratio is defined as nitrate uptake divided by nitrate plus ammonium uptake. Please justify your definition.

The f-ratio is defined in different ways across the literature. We follow the Eppley and Peterson (1979) and Sarmiento et al 2004 definition as the ratio of new to total production.

P 11126 line 27: The results related to this very large interval of export efficiencies must have overlaps with observational data. If you compare your results with observational data, which is not essential for this paper, do it more precise: Give tables or Taylor Diagrams.

Agreed. The point we wish to make is that model global estimates in the different remineralization cases are within the observational evidence. This is now better expressed in the text.

P 11127 line 3-7: This sentence is a mystery to me.

The whole passage is re-written. Awkward description is hopefully clear now.

P 11127 eq 7: Comparing the magnitudes of the different summands, it becomes clear that the last one dominates. Discuss the meaning of it.

We would like to avoid going into detailed discussion of Equation (7) as it is after all

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a parametric fit in our two models. However, it is interesting to recognize that export efficiency in Eq. 7 is seemingly constant with remin. rate, with only slight variations as we change remin. rate. However, what eq. 7 says is that when we compare the export between 2 different physical ocean models, the one that does better the Southern Ocean mixing will be able to get the right values of export efficiency. At least this is true for our two physical models.

P11128 line 26 ff: This argument is misleading. Vertical mixing should increase when the vertical gradient of a tracer gets larger (as long the diffusion coefficient allows it). The explanation of higher DIC in the upper water when using higher remineralisation rates is trivial: It's because most of the organic matter is remineralised near the surface when the corresponding remineralisation rates are high (or sinking speed low).

We agree with this explanation. This sentence was indeed misleading in the way it had been written and is now rewritten What we meant was that the decreases in DIC is not linear with depth as we linearly increase the reminearlization rate.

P 11129 line 1: Why "more diffuse"?

Since the vertical gradient is reduced, but at a different rate with depth, we end up with profiles are more unstable and therefore more turbulent diffusion (mixing) takes place. This effect is more pronounced in GISSER than GISSEH.

P 11129 line 10 ff: I have doubts whether this section can be used without simulating the carbonate chemistry.

In this model, alkalinity is a constant function of surface salinity. There is no carbonate pump which would change the distributions of alkalinity at depth. However, the effect of alkalinity on $p\text{CO}_2$ and the flux is included in our model. This is now clarified in the text.

P 11140 Table 1: Omit this table or give a substantial statistic for validation.

We now include the 5% significance level for the correlations presented in Table 1.

P 11142 ff Figures: Give for all figures correct values for longitudes and latitudes.
Done.

P 11142 Fig. 2: Indicate surface, 500m, 1000m, 3000m in relation to the graphs.
This figure has been redone and a third column with the difference of the first two columns has been added.

P 11146: Fig. 6 Give correct units for Chl and primary prod.
Corrected. The units are mg chl m^{-3} and $\text{mg C m}^{-2}\text{day}^{-1}$.

P 11147 Fig. 7: Enlarge.
Done.

P 11148 Fig. 8: Enlarge.
Done.

P 11149 Fig. 9: PgC yr-1 is not a concentration unit.
Corrected, it is supposed to be mmol C/m^3 .

P 11150 Fig. 10: "DIC surface along -179". Give the correct notation and possibly indicate the section in Fig. 9. The legend should say "red is ..".
Fig. 9 with the line plots along 179W is now dropped as we agreed with the 2nd reviewer's point that it did not add much information to the discussion.

P 11152 Fig. 12: "06 refers to .." I cannot see "06" or "08".
Corrected.

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P 11153 Fig. 13: "mmoles" you mean "mmol"?
Corrected.

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