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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.]

General statements

[..] the paper would in my opinion need a clearer presentation of the results. The paper does interpret results of the relevant compartments (detritus, nutrients, primary/export production, carbon cycle) in terms of both physical (circulation) and remineralization rate effects. However, the red thread sometimes gets lost in the amount of detail presented, partly because the underlying physical changes are not described or shown in this paper. This makes the explanations of the results hard to follow and to believe.

Following the reviewer's suggestions we have reduced the text where there was superflous information, rewrote several passages and tried to clarify the sections which were rather convoluted. We hope that the presentation is now clearer.

Specific statements

p11116, l7-p11117, l7 The model description provides a great amount of technical detail on physical model parameterizations. It lacks a description of how the differences between the physical models affects ocean circulation and relevant properties such as temperature, mixed layer depth etc. These differences are frequently referred to in the results section, and I believe they are explained in detail in Romanou et al., Ocean Modelling, 2013. For understanding the results described here, however, it is crucial to present at least the general picture of differences in physical properties between the models.

There is now additional information (a new paragraph) on the differences between the two ocean models that pertain to the ocean carbon pump. Also, there is more information on initialization and spin up and equilibrium procedures. There is more information on vertical coordinate systems in the GISSER model.

p11119, l14 you state that you explore remineralization rates from 0.01d to 0.5d why

not up to 0.8d, if that's the highest literature estimate (l8)?
We believe that such high values are not representative of global or at least recurrent conditions in the global ocean, but rather characteristic of one single location at the North Atlantic subtropical gyre. Moreover, the computational cost of these runs (full coupled model runs) is very high and we were not able to do as many simulations as we wanted.

p11118, l2-4 so is there a large/relevant difference between the two methods?
It is difficult to isolate the PPM/PLM effects in the two models. When used in the same model these differences are not shown to make much difference. However, the very different vertical coordinate and vertical discretization in the two models introduce much greater discrepancies between the two models than the PPM/PLM differences.

p11119, l16 can you remind the reader how long it takes to reach equilibrium, and what is meant by the term "equilibrium"? Is the deep ocean also at equilibrium?
The physical ocean is at equilibrium over centenial scales. The model was run for 3000 years to achieve equilibrium. This equilibrium is of course dynamic, in the sense that there are changes from year-to-year as well as interannual. However, over averaged over a decade, there is no decadal trend. This is now clarified at the beginning of the Model Description section.

p11119, l25ff in the results section you are frequently referring to the subantarctic front (SAF) and the subtropical convergence zone (STCZ). It would greatly ease following the results, if you could indicate the position of these fronts at least in one of the global maps.
This is now done in Figure 1.

p11121, l1-4 "..., then at the surface (Fig. 2a, b) the remineralization term is lower in the tropical upwelling regions and in the polar regions where the surface waters are

colder." - lower than what?
Lower than in the rest of the ocean. It is now explained in the text.

p11121, l5-9 are these differences supposed to be visible in Fig. 2? If so, they're not. Maybe a difference plot instead of absolute values for one of the models helps.
We have now included a third column in Figure 2 with the difference in eff. remin. at different depths.

p11121, l24 "the exponential 0.86" - I am guessing that you are referring to a parameter in the Martin curve, or is this number universally known?
Yes, we mean exponential decay by 0.86. We now explain this in the text as well.

p11122, l13 I'm a bit lost here: you say that in GISSEH detritus is near zero south of 60 S because temperatures are much lower in HYCOM in that region. My first thought is that low temperatures should lead to more detritus b/c of lower remineralization rates. Also, in l9 you say that HYCOM has shallower mixed layers due to higher SSTs south of 60 S - can you clarify? what do "higher" and "lower" relate to?
It is true that this paragraph was not clearly written. It is now broken into two pieces one that continues the discussion of Fig. 1 and the second continues the discussion of Fig. 3. References are also made to figures and discussion in Romanou et al. (2013).

p11123, l19-21 if I understand correctly, you say that increasing the remineralization rate increases nitrate in GISSER, but decreases it in GISSEH, because GISSER is warmer at the surface than GISSEH and effective remineralization rate changes are larger. But doesn't effective remineralization still increase in GISSEH when you increase the remineralization rate? So how can that aspect alone give a positive nitrate change in one model, and a negative change in the other?
As we increase remineralization rate in both models we will be increasing the surface nitrate distribution. However, other processes play a role at the same time, such as

vertical mixing which in conjunction with the very differnt profiles of temperature in this region lead to different results. MLDs are deeper in the HYCOM (see Fig. 7 in Romanou et al. (2013)) and SSTs colder (see Fig. 3in Romanou et al. (2013)) than in the Russell ocean model. The combination of these processes leads to redistributing nitrate such that we end up with decreasing nutrients as we increase the remineralization rate. This is now clarified in the text.

p11124, l24-p11125, l2 swapping the 2nd and 3rd sentence would make this paragraph easier to understand.
Done and the paragraph is slightly rewritten.

p11125, l27-29 absolute changes in carbon export are much more pronounced in GISSER than in GISSEH, but what about the relative changes?
This is a very interesting question. Indeed the absolute values in the carbon export in the two models are very different for both the high and the low remineralization cases as we already state in the text and as is shown in Figure 7. The largest changes occur in the frontal regions (40°- 60°) North and South. However, if we examine the relative change of exports within each model we see that they are quite similar. We find about 50-70% reduction in carbon export for an about 80% increase in remineralization rate. These reductions in export are found in the subtropical gyre frontal regions and at the equator in both models. This is now included in the text.

p11126, l20-22 "there is a limit of nutrient remineralization increase, beyond which limitations of other nutrients start playing a role" - do you mean that different nutrients are remineralized at different rates in your model? If nutrient stoichiometry is fixed, as I thought it was in the model, how can the limiting nutrient change?
No, different nutrients remineralize at different rates in the model. Details are now given in the text.

p11128, l11 I guess small DIC increases as percentage of total DIC are not too surprising, given the large inventory of DIC compared to, for example, the relatively small amplitude of the seasonal signal.
It is true that the amplitude of the seasonal cycle of DIC in the Southern Ocean is much smaller compared to the inventory of DIC, and in our model the magnitude of the seasonal cycle is about the same as the DIC changes due to remineralization.

p11128, l19-21 same confusion as in p11123, l19-21: how is the sign difference in changes related to one model being warmer than the other? isn't recycling still increasing in both cases?
As we discussed in response to comment on p11123, l19-21, several processes come into play the same time when we increase the remineralization rate. One should not only consider what happens at the surface but at near-subsurface levels as well. This is an important point and we now explicitly address in the text. In this situation, GISSER has higher temperatures than GISSEH therefore more remineralized DIC at the surface. As we increase the remineralization rate, initially both models will have more remineralized DIC at the surface. But the GISSER depth-profile of remineralized DIC will be more surface enhanced and GISSEH will have more remineralized GISSER below the surface. Vertical mixing comes into play then, as GISSEH has deeper mixed layers (see Fig. 7 in Romanou et al. 2013) and therefore mixing with subsurface DIC is such that GISSER ends up with more DIC whereas GISSEH ends up with less DIC. This is all now explained in the text as well.

p11128,l26/27 shouldn't a smaller vertical gradient reduce vertical mixing?
This sentence was indeed misleading in the way it had been written and is now rewritten. What we meant was that the decrease in DIC is not linear with depth as we linearly increase the reminearlization rate.

p11129, l16-p11130, l2 this section is hard to follow, since Fig. 12 apparently depicts

just the air-sea CO2 fluxes, but not their changes due to a change in remineralization rate. Also, the figure caption of Fig. 12 doesn't seem to fit the figure.
There should be four sub-figures in Fig. 12. It is unclear why only two are showing in the rendered version online. We will specifically ensure that the revised manuscript shows all Fig. 12. Further, we avoided difference figures for the flux due to the fact that flux is a signed (plus/minus) quantity and differences can be confusing. The caption is also changed.

p11140, Table 1 what do you mean by "correlations" - correlation coefficients of some sort? Which observations do you use, and what do you mean by "regional"?
We mean correlation coefficients. We have now included information about the 5% significance level for the correlations presented in Table 1. Observational data for chlorophyll abundances come from Watson Gregg's compilation dataset (described in detail in Gregg and Casey 2007, section 2.3). We now mention the data source in the text explicitly.

Figures

all global maps using tick labels -90 to 90 or 90 S to 90 N on y axes would make the maps a lot easier to use (check Fig. 12 for a particularly peculiar y axis). Same goes for larger font sizes in almost all figures. Fig. 1 the colours in the colour scale don't seem to fit those in the plot. Also please avoid using the same colour for land and 0 - it makes the map hard to read. Use grey for the land, for example.
Figure axis legends and fonts are now changed in all figures.

Fig. 2 missing labels for subplots a-f. can you use the same colour scale at least for the 500 m, 1000 m and 3000 m plots? Maybe differences between the model are easier depicted if GISSEH is shown as difference to GISSER, for example?

A new set of figures was produced and differences between the two models are also shown.

Fig. 5 why is the land area around Antarctica different in subplots a compared to c and e?
The figures were redone.

Fig. 8 are the captions texts for subplots c and d swapped, or is it the plot titles? Also, in subplots a and b it looks like you've tested individual values of remineralization rate - would you indicating the fact by using symbols (possibly with connecting lines) rather than piecewise linear lines? Y axis labels with units would make the plots easier to read.
The figures were redone.

Fig. 10 I am not sure how this plot adds new information to the manuscript. The difference in DIC is already shown in Fig. 9.
Figure is removed. We agree that it did not add much to the discussion.

Fig. 12 The figure caption doesn't seem to fit the figure: what/where is "06" and "08"?
Corrected.

Fig. 13 can you please identify which regions the acronyms stand for?
Corrected.

Technical corrections

p11112, l22 I would argue that the solubility and biological pump control DIC concentrations in the surface layers - the air-sea exchange of CO2 is controlled by temperature,

wind speed, carbon system properties etc.
We agree. As solubitility pump we refer to all such factors as wind, SST, SSS and DIC concentrations.

p11113, l28 "... secondary effects on the solubility pump" - sounds like these effects are not as important as those on the biological pump; is that what you mean or is it rather direct vs. indirect effects?
yes, “indirect effects" is now used.

p11114, l8 add dash in "... meso- to the bathypelagic ..."
done

p11115, l13 primary production here abbreviated "pp", later on "PP"
it is now everywhere PP

p11117, l9 typo: "diapycnal"
done

p11117, l8 what is PPM?
done

p11118, l20/21 "Gregg and Casey (2007)"
corrected.

p11120, l9/10 "Gregg and Casey (2007)"
corrected.

p11121, l9 typo: "Subantarctic Front"
corrected.

p11121, l26 typo: "... depths, however, ..."
corrected.

p11122, l6-8 "However, in the GISSEH model, the region ... lies between the ... APF ... and the STCZ, a much narrower region than in GISSEH ..." - is this a typo, and one should be GISSER?
correct.

p11122, l10 typo: missing the in "Although at the surface ... "
corrected.

p11122, l10 GISSEH instead of HYCOM?
corrected.

p11125, l26/27 grammar?
corrected.

p11126, l16 missing (in (Fig. 8a, b)
corrected

p11126, l21 no comma in "... beyond which, limitations ..."
The phrase is re-written now.

p11129, l1 typo: "deepens"
corrected.

p11129, l10 do you mean "changes in surface pCO2 depend only on changes in DIC"?
Yes, corrected now in the text.

p11129, l11 missing space in "T,S"
done

p11129, l22 missing "the" in "South of SAF"
done

p11131, l12 missing "in" in "of the SAF the Southern Ocean"
Corrected.

p11131, l13 and l17 missing "in" in "of the SAF the Southern Ocean"
Corrected.

p11131, l22 no "the" in "the atmospheric CO2"
Corrected.

p11131, l28 missing "the" in "along SAF"
Corrected.

p11131, l29 do you mean undersampled (too few samples) instead of subsampled?
Yes, corrected.

p11132, l2 no () in "decreases of CO2 of (about 6-10%)"
Done.

p11132, l18 Kwon et al. (2009)
Done.

p11133, l2 missing "of" in "the ratio carbon export to" Corrected.

p11141 mg C m3 instead of "miligrams C ..."
Corrected.

p11150 typos: "Units" and "mmol C m3".
Done.

p11152 typos: missing space in molCO2 m2 yr1, missing "In" before "Unshaded"
Done.

p11153 typos: "mmol C m2 yr1 " instead of "mmoles ...".
Done.