Response letter #2 for the manuscript titled "Anthropogenic carbon dynamics in the changing ocean" (os-2010-14)

The authors are very grateful for Referee#2 feedbacks and attention to details for this manuscript. We have now included all of the suggestions and comments in the revised manuscript. Below are more detailed point-by-point responses to Referee#2 comments:

Ref2.: On the other hand, due to the sparse observations, it is difficult to judge how reliable the model results are or what the 'error bar' for the quoted numbers could be. In my opinion, this item could be discussed a bit more detailed in the paper.

Response: In the revised manuscript, we have included error range for all the number quoted in this study. Here, the error range estimates represent the standard deviation of the residuals between the model simulated annual variability and the respected model smoothed (five-year running) mean. This information is now included in the first paragraph of the model validation section.

Ref2.: My question is whether EXP2 only includes physical climate-carbon feedbacks (as it is mentioned in the abstract) or also biological-chemical-physical-carbon feedbacks such as ocean acidification and its possible impacts on marine ecosystems.

Response: In the revised manuscript we stated, "EXP2 represents the fully coupled simulations, which include all feedbacks including biological, physical, and chemical with increasing atmospheric CO2 concentration."

However, the changes in ocean chemistry (i.e., pH) do not affect the biological production directly. For example, the calcium carbonate production in the model is still parameterized as a function of silicate concentration. But the dissolution of calcium carbonate in the model is allowed to feedback to the water chemistry. We will implement carbonate saturation dependent biocalcification once reliable quantifications is available.

Ref2.: If HACOMM5.1 includes biogeochemical feedbacks, the change of 22 Pg C is not only physical (in this case the formulation within the abstract is inadequate).

Response: The authors agree with the referee, the sentence is now revised from
"Another simulation indicates that changes in physical climate alone results in a release of natural carbon of about 22 Pg C"
to
"Another simulation indicates that changes in physical climate and its associated biogeochemical feedbacks results in a release of natural carbon of about 22 +/- 30 Pg C."

Ref2.: section 3 (p. 395, l. 20)
'.. recently published pCO2 climatology-based estimates (Takahashi et al., 2009) of 1.6 +/- 0.9 Pg C yr^(-1).' The value of 1.6 Pg C yr^(-1) given in Takahashi et al. (2009) is the number for the total air-sea carbon flux. Takahashi et al. (2009) assume a preindustrial outgassing of CO2 from the oceans of 0.4 Pg C yr^(-1), resulting in an oceanic uptake of anthropogenic carbon of 2.0 Pg C yr^(-1). (see Takahashi et al., 2009, p. 575)

Response: The referee is correct. We have revised the number accordingly.

Ref2.: section 4.1 p. 397, l. 8)
'Figure 1c ... also indicates a strengthening and poleward shift (i.e. approximately 40°S to 45°S) in the Southern Ocean surface wind speed' according to figure 1.c the wind speed is weakened between 40°S and 45°S and strengthened southward of 45°S, the discussion of fig. 1c in section 4.1 is thus misleading.

Response: We have replaced the statements as follows:
"Figure 1c also shows that our climate change simulation indicates a southward shift in the Southern Ocean westerlies wind by about five degree of latitudes (i.e., approximately between 35degree S and 55degree S, see also supplemental Fig. 2)."

Ref2.: section 5 (p. 401, l. 26)
'Since deep water formation associated with the AMOC ..., the weakening leads to weaker penetration of Cant into deeper layers.' A recent paper deals with reduced Cant storage over the last decade in the North Atlantic due to reduced formation of North Atlantic Deep Water: Steinfeldt, et al. (2009).

Response: The above study by Steinfeldt, et al. (2009) is now cited in the discussions section:
"For example, recent study by Steinfeldt et al. (2009) indicates that in the subpolar North Atlantic, weakening in the formation of Labrador Sea Water has led to a decrease of Cant inventory in this region."

Ref2.: section 5 (p. 403, l. 7)
'We also note that the stabilization of atmospheric carbon uptake in most regions is partly attributed to the long-term equilibration time for atmospheric CO2 into the ocean (Archer et al., 2009).' Archer et al., 2009, give an equilibration time scale of 200 to 2000 years, but I do not see, why this long time scale (which is probably due to the slow oceanic overturning) should lead to a stabilization of CO2 uptake under increasing emissions (which are imposed by the A2 scenario) only in some regions, but not in the Southern Ocean.

Response: The authors agree that the above statements are misleading. Therefore we have rephrased them to:
"We also note that the actual near stabilization of anthropogenic carbon uptake in all ocean regions will only occur after multi-centuries equilibrium period (between different carbon reservoirs) following the stabilization of Cant emissions (Archer et al., 2009).

Ref2.: How does the Delta pCO2 between ocean and atmosphere evolve with time? Maybe in the Southern Ocean, old deep water upwells, which can take up a lot of anthropogenic carbon, in contrast to central waters in the subtropics, which are younger and do not have such a large 'deficit' in Cant. If this is correct, the longer equilibration time for Cant in the Southern Ocean even leads to the increasing uptake there.

Response: As suggested, we have computed the temporal evolution of (latitudinal) delta pCO2 simulated by the model under the A2 scenario for the 1850-2099 period and added a supplemental Figure 3. The delta pCO2 indeed has larger variance in the high latitude regions. We have also included the following statement in the discussions section:

“In addition to the different transport mechanisms, the water masses in the high latitude regions are generally less exposed to the anthropogenic carbon as compared to the younger mid latitude water masses. Furthermore, high biological production during spring and strong winter cooling could lead to larger delta pCO2 values in high latitude regions (shown in supplemental Fig. 3), allowing more efficient anthropogenic carbon uptake (e.g. see also studies by Sarmiento et al. (1992), Sarmiento et al. (1998), Mignone et al. (2006), and Takahashi et al. (2009)).”

Ref2.: Another possible mechanism for the ongoing increase of Cant uptake in the Southern Ocean is the aforementioned decrease in sea ice. Could this effect somehow be quantified?

Response: While significant sea ice reduction in the Southern Ocean leads to considerably Cant uptake in our model, a multi-model intercomparisons study by Roy et al. (submitted to Journal of Climate) shows that all models (including the BCM-C) agree that the majority of anthropogenic carbon uptake in the polar Southern Ocean are mostly associated with increasing atmospheric CO2 rather than climate change.
In addition, we have also included the following statement in the discussions section:
“In the model, the opening of sea ice also contributes to the increase in Cant uptake across the air-sea interface in the region. Tjiputra et al. (2010) estimated that the sea-ice retreat increase the oceanic carbon uptake by an order of 20 Pg C for the same experiment period.”

If the referee would like to get access to the Roy et al. study, we kindly suggest contacting her directly (Tilla.Roy@lsce.ipsl.fr).

Ref2.: section 6 (p. 403, l. 25)
'Our model study thus implies that feedbacks from climate change induced circulation and other physical changes do not reduce the ocean's ability in taking up anthropogenic carbon substantially.' Could 'substantially' somehow be quantified and how do the authors prove this statement?

The question is how large this uptake would be without climate and other physical changes. This could be computed by another simulation with preindustrial climate and increasing CO2 according to the A2 scenario. Such a simulation is not mentioned in the paper. If it does not exist, I do not expect the authors to perform it, but nevertheless the discussion should be a bit more quantitatively, e.g. by comparing the fraction and total amount of the carbon emissions that are taken up by the ocean in the present day climate state (around the year 2000) and at the end of the model run.

Response: Fortunately, we have completed such simulation. We also carefully rephrased the statement “do not reduce the ocean's ability ... substantially”. The following statements are now included in the revised manuscript (in conclusions section):

“For the period of 1850-2099, outgassing of natural carbon is simulated in our model, which amounts to a total of 22 +/- 30 Pg C. The accumulated uptake of anthropogenic carbon for the same period amounts to about 538 +/- 23 Pg C. A similar simulation to EXP2 (not shown here) but without climate and other physical changes (see also Tjiputra et al (2010)) return an accumulated anthropogenic carbon uptake of about 560 +/- 23 Pg C. Our model study thus implies that feedbacks from climate change induced circulation and other physical changes is approximately one order magnitude smaller than the ocean capacity in taking up anthropogenic carbon.”

Ref2.: Figure 3, legend

'All observations value (correct: observational values) represent the modern day periods.' Figure 3 shows Cant fluxes, which cannot be observed directly. In section 3, these 'observations' are called 'estimates', and I would prefer the formulation 'observational based estimates' instead of 'observational values' also for this figure legend, as 'observational values' is misleading.

Response: The above suggestions have been included in the revised manuscript.

Ref2.: Figure 4
The storage term for anthropogenic carbon in each region at the end of the model run could also be written within the figure, which would be a useful additional information. These numbers might show up within the graphs for Cant fluxes from the atmosphere and the lateral fluxes. The Cant storage is just the sum of these two terms, but as the graphs are quite small, an addition of the 'red' and the 'blue' lines by eye results only in a rough estimate. For some regions (Arctic, Southern Ocean), the Cant storage term is provided in the text, but not for the other regions.

Response: We agree that these numbers can be useful, and consequently we have revised figure 4 to include these numbers as well as their respective error bars.

Ref2.: abstract
Long-term response of CO2 fluxes to climate change at the ocean surface and the ocean interior ...' In my opinion it should be 'The long-term response...' and ' ... within the ocean interior'

Response: The above suggestions have been included.

Ref2.: 'modern conditions' is used frequently throughout the paper. I would prefer 'present' or 'present day conditions'.

Response: Similarly, we have implemented the above suggestions to the manuscript.

Ref2.: section 5 (p. 403, l. 10) '537 Pg C' at all other places the Cant uptake is given as 538 Pg C

Response: the number has been replaced with 538.

Ref2.: Figure 1
legend of figure 1b the symbol for the unit 'mole' is 'mol', not 'moles' the symbol for the unit 'liter' is 'l', not 'L' the correct unit is probably mikromol l^(-1) ppm^(-1) ???

Response: The unit has been replaced with [mol l^(-1) ppm ^(-1)]. Note that the there is a "10^(-3)" factor shown in the top left figure.

Ref2.: caption of figure1
'... mean difference between EXP1 and EXP3'
I would prefer 'difference between EXP3 and EXP1', as EXP3 -EXP1 is meant and not EXP1-EXP3

Response: The caption has been revised accordingly.

Ref2.: Figure 4
Longitudes should be added along the x-axis.

Response: Longitude is now included in the revised figure.

Ref2.: Figure 5 and abstract (l. 21) the numbers given for the Cant uptake per area and year seem to be too small by a factor of 10! (area of ocean: $3.61*10^{14}$ m^2; assuming a Cant uptake of 2 Pg C yr^{-1}), the Cant uptake in g C m^{-2} yr^{-1} is about 6, in the figure the mean value for the year 2000 is smaller than 1) Also the numbers given in the abstract are too small by a factor of 10.

Response: This is indeed an error, the value shown in the old figure represents unit of [g C m^{-2} $month^{-1}$]. This error has been fixed in the revised manuscript.