The authors thank anonymous Referee1 for the critical and constructive feedback on the manuscript. Below, we address all of Referee1's comments in more detailed. We believe that after taking into account all of the Referees' comments into consideration, the revised manuscript is now substantially improved compared to its initial version.

Referee 1: The assessment of the model performance is too optimistic. Whenever a significant non-T contribution to fCO2 occurs, large discrepancies between the model and the measurements exist (Fig. 4). This indicates serious shortcomings in the biogeochemical component of the model.

In the revised version we note that the model has caveats, particularly in simulating the biological seasonality in high latitude regions (see section 4.1). However, due to the complexity of the model and lack of long-term pCO_2 observation, it is difficult to assess how these caveats affect the analysis results from this study. For example, Fig. 3 shows that the model seasonality generally is comparable in the BATS and Caribbean region, but there is problem in the mid-latitude NE-ATL, and the NASPG. We believe that similar analysis would be useful to be repeated with the improved model in the near future.
In the Summary section, by the end of the revised manuscript, we further stated our future plan in improving the model as well as validating the updated model with more recent data which will be released in the coming years (SOCATv2).

In addition, the two of the authors are presently involved in an ongoing multi-model study (which include a similar model used here) evaluating the effect of NAO on the carbon cycle component (Keller et al., in preparation), where pCO_2 from different models will be evaluated against observation to get a better insight of the problem.

Ref1: The comparison of the model results with the CARINA data is confined to the Taylor diagramme, to make it more illustrative show also modelled vs. measured data or omit this section.

We follow the suggestion from Referee1. In addition to the statistical summary by the Taylor diagram, Fig. 1 is now expanded to include four scatter-plots of SST, SSS, DIC, and ALK. They further illustrate the spread and variability of the model simulated parameters as compared to those from the CARINA data. Additional discussion is now added in the manuscript as well.

Ref1: Some of the interpretations of the trends in fCO2 and in the CO2 fluxes are also questionable (see below).

We have further clarified and addressed all the comments raised by the referee below.

Ref1: Specific comments Introduction: Please distinguish clearly between the uptake of anthropogenic CO2 and uptake, e.g. in the North Atlantic, caused by the natural cycling of CO2 between the ocean and the atmosphere.

In the revised manuscript (Introduction section), we have included statements calrifying the portion of carbon uptake in the North Atlantic attributed to the anthropogenic CO_2: "For the reference year 2000, a modeling study by Tjiputra et al (2010, Ocean Science) estimates a carbon uptake of 21.6 g C m^{-2} yr^{-1} in the North

Atlantic region between the 18°N and 66°N. Out of this amount, approximately half represents anthropogenic carbon uptake.”

Ref1: Observations: Fig. 3, right panel: Add the mean seasonality obtained from measurements;

The mean pCO_2 seasonality from both the model and observation are now included in Fig. 3, as suggested.

Ref1: 4.1 Regional seasonality of fCO2 p. 10195/line 7: “deviation” instead of “anomalies”;

The suggestion is now included in the revised manuscript.

Ref1: p.10195/10196, NASPG: To explain the phase shift in the pCO2 draw down in NASPG you should also discuss the temporal development of the mixed layer depth that affects the light conditions for plankton and thus has a large influence on the start of the spring bloom.

In the revised manuscript, within the same section (4.1, third paragraph), we have now included discussion on how well the model simulates the timing and magnitude variation of mixed layer depth as compared to the observational estimates. The combination effect of strong MLD and light condition that trigger the spring bloom is also discussed. In addition, we have also added a supplemental figure showing the model simulated mixed layer depth over the 2002-2007 period for all four regions studied in this paper.

Ref1: The differences in the seasonal DIC amplitude might be due to too low winter nutrient concentrations in the model. Nutrient regeneration produces also CO2 and has almost no net effect on DIC.

Referee1 is right that the model used here tend to underestimate the nutrient concentration, particularly in the North Atlantic region. This can certainly contribute to the weaker fCO_2-nonT amplitude shown in Fig. 4. We have mentioned this in the revised manuscript (Section 4.1, fourth paragraph).

Ref1: Explain briefly “sophisticated multi-functional groups of phytoplankton”.

We have included some explanation and briefly mentioned how they (Signorini et al., 2011) were able to produce the low surface pCO_2 in the summer, which our model could not (see. Section 4.1, end of fourth paragraph).

Ref1: 4.2 Regional trends in fCO2 and sea-air CO2 flux p. 10198/line 10: Only the signs of the trends agree.

We agree with Referee1 and have revised the sentence from
“Table 1 shows that both the observations and the model consistently produce the same trend signals for SST and SSS, though the magnitude is weaker in the model.”
to
“Table 1 shows that the model consistently produce the same trend signs with the

observations for SST and SSS, although the magnitude is weaker in the model."

Ref1: p. 10198/line 20 - 23: I can't see any agreement between the model and measurement derived interannual variability. Either abstain from this statement or document it in a more convincing way.

In the earlier version, the statement was meant to point out the agreement in the "amplitude" of the variability. But we follow the referee suggestion and remove the sentence to avoid any confusion.

Ref1: p.10199/line 5: If the data of one particular year determine the slope of a regression line, it is certainly not reasonable to interpret this as a trend. In view of the interannual variability the detection of trends require longer time series.

We agree that longer time series is undoubtedly necessary to provide a more accurate trend analysis from the observation. However, we think that it is still useful to analyze the trend based on the available observation. In the revised manuscript, we also stated what is the observed fCO_2 trend if the year 2007 were removed from the analysis (see Section 4.3, fourth paragraph). We also add the following statement:
"We note that, due to the high surface fCO_2 deviation in the year 2007, longer time series of observation is necessary to yield a more reliable trend analysis."

Ref1: Trends in air-sea fluxes: For the interpretation of the trends in the air-sea fluxes it is necessary to take into account also trends in the gas exchange transfer velocity (wind) and in the CO2 solubility (SST).

The authors agree with the referee and have taken into account the trends of surface wind speed in interpreting the overall trend in air-sea CO_2 fluxes. However, interpreting the contribution of wind speed change to the air-sea CO_2 fluxes is difficult as region such as BATS has two regimes of CO2 fluxes (i.e., carbon uptake during winter and outgassing during summer). So a positive trend in wind speed can translate to either stronger uptake or stronger outgassing, depending on the months where the wind trend signal dominates. Nevertheless, as suggested by Referee1, we have included the surface wind speed trend from both the observation and model to Table 1 and add more discussions where we found it relevant in the revised manuscript. Change in the SST, hence the CO_2 solubility, is already implicitly included in the trends of ocean surface fCO_2.

Ref1: I have a problem with explaining the flux trends by diverging trends in fCO2 and atmospheric CO2. If, for example, the fCO2 trend exceeds that in the atmosphere and if the fCO2 is below the atmospheric level, then partial pressure difference is decreasing and the fluxes are decreasing. Vice versa, if the fCO2 is above the atmospheric level, then partial pressure difference is increasing and the fluxes are increasing accordingly. E.g., Northeast Atlantic: What does the positive slope of the trend line mean? Increasing uptake or decreasing release of CO2? Even if I have misunderstood something, this needs a discussion.

The referee is correct with regards to the trend in fCO_2 and the fluxes. The positive slope represents the trend line of the "sea-air CO_2 fluxes", as labeled for example in Fig.6. Thus positive trend line means increasing sea-to-air CO2 fluxes (or less uptake) whereas negative trend means decreasing sea-to-air CO_2 fluxes (or more uptake). We have further clarified this in the revised manuscript (and in Fig. 6 caption as well) to avoid any confusion.

Ref1: Why don‘t you use annual flux balances to identify trends?

Since most of the underway observation in the three regions do not cover all months of each years studied (i.e., between 2002-2007), it is problematic to estimate the trends based on each individual year's annual flux. As also requested by Referee2, we have included a new subsection (4.2), which discusses the monthly sea-air CO_2 fluxes estimated from both the model and observation. Accompanying (new) Fig. 5 illustrates the model-data fitness as well as provides rough estimates of the annual flux balance.

Ref1: p. 10201/lines 19 -26: I can also not agree with the explanations of the trends in the surface water fCO2: If due to the hydrographic conditions (heat balance) a continuous trend in fCO2 exists that deviates from the trend in the atmospheric CO2 in some regions, the partial pressure difference will change continuously resulting in fluxes that counteract the diverging of the trends and surface water.

After careful reevaluation, we think that the statement on p.10201/lines 22-23, "… the surface pCO2 increases relatively slower than the atmospheric CO2 …" is misleading. As the referee pointed out, over long term, this could lead to increasing ΔfCO2 and fluxes, which is not the case. We have now revised the sentence to, "…the surface pCO_2 increases relatively slower than most other region …". However, we still think that the hydrographic condition plays an important role for long-term sea-air CO_2 fluxes. For example, in the paper Tjiputra et al. (2010, Ocean Sciences), they showed that over the 1850-2099 period the North Atlantic drift region responsible for most of the anthropogenic carbon uptake in the North Atlantic. The study is also mentioned in the revised manuscript.

Ref1: p.10205/line 15: NPP as such does not change the alkalinity. Or do you mean the consumption of nitrate that increases slightly the alkalinity? What‘s about calcifying organisms?

Yes, we meant to say the consumption of nitrate increases the alkalinity. Calcifying organisms reduce the alkalinity in the model but it is generally less significant than the nitrate consumption by biological production. We have clarified this in the revised manuscript (Section 4.4, eleventh paragraph).