Response to reviewers

We would like to thank the reviewers for the in-depth review and constructive comments. Below we provide point-to-point responses to each comment. Reviewer comments are given in italic and responses are given in bold. .

Response to reviewer 1

First of all, we would like to thank the reviewer for providing an annotated PDF file with detailed corrections.

General Comments

1. *While I appreciate the amount of work behind the manuscript, and the non-trivial nature of coupling even existing model components, regrettably I see the problem that there is no model development included in the study that would justify publication in GMD, given the well-known and partly not so new - model components ECHAM5 and NEMO that are employed (developed elsewhere, not at NUIST - this should be clarified). As it stands, I fail to recognize what the new aspect of the study for a wider readership would be, given that earlier CMIP experiments are repeated and no really new aspects of carbon cycle modelling are presented.*

Response: This manuscript presents a newly developed coupled climate system model, NUIST-CSM-2.0.1. We have shown in earlier studies that a previous version of the NUIST model is able to realistically reproduce observed physical atmosphere and ocean climate. The main aim of the manuscript is to show that the newly developed NUIST-CSM-2.0.1, which includes a marine biogeochemical component, is able to reproduce observed large-scale distribution of ocean biogeochemistry fields reasonably well, and the model-simulated oceanic CO_2 uptake is comparable with observations and CMIP5 model ensembles. Thus, we expect that this manuscript can serve as a documentation of the model performance in simulating current-day ocean biogeochemical cycles, which lays the foundation for future model improvement and application.

We agree that NUIST-CSM-2.0.1 is mostly a coupling of different existing model components, including ECHAM 5 for the atmosphere, NEMO3 for ocean circulation, and CICE 4 for sea ice, and PISCES 2 for ocean biogeochemistry. On the other hand, to improve the performance of the coupled model, we do make some non-trivial modifications in some components of the model.

We add a description of the improvements and specific strengths of the NUIST-CSM-2.0.1 in section 2 (Pg3 L28 –Pg4 L5).

2. *I also find it somewhat misleading to call their model an Earth System Model, as the 'NESM' does not include a land biosphere model such as JSBACH or LPJ that have been coupled to ECHAM5/6 in the past. Without such a model component, 'NESM' could not be used to diagnose carbon cycle climate feedback in emission-driven experiments such as analyzed in Friedlingstein et al. (2006), contrary to what is stated here at the end of the abstract. The land surface scheme of ECHAM5 is not a land biosphere model.*

Response: The land component is represented by a land surface scheme in ECHAM5, which is described in the model description section. We agree with the reviewer that this model does not include a land biosphere model that simulates the land carbon cycle. In the revised text, we term this model as climate system model instead of Earth system model.

Since the model does not include a land biosphere component, all experiments in this study use prescribed atmospheric CO2 concentration, and no emission-driven experiment is performed. The carbon-concentration and carbon-climate sensitivity parameters are diagnosed following the study of Arora et al. (2013), which uses prescribed atmospheric CO_2 concentration.

Specific points

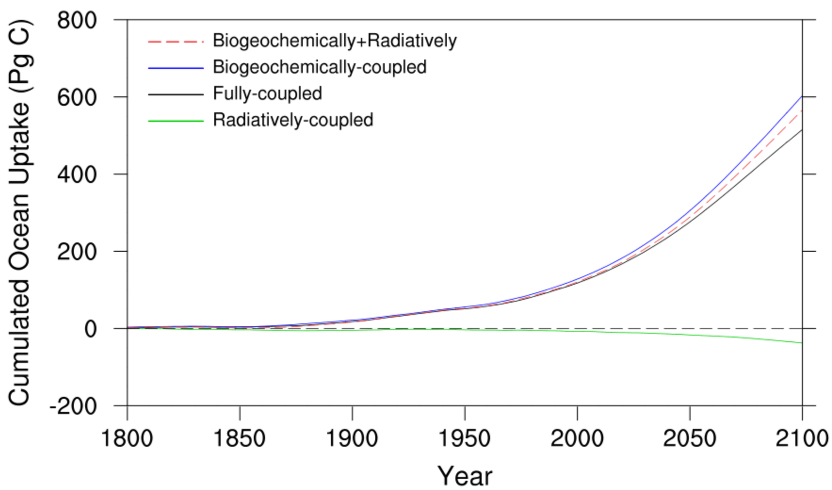
1. *I would prefer the term carbon-climate and carbon-concentration 'sensitivity' (as in Friedlingstein et al. (2006)) over 'feedback' parameter, as the parameters quantify not a feedback, but a sensitivity. I am aware, however, that other studies (e.g. Arora et al., 2013) use the term 'feedback', but do not find it appropriate. It should at least be made clear that the 'integrated' feedback parameters (as in Arora et al 2013, 4.c. 3)) are analyzed.*

Response: We use 'sensitivity' instead of 'feedback' in the revised manuscript.

2. Why are historical runs started at 1800 (not 1860) and how does this influence the integrated carbon uptake?

Response: In the model, the GHGs concentrations in PI simulation are set to 1800 values, and thus we started the historical runs from the year 1800.

The figure below shows the time series of the integrated ocean carbon uptake from 1800 to 2100. The model-simulated integrated ocean CO_2 uptake between 1800 and 1860 is 2.7 PgC, which is quite small compared to the simulated integrated uptake of 144.4 PgC between 1800 and 2011.In table 1, we compare model-simulated integrated CO2 uptake for the period of 1800-2011 with corresponding AR5 estimates.



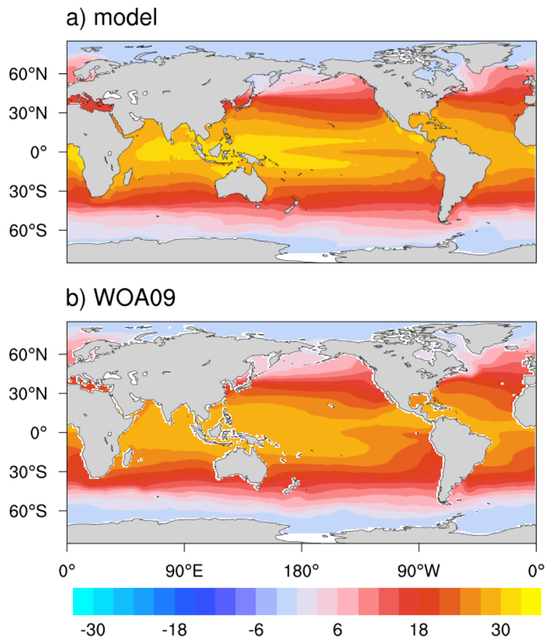
3. How does the cold bias in global mean SST/SAT influence the carbon uptake and why is SST discussed in SEC. 2.2. But SAT used to diagnose climate sensitivity for oceanic carbon uptake? Given that the model has only an ocean carbon cycle component, SST would seem more appropriate to understand the temporal evolution of oceanic CO2 uptake.

Response: We simulate a cold bias in the region of mid-western Pacific Ocean (cold tongue), but not a cold bias of global mean SAT. The NUIST-CSM-2.0.1 simulates a reasonable global mean temperature, compared with CMIP5 models (Pg12 L11-12).

We discussed SST in section 2.2 to validate the quasi -equilibrium state of dynamic ocean climate. The magnitude of the modeled global mean SAT is larger than SST, but their changes are similar. Therefore, the diagnosed carbon-climate sensitivity using either SST or SAT would be similar. Also, previous studies generally use SAT to diagnose carbon-climate sensitivity. We use SAT here to have a better direct comparison with previous studies.

4. It is stated that the simulated Pacific cold tongue is 'shifted' to the west (and that this might explain deficiencies in the biogeochemical fields, and that it will be a focus of future work to correct this), but Cao et al. 2015 state explicitely that in NESM the Pacific cold tongue is simulated 'very well' (their Sec. 4.1, 1st para, unfortunately without a figure) - *maybe this can be clarified.*

Response: As shown in the figure below, the Pacific cold tongue is successfully reproduced in NUIST model, as well as the warm pool in the Indian Ocean and the western Pacific Ocean. However, compared to observations, the model-simulated Pacific cold tongue in the fully-coupled historical run during the 1990s is extended westward. This westward extension is associated with the overestimated upwelling in this region, which would increase entrainment of nutrients from the deep ocean to the surface.



5. How different is NESM climate from IPSL-CM5A climate and from KCM climate?

Response: Compared with KCM, the NUIST-CSM-2.0.1 has an updated version of the ECHAM and NEMO-PISCES, and the NUIST-CSM-2.0.1 also uses different sea-ice model and coupling strategy. The PISCES v1 is used in the KCM while the PISCES v2 is used in the NUIST-CSM-2.0.1. Also, in NUIST-CSM, we made a series of modifications to improve the modeled internal modes, such as ENSO, MJO, ISO, etc. We add a detailed description of the NUIST-CSM-2.0.1 in the model description part.

In the supplemental material, we provide a detailed model comparison of dynamic ocean fields between NUIST-CSM-2.0.1 and CMIP5 models during the pre-industrial era and historical period. Also, NUIST-CAM simulated biogeochemical fields are compared with IPSL-CM5A-LR. In the revised manuscript, we provide corresponding discussion about the comparison in section 3 (Pg7 L6-8).

6. Why is the maximum AMOC 8-10 Sv higher than in Park et al. 2008 (also using ECHAM5/NEMO but for an atmospheric pCO_2 of 348 ppm and a T31L19 atmospheric resolution). Is this the result of tuning, or the lower atmospheric pCO_2, or model resolution?

Response: Compared with CMIP5 MME and observations, the AMOC in NUIST-CSM-2.0.1 is reasonable (figures can be found in the supplementary materials). There are many possible reasons that could account for the difference between the NUIST-CSM-2.0.1 and KCM:

(1) AMOC strength is closely related with the sea-ice behavior which, is different between the NUIST-CSM-2.0.1 and KCM. Also, the versions of ECHAM and NEMO in the NUIST-CSM-2.0.1 and KCM are different, which may result in different representations of ocean circulation.

(2) We have various modifications of ECHAM and NEMO model, including ocean mixing, background eddy viscosity and diffusivity profile, freshwater input. All of these processes would influence deep water formation in the North Atlantic.

(3) The atmospheric resolution may have some impacts on surface ocean wind-driven circulation and precipitation, which may also affect AMOC.

(4) In NUIST-CSM when atmospheric CO2 reaches about 348 ppm at year 2008, simulated AMOC is 20 Sv, which is 4 Sv lower than its pre-industrial value of 24 Sv. Therefore, higher atmospheric CO2 used in Park (2008) can also partly explain their weaker AMOC.

7. p. 4 ln 29 ff: is the chlorophyll-dependent light attenuation scheme applied also to ocean physics or only to the ocean biogeochemistry part? This should be made clear.

Response: Thank you for your comments. The chlorophyll-dependent light attenuation scheme is applied to ocean physics. This point is specified in the manuscript (Pg4 L20).

8. it is a bit counter-intuitive and perhaps confusing to the authors themselves to define the CO_2-flux between ocean and atmosphere as positive for a flux from the ocean to the atmosphere (sea-air flux), but name it air-sea flux... see e.g., p8 ln 21 where an air-sea CO_2-flux of 1.7 PgC is diagnosed for the year 2000, but according to the author's definition it should be -1.7 PgC (flux atmosphere to ocean).

Response: Thank you for your comment. In the ocean model of NUIST-CSM, positive flux represents flux from the ocean to atmosphere.

The manuscript is revised correspondingly, the flux from the ocean to the atmosphere is named sea-air flux.

The diagnosed sea-air CO_2 flux is -1.7 PgC, i.e., the ocean absorbs 1.7 PgC.

9. experiment names would be useful, like Hist-FC, RCP85-FC, 1%-FC; Hist-BC etc. e.g. to state which simulation is shown in the figures (not 'from NESM-2.0.1 simulations')

Response: The experiment names is specified in the manuscript (Pg7 L2) and the captions of the figures.

10. when describing model results, avoid 'is observed' as this is confusing, in particular when also observations are discussed (e.g., p10 ln. 5, 19; p.11 ln.20, 26)

Response: Thank you for your comments. The manuscript is revised correspondingly.

11. how is the anthropogenic carbon computed?

Response: Averaged over the last 100 years of the spin-up simulation, globally integrated sea-air CO_2 flux is -0.03 PgC yr^{-1}. We calculate the ocean uptake of anthropogenic CO_2 by subtracting simulated sea-air CO_2 flux from the pre-industrial value.

12. Fig. 3/4: why is the simulated surface Chl distribution so different from the vertically integrated NPP distribution?

Response: The distribution pattern of the chlorophyll is similar to the phytoplankton instead of the NPP. The distributions pattern of the NPP and chlorophyll in this manuscript is similar to the simulation results from CESM-BGC (Moore et al. 2013), which are as good or bad as other coarse-resolution model results.

In the model, the chlorophyll concentration is associated with the growth, mortality, aggregation, and grazing by zooplankton while the NPP represent the growth of the phytoplankton. In addition to the chlorophyll concentration, NPP is also associated with nutrients, temperature, and availability of the light.

13. Fig. 6 Perhaps as a result of the logarithmic depth-scale, it looks like the column inventory is larger in the GLODAP estimate than in the model from the sections, but the other way round from the column inventory map. Where does the local simulated maximum in anthropogenic CO_2 inventory near the Drake Passage originate from? There does not seem to be an equivalent in the section plot. Do the figures show CO_2, or anthropogenic carbon (i.e., excess DIC, I presume relative to 1800)? This should be included in the units (mmol C or mmol CO_2 m^-3). Why does the simulated high anthropogenic carbon in the NA not start at the surface? Why does oceanic uptake of CO_2 only start at 320 ppm? Which experiment is diagnosed?

Response: (1) The Y-axis of figure 6 in the manuscript is not linear. The amount of the observed anthropogenic CO_2 is larger than the modeled anthropogenic CO_2 only in the upper ocean (above 1000m), but lower in the deep ocean. We use a linear Y-axis to avoid confusion in the revised manuscript (Fig 9).

(2) The maximum in anthropogenic CO_2 inventory near the Drake Passage may be associated with the modeled ocean circulation, which needs further study.

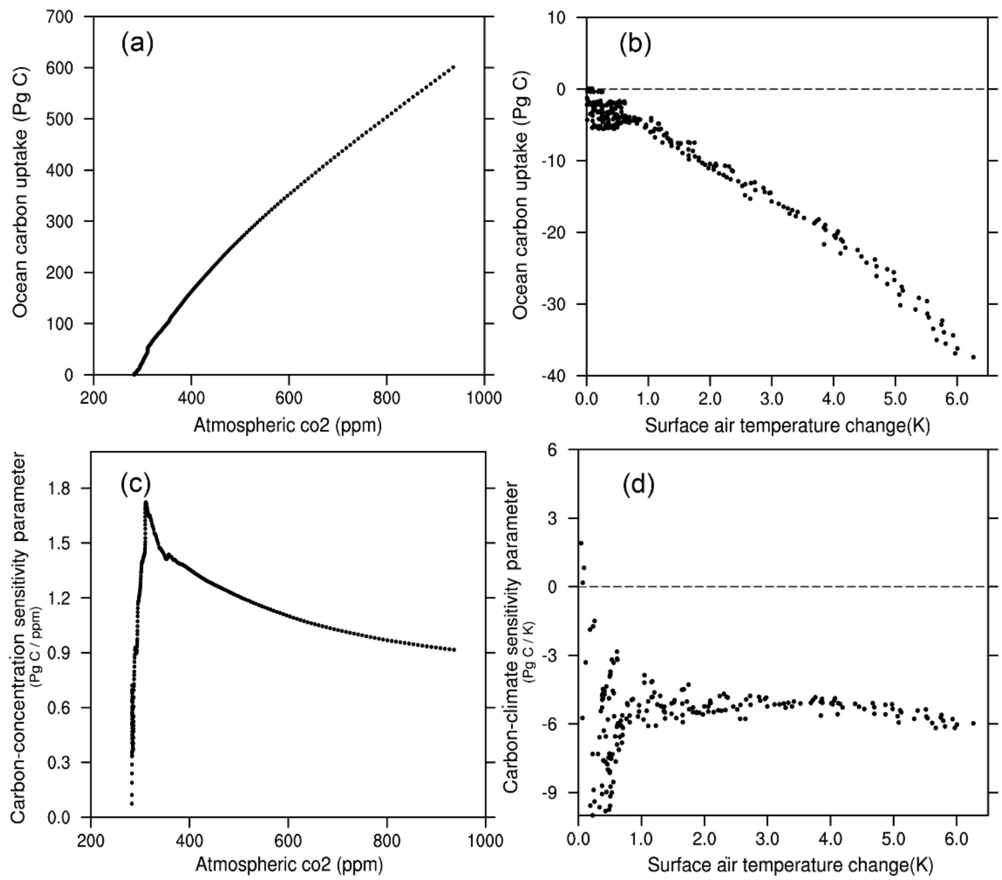
(3) The differences of anthropogenic CO_2 in NA may be attributed to different missing grids. All missing grids are set to be the same for the model simulation and observations in the revised manuscript.

(4) The figure shows the anthropogenic carbon that is specified in the figure caption. The unit is changed to mmol C m^{-3}

(5) In our simulations between year 1800 and 1950 or so, there is no obvious trend between diagnosed carbon-concentration sensitivity parameter and atmospheric CO_2, and between carbon-climate sensitivity parameter and CO_2-induced warming, as shown in the figure

below This is because of the small signals in the change of temperature and oceanic CO2 uptake, compared to their interannual variability, especially when we diagnose the carbon-climate sensitivity parameter from RC simulations. In the ms, we only diagnosed these two diagnosed parameters from 1950 to 2100 in figure 14. The figure caption is revised correspondingly.**

It is mentioned that the parameters compared with CMIP5 models (Fig 16) are diagnosed for the whole period (140 years) from idealized 1% CO_2 experiments, which is consistent with Arora et al. (2013).



14. search for and correct the following miss-spelled words:

Response: Thank you for your suggestions. We have checked throughout the manuscript and made corresponding corrections.

15. decide on macronutrients/macro-nutrients, Equator/equator, Tropics/tropics, PAR/par

Response: Thank you for your suggestions. We have made corresponding corrections.

16. last not least: model layers are not vertical - delete 'vertical'

Response: Thank you for your comments. We changed 'vertical levels' to 'layers'.

17. check references for captial letters (and 'technology' in affil. 1)

Response: Thank you for your comment. We have checked throughout the manuscript for capital letters.

18. some authors missing in Jones et al 2013

Response: Thank you for your comment. The missed authors are added.

19. Some typos in Madec and NEMO Team ref.

Response: Thank you for your comment. This reference is revised.

Response to reviewer 2

General comments

1. The first has to do with the fact that the specific combination of model components used here for the NUIST climate model (I would rather call it a climate model than an earth system model, since so far it does not involve a full interactive carbon cycle, but just an ocean carbon cycle, with atmospheric pCO2 prescibed) is quite similar to some other climate models, especially the Kiel climate model. This does not mean that the results have to be the same, since the equilibrium state of a model depends on many details of how the sub-models are set up, e.g. their spatial resolution, the choice of eddy parameterizations etc. Nevertheless I think the authors should do *some effort to discuss in which aspects their model results are different from similar other models. Does the physical circulation (be it atmosphere or ocean) in the NUIST model have any specific strengths (or maybe weaknesses), and does this have any effect on the modeled biogeochemical fields?*

Response: Thank you for your suggestions. The model's name has been changed to NUIST climate system model (NUIST-CSM-2.0.1).

Compared with the Kiel climate model (KCM), the NUIST-CSM-2.0.1 has an updated version of the ECHAM and NEMO-PISCES, and the NUIST-CSM-2.0.1 also uses a different sea-ice model and coupling strategy (more variables exchanged in the coupled system). The PISCES v1 is used in the KCM while the PISCES v2 is used in the NUIST-CSM-2.0.1. Also, in NUIST-CSM, we made a series of modifications to improve the modeled internal modes, such as ENSO, MJO, ISO, etc. We add a detailed description of the NUIST-CSM-2.0.1 in the model description part. Given these differences between the two models, different behaviors of the marine biogeochemical cycles and their responses to climate change are expected (Pg3L28-Pg4L5).

Direct comparison with of NUIST-CSM-2.0.1 and KCM results is not provided because we cannot obtain the simulation results from KCM group or available publications. According to Dr. Wensun Park, who is the leader of the KCM group, they have not ever done PI, historical, and RCP8.5simulations with an active PISCES model. Also, KCM employs an early version PISCES v1 (Aumont et al. 2003) to represent the marine biogeochemical cycle. A detailed introduction of the main differences between PISCES v1 and v2 can be found in Aumont et al. (2015). As an alternative, we compare our model with the IPSL-CM5A-LR instead, which also uses the NEMO-PISCES system. The results are submitted as supplementary materials and discussed in the manuscript (Pg7 L20-28).

2. My second comment is that at the present state of the manuscript the model-data (or rather model-climatology) comparison is not informative enough to really show that the model can be used for estimating the strength of climate-carbon cycle feedbacks. The main deficiency is that the comparison to data is limited to surface fields (nutrients, chlorophyll, productivity and air-sea carbon flux), and to global average vertical profiles of nutrients only. The surface nutrient fields look quite ok, but then this is not a very difficult task since they mainly depend on a reasonable distribution of up- and downwelling, which is largely pre-determined by the wind field, and by *representing some iron limitation. The chlorophyll and NPP fields, as well as the air-sea carbon flux look qualitatively reasonable, and probably are as good or bad as other coarse-resolution model results. But the vertical profiles of nitrate and phosphate clearly show that the model has a strong low bias in the upper 2000m and a high bias below that. The same pattern is then probable for dissolved inorganic carbon, for which no comparison is shown; I expect therefore DIC to be too high below 2000m as well. There are several possible explanations for that; these include (but are not limited to) a too deep remineralization, or a too sluggish deep ocean circulation, or a too strong DIC storage in AABW, caused by a too strong production in the Southern Ocean, or a completely wrong alkalinity distribution.*

To help in the interpretation it therefore would be good if the nutrient comparison had been extended over the full water column, and not just the surface, and the deep fields would have been shown, too. It would also be informative to show the DIC and Alkalinity fields, and not just anthropogenic carbon, which is not much affected by biology. Some information on which nutrient is the most limiting in which part of the ocean would also be helpful in assessing the validity of the modeled biogeochemical cycles. The author should therefore extend their analysis of model results and the comparison with data substantially.

Response: Thank you for your comments. In the revised manuscript, we have made a more comprehensive comparison between model-simulated biogeochemical fields and available data-based observations, including the geographic distribution of upper 100m nutrients, alkalinity, and DIC (Fig. 1 and 6), the depth-latitude distributions of nutrients, alkalinity and DIC in the Atlantic Ocean and the Pacific Ocean (Fig. 2 and Fig. 7).

We have also diagnosed the spatial pattern of nutrients limitation for nanophytoplankton and diatoms (Fig. 4). Corresponding discussions can be found in Pg8 L7-L23, Pg9 L8-L20, and Pg10 L11-L20.

3. *In addition to these central points the manuscript contains quite a number of slightly wrong statements, in the description of the biogeochemical model component and the biogeochemical results. Just one typical example here, more are listed in the detailed comments: On page 6, line 7-8 growth of phytoplankton is said to be limited by the availability of nutrients, but the dependence on light is forgotten.*

Summing up, the paper cannot be published in the present form and would need substantial revisions. The revision should include a better description of how the model set-up and results differ from other very similar models, especially the Kiel climate model, and a much more comprehensive evaluation of the comparison to climatological nutrient data.

Response: Thank you for your suggestions. Because the Kiel modeling group haven't published their model and simulation dataset, we compare our results with IPSL-CM5A-LR simulation results instead, which is also using NEMO-PISCES system to represent dynamic ocean circulation and biogeochemical cycle. The results are submitted as supplement material.

As suggested, in the revised manuscript, we have performed a more comprehensive evaluation of the simulated distribution of marine biogeochemical fields, including latitude-depth distribution of DCI, alkalinity, and nutrients.

In PISCES v2, the growth of phytoplankton is limited by the availability of nutrients, light, and temperature, and the formulation could be found in Aumont et al. (2015). We rewrite the whole model description in section 2.

In addition, a detailed model comparison of dynamic ocean fields between NUIST-CSM-2.0.1 and CMIP5 models during the pre-industrial era and historical period is provided as supplements. Also, the biogeochemical fields during the pre-industrial era are compared with IPSL-CM5A-LR, and the results are provided as supplements. In the revised manuscript, we give a brief discussion of the comparison (Pg7 L6-L28).

Specific points

1. Abstract: I would mention that at present the model does not include a fully prognostic carbon cycle.

Response: Thank you for the comment. We modify the sentences in the abstract and add a description in the section 2 (Pg5 L33-L34). The model name is changed to NUIST climate system model (NUIST-CSM-2.0.1) instead of NESM.

2. page 2 line 7 (p2l7 in the following): Menon et al 2007 is not the right citation for that statement

Response: Thank you for your comment. This citation is revised to Denman et al. (2007). Pg19 L13.

3. p2l11-12: 'transport of inorganic and organic carbon' probably means particulate, produced by the surface biota?

Response: These processes refer to the carbon pumps. Solubility pump transports DIC from the surface to the deep ocean, and soft tissue pump and CaCO3 pump transports organic carbon to the deep ocean. The latter two pumps is related to the particulate produced by the surface biota (Pg2 L7-L10).

4. p2l17: No, it is not just the solubility which is different, but also the buffering, i.e. the distribution of DIC over CO2, HCO_3^- and CO_3^{2-}

Response: Thank you for your comments. The manuscript is revised (Pg2 L16-25).

5. p2l20-21: There is more to the increase in stratification than just the slower uptake of anthropogenic carbon: It reduces nutrient supply but at the same time increases average mixed-layer light. The authors should do some reading.

Response: Thank you for your comments. In the revised manuscript, we give a more comprehensive description (Pg2 L20-25).

6. p3l17: But it should be mentioned that the model in Seferian et al. included a full carbon cycle, unlike the present model.

Response: Thank you for your suggestion. The manuscript is revised (Pg3 L11-L13).

7. p5l24: the statement 'no modification is made' is unsufficient to know exactly which model parameters were used. Is it exactly the same as those presented in the recent model description of PISCES, Aumont et al, 2015?

Response: We provide a more detailed description of the model and experiments set up in section 2. The whole model description is rewritten. All the biogeochemical parameters in PISCES are the same as those used in Aumont et al. (2005).The only exception is that the advection scheme for passive tracer used in this manuscript is Total Variance Dissipation (TVD) formulation that is used in the physical ocean component, while Aumont et al. (2015) used Monotone Upstream Scheme for Conservative Laws (MUSCL) formulation (Lévy et al., 2001).

8. p6l2: Is the advection scheme different from the one used in the Kiel climate model?

Response: We are not aware of any publication that mentioned advection scheme used in Kiel model. There is only one paper of Kiel model that involves marine biogeochemical cycle, discussing oxygen variation in tropical Pacific. It is noted that the model used different versions of PISCES (version 1 for Kiel model and version 2 for NUIST model). The main difference between the two versions of PISCES can be found in Aumont et al. (2015).

9. p6: The whole description of the biogeochemical model is confusing, often plain wrong, sometimes misunderstandable. What are 'the processes between nutrients, phytoplankton and zooplankton'? (l5) Isn't detritus missing here? Is there more than one group of phytoplankton and zooplankton? Doesn't growth of phytoplankton also depend on light? Why is the mortality and aggregation of phytoplankton associated with calcifying organisms and biogenic silica? Who are the producers? What does the sentence'50% of the calcified organicsms are associated with the shell' mean? What is the difference between 'degradation' and 'remineralization' here? I stop here, but this desription is probably not worth improving, it must be written new.

Response: The whole model description (section 2.1) is rewritten in the revised manuscript.

Detritus is described by different types, including particulate organism matter, calcite, iron particles, and diatoms silicate. The sinking speed of detritus increases with depth in the model.

Remineralization and degradation represent the same process in the PISCES. The processes of remineralization (degradation) of DOC can occur in either oxic or anoxic water that

depends on the local oxygen concentration. The parameter of degradation rate depends on the local temperature with a Q10 of about 1.9. The description is rewritten.

10. p7, l7-8: how do the two numbers on air-sea flux and linear drift fit together? Shouldn't they be equal?

Response: The air-sea flux is the air-sea CO_2 exchange per year in the unit of PgC/yr. The linear drift is the trend of air-sea flux during the last 100 year in PI control simulation in the unit of PgC/yr per year.

11. p8, l18: You do not evaluate the ecosystem, but only the biogeochemistry.

Response: Thank you for your comment. The title of section 3.1 and Pg7 L30-L31 is revised.

12. p8, l24: One cannot say that 'Nutrients limit the growth', but rather that a lack of nutrients limits growth, or that nutrients are required for growth.

Response: Thank you for your comment. The description is revised (Pg8 L4).

13. p9: Just showing surface distributions and global average profiles of macronutrients is simply not enough. The deep concentrations, especially the gradient between the deep Atlantic and Pacific is an important additional quantity. Without information on the distribution of alkalinity one also cannot evaluate the DIC distribution (which is also not shown).

Response: Thank you for your comments. In the revised manuscript, the spatial distributions of macro-nutrients, DIC, and alkalinity are shown for the upper 100m ocean (Fig 1, Fig. 6). We also show the latitude-depth distribution of nutrients, alkalinity, and DIC averaged over the Atlantic Ocean and the Pacific Ocean (Fig.2 and 7).

14. p9, l14-15: The sentence 'The modeled deficiencies of nutrient distribution (which ones are meant here?) is associated with the modeled deficiencies in the ocean dynamics and/or parameterization of the ocean biological processes' is far to general and does not explain *anything. Which deficiencies are explained by which? As it stands, this sentence says: we do not care.*

Response: Thank you for your comments. We provide a more comprehensive discussion in Pg8 L20-25. The modeled deficiencies here mainly refer to the model-simulated bias in vertical distribution of nutrients. In the future, we plan to implement the simulation of Delta 14C in the model. The large-scale distribution of Delta 14C in the ocean is primarily controlled by ocean dynamics. Therefore, the simulation of Delta 14C would provide a good benchmark to test model-simulated ocean dynamics and help to single out dynamic and biological effects on simulated distribution of carbon-related tracers.

15. p9, l22 and following: It is unclear which regions are meant by 'coastal' here? I have the suspicion that it is the subpolar gyres, which are not coastal at all. Maybe an oceanographer should have a look at the manuscript.

Response: Thank you for your comments. The underestimate of NPP can be found in large parts of the high latitude regions. However, the underestimate of NPP is largest along the coastal regions. Thus, we emphasize the 'coastal' region.

16. p10,l7-8: an underestimation of Chl by satellites is one possible reason, but another is a lack of iron-limitation in the model. The iron field is nowhere shown.

Response: Thank you for your comments. Because of the lack of the observed global iron distribution, we do not show the modeled iron field in this manuscript. In the revised manuscript, we show the distribution pattern of nutrients and light limitation on nanophytoplankton and diatoms (Fig. 4). As shown in the figure, In the Southern Ocean, iron is the most limiting factor.

17. p10, l9-10: Chlorophyll does not only affect photosynthesis by attenuating light, but it is the main driving factor in NPP.

Response: Thank you for your comment. I agree with you that the chlorophyll is the main driver of NPP. The corresponding texts have been revised (Pg9 L21-22).

18. p10, l12: which Satellite-NPP product is used here?

Response: The detailed description of Satellite-NPP product can be found in Appendix. NPP is calculated based on the Vertically Generalized Production Model (VGPM) that was first proposed by Behrenfeld and Falkowski (1997a, 1997b) and is widely used in the estimate of global marine net primary production. The description in Pg9 L22-25 is also revised in the manuscript as follow to give more information.

19. p11, l23-24: In my experience, in models with prescribed atmospheric pCO2, the air-sea flux of CO2 does not depend strongly on the gas exchange piston velocity; it is mostly the oceanic pCO2 that adjusts. I think the model bias in the equatorial Pacific has probably more to do with the modeled upwelling.

Response: Thank you for your comments. We agree with you that pCO2 is the main contributor here. We have compared the geographic distribution of modeled bias in air-sea CO2 flux and pCO_2. The modeled pCO_2 shows a similar bias pattern as modeled sea-air CO_2 flux, but the percentage of the bias of pCO_2 is smaller than that of sea-air CO_2 flux. We agree that the model bias in pCO2 in the equatorial Pacific is probably mainly associated with the bias in modeled upwelling, and overestimated wind speed also contributes to the bias in air-sea flux of CO_2. The manuscript is revised correspondingly (Pg11 L4-8).

20. p12, l24: The comparison of surface silicate is far too general. Where, by how much?

Response: Thank you for your comments. We give a more detailed discussion of surface nutrients in Pg8 L8-16, and we also add the geographic distribution of the difference between modeled nutrients and observations in figure 1.

21. p16, l4: what is meant by 'the CO2 biogeochemical effect'?

Response: It refers to the direct effect of the increasing atmospheric CO2 concentration on the oceanic CO2 uptake.

22. p16, l18-20: I did not understand what the authors want to say here.

Response: Here, we explain that the different background state of the temperature, DIC, atmospheric CO_2 concentration in BC run, RC run, and FC run may lead to the nonlinearity among the carbon-climate feedback and carbon-concentration feedback. The manuscript is revised (Pg14 L3-9).

23. Typographical and spelling errors

Response: Thank you for your comments. The spelling errors are corrected.

24. p6, l21: Expect -> except. But even with this I don't understand the sentence.

Response: The corresponding model description part is rewritten in the revised manuscript.

25. p6, l21: 'The amount of unburied parts': what is meant here?

Response: In the sediment model, not all tracers are buried into the sediment when they reach the ocean bottom. The part of tracers which is unburied would dissolve back into the ocean water instantaneously. The sentence is rewritten in Pg5 L5-8.