Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-68-RC2, 2018

Geoscientific Model Development Discussions
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Interactive comment on "Marine biogeochemical cycling and climate-carbon cycle feedback simulated by the NUIST Earth System Model: NESM-2.0.1" *by* Yifei Dai et al.

Anonymous Referee #2

Received and published: 9 June 2018

1 General evaluation and recommendation

The manuscript by Dai et al. presents a set of model runs from a variant of the Nanjing University Earth System Model that includes an atmospheric and an oceanic circulation model, but now also a representation of marine biogeochemistry. The model, like most earth system models, is a combination of several well-known components (in this case ECHAM 5 for the atmosphere, NEMO 3 for the ocean circulation, CICE 4 for the sea-ice, OASIS 3 for the coupling, and PISCES 2 for ocean biogeochemistry) and the new aspect is mainly the conjoint behaviour of these different components.

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The main aim of the manuscript is to show that the model in this configuration produces results, concerning the present-day distribution of ocean biogeochemistry and projections of carbon cycle changes until the end of this century which are in agreement with the findings from CMIP5, presumably as a preparation for using this model also in future model intercomparisons like CMIP6.

I have mainly two criticisms of the manuscript. 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? By answering this question, the paper would gain an aspect of novelty which at the moment it is lacking.

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, chlorphyll, 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 an a reasonable distribution of up- and downwelling, which is largely pre-determined by the wind field, and by representing some iron limitation. The chlorphyll 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 Alkaliity 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 authors should therefore extend their analysis of model results and the comparison with data substantially. A typical example how this is done is for example found in

Schneider, B., Bopp, L., Gehlen, M., Segschneider, J., Frölicher, T. L., Cadule, P., . . . Joos, F. (2008). Climate-induced interannual variability of marine primary and export production in three global coupled climate carbon cycle models. Biogeosciences, 5(2), 597–614. doi: 10.5194/bg-5-597-2008

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.

2 Minor points, errors in statements

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

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

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

p2l17: No, it is not just the solubility which is different, but also the buffering, i.e. the distribution of DIC over CO_2, HCO_3^- and CO_3^{2-}

p2l20-21: There is more to the increase in startification 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.

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

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?

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

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.

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

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

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.

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).

p9, l14-15: The sentence 'The modeled deficiencies of nutrient distribution (wehich 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.

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

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oceanographer should have a look at the manuscript.

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.

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

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

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.

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

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

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

3 Typographical and spelling errors

page 1, line 14 (in the later listings I abbreviate this as p1l14): estimate -> estimates

p1l16 and l17 *the* carbon-climate

l1l18: *an* increase

There are many more instances of missing 'the' or 'an'; I stop listing them here. The authors shoudl give the manuscript someoone to read who can correct that.

p2l2: has -> have

p2l7: more than 50 times of carbon than that of the atmosphere -> contains more than

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50 times the amount of carbon than the atmosphere

p2l10: molecular -> molecule?

p6, l11: bigenic -> biogenic

p6, l20: would BE buried

p6, l21: 'The amount of unburied parts': what ie meant here?

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

p6, l23: recharge -> discharge

p7, l7: spun-up -> spin-up

p10, l17: veridical -> vertical?

p17, l6: sensitive -> sensitivity

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