

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.

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Received and published: 4 June 2018

Review of

Marine biogeochemical cycling and climate-carbon cycle feedback simulated by the NUIST Earth System Model: NESM-2.0.1

by Yifei Dai, Long Cao, and Bin Wang

In their study, the authors repeat a series of CMIP5-style experiments to estimate oceanic uptake of anthropogenic carbon with a coupled atmosphere - ocean - sea-ice model (ECHAM5/NEMO), with a marine biogeochemistry module (PISCESv.2) coupled

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to the ocean model. In particular, atmospheric pCO₂ is prescribed for the IPCC historical period (here starting at 1800 AD, not 1860 AD) and IPCC future period (RCP8.5 scenario 2006 until 2100 for the latter), and idealized 1% p.a. atmospheric pCO₂ increase experiments until atmospheric pCO₂ has quadrupled (140 years). For each of these CO₂ trajectories, a set of three experiments, namely a fully coupled, a radiatively coupled, and a biogeochemically (CO₂) coupled model run following the CMIP5 protocol is performed and analyzed.

The authors find that the simulated present day biogeochemical fields agree reasonably well with observations (even though this is formulated less optimistic in the discussion section of the ms.), and diagnose the sensitivity of oceanic carbon uptake to climate and CO₂ concentration (called feedback parameters in their study). The authors find that their diagnosed values of CO₂ uptake and sensitivity parameters are in the range of observation-based estimates for the historical period, and within the range of CMIP5 model results for the 21st century.

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.

Moreover, the authors do not seem to be aware of the earlier Kiel Climate Model (Park and Latif, 2008, Park et al., 2009). KCM already combines MPI's ECHAM5 atmospheric and IPSL's NEMO ocean model. KCM has also been used with PISCES (see Xu et al. 2015). 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,

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'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.

In addition to the above points, the paper is sometimes difficult to read, as too often formulations are confusing, numerous definite and indefinite articles are missing, sometimes plain wrong words are used, words seem to be missing, singular and plural are mixed up and wrong grammatical endings are used. Some editing by a more English proficient person before submission would have been appreciated.

I add the following more specific comments for consideration:

- 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.
- 2) why are historical runs started at 1800 (not 1860) and how does this influence the integrated carbon uptake?
- 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.
- 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 explicitly that in NESM the Pacific

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cold tongue is simulated 'very well' (their Sec. 4.1, 1st para, unfortunately without a figure) - maybe this can be clarified.

5) how different is NESM climate from IPSL-CM5A climate and from KCM climate?

6) why is the maximum AMOC 8-10 Sv higher than in Park et al. 2008 (also using ECHAM5/NEMO but for an atmospheric pCO₂ of 348 ppm and a T31L19 atmospheric resolution). Is this the result of tuning, or the lower atmospheric pCO₂, or model resolution?

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.

8) it is a bit counter-intuitive and perhaps confusing to the authors themselves to define the CO₂-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 CO₂-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).

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')

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)

11) how is the anthropogenic carbon computed?

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

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

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maximum in anthropogenic CO₂ inventory near the Drake Passage originate from? There does not seem to be an equivalent in the section plot. Do the figures show CO₂, or anthropogenic carbon (i.e., excess DIC, I presume relative to 1800)? This should be included in the units (mmol C or mmol CO₂ m⁻³).

Why does the simulated high anthropogenic carbon in the NA not start at the surface?

Why does oceanic uptake of CO₂ only start at 320 ppm? Which experiment is diagnosed?

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

meanpreindustrial -> mean preindustrial

molecular-> molecules

Lappace/IPPSL -> Laplace /IPSL

tropic -> trophic

bigenic -> biogenic

Expect -> ??

spun simulation, spun-up simulation -> spin-up simulation

veridical -> vertical

21th -> 21st (or twentyfirst)

phosphate (P), nitrate (N) -> phosphate (PO₄), nitrate (NO₃)

nitrite -> nitrate (p9 ln 10)

e.g. -> i.e. (p14 ln 8,9)

preindustry -> pre-industrial (Tab.2)

15) decide on macronutrients/macro-nutrients, Equator/equator, Tropics/tropics,

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- 16) last not least: model layers are not vertical - delete 'vertical'
- 17) check references for captial letters (and 'technology' in affil. 1)
- 18) some authors missing in Jones et al 2013
- 19) some typos in Madec and NEMO Team ref.
- 20) you may also want to consider the annotated pdf attached below

References

Park, W. and Latif, M., Multidecadal and multicentennial variability of the meridional overturning circulation (2008), GRL, VOL. 35, L22703, doi:10.1029/2008GL035779.

Park, W., N. Keenlyside, M. Latif, A. Ströh, R. Redler, E. Roeckner, and G. Madec (2009), Tropical Pacific climate and its response to global warming in the Kiel Climate Model, J. Clim., 22, 71–92, doi:10.1175/2008JCLI2261.1.

Xu, X., Segschneider, J., Schneider, B., Park, W., Latif, M. (2015), Oxygen minimum zone variations in the tropical Pacific during the Holocene, GRL, VOL 42, 8530-8537.

Please also note the supplement to this comment:

<https://www.geosci-model-dev-discuss.net/gmd-2018-68/gmd-2018-68-RC1-supplement.pdf>

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2018-68>, 2018.

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