Earth System Dynamics Discussions

Earth Syst. Dynam. Discuss., referee comment RC1
https://doi.org/10.5194/esd-2021-64-RC1, 2021

Comment on esd-2021-64

Anonymous Referee #1

Referee comment on "Indian Ocean marine biogeochemical variability and its feedback on simulated South Asia climate" by Dmitry V. Sein et al., Earth Syst. Dynam. Discuss., https://doi.org/10.5194/esd-2021-64-RC1, 2021

Review of Sein et al. entitled "Indian ocean marine biogeochemical variability and its feedback on simulated South Asia climate"

General comments:

Based on two numerical experiments run with the ROM model, Sein et al. investigate how the attenuation of the incoming radiation by chlorophyll may perturb the ocean response. Then they evaluate the resulting feedback from the altered ocean on the coupled ocean-atmosphere system. This topic is of interest as it sheds light on the uncertainties associated with model configuration when modelling climate and ocean-atmosphere interactions.

Major comments:

The scientific questions are of great interest, and the paper is in general well-structured and well-written.

Figures:

I recommend to put more care on the readability of your figures. Below are few examples of the improvements you may bring to them:

- Figure 2: to add a title per row on the plot (DJF or JJAS) would help the reader to

quickly understand the results.

- Figure 8 has no x- and y-ticks.
- Figure 13 has no x- or y-ticks. Maybe a) JJAS, b) DJF, and c) (INDB-INDJ) with inside subtitle JJAS (left) and DJF (right).
- Figures 14, 15 and 17: This is a cosmetic suggestion that you may take or not into account: I think it would improve the readability of those Figures to put a title for each column, and each line. Something like (this example is done for Figure 15):

ERA5 INDJ (INDB-INDJ)

Total A B C

convective D E F

large-scale G H I

- In general, for most of your Figures, ticks are too small.

Minor comments:

l. 114: "The oceanic component of ROM is the Max Planck Institute Ocean Model" -> On the basis of what I understand I would suggest to write: "The oceanic component of ROM is the global Max Planck Institute Ocean Model". When reading your model description it is not straightforward to understand that your oceanic model has a global configuration, and that's why "MPIOM provides the possibility to refine the grid resolution in the region of interest and to avoid the lateral boundary conditions in the ocean while performing calculations". Please clarify.

l. 125: Please specify how many vertical levels has your ocean configuration.

l. 166: WOA13 ? why don't you use the latest release WOA2018 ?

l. 169: "NE winds": north-easterly winds ? Define "NE".
l. 171: "SW winds": south-westerlies ? Define "SW".

l. 171: I find a bit strange to use 3 months for winter mean (DJF), but 4 months for the mean of the summer season (JJAS). Please explain briefly why, or change it.

l. 190: Following your draft structure, you have to put in italics “Sea surface concentration of dissolved nitrate”.

l. 250: given that the amplitude of your Chl-a improves when using the variable C:Chl ratio, are you going to try to replace the fixed C:Chl ratio by the parameterization of Anderson directly in HAMOCC in a future study ?

l. 219: “While these changes are in accordance with the changing wind regime, the satellite data also shows higher concentrations during summer.” In its actual form the sentence is not easy to interpret for me. I would mark more clearly the opposition between your model results that are in accordance with the wind regime but not with the satellite data. -> something like “ These modeled Chl-a changes are in accordance with seasonal changes of the wind regime. However the satellite data show high concentrations during summer in that coastal area that our model did not represent.”

l. 220: “The most plausible explanation for this is a persistently high supply of riverine nutrients around the year.” Do you want to say that this persistent high supply is not represented in your model ? From l. 139-141 I understand that you did not represent riverine inputs: “Secondly, coastal characteristics, especially in front of large rivers with high nutrient load and limited exchange with the open ocean, are not resolved which is however crucial in high resolution downscaling simulations.” Please, clarify.

Figure 3: Surface Chl-a in DJF show a strong equatorial tongue not presents in the observations. In l. 223-224 you stated that “the enhanced model’s equatorial surface phytoplankton concentration cannot simply be related to incorrect wind simulation”. However I wonder if it may not be inherited from the forcing fields outside the modeled domain ? If I have correctly understood your coupled configuration, the atmospheric forcings outside your simulated region come from MPI-ESM, and so may imprint direct biases to 1) the atmospheric fields inside your simulated domain (but you found “good agreement, both qualitative and quantitative, between model’s winds and ERA5’s winds”), as well as 2) to the ocean outside the modeled domain. Then 1) and 2) would indirectly affect the ocean inside the modeled domain.

Regarding forcings description:

- l. 127: “The model is driven by data from a CMIP5 20th century simulation with the MPI-

ESM LR setup." Following my previous comment, I would have rather written: "The model is driven by atmospheric data from a CMIP5 20th century simulation with the MPI-ESM LR setup."

- l 450-451: "It should be noted that in both INDJ and INDB experiments ROM is forced by MPI-ESM and the biases of the driving ESM influence the results (e.g., Cabos et al., 2020)." Here also I would specify "[…] and the atmospheric biases of the driving ESM […]".

l. 224: "The problem may be related to relatively coarse vertical resolution of MPIOM in the upper layer (16 m) together with simple turbulence closure scheme in MPIOM based on (Pacanowski and Philander, 1981)." For me Pacanowski and Philander (1981) allow to diagnose vertical mixing coefficients from the large scale variables computed by the model. Does it mean that you are not really taking advantage of your configuration at 15km of horizontal resolution (by using parameterizations not suitable for your resolution) ?

l. 225: "The overestimation or underestimation of ocean productivity along the equatorial divergence zone is a common problem of many ocean general circulation models (e.g., Steinacher et al., 2010)." By reading the paper of Steinacher I rather get the feeling that only 2 models had this problem (MPIM and CSM1.4), in which is yours.

l. 257: "However, during several short periods the MODIS's daily-mean climatic concentrations (MODIS, 2020) appear to be higher than in the model."
Are model time series based on daily or monthly outputs ? I guess they are daily, because if based on monthly outputs it would be not surprising that MODIS daily-mean concentrations exceed model outputs. Could you please clarify the modelled outputs frequency you use for that analysis ?

Figure 4: I have some reservations superimposing MODIS and SeaWifs observations on that figure. I understand that you want to validate your model configuration with integrity and also show that observed daily concentrations may be higher than your model prediction. However, I think that these observational seasonal cycles would be valuable to show only if in good accordance with your modeled seasonal cycle, which is not particularly the case (except maybe in the Somali upwelling area). I am afraid it discredits your configuration, while the sensibility experiment you performed and associated effects are of great interest. I guess that you tried to compare your model and observations on a more aggregated region, and that was not better ?

l. 261: "time series" -> I suggest to write rather "seasonal cycle".

Figure 6: I am surprised that your comparison with observations for temperature and salinity are not better with a regional configuration. By curiosity, have you tried to force an INDB-type configuration by a reanalyse product ?

l. 367 put in italics “Thermocline dynamics”.

l. 376: WOA 2013 vs WOA 2001 -> why don’t you compare data of WOA2013 with the latest release WOA2018 (https://www.ncei.noaa.gov/products/world-ocean-atlas) rather than with an older one (WOA2001) ?

- Boyer, Tim P.; Garcia, Hernan E.; Locarnini, Ricardo A.; Zweng, Melissa M.; Mishonov, Alexey V.; Reagan, James R.; Weathers, Katharine A.; Baranova, Olga K.; Seidov, Dan; Smolyar, Igor V. (2018). World Ocean Atlas 2018. NOAA National Centers for Environmental Information. Dataset. https://accession.nodc.noaa.gov/NCEI-WOA18.

l. 402-403: Fig 13c for DJF suggest that the equatorial overestimation of CHLa in INDJ (Figure 3) may be due to a too deep thermocline depth when considering a constant light attenuation coefficient. But you did not present or discuss surface CHLa differences between experiments (INDB-INDJ), why ? PP differences (Figure 11) show a small increased production in the equatorial area in DJF, but it seems weak. On the contrary your Figure 16 suggests that the equatorial bias is still present in your INDB simulation. Please could you discuss that aspect ?

l. 412: “In both seasons, the mean surface temperature in ERA5 is clearly influenced by topography (Figs. 14a, 14d).” On that topic I suggest you to read and cite Samson et al. (2016). They show that land surface temperature errors are a major source of low-level circulation and rainfall biases for your modelled region. Your Figure 14E shows the bias they describe for JJAS with a cold bias over the Middle-East (impacting the Findlater jet) and a warm bias over India (although yours appears restricted to the north of India: see their Figure 2c).

- Samson, G., Masson, S., Durand, F. et al. Roles of land surface albedo and horizontal resolution on the Indian summer monsoon biases in a coupled ocean–atmosphere tropical-channel model. Clim Dyn 48, 1571–1594 (2017). https://doi.org/10.1007/s00382-016-3161-0

l. 424-425: “In general, the most considerable T2M biases are located in regions where larger temperatures are obtained, pointing to a role of the simulated nocturnal boundary layer and/or radiative fluxes”. I suggest to add the role of the good representation of the land surface albedo (impacting surface heat budget, winds and precipitations) described in Samson et al. (2016).

l. 453: “and in 15g-h” -> and in Fig. 15g-h

l. 511-515: You wrote “The climatic annual-mean values of attenuation coefficient in the INDB experiment (0.057 m-1) and in SeaWiFS data (0.052 m-1) do not differ too much” and “Compared to the fixed attenuation coefficient used in INDJ, in the INDB experiment, the attenuation coefficient seasonally varies in time and space in a relatively good accordance with that of SeaWiFS, except for the winter period (DJF).” However amplitude along the Indian west coast and the coast surrounding the Bay of Bengal are not so comparable. And spatial patterns at the equator and along the Somalia upwelling are quite different. Please rephrase.

Figures 17 and 18: Please complete your captions to explain that shading shows the wind (cloud water transport) module and arrows show its direction.

l. 523: “Figure 12 clearly demonstrates that the water temperature differences in the surface layers between INDB and INDJ experiments are less than the differences between them in the deeper layers”. Vertical profiles in Figure 12 only go down to 100m: have you checked the temperature difference below 100m ? Are you sure it is maximal at 100 m depth ? It would be interesting to see a vertical section or profiles up to 500 m depth. I guess the differences extend up to 300m.

l. 536: “Phy=0” ? What does it mean ? With fully absent phytoplankton I guess. Please clarify the text.