Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-130-SC5, 2018

Geoscientific Model Development Discussions

Interactive comment on "Simulation Improvements of ECHAM5-NEMO3.6 and ECHAM6-NEMO3.6 Coupled Models Compared to MPI-ESM and the Corresponding Physical Mechanisms" *by* Shu Gui et al.

K. Matthes

kmatthes@geomar.de

Received and published: 3 September 2018

Comments on behalf of the FOCI development team at GEOMAR

General comments

The authors try to disentangle the oceanic and atmospheric impact on the SST bias in a coupled GCM by replacing the ocean and atmosphere components separately. While the idea seems very innovative at a first glance, the paper unfortunately lacks a discussion on the difficulties and side effects of replacing a model component in a coupled GCM. The large effect of coupled ocean-atmosphere feedbacks onto the development of the SST bias is not discussed properly. The replacement of a component in a coupled GCM is not as straightforward as the paper suggests in its current form, and the conclusions drawn are hence questionable.

Some more information on the experimental setup would be desirable. How is the ocean initialized, e.g. are World Ocean Atlas ('Levitus') data used? This is in particular of interest since the authors claim that their model ocean is in equilibrium after only 100 years of spin up whereas other modeling groups perform multi-century (Delworth et al., 2006) or even multi-millennial (Müller et al., 2018) spin-up runs to significantly reduce the temperature drift in the ocean where clearly a drift is still visible after 300 or 500 years (Delworth et al., 2012, Fig. 1; Delworth et al., 2006, Fig. 3). Such a drift is best visible in timeseries of the global mean temperature for the surface but also deeper ocean layers, which unfortunately are not provided by the authors and should be added. It is essential for other modelling centres to provide at least a number or even better a timeseries of the TOA radiation (im)balance.

Other major concerns are:

1. For both the Atmosphere (ECHAM5 and ECHAM6) and the ocean models (NEMO, MPI-OM) very little information is provided on the technical details except for the configuration of the coupler (e.g., which parametrizations are active, which model options are switched on or off, how are the model components initialized, is nudging or restoring used in the ocean or atmosphere, model tuning)

2. The most prominent feature of no North Atlantic cold SST bias in a 2° ocean model coupled to ECHAM5 in their ECHAM5-NEMO3.6st configuration is not properly discussed. This bias has been around for decades in coupled climate models at the given resolution, and numerous papers discuss it. None of this work is mentioned or compared to. See also our detailed comments on Figure 3 below.

3. No figures or information on the stability of the control simulation (e.g., timeseries of

surface air temperature, TOA radiation budget, etc.) are provided which are crucial to evaluate coupled GCM performance.

4. A new coupled model system is presented and key ocean parameters such as the Atlantic Meridional Overturning circulation (MOC) or important coupled atmosphere ocean variability patterns (e.g., ENSO, NAO), their difference amongst the different GCM configurations and their possible impact on the SST bias are not discussed and should be added to the paper.

5. In our opinion, the pattern correlation method (table 1, with pattern correlations always below 0.4) cannot be used to explain the inter-model differences as it completely ignores both the physical dependencies of the parameters used in the correlation as well as the impact of ocean dynamics and coupled ocean-atmosphere feedbacks onto the SST bias in a GCM. In addition the presented pattern correlation values are very low.

6. Unfortunately, no information about the setup of the land component in the new coupled GCM is provided. We assume it is using JSBACH, the new land model component within ECHAM6. Is JSBACH running interactively? Why are the pattern correlations for albedo that weak? The interpretation of simulated precipitation is questionable, as differences in the extra tropics are not really visible (scale inappropriate). Additionally, the paper lacks also information about 2m temperatures (also referred to as SAT - surface air temperature) simulated over land.

7. No information on sea ice in the different GCM configuration is provided. To be able to judge the SST differences between the different model configurations properly, some information such as sea ice extent and sea ice thickness should be added to the paper.

8. Some of the presented model configurations (ECHAM5 coupled to NEMO) have been developed almost 10 years ago (Park et al., 2009), and have been used extensively during the last 10 years including work on the SST bias (Wahl et al., 2009,

Harlass et al., 2015). Unfortunately, none of this work is mentioned in the introduction or in the discussion.

Detailed major comments:

1. Page 5, line 31: The model experiments are performed using the piControl standard scenario of the MPI-ESM (p. 5, line 31). Most of the observational or reanalysis products used to compute model biases cover more recent periods than pre-industrial (1850 or 1870). Which reference period is used to compare the model runs to?

2. Page 6, line 1: "Model initialization is started from the climatology basic state recalculated with the AMIP run input data from 1981 to 2010." This suggests that the initial conditions in the atmosphere are based on a climatology calculated from AMIP simulations. Due to the chaotic nature of the atmospheric circulation, the choice of initial conditions of the atmosphere are not crucial for the performance of a coupled GCM. Hence we would strongly suggest to provide more information on the ocean initial conditions (see also our general comment above).

3. Figure 3c/d clearly shows the absence of a North Atlantic (NA) cold bias in a 2° ocean model coupled to a coarse resolution atmosphere (ECHAM5/6-NEMO3.6st configuration) which is a very striking result. The NA cold bias has been around for decades in coupled climate models at the given resolution, and numerous papers discuss it (e.g. Zhang and Zhao, 2015). None of this work is mentioned or compared to. Please provide details (e.g. namelists for both ocean and atmosphere model) to make it possible for other modelling centres to understand how you were able to achieve this immense improvement in the North Atlantic.

4. Figure 3: The authors note that the SST bias is largest in the polar regions exceeding 4degC as shown on Figure 3. This large bias clearly coincides with sea-ice coverage. There, the HadISST data set, which is used as a reference here, provides temperatures near the freezing point of sea water ($\sim$-1.9degC). While the authors are right that HadISST and other reanalysis products have deficiencies in high latitudes

due to the lack of observations, it is quite astonishing how the model bias can exceed 4degC where the sea water should be at or near the freezing point. Large biases can be expected at the sea-ice edge, which position may quite differ among coupled models.

5. Page 9, section 4.4 on ocean currents: The section on ocean currents is confusing as the differences in ocean currents are not related to the underlying ocean currents. When discussing differences in ocean currents please term the ocean currents that are enhanced/weakened, for example “enhanced/weakened Kuroshio transport is present in Model A compared to observations.”

6. Page 11, lines 11-15: In the first sentence you claim that the differences in NPMOC in the two MPI-ESM model simulations are not caused by the increased coupling frequency while in the second sentence you argue that “suggesting obvious improvements after decreasing coupling interval” are present. Please clarify. Additionally please provide more information on the MPI-ESM model data (e.g. MPI-ESM model version and a reference paper) cited as “The piControl experiment result (available in http://esgf-node.llnl.gov/)”. To our knowledge, the publicly available MPI-ESM output available at http://esgf-node.llnl.gov is based on the CMIP5 version of MPI-ESM which implements older versions of both ECHAM6 and MPIOM. It means that the two models differ by far more than just the coupling frequency.

7. page 12, line 15: "The analysis on oceanic and atmospheric circulation has made it clear that the SST bias is consistent with meridional overturning circulation in North Pacific, driven by surface wind stress anomalies that are maintained by anomalous Walker circulation over the tropical Pacific. Cumulus convection process is found to be a major contributor to inter-model differences". The authors should explain in more detail why they assume that cumulus convection is the key in the chain of arguments provided.

8. page 13, line 11: This sentence is confusing. Your statement that enhanced

Printer-friendly version

Discussion paper

northerly winds (which we cannot find on Figure 10b as you indicate in the text) in ECHAM6-NEMO3.6 compared to ECHAM5-NEMO3.6 in a region dominated by easterly trade winds are responsible for stronger evaporative cooling of SSTs in ECHAM6-NEMO3.6 compared to ECHAM5-NEMO3.6 is unclear. Please clarify.

9. page 13, line 18: "Since the latent heat and surface wind differences are caused by replacing the AGCM,..." The statement is challenging, as for example the latent heat flux between ocean and atmosphere is a coupled process (see also 12. below).

10. Page 13, line 27: “It turns out that deviations in shortwave flux and latent heat are more significant than those in longwave and sensible heat fluxes.” Additional information on the physics behind this statement would be helpful. In its current form it completely ignores the fact that there are large differences in the regional importance of the different fluxes.

11. Page 14 first lines: "...can be confirmed that the AGCM replacement first alters cumulus convection that modulates temperature, specific humidity and atmospheric circulation, which in turn accommodates cloud radiation feedback to a consistent change and affects the radiation budget". Some references that underpin the postulated process chain should be added.

12. Page 16, line 8: “Through analysis on circulation and radiation terms, it has been clear that latent heat of evaporation plays a predominant role in the SST differences after changing the OGCM.” This statement does not take into account that LH flux is a coupled process. LH heat flux may impact SSTs in the tropics where atmospheric temperature is high and hence strong evaporation is possible, but is mainly dominated by stability of the atmospheric stratification, windspeed, moisture and temperature in the lowest atmospheric level. It’s not as simple as the sentence suggests.

13. Page 16, line 10: What are “conduction processes” and how do they affect sensible heat flux? Additionally, this sentence indicates that the authors don’t take into account that e.g. sensible heat flux is a coupled ocean-atmosphere process that mainly de-

pends on the ocean atmosphere temperature difference. The importance of sensible heat flux for SST depends on the region. The authors should also provide a reference if and to what extend the surface flux parameterizations have changed in ECHAM6 with respect to ECHAM5.

14. page 16: line 11: "Attributing simulation deviations to latent heat in the OGCM case is consistent with Cao et al. (2015), which points out that amplitude and meridional variability of latent heat flux over Pacific are the most diverse in CMIP5 models." From our understanding, Cao et al. summarize that LH flux is very diverse amongst coupled models due to the large differences in simulated SST but not that the LH flux differences between the models can explain the bias (From the abstract of Cao et al., 2015: "Regression analysis indicates that the inter-model diversity [in LH flux] may come from the diversity of simulated SST and near-surface atmospheric specific humidity").

15. Page 16, line 14: Please explain what the term “reverse transformation of model bias” means.

16. Page 16, line 25: More details on the “coupling processes” (line 26) that you claim to be responsible for the differences in the wind field in the different GCMs should be added.

17. Page 16, line 28: Please explain what you mean by “net surface radiation is warmer in the east and colder in the west.”

Minor comments:

1. Fig. 8: Unit of color contours missing.

2. Fig. 8: Why does the MOC plot stop at 1600m depth?

3. Page 8, Line 6: Details for the reference Huang et al. (2014) are missing.

4. Page 5, Line 29: It is not clear whether the control experiments for the

ECHAM5/NEMO3.6st and ECHAM6/NEMO3.6st setup use present day or piControl external forcing. Please clarify.

5. Page 11, line 5: The Wang et al., 2014 paper cited focuses on the Atlantic MOC and not on the Pacific MOC, hence the citation in this context is not appropriate.

6. Page 11, line 15: The Ge et al., 2017 reference is not appropriate in the context, as Ge et al., 2017 focus on the impact vertical resolution on the SST bias in a ocean model (MOM5) driven by reanalysis data and not the impact of coupling frequency.

7. Page 11, line 31 and Figure 9: Please provide the coordinates you use to determine the SEC region. Figure 9 does not show zonal averages (longitudes on the x axis) as indicated in the text. Please correct.

8. Page 12, line 28: LH flux and evaporation describe the same physical process, so there is no need to discuss the two separately.

References:

Delworth, TL, and co-authors, 2006: GFDL's CM2 Global Coupled Climate Models. Part I: Formulation and Simulation Characteristics. J. Climate, 19, 643–674, https://doi.org/10.1175/JCLI3629.1

Delworth, T.L., A. Rosati, W. Anderson, A.J. Adcroft, V. Balaji, R. Benson, K. Dixon, S.M. Griffies, H. Lee, R.C. Pacanowski, G.A. Vecchi, A.T. Wittenberg, F. Zeng, and R. Zhang, 2012: Simulated Climate and Climate Change in the GFDL CM2.5 High-Resolution Coupled Climate Model. J. Climate, 25, 2755–2781, https://doi.org/10.1175/JCLI-D-11-00316.1

Harlass, J., Latif, M. and Park, W. (2015) Improving Climate Model Simulation of Tropical Atlantic Sea Surface Temperature: The Importance of Enhanced Vertical Atmosphere Model Resolution. Open Access Geophysical Research Letters, 42 (7). pp. 2401-2408. DOI 10.1002/2015GL063310.

Müller, W. A., Jungclaus, J. H., Mauritsen, T., Baehr, J., Bittner, M., Budich, R., et al., 2018: A higherâĂŘresolution version of the Max Planck Institute Earth System Model (MPIâĂŘESM1.2âĂŘHR). Journal of Advances in Modeling Earth Systems, 10, 1383–1413. https://doi.org/10.1029/2017MS001217

Park, W. et al.: Tropical Pacific Climate and Its Response to Global Warming in the Kiel Climate Model. Journal of Climate, http://doi.org/10.1175/2008JCLI2261.1

Wahl, S., Latif, M., Park, W., & Keenlyside, N. (2009). On the Tropical Atlantic SST warm bias in the Kiel Climate Model. Climate Dynamics, 36(5–6), 891–906. http://doi.org/10.1007/s00382-009-0690-9

Zhang, L., & Zhao, C. (2015). Processes and mechanisms for the model SST biases in the North Atlantic and North Pacific: A link with the Atlantic meridional overturning circulation. Journal of Advances in Modeling Earth Systems, 7(2), 739–758. http://doi.org/10.1002/2014MS000415

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-130, 2018.