Anonymous Referee #2

Referee comment on "AWI-CM3 coupled climate model: Description and evaluation experiments for a prototype post-CMIP6 model" by Jan Streffing et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-32-RC2, 2022

This manuscript describes the newly developed climate model AWI-CM3. The global coupled climate model consists of the atmosphere model OpenIFS and the ocean model FESOM2 which are coupled using the OASIS MCT4 coupling software. For the AWI-CM family the new development is mainly the replacement of the ECHAM atmosphere, which was used in AWI-CM2, by OpenIFS. The authors describe the component model and assess the performance by evaluating a set of CMIP6 DECK-like experiments in comparison with observational data sets and other CMIP6 models. This is a standard procedure and the evaluation of the model at moderate resolution shows good and above-CMIP6 standard performance. The authors also comment on computational performance and conclude that this model is quite suitable for CMIP6-like experiments. Finally, they give some hints on the performance of a higher-resolution version.

First of all, achieving such good performance in an early stage of development is a great success and I congratulate the authors. As both, the ocean and atmosphere components have potential to work even better at higher resolution and using more of the flexible grid properties of FESOM, I expect to see interesting configurations as coupled climate model and full Earth System Model in the future.

Overall, the paper covers all relevant aspects of a model documentation and the presentation is mostly clear and concise. However, as I outline below, another iteration seems to be necessary. I feel that important information is missing at some places and the evaluation could be more quantitative at other sections. Often the reader would need to consult other publications for very basic information. Also, the text needs another revision. For example, acronyms and abbreviations used either without spelling out what they stand for or with their definitions given later in the manuscript elsewhere.

I therefore recommend that the paper should be accepted for publication after taking into account the points below and including further discussions on various points. I would rate the revisions needed somewhere between “minor” and “major”.

We would like to thank the reviewer for the encouraging feedback.

Specific comments:

Introduction

General: I recommend to discuss where AWI-CM3 stands in comparison to other recent developments for CMIP6, but also beyond (e.g. new grid systems in MPAS, FESOM, ICON, improved dynamical cores, etc.).

We added some additional information about MPAS, new grids and new core design.

Ln 16ff: the reader may not be familiar with the differences between AWI-CM and AWIESM models. This should be briefly explained.

We added brief explanations where these models are introduced.

Ln 22: be more specific of which range of resolution you are talking about.

Section 2

We added a clarification stating that we are talking about the range from current CMIP6 to NWP models (~100->~10km)

Ln 56 ff and figure 1: are WAM and H-TESSEL actually used here? Later you say that another hydrology model (mHM) shall be introduced later. The description of the atmosphere model should also include some information how land processes are treated.

We added a description of the two way WAM ↔ OpenIFS coupling. We also clarify that WAM is currently not directly coupled to FESOM2.

We added information about the column model nature of H-TESSEL soil moisture updates, and the need for a Runoff-mapper for horizontal water transport on land. It is this water transport that may be replaced with a more sophisticated scheme.

Ln 76: what is the vertical lay-out in the atmosphere, which coordinate is used, how is the stratosphere resolved?

We added information about the vertical layout.

Ln 82ff: give some more details on FESOM2: e.g. physical paramterizations, like vertical mixing or eddy-induced (GM) mixing

We added information about GM, KPP, the viscosity closure, & MOMIX.

Ln 87: a few words more about the sea-ice model, what kind of dynamics and thermodynamics are used in FESIM, how is it coupled to FESOM?

We added a section of FESIM and how it is integrated into FESOM, and the potential upgrade option Icepack.

Ln 91: what is the vertical distribution of the levels, how is the mixed layer resolved?

We added a sentence on level distribution in the upper 100 meters.

Ln 113: see above

Answered above.

Section 3

Ln 144ff and Table 1: In the introduction you quote Renault et al (2016) and say that “local energy transfer” is important. Why didn’t you include then the coupling of ocean surface currents for the calculation of the wind stresses?

While we have recently identified this flux as important, this was not the case at the onset of the project. Neither of the predecessor CMIP6 models AWI-CM1 or EC-Earth3 featured *ocean surface current coupling. Furthermore the interface to integrate the ocean surface current into OpenIFS was only recently provided by Barcelona Supercomputing Center.*

The feature will be added in the next minor release of AWI-CM3.

Ln 157: I don't understand what "converged solution" means here

Added explanation that this refers to the theoretical option of using an iterative solver for the ocean-atmosphere surface update.

Section 4

Ln 175: How is the initial state of the ocean defined? Did you run ocean stand-alone simulations before coupling or start with climatology (which)? From Fig3, I assume it is PHC, but you should describe it in the text.

Added a sentence on the initialization with PHC3.

Fig 3: Define PHC

Definition added.

Line 197: define KPP

Definition added.

Ln 214: As your models runs at above 60SYPD, why don't you provide a control run of decent length (I think CMIP6 asks for 500 years). This would allow you to asses aspects of low-frequency variability (e.g. AMOC, AMV, sea ice extent).

Indeed we did not follow the CMIP6 DECK protocol here. If we were to repeat the work, we likely would. This was mainly motivated by the fact that we consider this a prototype version. This is not a model with e.g. cmorized output and upload to ESGF. It is however a version that some of our colleagues have started to put to limited scientific use, motivating us to document its features and calling it release 3.0.

Fig. 4: "Mean absolute error…" aren't all these "relative" errors?

We reformulated this sentence to hopefully better express what the Kim and Reichelt performance indices are:

Performance indices after Reichler and Kim (2008) that give the fraction of absolute error of climatology of the last 25 years in AWI-CM3 historic simulation to the absolute error averaged over CMIP6 models.

Reichler, Thomas, and Junsu Kim. "How well do coupled models simulate today's climate?." Bulletin of the American Meteorological Society 89.3 (2008): 303-312.

Ln 221ff: the Reichler indices are fine, but I would like to see at least a few vertical plots for the atmosphere, e.g. zonal averages of zonal winds and temperature to see how the models performs in the upper troposphere/lower stratosphere.

We added a figure showing vertical plots of zonal mean temperature and zonal mean zonal wind. The latter showed obvious issues in the QBO region, encouraging us to add a QBO plot as well. From this QBO plot a clear issue of the model was found, that we recommend shall be tuned in an upcoming release of the model. We found that other of OpenIFS users had found similar issues and already identified some tuning parameters: https://confluence.ecmwf.int/pages/viewpage.action?pageId=222481088

Ln 248, Figure 5: include an estimate from observations, e.g. sea-ice extent

We added GIOMAS reanalysis for the spatial distribution of sea ice thickness. We believe that the sea ice extent plot is already quite busy, and that the thickness reanalysis gives a good overview of the extent match/mismatch.

Ln 275, Figures 7,8: a more quantitative evaluation could be done including RMS and mean errors.

We added area weighted rmsd and md for figues 7 & 8.

Ln 307: what kind of work on the mixing schemes would help?

We have since found some indication that replacing MOMIX with a salt plume parameterization might work. However, we think there are still too many issues with the approach to put this into writing.

Ln 315: are there plans to combine the Langmuir-associated mixing with WAM, as in

CESM2 (see Danabasoglu et al., JAMES, 2020).

We added a note on our plans to include Langmuir circulation parameterization.

Ln 320: any idea what causes the strong warm bias in the Atlantic Subpolar Gyre?

We added the following section outlining different attempts to explain this fairly common model bias:

In Sidorenko et al. 2014 we noted a similar bias when we analyzed a coupled setup with FESOM1.0 coupled to ECHAM6.3. This bias is shared between many climate models which contributed to CMIP. A similar drift in ocean hydrography is also described in Sterl et al. (2012), Delworth et al. (2006, 2012), and Jungclaus et al. (2013). These authors discuss different factors that may be responsible for the bias. Sterl et al. (2012) show that overestimation of the Mediterranean outflow can significantly increase the deep-ocean salinity bias. Delworth et al. (2012) attribute this anomaly to the insufficient eddy transport required to compensate for the wind-driven subduction in the subtropical gyres. They show that moving towards an eddy-resolving setting or a parameterization of the eddy stirring reduces the temperature biases significantly. Jungclaus et al. (2013) suggest that part of the problem arises from the improper interbasin exchange between the Indian and South Atlantic oceans.

Sidorenko, D., Rackow, T., Jung, T. et al. Towards multi-resolution global climate modeling with ECHAM6–FESOM. Part I: model formulation and mean climate. Clim Dyn 44, 757–780 (2015). https://doi.org/10.1007/s00382-014-2290-6

Sterl A, Bintanja R, Brodeau L, Gleeson E, Koenigk T, Schmith T, Semmler T, Severijns C, Wyser K, Yang S (2012) A look at the ocean in the EC-Earth climate model. Clim Dyn 39(11):2631–2657

Delworth TL et al (2006) GFDL's CM2 Global coupled climate models. Part I: formulation and simulation characteristics. J Clim 19:643–674

Delworth TL et al (2012) Simulated climate and climate change in the GFDL CM2.5 high-resolution coupled climate model. J Clim 25:2755–2781

Jungclaus JH, Fischer N, Haak H, Lohmann K, Marotzke J, Matei D, Mikolajewicz U, Notz D, von Storch JS (2013) Characteristics of the ocean simulations in the Max Planck Institute Ocean Model (MPIOM) the ocean component of the MPI-Earth system model. J Adv Model Earth Syst 5(2):422–446

Ln 329 ff: The section on variability could be extended a bit. At least some spatial regressions of ENSO could be included. ENSO is not the only mode of variability; several recent papers on CMIP6 models (e.g., Voldoire et al., 2019; Danabasoglu et al., 2020) mhave included, for example MJO, which is quite revealing for the atmosphere.

We have added the spatial regression of ENSO and information about QBO.

Unfortunately our outgoing thermal radiation, the standard field for plotting MJO was only stored at monthly frequency. Too coarse for MJO detection or a Wheeler Kiladis plot.

Ln 379: here as well, I would expect a little bit more quantitative evaluation and putting

these results into context of other models.

We have extended this sub-subsection as follows:

Figure 11b shows the simulated changes in the precipitation pattern resulting from historic well-mixed greenhouse gas and solar forcing. The most important features are: the high latitudes nearly uniformly receive more precipitation; the monsoonal precipitation in North Africa and China intensifies; the ITCZ is enhanced in the western Pacific and more focused on the equator in the eastern Pacific and in the Atlantic; considerable parts of the subtropics tend to receive less precipitation. These patterns are largely consistent with precipitation changes simulated in CMIP6 models where transient aerosols are included although precipitation increases over the Indian Ocean and Northern Central Africa are not as pronounced as in the CMIP6 model mean and more pronounced over the Indonesian warm pool (compare with Figure SPM.5c in IPCC (2021)).

Ln 398: what are spurious transformations? Water masses?

We replaced spurious transformations with "numerical mixing in the model caused by the advection operator"

Spurious transformations (or mixing) refers to numerical mixing in the model caused by the advection operator. Dissipative truncation errors in horizontal advection lead to diapycnal mixing in places where isopycnals are sloping. In the ocean we use vertical advection without dissipative truncation errors. (Of course, we use FCT which can introduce additional dissipation, but we see the effect where the isopycnals are sloping, which indicates that it comes from artificial horizontal diffusion). It is generally expected that increase in horizontal resolution will lead to a reduction of numerical mixing.

Ln 427, 432: I feel that Scafetta et al is not the best reference here. More original papers

are probably Sherwood et al (Rev. Geophys. 2020) and Meehl et al. 2020.

We added the citation for Meehl et al. 2020

References

IPCC, 2021: Summary for Policymakers. In: Climate Change 2021: The Physical Science Basis. Contribution of Working Group I
to the Sixth Assessment Report of the Intergovernmental Panel on Climate Change [Masson-Delmotte, V., P. Zhai, A. Pirani, S.L.
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