



EGUsphere, referee comment RC2  
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## **Comment on egusphere-2022-32**

Anonymous Referee #2

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

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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".

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.).

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

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

## **Section 2**

Ln 56 ff and figure 1: are WAM and H-TESEL 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.

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

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

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?

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

Ln 113: see 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?

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

### 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.

Fig 3: Define PHC

Line 197: define KPP

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).

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

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.

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

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

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

Ln 315: are there plans to combine the Langmuir-associated mixing with WAM, as in CESM2 (see Danabasoglu et al., JAMES, 2020).

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

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) have included, for example MJO, which is quite revealing for the atmosphere.

Ln 379: here as well, I would expect a little bit more quantitative evaluation and putting these results into context of other models.

Ln 398: what are spurious transformations? Water masses?

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.

