

# ***Interactive comment on “Evaluating the physical and biogeochemical state of the global ocean component of UKESM1 in CMIP6 Historical simulations” by Andrew Yool et al.***

## **Anonymous Referee #2**

Received and published: 18 January 2021

In this manuscript, Yool et al. present in detail the performance of the ocean and marine biogeochemical component of UKESM1, a novel Earth system models contributing to CMIP6. The manuscript is clearly written and provides a large array of standard analysis to understand the performance of UKESM1 at replicating observed features of the ocean and marine biogeochemical dynamics. This work presents an important basis for the users and other climate research groups in the context of multi-model analysis. I only have three majors and a set of minor comments that aims to clarify some point of the paper.

Major comments:

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1- Although the authors did a great job assessing model modern climatology (2000-2009) against available modern observations, they didn't provide any key statistical metrics that might be useful to support their assessment (correlation, total root-mean squared errors, etc.). This would represent an added value in the current manuscript. In addition, The abstract states that this paper investigate the driving mechanisms behind model-data errors. This is misleading. The manuscript remains largely speculative on what causes model biases and what mechanisms are at play to explain errors propagation or amplification. Within digging to much in those mechanisms (I reckon the paper is already long), I would like to see more properties-to-properties diagrams. This kind of analysis would strengthen the manuscript and support the conclusions. Those limited range of analysis mirrors the paper structure. Indeed there is no Methodology section. The reader remains without information on how model data are compared to observation (regridding for instance); how the mixed-layer depth are calculated; what is the working hypothesis to compute anthropogenic carbon and so on. This section, even short, would be useful.

2- Following major comment #1, I find it difficult to understand the choices of the authors team for the analysis. For instance, why DJF or JJA are taken for ocean analysis instead of JAS more widely accepted to studied the Southern Ocean winter dynamics. Does it has something to do with biases in the atmospheric model? Further explanation would be useful.

3- Manuscript structure: Although the manuscript is well written, some parts could be improved. For instance, I find it unexpected to find the description of the ocean and marine biogeochemical model in the appendix (while central in this work) but not in the main text to guide analyzes. Besides, the opening to the future scenarios seems out of the scope in the manuscript. This latter could be removed to give more space to develop key aspects (see point 1).

Minor comments: L6 observational properties = observed fields ?

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L15 compares favourably = you mean “outperforms” ? or just compares to other models ?

L29 in response to the release = driven by the release

L30 chemical composition = CO<sub>2</sub> airborne fraction

L57 to identify avenues for future. . . addressing model limitation and weakness

L70 built to “simulate”

L80 do you simply mean that land-surface model is a submodule of the atmosphere model ?

L92 Mulcahy et al. (subm.) I don't know if it match GMD standard to refer to submitted papers (I counted two papers with this status)

L102-123 I would further develop this paragraph because it describes the model description central to this paper. In addition I suggest the authors to include the MEDUSA description here. Further details on how couplings may influence MEDUSA results (for instance, dust deposition).

L145 physics=hydrodynamics

L150 it is unclear what is meant with “ocean circulation”, please provide further detail

L162: Please include Khatiwala et al. (2009) dataset

L175 “% citep. . .” I guess there is a typo here. Please check the following sentence

L215 “thermohaline transects” or “thermohaline circulation” Figures: this naming is misleading. What about “basin-averaged section” ?

L213 Section 3.2 please include detail on how to determine NADW and AABW properties in the model

L254: RAPID-MOCHA time coverage poorly matches with the time period chosen for

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computing the model climatology. Does this difference in time period influences model assessment?

L295 “63 vs.” ? please check

L317 biological community = marine biology ?

L334 three models = three algorithms ?

L348-362: All the Figures referred in these paragraphs should be included in the manuscript. Otherwise please consider moving this paragraph and associated text in the suppl. Mat.)

L596: Section 4.2: While interesting, I would suggest to stratify this section by comparing first NEMO-based model (IPSL, CNRM, CanESM) and then the other models. This would help to discuss the performance of the marine biogeochemical in a more constrained modelling framework. Please note that the Danabasoglu et al. and Voldoire et al. are inaccurate.

L681-695: Section 5: some key points would require further detail: resolution-dependent surface biases : is it assessed in this paper or based on published literature ?; same does for the aerosol-driven strengthening of the AMOC.

Figure 1: please detail somewhere in the ms why JJA and DJF has been used for seasonal analyzes

Figure 2: 0.15 isolines may help to compare model and observation

Figure 3: please considering remove indiv ens members and show  $\pm 1$  standard deviation. Please explain what is behind seasonal minima/maxima.

Figure 6: please see the comments above

Figure 17: further discussion would be required to explain why model and obs-derived estimates differ over the preindustrial period (1800-1850) and why models fail at cap-

turing carbon uptake over the recent years (2000-2014). Please include the database in the reference list

Figure 18: please explain what is your working hypothesis to compute anthropogenic carbon

Figure 20: typo on the Figure “CO 2”

Figure 21 and 22: These two Figures merits further explanations. They show major model biases in the subsurface Atlantic waters whereas the modelled meridional transport matches well with the observed one.

technical suggestion: please consider to adjust color scales for some figures and use red-green color-blind color palette (where relevant)

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Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-333>, 2020.

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