

Geosci. Model Dev. Discuss., author comment AC1  
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## Reply on RC1

James R. Christian et al.

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Author comment on "Ocean biogeochemistry in the Canadian Earth System Model version 5.0.3: CanESM5 and CanESM5-CanOE" by James R. Christian et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-327-AC1>, 2022

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RC1: 'Comment on gmd-2021-327', Anonymous Referee #1, 02 Dec 2021

This manuscript describes the incremental development of the marine biogeochemical component of the Canadian Earth System Model(s) v.5 and the contribution of these models to the 6th phase of the climate model intercomparison project (CMIP6).

My overall judgment is that this is a good technical publication, whose main aim is to describe the features of the new marine biochemistry component (CanOE) and provide useful insights on the historical simulations performed with both versions of CanESM5.

In commending the authors for their achievement, I would however point out that the discussion of the results obtained with CanESM5 and CanESM5-CanOE is not as detailed as one would expect and it should be revised to better exploit the material presented in the results section (see detailed comment below).

I would also recommend the authors to revise the ending section of the manuscript with clearer perspective on future research directions and foreseen model development.

**We thank the reviewer for the constructive review. We have restructured the Discussion section in accordance with the comments of this and other reviewers.**

Specific comments

Section 3: In the results section the authors widely discuss about differences and biases in the comparison of the model outcomes with observations and CMIP6 multi model ensemble. So, it would be more effective to swap the Figures 2, 3, 6, 7 with the corresponding ones of the Supplementary Material (which directly show the differences against observations).

**We have swapped out the main and Supplementary figures as the reviewer suggests.**

L46: Is the NEMO model implemented on a T63 horizontal grid? I think this statement is not correct and should be modified to correctly address the ocean model configuration (likely ORCA1 grid). I suggest to add more details on the configuration and resolution of the different CanESM5 components in Section 2.

**No, the T63 grid is the atmosphere model. We have clarified this in the revised MS. As the reviewer suggests, the ocean is on the ORCA1 grid. We have added a few more details about the ESM as a whole as the reviewer suggests.**

L123: This aspect could be improved by adopting the SolveSAPHE solver (Munhoven, 2021 <https://doi.org/10.5194/gmd-14-4225-2021>) in the future development of the model

**We will consider the reviewer's suggestion for our future development. As the present contribution is part of CMIP6, we simply followed the OMIP-BGC protocols as outlined by Orr et al (2017).**

L127: I think it should be stated in here that carbon chemistry variables are computed offline instead of discovering it at L360

**done**

L360: GLODAP is a versioned dataset. It would be clearer to refer to it as GLODAPv2.

**done**

L360-363: How different is the carbon chemistry obtained with the online computation? If applicable, I suggest authors to detail this aspect to support the offline approach.

**We have done this calculation many times with many data sets. The differences are negligible from the perspective of the kind of global-scale analysis with which we are currently concerned.**

L382: A more substantiated explanation should be provided to explain the use of such a coarse horizontal sampling ( $2^\circ \times 2^\circ$ ) of CanESM2 and CMIP6 datasets.

**Again we are primarily concerned with documenting the global-scale distributions of major tracers. For this purpose, whether the data are regridded at 1deg or 2deg makes little difference. For example, when a 1x1 grid is used, none of the correlation coefficients for CanESM5 vs observed oxygen on the six depth levels shown in Figure 4 changes by more than 0.005 (max 0.0028, mean 0.0011).**

L401: The CMIP6 multimodel ensemble data are treated here (and in the following paragraphs) as a "single model" results, but I think that authors are missing the opportunity to exploit this information to better characterize CanESM5 performance in the broad CMIP context

**We chose this particular method of presentation because in this case presenting all of the individual CMIP6 models seemed to us to be potentially overwhelming the reader with questionably relevant information. We have modified the text to try to address the reviewer's concern.**

L410: A description of the Oxygen Minimum Zones spatial patterns would a good complement to this paragraph.

**done**

Figure 5: Axis labels should be increased in size to make them easily readable

**done**

L460-464: The observation-based Aragonite saturation state is here recomputed using GLODAPv2 and WOA2013 data instead of using the original field made available within the GLODAPv2 dataset. The rationale for this choice should be specifically addressed.

**We do all carbon chemistry calculations offline to make sure they are done in a consistent way across models and observations. We have checked these calculations against the published derived fields and the differences are minor.**

L484-485: I don't think that the conclusion made by Lambert and Boer (2001) in the analysis of atmospheric fields (air temperature, precipitation, sea level pressure) from CMIP1 exercise can be extended in such a way to the DIC, and more generally, to any ocean biogeochemistry.

**There are differences of opinion about the appropriateness of citing older vs more recent literature. We prefer to cite the reference that first (to our knowledge) articulated an idea. But this idea has proved over time to be quite robust, and it is generally true for all sorts of fields including ocean biogeochemistry fields (see e.g., Figure 22 of Chapter 5 of the AR6 (WG1) report). It can also be deduced from the data shown in the paper itself, which include both ensemble means and individual models for several example ocean biogeochemistry fields (e.g., Figures 4, 8, 9, and S2), as noted by reviewer #2.**

L515: Figure 11b could be moved to Supplementary material.

**Figure 11b has been moved to Supplementary as requested.**

L526: The comparison of dFe observations with different models outcome in Figure S4d could be improved by reporting also the tendency lines of each model along with the ideal fit (1:1) black line.

**Yes good idea. This was added.**

Besides, these results could be further discussed in the light of the findings from Seferian et al. (2020, <https://doi.org/10.1007/s40641-020-00160-0>)

**We agree with this suggestion and will work this into the revised Discussion.**

Figure 14: revise caption by adding "CanESM5 is not included because it does not have prognostic iron"

**done**

L568: here it would be interesting to specify which are the two models that do not fall along the spectrum.

**done**

L574 and L577: Use 260°E instead of 100°W, coherently with the Longitude units used in Fig. 15.

**done**

L577: It would be useful to have this "not shown" figure in the Supplementary material

**Not shown in this context indicates analysis of a number of data sets, all of which are in the public domain. The point at issue is relatively trivial: whether**

**this particular local maximum in surface nitrate concentration is due to undersampling. Such localized maxima certainly do exist in this data product, but in this case the maximum is associated with equatorial upwelling at the longitude where the thermocline is nearest the surface. We have reworked the wording slightly, and added a literature reference that shows that the flow is strongly divergent at this location.**

Section 3.4: Differently from the previous ones, this part is largely intertwined with comments on results that better fits the discussion section.

**We have addressed this as part of a more general restructuring as per the reviewer's general comments above.**

L587: Add reference to Tesdal et al. (2016)

**The reference is given in the figure caption. It seems like overkill to repeat in the text especially given that the reference given is for a climatology of chlorophyll concentration. The text refers to biomass and the figure caption explains how biomass was estimated from chlorophyll.**

Figure15: Authors should consider to add a shaded area showing, e.g., the min-max range obtained from the CMIP6 model ensemble and include some considerations with respect to CanESM5.

**We have tried several different versions of this, but we don't think it adds very much. The range is very large (see attached image), as the reader can deduce from other figures shown in the paper.**

Figure 16: Axis labels should be increased in size to make them easily readable

**done**

L601-603: The expected behavior of phytoplankton size distribution is clearly visible only in subarctic regions, while in the North Atlantic the monthly variability is rather similar between small and large classes.

**The reviewer may have mistaken the Total line for one of the size classes. The figure clearly shows that the amplitude of the seasonal cycle is greater for large phytoplankton which exceed 50% of the total only in summer and fall to near 0 in winter.**

Figure 17: The supplementary Table S4 could be easily replaced with a map illustrating in a more straightforward way the location and extent of selected marine regions.

**We think the Table is necessary so that the reader can see the actual numbers for the region bounds, but will consider including a map as well.**

L655: I guess it should read as "... with the range of other CMIP6 models."

**CMIP6 added**

L671-673: It could be useful to address in a dedicated table the residual drifts of the piControl simulation for the CO2 uptake and also the other biogeochemical variables presented in the previous sections.

**This is a good suggestion and overlaps with one made by reviewer #2. We will**

**include a Supplementary Table to illustrate the magnitude of historical trends relative to both drift and internal variability.**

Section 4: There are several parts of the discussion section that are not fitting the real purpose. For example, L704-716 describes the evolution of the model which in my option should pertain to the introduction. L729-741 focuses on the differences between CanESM2 and CanESM5 formulations which was already stated previously in the manuscript. Lastly, in many points the authors refer to not shown figures that is not helpful to the discussion. I suggest to revise the entire section by tackling the various outcomes of the result sections, which is very rich in content and material.

**The Discussion section has been restructured according to the reviewer's comments.**

L771: Typo in the model name "CanESM5"

**fixed**

L774-776: This sentence is not clear

**Sentence has been restructured to make the meaning clearer.**

L792-794: The impact of the different ocean circulation between CanESM2 and CanESM5 is supported only by the comparison of DIC (Fig.8) and the one "not shown" at Line 750. I think that this part should be better supported (maybe with some additional analysis on other variables) to robustly claim that only ocean circulation is responsible for the observed differences between the two model versions.

**We have expanded our discussion of the relative roles of circulation and biogeochemistry in a number of places in response to comments by several reviewers. The text here refers to "e.g., Figure 8" but the assertion is also substantiated by Figure 9 and Figure S2. The text has been modified to reflect this. That there is no similar comparison for O2 is unfortunate; there are no CanESM2 O2 data because at the time we were under a lot of pressure to keep the number of tracers to a minimum. We chose not to include in the Supplementary material the geographic distribution of DIC referred to as "not shown" on 489 because all of the relevant data are in the public domain and the interested reader can easily verify this.**

Please also note the supplement to this comment:

<https://gmd.copernicus.org/preprints/gmd-2021-327/gmd-2021-327-AC1-supplement.pdf>