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Geoscientific Model Development Discussions

Interactive comment on "Evaluation of NorESM-OC (versions 1 and 1.2), the ocean carbon-cycle stand-alone configuration of the Norwegian Earth System Model (NorESM1)" *by* J. Schwinger et al.

Anonymous Referee #1

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Overview

This manuscript presents a description of an updated version of a previously published earth system model. The focus here is on the ocean physics and marine biogeochemistry components, and the work presented uses these in forced mode under reanalysis atmospheric forcing. The configurations presented span old to new, and cover resolution differences, physics updates and biogeochemistry tuning and updates. Evaluation is performed for select fields between the model configurations and observational climatologies. The manuscript includes an evaluation of four schemes for the water col-

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umn remineralisation of export production, though similarities in performance preclude a definitive selection.

Summary

In general, the manuscript does well at describing the model and outlining its performance. I have a number of relatively minor specific comments dotted throughout the manuscript (see below). My only major comment would be that, while intercomparing the various versions of the model, the manuscript does nothing to contextualise the performance of NorESM-OC within the context of other Earth system models. The CMIP5 archive is something of a treasure trove on this point, and most of the evaluations made in the manuscript could be repeated with output from it. However, I would only suggest adding an overview of this (e.g. Taylor diagrams?), and not amending the manuscript throughout – the main task here, and I would agree with the authors on this, is to evaluate performance between traceable versions of NorESM-OC.

My recommendation is publication following addressing of these comments.

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Pg. 2, ln. 4: "We present simulation results . . ." – this sentence could perhaps be a little clearer on how many configurations are examined; the mention of multiple resolutions makes it a little opaque

Pg. 3, ln. 12: The authors simulate the ocean component of an ESM under reanalysis forcing; did they consider running under atmospheric output from the ESM instead?

Pg. 8, ln. 1: "We anticipate . . . of 10,000 years . . ." – would I be correct in assuming this "anticipation" is either too computationally expensive to test, or is not actually possible because v1.2 of the model rolls in several changes and not just this one (i.e. it is not possible to separate differences due to the time-stepping change from those from other sources)?

Pg. 8, ln. 13: "derived by Wanninkhof (1992)" – it might be worth examining Wanninkhof (2014) for updates here; I believe that some of the issues mentioned subsequently are tackled there

Pg. 10, ln. 15: A remark about the shape of the nutrient limitation relationship would be useful; also something about the relationship between nutrients (e.g. Leibig's Law or something else?)

Pg. 10, ln. 23: Presumably this means that opal "production" is associated with export only, and is not representative of all of the opal production by diatoms (some of which is dissolved before it can be exported)?

Pg. 11, ln. 4: "we performed a re-tuning" – by eye?

Pg. 11, ln. 16: Is there a potential problem here because Si:P ratios in the real world are not constant?; in iron-limited HNLC regions, for instance, Si:P is typically elevated (since diatom cell cycle length is increased affording more time to uptake silicon); the global nature of the "solution" here to DIC / TA biases potentially promises trouble regionally

Pg. 14, ln. 16: It would perhaps be helpful to illustrate to the reader the depth distribution of sinking particles, as well as remineralisation, produced by these different schemes (e.g. for the same quantity of POC at 100m, what's its fate down a static water column?)

Pg. 16, ln. 10: ". . . a restoring time scale of 365 days (version 1) and 350 days (version 1.2) . . ." – any reason why this is 15 days shorter?; presumably a parameterisation oversight?

Pg. 17, ln. 13: "with only small trends of 0.00007, 0.021, and −0.048 PgCyr−1 century−1 for Mv1, Mv1.2, and 15 Lv1.2, respectively" – any explanation for why the longer duration simulations with v1.2 have markedly higher CO2 trends than the shorter duration v1 model?; one might instinctively expect smaller values for longer simulations; perhaps plot up the net CO2 flux with time?

Pg. 18, ln. 8: “Since the unbalanced salinity relaxation flux removes salt . . .” - this sentence reads as if it is saying that the reduction in S in Mv1 is due to the relaxation flux being applied across *all* model configurations; I think this sentence and the preceding one should be combined into: "While all three configurations include salinity relaxation, this is not balanced in the case of Mv1, with the result that average salinity falls by 0.2 psu during the course of the integration."

Pg. 18, ln. 14-16: Worth reporting these sorts of numbers in a table?

Pg. 19, ln. 12: “. . .a long transient increase in strength for about 300 years . . .” – Any chance of including a plot of the AMOC strengths of the models from their spin-up phases?; not least to give some idea of interannual variability in the absence of interannual forcing variability (if any)

Pg. 20, ln. 15: Since the plot makes a point of examining the time-series of AMOC, would it be possible to present the RAPID estimates on the same plot?

Pg. 20, ln. 19: “. . . the climatology of de Boyer Montégut et al. (2004)” – This is calculated how?

Pg. 20, ln. 22: On a related point, are model MLDs comparable to the climatology?; e.g. could MLD be calculated for the model in the same way as it’s done for the observation-based climatology?; if this is already the case, please make this clear

Pg. 21, ln. 12: “clearly indicates a too deep mixing” – Grammar

Pg. 21, ln. 13-16: Does this have anything to do with sea-ice?; some models can exhibit large polynas in the SO, with the result that mixing, and ventilation, can be extreme

Pg. 22, ln. 5-7: “We note that the Eppley-VGPM algorithm produces global PP estimates at about the mean value . . .” – This might well be true, but in my experience the spatial patterns of different estimates are wildly different, making the choice of such an "intermediate" product less clear

Pg. 22, ln. 12-14: “These large discrepancies are reduced in model version 1.2 . . .” – Is it possible (e.g. via run models that are not shown here) to be sure that the improvements stem from the BGC changes as opposed to the physics changes?

Pg. 23, ln. 5-6: “This missing PP on the shelves . . .” – You could make this clearer by calculating PP in open ocean areas only for VGPM and the model runs

Pg. 23, ln. 18-26: Is it possible to determine a map of nutrient limitations from the model?; it might help diagnose another reason for differences between them

Pg. 24, section 3.3: BGC tracers are the core of the model, while production is just one process within the model; I’d suggest swapping the sections around and making this 3.2; production could come just before export - which is arguably more natural anyway

Pg. 24, section 3.3: Why no chlorophyll?; is this because the fixed chl:C ratio here causes problems?

Pg. 25, ln. 16: “Moreover, nitrogen fixation, . . .” – A map of this perhaps?

Pg. 25, ln. 17: “. . .which occurs in the surface ocean as soon as [NO3] < RN:P[PO4], . . .” – Does this mean that all N2-fixation occurs in the right place?; cf. ostensible temperature limits, etc.

Pg. 26, ln. 2-6: What do the distributions of biogenic opal and CaCO3 export look like?; and how do they compare to observationally-derived estimates (e.g. in total)?

Pg. 27, ln. 15: While iron isn’t quite at the stage of having a global observational climatology, Geotraces has some fields that might help; and even in the absence of an observational comparison, it might be helpful to compare the models to elucidate differences; iron’s reach is longer than simply total iron concentration

Pg. 28, ln. 22: “. . .results in increased CaCO3 production and a considerable reduction of the alkalinity and DIC biases . . .” – Can this be squared with any observational evidence?; it can certainly be squared with model evidence (cf. Kwiatkowski et al., 2014;

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here, a number of models have low $CaCO_3$ production in the tropics and excessive alkalinity and DIC)

Pg. 30, ln. 25: "…the GLODAP data base" – The Khatiwala et al. (2013) estimate of anthropogenic CO_2 is probably a better estimate

Pg. 31, ln. 16: As mentioned previously, having a figure that illustrates how each of these schemes remineralises organic matter down the water column would also be helpful (or a plot of how the OM is attenuated)

Pg. 31, ln. 28: "At the end of the spin-up runs … " –How do these fit with the long spin-ups already done?; also, are these long enough to approach equilibrium?; or is the assumption that they are long enough for only transient drift to remain?

Pg. 33-34: I don't know the answer myself, so it's perhaps cheeky to ask, but could the authors comment on whether direct POC flux measurements or indirect AOU (or other tracer) measurements better constrain export and remineralisation; there may be no good answer at the moment, so the authors' use of both is probably best

Pg. 35, ln. 16: A more general comment – some studies (e.g. Kwon et al., 2009; Kriest, Oschlies & Khatiwala, 2012) examine the tuning of such models of export, whereas the manuscript uses them "as is"; while the authors do mention alternative sinking velocities for STD-fast at one point, they could help here by drawing further attention to this and / or commenting on the tuning of such models (e.g. if they have any unreported experience on the success or otherwise of this)

Pg. 36, ln. 10: So, paradoxically, excessively high and excessively low O_2?

Pg. 37, ln. 18: "Part of this problem is the distribution of primary production which is too high in a narrow band along the upwelling …" – Is this in any way related to the model being an isopycnal model?

Pg. 38, ln. 1: "In the Southern Ocean …" – Since this paragraph deals with the model as it is, rather than - per the preceding paragraph - the model as it might become, it should precede the paragraph on shelf improvements

Pg. 38, ln. 9: It's a wholly personal preference, but I think papers finish better with a short, bulletpointed list of the main points / findings

Pg. 38, ln. 9: A general criticism I'd make of the manuscript's validation of the model is that the performance of the model is not properly put within the context of similar models; the CMIP5 archive, for instance, offers a range of similar resolution models that could profitably provide such context; most of which aren't isopycnal models

Table 1: "kmol m−3" – At the risk of both being a pedant and missing the wood for the trees, presumably kmol / m3 is being used here because it is equivalent to molar units (i.e. mol / l); if so, why not just use mol / l?

Table 1: "Laughing gas" – While - appropriately enough - I laughed when I saw this, I don't think it can be called this in the final manuscript; nitrous oxide, perhaps?

Table 2: "Fraction of grazing egested 1−ezoo" – This is a little bit confusing; is the symbol really "1 - ezoo"?; why not "ezoo" and give it a value of 0.2 or 0.1?

Figure 1: Sometimes putting the y-axis on a logarithmic scale is helpful for showing what's happening near-surface

Figure 1: Since the colour map includes white, these panels could do with having the seafloor drawn on; that would help separate places that have zero difference from those that are rock; also, this would help clarify the bathymetry differences between different model grids

Figure 3: What happens with AMOC during the long spin-ups?

Figure 3: "109 kg s-1" – Convert to Sverdrups?; or is this awkward for an isopycnal model?

Figure 3: Observational data from RAPID appropriate for comparison?

Figure 4: “The range given for the observation based estimates is solely due to different criteria used to define MLD and not due to other uncertainties” – Per a previous comment, how does the MLD method used for the models compare to that of the observations?; also, given how variable different MLD methods can be from one another, reporting uncertainty in this way here seems potentially risky

Figure 5: Again, a log-scale y-axis might help here; most of the structure here is in the upper water column

Figure 5: Rotate panel e so that its y-axis is aligned with the x-axes on the panels to the left?; i.e. 90S to the left, 90N to the right

Figure 6: It’s a weakness on my part, but I prefer my plots to omit unnecessary grid lines (and have coastlines if possible)

Figure 6: Rotation would make panel e easier to understand (though I appreciate it would not then be aligned as in Figure 5)

Figure 6: Rather than only use VGPM, you could average VGPM with other estimates; it’s a poor way of simplifying the diversity in observational estimates of PP, but it can be useful given their spread, and it’s not without precedent

Figure 7: Why 40 in one hemisphere and 60 in the other?

Figure 7: Again, why just use VGPM?

Figure 8: In panel c, including the Indian Ocean is complicated by the presence of the monsoon

Figure 9: Rotate panel e again please

Figure 10: Fewer colours in the colour maps here (especially for the delta plots) might make it easier to discern patterns in match-mismatch (e.g. the reds are quite homogeneous)

Figure 12: These panels hint at some odd ventilation feature that elevates N. Pac. silicic acid (which then bleeds into the Arctic); does the same appear in CFC-11?

Figure 13: Add a key if possible; also, different symbols might help with the plot (especially for colour blind readers)

Figure 13: Amend to “Panel (d) shows results for the 500m depth level with error prone grid points located in the tropics (between 20 and 20S) omitted from the analysis”?

Figure 14: Why not go entirely east-west in the Pacific here?; since oxygen is particularly low in the East Pacific, this could be important

Figure 16: Nicely improved!

Figure 17: Uncertainty from Takahashi (and / or other pCO2 products)?

Figure 18: There’s quite a bit of a dip in the observationally-derived fluxes; is its origin explained in the main text?

Figure 19: “averaged over the years 1990 to 1998” – Why not a full decade?; being unreasonably suspicious, such odd ranges always raise my eyebrows

Figure 20: The mismatched sizes of bars shown in panel b seem a bad idea; log scale again?

Figure 21: Why zero in the Arctic of panels a, c, e and g?; does the main text say?

Figure 21: A global integrated profile plot of a, c, e and g might be helpful

Figure 22: Bigger dots?; also, a key would be nice; perhaps a little map that shows the regions in the appropriate colour?

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